# It Was Already Dobbs:
# Abortion Bans and Maternal Health

*By* Gabrielle Sorresso*

Cornell University, Brooks School of Public Policy

Last updated: November 1, 2026

Click here for the latest version

Clinicians warned that abortion bans enabled by *Dobbs* would harm maternal health, but ban states had restricted abortion for over a decade, which may limit the marginal effect of a full ban. Using insurance claims covering nearly 1 million pregnancies and a stacked difference-in-differences design, I find that, on average, the bans caused minimal change in maternal health or care. Fertility rose modestly and the pregnant population's health changed little. Results imply that restoring the federal right to abortion alone may not meaningfully change maternal health in these states.

* gns36@cornell.edu. This research was supported by the Irving Louis Horowitz Award from the Horowitz Foundation for Social Policy. I am grateful to Colleen Carey, Amanda Agan, Nick Sanders, and Doug Miller for helpful comments; and to attendees of the PIERS seminar at Cornell for additional feedback.

Between September 2021 and August 2023, fifteen U.S. states banned abortion at six weeks of gestation or earlier (Curhan, 2026). These laws were enabled by the Supreme Court's June 2022 decision in *Dobbs v. Jackson Women's Health Organization*, which overturned the constitutional right to abortion recognized in *Roe v. Wade*. The decision was the largest change to abortion law in half a century, what Dench, Pineda-Torres and Myers (2024) call "the most profound transformation of the landscape of abortion access in 50 years." As the bans took effect, clinicians, medical societies, and journalists warned that the harm would reach beyond women seeking an elective abortion for reasons unrelated to their health (American College of Obstetricians and Gynecologists, 2022; Harris, 2022; Arey et al., 2022; MacDonald, Gershengorn and Ashana, 2022; Simmons-Duffin, 2022).

Three features of abortion care and of the bans drove this concern. First, the medications and procedures used to end a pregnancy are also the standard treatment for miscarriage, ectopic pregnancy, and several severe complications of pregnancy, so their provision could be mistaken for an abortion. Second, the line between an "elective" and a "medically necessary" abortion is often unclear. For instance, a pregnancy with a severe fetal anomaly may pose no imminent risk to the woman when the anomaly is diagnosed but can become riskier as it continues (Nambiar et al., 2025). Third, although the bans include exceptions for the woman's life and sometimes her health (Felix, Sobel and Salganicoff, 2024), they say little about how a physician should act in these ambiguous cases, and bans threaten physicians who violate them with severe penalties (Felix, Sobel and Salganicoff, 2025). Investigative reporting has since documented women with serious pregnancy complications sent home from emergency departments until their condition deteriorated enough to meet an exception (Grossman et al., 2023; Seitz, 2024; Presser, 2024). The prevailing expectation was therefore that the bans would mark a sharp break in the care pregnant women received and in their health.

The abortion bans enabled by *Dobbs* followed a decade of tightening restrictions on abortion in the same states that would ultimately go on to ban the procedure. By the start of 2022, many states that would later ban abortion already imposed waiting periods and regulations targeting abortion providers that closed clinics and lengthened travel distances (Guttmacher Institute, 2022, 2021). In 2021 alone, states enacted 108 new restrictions, more than in any earlier year since *Roe* (Nash, 2021), and some of these rules already carried criminal penalties for physicians. Further, insurance had stopped paying for most abortions long before any formal ban. The Hyde Amendment has barred federal Medicaid funds from paying for abortion in most circumstances since 1977 (Salganicoff et al., 2024), and many of these states also limited coverage in private plans and in plans sold on the Affordable Care Act Marketplaces, the health insurance exchanges (Thompson, 2019; Ranji

et al., 2025). Even before *Dobbs*, the abortions that women in ban states did obtain were largely paid for outside insurance (Roberts et al., 2014), and they increasingly took place out of state or by telehealth (Guttmacher Institute, 2023; Society of Family Planning, 2025). In these states, access to abortion likely already resembled a post-*Dobbs* environment well before *Dobbs*.

How the largest change in abortion law in half a century changed the maternal health landscape is therefore an empirical question of great interest and significance. This paper provides the first comprehensive evidence on this question by estimating the effect of abortion bans on maternal health, the timing and intensity of care, and pregnancy outcomes. From the 350 million insurance claim lines of the women in my core sample, I construct pregnancy episodes that follow each woman from a year before conception through the postpartum period and record the diagnoses, procedures, and providers for services billed to her insurance. My core sample follows the nearly 1 million pregnancies of women continuously insured from 2020 through 2024. Using a stacked difference-in-differences design, I compare pregnancies in ban states with pregnancies in states that continuously protected abortion, before and after each state imposed its ban. I study three core sets of outcomes. First, maternal health outcomes, which include a composite of adverse pregnancy outcomes, severe maternal morbidity, hypertensive disorders of pregnancy, and preterm birth, together test the warnings of harm directly. Second, timing and intensity outcomes, which include the days to the first visit, whether a delivery stay ran past the federal minimum, the length of pregnancy, and the number of visits, would register delayed or deferred care before it reached health. Lastly, outcomes that describe the ending of a pregnancy, the shares ending in a live birth, a miscarriage, or an abortion billed to insurance, show whether the bans changed how pregnancies ended. Finally, to understand why these outcomes moved as they did, I examine whether the bans changed fertility and who became pregnant.

Additionally, these data allow me to document the pre-ban landscape in the states that subsequently banned abortion. Before any formal ban, I show that insurance-billed abortion was already extremely rare, ending about 1 percent of ban-state pregnancies compared to 14 percent of protected-state pregnancies in 2020. Because so little abortion was billed to insurance, the claims can show only a small fall in insured abortion in these states, a limit the health and care outcomes do not share, because they are measured for every insured pregnancy. Correspondingly, a live birth ended a far larger share of ban-state pregnancies. Furthermore, ban-state pregnancies had worse health outcomes even before *Dobbs*, and more often carried a hypertensive disorder or an adverse pregnancy outcome such as infection or gestational diabetes, and, even among live births, more often ended preterm. These descriptive patterns are, to my knowledge, new facts about pregnancy

care and outcomes in the years leading up to *Dobbs*. In part, this may be because the sources that commonly measure pregnancy outcomes, such as birth certificates and abortion surveillance records, do not follow a pregnancy from its start to its ending.

My stacked difference-in-differences estimates show that, on average, the bans caused little change in the health of pregnant women and the care they received. The only health outcome that rises by a margin approaching significance is a composite of common adverse pregnancy outcomes, such as infection, preterm birth, and gestational diabetes, which is suggestively 3.7 percent higher ($p = 0.06$), but its 95 percent confidence interval rules out increases larger than 7.6 percent. Among live births it changes by 0.8 percent, so most of that rise reflects more pregnancies carried to delivery rather than worse outcomes for each. The most common complications moved even less. Hypertensive disorders change by $-0.1$ percent, with an interval that rules out increases larger than 5.0 percent, and preterm birth among live births changes by 0.4 percent, with an interval that rules out increases larger than 9.2 percent. Changes in severe maternal morbidity, a rare but serious outcome at the center of the warnings, are also functionally null, at 1.7 percent, although because it is rare, the interval rules out only increases larger than 15.4 percent. The timing and intensity of care also changed little. The days to the first visit changed by 1.5 percent, with an interval that rules out delays longer than 5.9 percent, and delivery stays longer than the federal minimum became 5.0 percent less common. The share of pregnancies ending in a live birth rose 1.7 percent and insured abortion fell relative to protected states, but the raw series show both to be largely continuations of earlier trends, so they should not be read as causal effects. These patterns are stable across subgroups, despite differences in the levels of pregnancy and health outcomes. They describe the average insured pregnancy, most of which are routine pregnancies carried to delivery or lost early, and the design is not built to detect harm concentrated in the small group of pregnancies whose complications call for urgent care or a termination, which I leave to future work.

Because a pooled estimate could hide harm among few women, I also study pregnancies with a severe fetal anomaly, early severe preeclampsia, or an ectopic pregnancy. The share of pregnancies in these groups does not change detectably relative to the quarter before the ban, although the severe anomaly share is higher against earlier quarters. Within these groups I do not detect a rise in severe maternal morbidity, although the intervals are wide and transfusion rises suggestively in early severe preeclampsia (Section V).

Turning to the channels through which the bans could have acted, I find suggestive evidence that births rose. The birth rate among enrolled women is 3.4 percent higher relative to protected states ($p = 0.09$), with positive estimates in every subgroup, though only the under-25 estimate is

significant. However, despite this potential rise in fertility, the women who became pregnant after the bans were not measurably less healthy, as the share entering pregnancy with a chronic condition changed by −1.1 percent, with an interval that rules out increases larger than 0.5 percent.

I contribute to three active streams of literature. The first studies how abortion restrictions shape maternal health. Studies of the legalization of abortion around the time of *Roe* find that it lowered maternal mortality, by 30 to 50 percent among nonwhite women (Farin, Hoehn-Velasco and Pesko, 2024), and studies of later restrictions find that they worsened maternal health. Regulations targeting abortion providers raised hypertensive disorders of pregnancy (Gardner, 2024), sepsis and critical illness rose among hospitalized pregnancies in Texas after its 2021 ban (Presser et al., 2025; Chen et al., 2026), and blood transfusions for pregnancy loss rose after its 2022 total ban (Nagle et al., 2025). Women denied a wanted abortion also report worse physical health and more financial distress in the years that follow (Ralph et al., 2019; Miller, Wherry and Foster, 2023). In Mexico, by contrast, harsher penalties in states where abortion was already illegal had small or null effects on women's health (Clarke and Mühlrad, 2021). Early evaluations of *Dobbs* find that pregnancy-associated mortality may have increased, although the estimates have wide intervals (Bell et al., 2026; Abraha et al., 2026; Stevenson and Root, 2024; Nuss et al., 2025), and that infant deaths rose, largely from congenital anomalies (Gemmill et al., 2024; Singh and Gallo, 2024; Gemmill et al., 2025) and most among Black infants in Texas (Caraher, 2024). I extend this literature to a broad set of health outcomes and pregnancy endings, measured in detailed claims that describe care from conception through the postpartum period. To my knowledge, these are among the first estimates of the effect of the post-*Dobbs* bans on a broad range of maternal health outcomes in a population-level sample. However, in contrast to some of this work, I find that the bans enabled by *Dobbs* did not greatly change maternal health outcomes, a difference potentially explained by my broader sample and my focus on the later, *Dobbs*-era restrictions.

The second literature measures the fertility response to abortion restrictions. It generally finds that restrictions raise births modestly, and that the bans after *Dobbs* raised them by about 2 percent (Fischer, Royer and White, 2018; Venator and Fletcher, 2021; Myers, 2024; Dench, Myers and Pineda-Torres, 2025; Bell, Stuart and Gemmill, 2023; Caraher, 2024; Dench, Pineda-Torres and Myers, 2024; Bell et al., 2025). Restrictions short of a ban had already lowered abortion rates, most of all those aimed at the women seeking care rather than at providers (Caraher, 2026). In my population, which, unlike the birth records these studies use, excludes uninsured women and women whose coverage changed between 2020 and 2024, I find modest and suggestive evidence of a rise in births that partly continues earlier trends. Because claims also record pregnancies that do not end

in a birth, I can measure the pregnancy rate, which vital statistics cannot, and separate pregnancies carried to term from added conceptions. Claims also record a woman's health before conception, which natality records often do not. I can test whether the bans changed who became pregnant, as a ban can (Pop-Eleches, 2006), and hold pre-pregnancy health fixed in a decomposition.

Finally, I contribute to a literature on how physicians and patients respond to the regulation of health care. Care changed on some margins after *Dobbs*. Among the commercially insured, the management of miscarriage shifted from medication toward waiting (Rodriguez, Fuerst and Schrote, 2026), and the use of permanent contraception rose after the decision (Xu et al., 2024; Ellison, Brown-Podgorski and Morgan, 2024). Across all pregnancies, I find that the timing and intensity of women's care changed little.

I confront several threats to the validity of my design. First, protected states may not be the ideal comparison group, because they expanded access to abortion over the same years that ban states continued to restrict it, potentially leading to the observed increase in insurance-billed abortion. To address this difficulty, I reweight protected states to match the pre-ban paths of ban states with synthetic difference-in-differences (Supplemental Appendix A10), and in Supplemental Appendix A11 I compare women near and far from protected-state care within ban states, which removes protected states from the comparison entirely. Neither points toward harm to maternal health. Second, women enter and leave my data for a variety of reasons, some of which may be correlated with treatment. Therefore, my analysis sample includes only women continuously enrolled from 2020 through 2024, which ensures a stable denominator and lets me follow women before and after pregnancy (Section II.D). However, this requirement may select women better able to circumvent a ban, for example by traveling, which would lead the estimates to understate rather than overstate the effect of the bans. To check the effect of this restriction, in Supplemental Appendix A5 I use a weaker enrollment requirement and find similar results. I further show that the births in my sample broadly resemble all U.S. births on a variety of demographic measures (Supplemental Appendix Table A7). Finally, by raising fertility, the bans may change who becomes pregnant, toward women at higher or lower baseline risk. Because claims record each woman's health before conception, I measure this directly and hold it fixed in a reweighting decomposition, and I find little evidence that a change in composition masks harm (Section VI.B).

My results place the effect of the *Dobbs* decision on maternal health in the ongoing environment of restrictions that preceded it. Together, my descriptive results and difference-in-differences estimates suggest that maternal health in ban states was already worse than in protected states before the *Dobbs* bans and changed little after. These findings point to the restrictions that came before the

bans. For example, Gardner (2024) finds that targeted regulations of abortion providers, which most later ban states had adopted, raised hypertensive disorders of pregnancy by 11 to 16 percent, while the 95 percent confidence interval for the effect of the bans on the same outcome runs from −5.1 to 5.0 percent. However, my estimates do not rule out harm. Because they average over every insured pregnancy, they cannot exclude severe harm concentrated among the few percent of pregnancies with the complications the bans made hardest to treat, but they bound how common such harm could have been, despite a prevailing narrative that harm was likely. Finally, these findings carry a clear policy lesson: returning the federal right to abortion alone will not meaningfully change the maternal health and care landscape for women.

The paper proceeds as follows. Section I describes how abortion restrictions could affect maternal health and the history of restrictions before and after *Dobbs*. Section II describes the data, samples, and outcomes, and Section III the empirical strategy. Section IV presents the effects on maternal health, the timing and intensity of care, and pregnancy endings, and Section VI the channels through which the bans could act. Section VII concludes.

## I. Background and Expected Effects

### *A. How Abortion Restrictions Could Affect Maternal Health*

Restricting access to abortion, and leaving unclear when exceptions apply, can have potentially wide-reaching effects on maternal health during pregnancy. An abortion early in pregnancy is induced either with medication, mifepristone followed by misoprostol, or by a procedure that empties the uterus, such as uterine aspiration (American College of Obstetricians and Gynecologists and Society of Family Planning, 2020). However, the medications and procedures that make up an abortion are also the standard of care for several conditions unrelated to elective abortion. For example, an ectopic pregnancy, which implants outside the uterus and cannot survive, is treated with methotrexate or surgery (American College of Obstetricians and Gynecologists, 2018*a*). An incomplete, retained, or septic miscarriage is completed with dilation and curettage or with the same medications used for abortion (American College of Obstetricians and Gynecologists, 2018*b*). In previable rupture of membranes, preeclampsia with severe features, a hypertensive disorder, or intra-amniotic infection, ending the pregnancy is sometimes the only treatment that prevents sepsis, hemorrhage, or organ failure (Arey et al., 2023). Additionally, terminations for a lethal fetal anomaly, including ones that may risk the mother's health if continued, use the same procedures and are ambiguously elective (Grossman and Chasen, 2020).

Further, a pregnancy that does not end in a live birth is common, and its management is routine

obstetric care, so many women encounter the procedures a ban restricts. In 2019, it is estimated that 66.9 percent of U.S. pregnancies ended in a live birth, 13.1 percent in an induced abortion, and 20.0 percent in a miscarriage, ectopic pregnancy, or stillbirth (Rossen et al., 2023). Further, even before any formalized abortion ban, 12 percent of women obtaining an abortion cited concerns about their own health and 13 percent possible problems with the health of the fetus (Finer et al., 2005). Therefore, restrictions on abortion potentially have a large scope to impact pregnant women's experiences in health care.

One potential channel through which these restrictions could harm maternal health is delayed management of complications. Because the procedures overlap, a physician unsure whether an exception applies may wait until a miscarriage becomes septic or an ectopic pregnancy ruptures (Sabbath et al., 2024; Arey et al., 2023). Clinicians practicing in environments where abortion is restricted describe waiting to intervene in ruptured membranes, infection, and severe preeclampsia until the fetus has no cardiac activity or the patient has deteriorated enough to meet a narrow reading of the exception (Sabbath et al., 2024; Arey et al., 2023; Lift Louisiana et al., 2024). Waiting can be costly. In Texas hospital discharge records, sepsis among women hospitalized with a second-trimester loss rose from 2.9 to 4.5 percent after Senate Bill 8 (SB8) (Presser et al., 2025), and reporting describes ectopic pregnancies that ruptured before treatment (Seitz, 2024).

A second way restrictions on abortion can harm maternal health is through reduced access to terminations for medical reasons, which can follow the diagnosis of a fetal anomaly or maternal condition, usually later in pregnancy than medication abortion can address (Grossman and Chasen, 2020). A woman who cannot end such a pregnancy carries it longer, and carrying it is not without risk. Among patients at one Texas center with a life-limiting fetal condition, maternal morbidity among those managed expectantly doubled, from 35 percent in 2017 and 2018 to 72 percent in 2022 and 2023, after the state's abortion bans left most of them to expectant management, against 15 percent among patients who terminated before the bans (Nambiar et al., 2025).

A separate channel for harm is the timing of care. Some practices declined to see pregnant patients before 12 weeks to avoid managing early losses (Lift Louisiana et al., 2024). If practices stop seeing women early in pregnancy, care could deteriorate for every pregnancy.

Further, abortion restrictions may change average maternal health solely through who is pregnant, as I test in Section VI.B. If restrictions prevent women who would have ended a pregnancy from ending it, and these women are on average at higher baseline risk than women already carrying pregnancies, average outcomes would worsen even if no woman's care changed. If instead they are at lower risk, average outcomes could improve and hide a deterioration in care.

### B. *Abortion Policy Before and After Dobbs*

The history of abortion restrictions in states that enacted post-*Dobbs* abortion bans began long before *Dobbs*, and the restrictions grew tighter in the years leading up to the decision. By the start of 2022, every one of the fifteen states that would later ban abortion already imposed a gestational limit on abortion and required a parent's consent or notice before a minor could obtain an abortion, and all but Tennessee made women wait between counseling and the procedure (Guttmacher Institute, 2022). Eleven states enforced targeted regulations of abortion providers, rules that go beyond what patient safety requires (Guttmacher Institute, 2021). Studies of these rules find that, together, they closed clinics and lengthened the distances women traveled to reach a provider (Lindo et al., 2020; Smith et al., 2022; Lindo and Pineda-Torres, 2021; Myers and Ladd, 2020). Supplemental Appendix A1 describes these restrictions and the legal history of the bans in more detail.

Insurance coverage was minimal in states that ultimately banned abortion as well. Beyond the federal Hyde Amendment's limits on Medicaid (Salganicoff et al., 2024), none of the fifteen states with post-*Dobbs* bans used their own funds to pay for most medically necessary abortions for women on Medicaid (Guttmacher Institute, 2022), and fourteen of the fifteen also restricted abortion coverage in plans sold on the health insurance exchanges (Supplemental Appendix A1). As a result, insurance pays for only a minority of abortions in the United States (Roberts et al., 2014). Coverage also matters for access, because cost has been shown to be a barrier to receiving an abortion. Previous work has found that state restrictions on Medicaid funding reduced abortions, with the response concentrated among low-income women (Levine, Trainor and Zimmerman, 1996), and a review of this literature concludes that about one in four women who would have had a Medicaid-funded abortion give birth when that funding is unavailable (Henshaw et al., 2009).

In addition, several of these states had already passed near-total or early gestational bans, only to see them enjoined under *Roe*, so the legal status of abortion in these states shifted from year to year, making the practice environment already potentially unstable and stress-inducing. For example, Georgia's six-week LIFE Act passed in 2019 and stayed blocked until the Eleventh Circuit allowed it to take effect weeks after *Dobbs* (*SisterSong v. Governor of Georgia*, 2022), and similar bans in Arkansas, Kentucky, and Ohio were blocked by courts before or shortly after they took effect (Nash et al., 2019; DeMillo, 2021).

Against this unsettled background, Texas became the first state to enforce an early abortion ban under *Roe*. Texas SB8 took effect on September 1, 2021, nearly ten months before *Dobbs*, and banned abortion after about six weeks of gestation. Because private lawsuits rather than state officials enforced the law, there was no official a federal court could enjoin to stop it. The law

nonetheless stood on uncertain legal footing. When the Supreme Court declined to block it when it took effect, the Court noted "serious questions regarding the constitutionality of the Texas law" and stated that its order was "not based on any conclusion about the constitutionality" of the law (*Whole Woman's Health v. Jackson*, 2021). After *Dobbs*, Texas's ban came to rest on the decision itself, because the state's trigger law, which banned nearly all abortions once *Roe* was overturned, took effect on August 25, 2022 (Supplemental Appendix A2 gives the legal sources).[1]

Then, in June 2022, the Supreme Court held in *Dobbs v. Jackson Women's Health Organization* that the Constitution confers no right to abortion. Trigger laws and previously enjoined bans then took effect in eleven states (Alabama, Arkansas, Georgia, Idaho, Kentucky, Louisiana, Mississippi, Missouri, Oklahoma, South Dakota, and Tennessee) between June and August 2022 (Nash and Guarnieri, 2022). West Virginia passed a total ban in September 2022, and Indiana and South Carolina enforced bans in August 2023 after litigation (Curhan, 2026).

Every ban came with potential health exceptions, but their reach was unclear. Each preserves an exception to prevent the death of the pregnant woman, and some also for a serious risk to her health (Felix, Sobel and Salganicoff, 2024). However, physicians describe the language of these exceptions as too vague to apply at the bedside (Sabbath et al., 2024; Landay et al., 2026), and guessing wrong can mean license revocation, civil liability, or prison for providers (Felix, Sobel and Salganicoff, 2025). More than 60 percent of obstetrician-gynecologists in ban states report concern about their own legal risk when deciding whether an abortion is medically necessary (Frederiksen et al., 2023), and qualitative work documents clinical decisions that departed from the prior standard of care (Sabbath et al., 2024). However, this uncertainty did not begin with *Dobbs*. Long before the decision, ethics committees at Catholic-owned hospitals denied approval to evacuate the uterus of a miscarrying patient while fetal heart tones were present, forcing physicians to delay care or transfer the patient elsewhere (Freedman, Landy and Steinauer, 2008). In Texas, clinicians interviewed before *Dobbs*, some of them before SB8, described narrow hospital criteria for abortion that limited care for patients whose health was at risk (Arey et al., 2023).

## II. Data

### *A. Insurance Claims*

To study the effects of these bans on maternal health, care, and pregnancy outcomes, I use Inovalon closed claims real-world data, which are comprised of post-adjudication medical and pharmacy claims received directly from more than 160 primary payer sources, including national, regional,

[1]Given this background, I treat SB8 as a ban enabled by *Dobbs*, but one enacted earlier.

and local health plans across all 50 states, the District of Columbia, and Puerto Rico. The dataset includes more than 246 million covered lives over a 10-year lookback across commercial, Medicare Advantage, and Medicaid lines of business. Patient identity is resolved before de-identification using a conservative deterministic matching approach, producing a persistent identifier that follows each individual across health plans and over time. Only birth year, gender, and three-digit ZIP code carry into the de-identified dataset. Data are de-identified under the HIPAA Expert Determination standard (45 CFR §164.514(b)(1)). Available elements include claim type, place of service, service and admission and discharge dates, diagnosis and procedure codes, DRG, HCPCS, NDC, proxy cost data, enrollment and continuous enrollment, insurance type, ACA indicators, and a provider's National Provider Identifier (NPI), taxonomy, specialty, and facility type.

Within these data, the claims file records each woman's health care, and the enrollment file records her demographics, state of residence, and payer each month. I also merge in the National Plan and Provider Enumeration System (NPPES), the federal NPI registry, which gives each provider's practice address by year (Centers for Medicare & Medicaid Services, 2026).

These data provide unusually rich detail on the health care of millions of insured women. In my analysis sample, I record the services billed to a woman's insurance from a year before conception through 42 days after her pregnancy ends, so I can follow a pregnancy and its outcomes. One feature of these data is that they are a convenience sample, meaning that insurers contribute data under contracts with the data provider that begin and end over time. Because the data carry no plan identifier, a woman who leaves her plan cannot be distinguished from one whose plan stopped contracting with the data provider. Additionally, because insurance plans can be regional, such as Medicaid managed care plans, which each state contracts for separately (Medicaid and CHIP Payment and Access Commission, 2019), an insurer's entry or exit can add or remove a whole population and potentially shift the risk profile of a state's members. I address this by restricting my analysis sample to women continuously enrolled from 2020 through 2024 (Section II.D).

From these files I construct two analysis samples: the pregnancy sample, the core of the analysis, with one observation per pregnancy episode of a continuously enrolled woman, and a panel of continuously enrolled women aged 15 to 45 by state and quarter, which gives state pregnancy and birth rates (Section VI.A).

### *B. Ban and Protected States*

Thirteen states form three treated cohorts in my analysis: Texas; the eleven states that banned abortion directly after *Dobbs*; and West Virginia. The comparison group is comprised of 24 states

and the District of Columbia where abortion remained legal throughout the period, to viability or later (Curhan, 2026). I exclude from my analysis eleven states whose bans were enjoined, partial, or changing during the window; these states do not enter the analysis at all, and the protected states enter only as never-treated controls. I also exclude Indiana and South Carolina, which banned abortion in August 2023, from the pregnancy estimates, because the data end in December 2024, before a complete post-ban window of their pregnancies can be followed to its ending. Supplemental Appendix Table A2 further describes the rationale behind the choice of treatment, control, and excluded states, and Table A1 lists each state with its effective date and statute.

### *C. Constructing Pregnancy Episodes*

I build pregnancy episodes following validated claims-based algorithms (Ailes et al., 2016; Matcho et al., 2018; Moll et al., 2021). I begin by using pregnancy-specific codes to identify each pregnancy and date its start and its ending.[2] I then gather the woman's complete claim history around these dates. Each episode has three windows. The pre-pregnancy window covers the 365 days before the pregnancy start and supplies the measures of health before pregnancy, the pregnancy window runs from the start to the ending, and the postpartum window runs from the day after the ending through 42 days. This process captures all of the insured care a woman receives during pregnancy, not just pregnancy-specific care, which matters because some care is billed with no code indicating pregnancy. For example, sepsis treated in an emergency department may be billed with only the presenting condition, which outcomes measured only on pregnancy-coded claims would miss.

#### Endings

I identify each ending from diagnosis, procedure, and diagnosis-related-group codes for a live birth, stillbirth, molar pregnancy, ectopic pregnancy, induced abortion, or spontaneous abortion, and where an ending carries codes for more than one type, I apply the hierarchy that Moll et al. (2021) validated against physician-adjudicated records. Because the abortion codes include the billing codes for mifepristone and misoprostol, which also treat miscarriage, the classification errs toward counting too many abortions rather than too few. Supplemental Appendix A3 describes how I link ending codes and exclude endings that imply a pregnancy too close to the previous one.

[2]The pregnancy and ending codes draw on the Maternal and Infant Health code lists that the Centers for Medicare & Medicaid Services publish for identifying pregnant and postpartum beneficiaries (Centers for Medicare & Medicaid Services, 2023), with the ending hierarchy of Moll et al. (2021). Supplemental Appendix Table A21 lists the codes by outcome.

### Pregnancy start

I date each pregnancy from the first day of the estimated last menstrual period (American College of Obstetricians and Gynecologists, 2017), and call this date the pregnancy start. Most pregnancies are dated from Z3A diagnosis codes, which record the gestational week at a visit, by a validated algorithm with a median absolute error of four days (Chomistek et al., 2023). Dating is least accurate for early losses. Supplemental Appendix A3 gives the full procedure and the share of pregnancies dated by each source.

### Pregnancies without an observed ending

Some pregnancies carry pregnancy care but no ending code. In my main analysis sample, they are 6.2 percent of ban-state pregnancies conceived in the six quarters before each state's ban (Section II.D). I build these episodes from clusters of pregnancy-indicating claims, such as prenatal visits and gestational-age codes, that fall outside every other observed pregnancy for a woman, as Sarayani et al. (2020) do for pregnancies with unknown outcomes, and end each on the date of its last such claim (Supplemental Appendix A3).

I keep pregnancies without an observed ending rather than excluding them, because their share is itself informative. For a woman who is insured throughout, a pregnancy with prenatal care and no billed ending is most plausibly one that ended in care insurance did not pay for, such as an abortion paid out of pocket, obtained out of state, or obtained by mail, or a loss that received no care. I exclude them from the outcomes that require an observed ending, namely the live-birth, miscarriage, and insurance-billed abortion shares, preterm birth, the length of pregnancy, and the days to the first visit.

## *D. Analysis Samples*

The core analysis sample, the *continuous-enrollment sample*, requires a woman to be continuously enrolled from January 2020 through December 2024 to appear in the pregnancy data. This requirement lets me observe, for nearly every pregnancy, the full year preceding it and the full postpartum period.[3] Holding the same women in the data throughout also keeps the sample's membership from shifting with other changes in who appears in the claims over the years the bans took effect, such as the end of the pandemic-era Medicaid continuous-enrollment provision, which had kept enrollees covered without annual eligibility checks from March 2020 until March 31, 2023 (Tolbert and Ammula, 2023).

[3]Supplemental Appendix A4 reports how often the pre-pregnancy year is fully observed for pregnancies that start in 2020.

While the continuous-enrollment sample keeps a constant group of women at risk of pregnancy, each observed in detail, it is not without trade-offs. First, it retains only 19 percent of the pregnancies that pass every other restriction when no enrollment requirement is imposed (995,377 of 5,325,806). Second, it may select certain kinds of women. For example, women with stable coverage of either kind are plausibly better able than most to travel around a ban, and nationally, continuous coverage is more common among higher-income and married women (Daw et al., 2017). Despite this, the births in my sample closely resemble all U.S. births on a number of demographic features (Supplemental Appendix Table A7). For example, 22.7 percent of sample pregnancies are to women under 25, against 21.1 percent of U.S. births, and 15.1 percent are to Black women, against 13.7 percent. The sample also includes more Medicaid-covered pregnancies than the nation does, as Medicaid covers 50.7 percent of sample pregnancies against 41.5 percent of U.S. births (Osterman et al., 2025), which, if the bans harmed Medicaid-covered women more, would push the estimates toward larger effects rather than smaller ones.

In addition, my enrollment requirement excludes, by construction, every woman who died before December 2024, because a woman who dies loses her insurance enrollment.[4] Pregnancies that ended in or led to a maternal death, the most severe outcomes, are therefore missing from the main sample, which tilts it slightly toward healthier pregnancies. If the bans raised maternal deaths, the excluded deaths would come disproportionately from ban states after the bans, and the estimates for severe maternal morbidity would understate the true effect. However, maternal death, at 17.9 per 100,000 live births in 2024 (Hoyert, 2026), is roughly a hundred times rarer than severe maternal morbidity, which affects about 2.5 percent of deliveries once hospitalizations during pregnancy and in the year after delivery are counted (Boghossian et al., 2024), so the understatement is small.

### Additional restrictions

I keep pregnancies in the 15 ban states and 25 protected jurisdictions that started from January 2020 through February 2024, so that a typical 40-week pregnancy can be followed to its ending, and I exclude observed endings with an implied gestation shorter than 28 or longer than 315 days, which likely reflect coding or dating errors. The continuous-enrollment sample contains 995,377 pregnancies, and Supplemental Appendix A4 reports counts after each step.

[4]The data also do not let me identify deaths directly, because the data provider withholds them to protect privacy.

### Demographics, health, and treatment

A woman's demographics for each pregnancy come from the enrollment file in the month the pregnancy started. The regressions control for age and its square;[5] race and ethnicity, in six mutually exclusive groups (Hispanic of any race, and non-Hispanic White, Black, Asian, other or multiple races, and missing);[6] Medicaid coverage; indicators for a first observed pregnancy and a multiple gestation; and a pre-pregnancy comorbidity score. I measure this score from available claims in the 365 days before the pregnancy start, using the conditions of the obstetric comorbidity index of Leonard et al. (2020), a validated predictor of severe maternal morbidity. Supplemental Appendix A4 lists the conditions and how they are weighted. One limitation of this measure is that a condition appears in the claims only if the woman sought care for it, so women who rarely see a doctor may appear healthier than they are. However, every woman in my sample is insured throughout the year before conception, and the index was developed and validated on administrative hospital records that share this limitation (Leonard et al., 2020).

I assign treatment by the state of residence in the enrollment file rather than by where care was delivered, because claims locate care only through the registered address of the provider's NPI (Supplemental Appendix A4). The enrollment file records one state per woman, so a woman who moved from a ban state to a protected state would be counted as untreated, which would bias the estimates toward zero. To address this in part, the within-ban-state comparison in Supplemental Appendix A11 assigns location from the providers a woman saw before conception.

Several of these demographic and health characteristics define the subgroups in Section IV.D. Medicaid-covered pregnancies are those with a Medicaid payer in the month the pregnancy started, minority women are those with a Black race code or a Hispanic ethnicity code, and high-risk women are those with at least one condition in the obstetric comorbidity index, about 42 percent of ban-state pregnancies before the bans.

## *E. Outcomes*

Each channel in Section I.A predicts a pattern of maternal harm from abortion restrictions that may be observable in health care claims. I therefore define twelve outcomes across three groups that capture the parts of pregnancy which abortion bans are most likely to affect. Supplemental Appendix Table A21 provides code-level definitions and sources for each.

[5] Age enters as a quadratic because the risk of severe maternal morbidity rises nonlinearly with maternal age, low from the early twenties to the mid-thirties and rising steeply after about 39 (Lisonkova et al., 2017).

[6] Race and ethnicity are imputed by the data provider using a proprietary algorithm and can be unknown.

### Maternal health

*Severe maternal morbidity* measures the life-threatening complications at the center of the warnings, such as sepsis and hemorrhage, and would rise if delayed management let a miscarriage become septic or an ectopic pregnancy rupture. To define it, I use the Centers for Disease Control and Prevention's definition (Centers for Disease Control and Prevention, 2024), as specified in ICD-10 diagnosis codes by Leonard et al. (2020, Appendix 1), and count a pregnancy as having severe maternal morbidity if any of its 16 diagnosis indicators, among them sepsis, shock, eclampsia, and acute renal failure, appears on any claim from the pregnancy start through 42 days after the ending.[7] Because severe morbidity is rare, affecting about 2.6 percent of ban-state pregnancies before the bans, I also measure a composite of more common *adverse pregnancy outcomes*, adapted from Crump, Sundquist and Sundquist (2024), which captures the harms of a pregnancy carried longer or managed later. If terminations for medical reasons became harder to obtain, pregnancies that would have ended in a managed termination would instead end in a complicated birth or a loss, and a delayed miscarriage can become infected. It equals one if a pregnancy has a diagnosis of infection or sepsis, preterm birth, gestational diabetes, poor fetal growth, or placental abruption.

I also study two of the most common serious complications of pregnancy. *Hypertensive disorders of pregnancy* are the most common, affecting 21 percent of ban-state pregnancies before the bans. They are disorders of high blood pressure that arise in pregnancy, among them gestational hypertension and preeclampsia, and in their severe forms they can damage the kidneys, liver, and brain. Delivery or termination is the treatment for these severe forms, so delayed management could let them progress, and they are the outcome earlier restrictions on abortion providers affected (Gardner, 2024), so they give the sharpest comparison with prior work.

Finally, I measure *preterm birth* only among live births. Because the mix of pregnancy endings may itself be changing due to the bans, this outcome isolates harm to pregnancies carried to delivery, where lost access to terminations for medical reasons may show up, as a pregnancy with a severe anomaly or complication that would have been ended is instead carried until an early delivery.

### Timing and intensity of care

The second group of outcomes asks whether the timing and intensity of care changed, which is where delayed management or deferred early care would appear first, before or without a change in health. The *days to the first visit*, the log number of days from the pregnancy start to the woman's first claim of any kind during the pregnancy, tests whether practices deferred early care, as some

[7]Supplemental Appendix A13 explains why I omit the definition's five procedure indicators.

practices in ban states said they would to avoid managing early losses (Lift Louisiana et al., 2024). A *prolonged delivery stay*, a live birth whose delivery admission ran past the federal minimum stay of two days after a vaginal delivery and four after a cesarean (42 U.S.C. 300gg-25), tests for delayed management of complications, which would lengthen hospital stays at delivery. The log *length of pregnancy* and the log *number of visits* describe how long a pregnancy lasted and how much care it received. However, I expect these two to rise mechanically when fewer pregnancies end early, so I read them alongside the endings rather than as measures of the quality of care.[8]

#### Pregnancy endings

The third group of outcomes records how each pregnancy ended. A ban that prevented terminations should change how pregnancies end, with fewer insurance-billed abortions and more pregnancies ending in a live birth, a miscarriage, or an unknown ending.[9] I study four outcomes, whether a pregnancy ended in a *live birth*, a *miscarriage*, or an *insurance-billed abortion*, an induced abortion identified from any diagnosis, procedure, or medication code for one, and whether its *ending was not observed* (Section II.C). The first three are shares of pregnancies with an observed ending and the fourth is a share of all pregnancies, so the four do not sum to one. Insurance-billed abortion is the most direct measure of the policy for insured women, although it captures only a fraction of all abortions. As Section II.C explains, this measure errs toward counting too many abortions rather than too few.

### *F. The Pre-Ban Landscape*

Table 1 reports means in the continuous-enrollment sample for ban and protected states before and after the bans; the protected-state post-period begins with pregnancies that started in or after June 2022. Before the bans, ban-state mothers were younger than protected-state mothers (29.0 against 30.4 years), and the racial and ethnic composition of insured pregnancies differed between the two groups of states. Medicaid covered 42.4 percent of ban-state pregnancies before the bans and 53.4 percent in protected states, and ban-state women entered pregnancy with a higher mean comorbidity score, 8.7 against 8.4. Supplemental Appendix Table A6 reports normalized differences for these characteristics in the pre-period (Imbens and Wooldridge, 2009).

In addition to these baseline differences in characteristics, the pregnancies themselves differed, and the gaps persist in the rows of Table 1 adjusted for characteristics. First, the difference in how

[8]These outcomes enter in logs because their distributions have a long right tail, which in levels would let a few extreme pregnancies drive the estimates. Counts are winsorized at the 99th percentile.

[9]Ectopic, stillbirth, and molar endings, 2.5, 0.9, and 0.1 percent of observed endings, are too rare to estimate.

pregnancies ended is striking. Before the bans, insurance-billed abortion ended about 1 percent of ban-state and 14 percent of protected-state pregnancies with an observed ending in 2020, and a live birth ended a correspondingly larger share of ban-state pregnancies. Some of this gap likely reflects ban-state pregnancies that ended in an abortion paid outside insurance, which never appear in the claims, and some likely reflects differences in the insured populations the data cover in each state.

Second, ban-state pregnancies fared worse, particularly in the states that banned at *Dobbs* rather than in Texas (Figure 1). They more often involved an adverse pregnancy outcome and a hypertensive disorder. Part of this gap perhaps follows from more ban-state pregnancies being carried to term, which exposes more of them to the complications of later pregnancy, but even among live births, and adjusted for characteristics, preterm birth was more common in ban states before the bans. Finally, ban-state pregnancies lasted longer, with about as many visits and prolonged delivery stays, while the first visit came at about the same point in both groups of states.

Figure 1 shows the raw series for the twelve outcomes over calendar time for Texas, the states that banned at *Dobbs*, and protected states. The gaps were largely stable, and in some cases widening, before the bans. For maternal health and for the timing and intensity of care, the ban-state and protected-state series move roughly in parallel, apart from the length of pregnancy and the number of visits. However, there are clear divergences in the pregnancy endings. The ban-state series for insured abortion and live birth are nearly flat, while in protected states the live-birth share fell and insured abortion rose, as several states widened protections and coverage (Supplemental Appendix A2) and access to abortion by telehealth expanded after the COVID-19 pandemic (Society of Family Planning, 2025). No maternal health or care series shows a sharp break at either ban, and Section IV tests whether the smaller movements in the adverse-outcome composite and preterm birth exceed the changes in protected states.

## III. Empirical Strategy

In this section I describe my core empirical design, which compares pregnancies in ban states with pregnancies in protected states, before and after each ban. The unit of observation is a pregnancy $p$ in state $s$ conceived in quarter $q$. Because the bans took effect at different times, I estimate the stacked difference-in-differences design of Wing, Freedman and Hollingsworth (2024), which formalizes the stacked event studies of Cengiz et al. (2019) and Deshpande and Li (2019).[10] For each adoption quarter $a$, the first fully treated quarter of one or more ban states (the fourth

[10]Stacking compares each treated cohort only with never-treated states, which avoids the comparisons with already-treated states that bias two-way fixed effects estimates when treatment is staggered and effects vary over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak, Jaravel and Spiess, 2024). Supplemental Appendix Figure A4 compares the stacked event studies with two-way fixed effects.

quarter of 2021 for Texas, the third quarter of 2022 for the *Dobbs* cohort, and the fourth quarter of 2022 for West Virginia),[11] I build a sub-experiment containing those states and, as controls, the protected states. A ban state never serves as a control. Event time is $e_{pa} = q - a$, with a window of $e_{pa} \in [-6, 4]$. A sub-experiment enters the stack only if its full window lies inside the data and it has at least one clean control, so every event-time coefficient is estimated on the same cohorts. Supplemental Appendix A9 gives each cohort's window, and Supplemental Appendix Table A15 the pregnancies in each sub-experiment. I then estimate

$$Y_{psqa} \;=\; \sum_{\substack{h=-6 \\ h\neq -1}}^{4} \delta_h \cdot \mathbf{1}\{e_{pa} = h\} \cdot D_{sa} \;+\; \sum_{h} \lambda_h \cdot \mathbf{1}\{e_{pa} = h\} \;+\; X_p'\gamma \;+\; \phi_{sa} \;+\; \varepsilon_{psqa},$$

where $D_{sa}$ indicates the treated states of sub-experiment $a$, $\lambda_h$ are event-time effects shared by treated and control states, $\phi_{sa}$ are sub-experiment by state fixed effects, and $X_p$ contains the individual controls described in Section II.D. The reference period is $h = -1$. Control observations in sub-experiment $a$ are weighted by $(N_a^D/N^D)/(N_a^C/N^C)$, the sub-experiment's share of treated observations divided by its share of control observations; without these weights a stacked regression does not identify a convex combination of cohort-specific effects, and with them $\delta_h$ is the trimmed aggregate average treatment effect on the treated at event time $h$ (Wing, Freedman and Hollingsworth, 2024). For the pooled estimate I replace the post-period interactions with one indicator $D_{sa} \cdot \mathbf{1}\{e_{pa} \geq 0\}$, on the same stacked sample and with the same weights, so the pooled and event-study estimates use the same pregnancies. Standard errors are clustered by state, as the same control states appear in several sub-experiments. I report two specifications. The Raw specification omits $X_p$. The Controls specification is the equation above, and it is my preferred specification: it measures the effect of a ban among observably similar women.

Event time is measured in quarters, because conception dates are much less accurate for early losses and quarters estimate each coefficient on more pregnancies (Supplemental Appendix A3).

A causal reading of these estimates requires that, absent the bans, outcomes in each treated cohort would have followed the path of its controls. Several features of the setting strain this assumption. The first is that for some outcomes the trends clearly do not move together before the bans. This is most true for the pregnancy endings, whose protected-state series diverge from the ban-state series well before the bans (Section II.F), consistent with the argument that it was

[11] The third quarter of 2022 is only partly treated for Georgia, Idaho, and Mississippi, whose bans took effect in July and August, and for Kentucky and Louisiana, where state courts blocked enforcement for much of July, so the estimate at $h = 0$ may be attenuated for these five states. The later event-study coefficients, in fully treated quarters, show whether an effect appears once the bans were fully in force.

already *Dobbs* in ban states. I therefore read the event studies for the endings as mainly descriptive evidence of ongoing patterns. However, the maternal health outcomes and most of the timing and intensity outcomes move together before the bans, apart from the length of pregnancy and the number of visits, which rise with the share of pregnancies carried to term, so I read their estimates around the ban dates as more causal in nature.

An additional strain is on the stable unit treatment value assumption, which requires that the bans not change outcomes in the comparison states. Protected states absorbed travel from ban states and expanded access to abortion after *Dobbs*. Expanded access would bias the estimates toward finding harm and crowding would bias them toward zero, and the within-ban-state comparison in Supplemental Appendix A11 avoids protected states. Synthetic difference-in-differences (Arkhangelsky et al., 2021) reweights protected states and pre-ban quarters to match ban states' pre-ban paths, in case protected states are a poor comparison (Supplemental Appendix A10).

## IV. The Effects of the Bans

I report pooled estimates from the Controls specification in the continuous-enrollment sample, each scaled by the pre-ban mean, with the quarterly event studies (Figure 2). Table 2 also reports the Raw specification and the $p$-value of a joint test that the pre-period coefficients are zero.

### *A. Maternal Health*

The only suggestion of harm in the maternal health estimates is in the composite of common adverse pregnancy outcomes. The estimate is 0.011 (standard error 0.005), or 3.7 percent of the pre-ban mean, marginally but not conventionally significant ($p = 0.06$), and 4.1 percent ($p = 0.03$) in the Raw specification. Its event study is flat before the bans and positive in every quarter after (Figure 2). However, the 95 percent confidence interval rules out increases larger than 7.6 percent, so any rise in these complications was likely small. Against quarter $-2$, the estimate falls to 1.8 percent (Supplemental Appendix Table A16). Of its five components, only poor fetal growth rises, by 7.1 percent ($p = 0.02$), a diagnosis made only in pregnancies that continue, while infection or sepsis, the complication delayed management would produce, falls 11.8 percent, although its pre-period coefficients jointly differ from zero ($p = 0.03$; Supplemental Appendix Table A11).

Severe maternal morbidity, a much rarer outcome, is measured less precisely but is still a null. It changes by 4.6 per 10,000 pregnancies (standard error 17.9), or 1.7 percent of a pre-ban rate of 264.5 per 10,000, and the 95 percent confidence interval rules out increases larger than 15.4 percent. Its post-period coefficients scatter around zero. Hypertensive disorders of pregnancy, the

most common serious complication and a well-studied outcome, show a flat null. The estimate is −0.1 percent ($p = 0.98$), and the 95 percent confidence interval rules out increases larger than 5.0 percent, below the 11 to 16 percent rise that earlier restrictions on abortion providers produced (Gardner, 2024). Hypertensive disorders rose in both groups of states over the period (Figure 1), and the bans added little to that trend in ban states relative to protected states. Preterm birth among live births changes by 0.4 percent, with an interval from −8.3 to 9.2 percent, which likewise bounds harm through a common adverse outcome among live births.

### *B. Timing and Intensity of Care*

The raw series for the timing and intensity of care show no clear break at either ban (Figure 1), and where the gaps with protected states widen, they widen gradually, mostly because the protected-state series fall. The days to the first visit, which would lengthen if practices deferred early care, change by 1.5 percent ($p = 0.50$), with pre-period coefficients that do not jointly differ from zero (joint $p = 0.12$), and the interval rules out delays longer than 5.9 percent.

Hospital care at delivery did not lengthen. The share of live births whose delivery stay ran past the federal minimum falls 5.0 percent ($p = 0.03$; 95 percent confidence interval −9.6 to −0.4 percent), but its pre-period coefficients jointly differ from zero ($p = 0.03$), so I do not read the decline as an effect of the bans. It gives no sign that complications were managed later.

The length of pregnancy rises 1.7 percent ($p = 0.06$), as expected when fewer pregnancies end early, and the number of visits is 1.6 percent higher ($p = 0.25$; 95 percent confidence interval −1.2 to 4.5 percent). Both climb toward the reference quarter before the bans as live births do (Section IV.C; joint $p$ <0.001 for the length of pregnancy and <0.001 for visits), so, as anticipated in Section II.E, I read them as part of the shift in endings rather than as evidence about the quality of care. None points to a change in how physicians managed pregnancies on average.

### *C. Pregnancy Endings*

Because the pregnancy endings move before the bans, these results are largely descriptive. If the bans prevented terminations, the most direct place to see it is abortion billed to insurance, although it is an imperfect proxy for the abortions a ban prevents. However, my raw data and event studies show no fall in ban states. The pooled estimate is −0.013 (standard error 0.003), or −123.6 percent of the pre-ban mean of 1.06 percent, a magnitude larger than the mean that is possible only because the estimate is measured against protected states. The raw series show that the protected-state side accounts for most of this (Figure 1). In ban states, an insurance-billed abortion ended between

0.8 and 1.2 percent of pregnancies with an observed ending in every quarter from 2020 through 2023, while in protected states the share rose from 14 percent in 2020 to 17 percent in the quarter of the decision and 20 percent by the end of 2023. The event study traces the same divergence and rejects parallel pre-trends ($p$ <0.001).

Additionally, if the bans moved terminations outside the insurance system, to other states or to medication by mail, those pregnancies may appear as a higher share of pregnancies with care but no recorded ending, because in the continuous-enrollment sample every woman is insured and enrolled throughout. In Texas this share was 4.3 percent among pregnancies conceived from 2020 through the first quarter of 2021, rose to 8.4 percent among pregnancies conceived in the three quarters around SB8, and returned to 4.4 percent afterward (Figure 1). Over the same quarters it moved from 4.7 to 5.2 percent in protected states and from 6.2 to 7.0 percent in the states that banned at *Dobbs*, so part of the Texas rise is shared with states whose laws had not yet changed. No comparable rise follows *Dobbs*, perhaps because by mid-2022 women who meant to end a pregnancy could do so before any prenatal care was billed, or after a single visit, which forms no episode, increasingly through telehealth (Society of Family Planning, 2025).

The share of pregnancies with an observed ending that ended in a live birth rises 1.7 percent ($p = 0.03$; 95 percent confidence interval 0.2 to 3.2 percent), while the miscarriage share, which would rise if pregnancies that would have been terminated instead ended in a loss, changes by 1.0 percent, with an interval from −5.1 to 7.0 percent. The live-birth estimate is weaker than its $p$-value suggests, because its event study sits 2 percent of the pre-ban mean below the reference quarter from six to four quarters before the bans and level with it from three quarters before (joint $p$ <0.001), and without Texas the estimate falls to 0.9 percent ($p = 0.48$). I therefore read the rise in live births as suggestive. This share describes how pregnancies ended, not how many women gave birth, so Section VI.A turns to birth and pregnancy rates among all enrolled women.

### *D. Heterogeneity Analysis*

A small average effect can hide large effects in particular groups, so I define five subgroups whose effects have reason to differ from the overall average. I first look at Medicaid-covered pregnancies, because women with low incomes may be the least able to travel for abortion or pregnancy care or to pay for an abortion out of pocket (Jerman, Jones and Onda, 2016; Hill et al., 2024). I next look at minority women, Black and Hispanic women reported together, whose outcomes were worse before the bans. Black women in the United States die of maternal causes at about three times the rate of White women (Hoyert, 2026) and experience severe maternal morbidity at nearly twice

the rate (Leonard et al., 2019). I then look directly at women who enter pregnancy with a high-risk condition, who are the most likely to need potentially lifesaving care, such as a termination for medical reasons, that the bans may have called into question. The opposite concern is that the bans led many young, healthy women to carry pregnancies to term, which could mask worse outcomes for everyone else, so I look at pregnancies to women under 25. Finally, I exclude Texas, whose ban came first, was enforced through private lawsuits, and was in place longest, to ensure the results are consistent in its absence.

For each group, I rerun the stacked design and report the percent change over the group's own pre-ban mean, so each estimate is the change for that group rather than its contribution to the pooled effect (Figure 3 and Supplemental Appendix Table A9). The estimates vary little across these groups. For each of the twelve pregnancy outcomes, every subgroup's 95 percent confidence interval contains the pooled point estimate. Two estimates stand out, both among high-risk women. Severe maternal morbidity rises 12.1 percent ($p = 0.04$; 95 percent confidence interval 0.5 to 23.7 percent), and the adverse-outcome composite rises 4.1 percent ($p = 0.04$). They are the only two of 20 subgroup estimates for maternal health whose intervals exclude zero, close to the one expected by chance at 5 percent, but they fall in the group of most concern, so I read them as suggestive.

Because the bans changed how pregnancies ended, I also estimate the health and care outcomes within live births and within losses (miscarriage, stillbirth, and ectopic pregnancy; Supplemental Appendix Table A10). These estimates condition on an ending the bans changed, so they describe the pooled estimates rather than isolate effects of their own. Within live births, the adverse-outcome composite changes by 0.8 percent (95 percent confidence interval $-2.0$ to 3.6 percent), the length of pregnancy by $-0.2$ percent, and the number of visits by $-1.6$ percent, so the pooled rises in these outcomes mostly reflect more pregnancies carried to delivery. Losses, the small group the warnings most concerned, are measured imprecisely. Among them, severe maternal morbidity changes by 19.5 percent, with an interval that does not rule out a rise of 53.1 percent, and the first visit comes 2.6 percent later ($p = 0.07$).

## V. Complicated Pregnancies

The warnings concerned particular pregnancies, and a pooled estimate spreads any harm across every insured pregnancy in a ban state. I therefore follow three groups in which a ban could delay or withhold care: pregnancies with a severe fetal anomaly, where a ban ends terminations for medical reasons; early severe preeclampsia, where delivery may be the only treatment; and ectopic pregnancy, whose treatment bans generally exempt but which resembles abortion care. I identify

each group from its own diagnosis, and because a ban acts on a woman when she is diagnosed rather than when she conceives, I date each pregnancy by the quarter of that diagnosis (Supplemental Appendix A12). The severe anomaly group includes conditions that are not lethal. Within each group I estimate the design of Section III on four severe outcomes from the diagnosis through 42 days after the ending: severe maternal morbidity, transfusion, hysterectomy, and critical care.

These estimates are only as informative as the groups are comparable, and the bans could change how many pregnancies fall in each. More pregnancies could continue long enough to be diagnosed, or fewer could reach a diagnosis billed to insurance. Panel A of Table 3 therefore estimates the share of all pregnancies in each group, which does not condition on a diagnosis. The share changes by 0.33 percentage points for severe anomalies (95 percent confidence interval −0.19 to 0.86), 0.15 for early severe preeclampsia (−0.06 to 0.36), and −0.15 for ectopic pregnancies (−0.48 to 0.18). None of these intervals excludes zero, so I do not detect a change in the size of these groups, although the estimates within them still condition on a diagnosis the bans could have affected and should be read as describing the women diagnosed. The pre-trend test passes for severe anomalies ($p = 0.29$) and early severe preeclampsia ($p = 0.11$) and fails for ectopic pregnancies ($p = 0.002$). The severe anomaly share is the least settled. Its pre-period coefficients all sit below quarter −1, and women conceiving in quarter −1 reached the anatomy scan after the ban, so that quarter may be affected. Against the average of quarters −6 to −2, the share rises by 0.75 points.

Within these groups, severe maternal morbidity does not rise detectably (Panels B to D of Table 3). It changes by −1.1 percentage points for severe anomalies, with an interval that rules out increases larger than 0.5 points, by 0.2 for ectopic pregnancies, ruling out increases larger than 1.8 points, and by 2.9 for early severe preeclampsia, where the interval reaches 10.8 points, and all three pass the pre-trend test. Of the twelve estimates, four have intervals that exclude zero, and three of these fail the pre-trend test: transfusion (2.0 points) and hysterectomy (0.6 points) for severe anomalies, and critical care for ectopic pregnancies (3.6 points). Figure 4 shows that their pre-period coefficients are already positive, so a low quarter −1 accounts for much of each estimate. The fourth, transfusion in early severe preeclampsia, rises 4.2 points (95 percent confidence interval 0.7 to 7.8; pre-trend $p = 0.35$). Taken together, I do not detect a rise in the most severe outcomes among these pregnancies, but they are few, their intervals are wide, and the data withhold deaths.

## VI. Understanding the Channels

Beyond the outcomes in Section IV, the bans could also change how many pregnancies were carried to term and who became pregnant. I show below that each of these channels is muted,

consistent with the small effects above.

### *A. Fertility*

A ban that prevented abortions, insured or not, should raise births, as earlier restrictions to abortion access do (Fischer, Royer and White, 2018; Venator and Fletcher, 2021). Conceptions may also fall if women use more contraception after a ban (Xu et al., 2024; Ellison, Brown-Podgorski and Morgan, 2024), so the net effect on births is ambiguous. I test this prediction by estimating the stacked design on the panel of enrolled women aged 15 to 45 by state and quarter, weighted by enrollment. The outcome is live births or pregnancies per 1,000 enrolled women per year, counted by the month the pregnancy started, and I report each estimate in percent of its pre-ban mean (Supplemental Appendix Table A12). The birth rate is 3.4 percent higher relative to protected states ($p = 0.09$; 95 percent confidence interval $-0.5$ to 7.3 percent), the same sign as the 2.3 percent increase that Dench, Pineda-Torres and Myers (2024) estimate from birth certificates. The raw rates in panels (c) and (d) of Figure 5 show where that gap comes from. Both the pregnancy rate and the birth rate fall over the period in both groups of states, as the women in a continuously enrolled panel age, and birth rates in ban and protected states move together through 2022. From 2023 on they separate, as the ban-state birth rate holds near its 2022 level while the protected-state rate keeps falling, so relative to protected states, births held up in ban states.

The pregnancy rate, which counts conceptions rather than births, changes by 0.1 percent ($p = 0.96$; 95 percent confidence interval $-4.2$ to 4.4 percent). Comparing the two rates shows where any added births came from. Together with the rise in the live-birth share of observed endings (Section IV.C), these estimates suggest that about as many women became pregnant after the bans but more of their pregnancies ended in a live birth. Across the subgroups in Supplemental Appendix Table A12, the birth-rate estimate is always positive and never differs much from the overall estimate.

### *B. Who Became Pregnant*

The bans may have changed who was pregnant if they changed how pregnancies ended (Section I.A). Because the claims record women's health before they conceive, I test this directly in two complementary steps. First, I ask whether the pregnant population changed, by estimating the stacked design with each pregnancy's pre-pregnancy characteristics as the outcome. Because a ban cannot change any one woman's health before conception, a nonzero estimate means that the set of women who became pregnant changed. Second, I ask whether any such change mattered for the pregnancy outcomes, with a reweighting decomposition that splits each outcome's change into

a part due to who became pregnant and a part due to care, holding baseline health fixed.

The pregnant population's health before pregnancy barely moved (Supplemental Appendix Table A13). After the bans, the comorbidity score changes by −1.9 percent ($p = 0.33$), the number of comorbid conditions by −0.9 percent, and the share with any condition by −1.1 percent ($p = 0.16$), and the last, whose pre-period is closest to flat, rules out increases larger than 0.5 percent. Each point estimate is negative, so if anything the women who became pregnant after the bans were slightly healthier. Their demographics changed little as well. The Medicaid share of pregnancies, the most direct test of whether the added pregnancies came from women with low incomes, changes by 0.2 percent ($p = 0.91$), with an interval from −3.2 to 3.5 percent, and the share of pregnancies to women under 25 rises by 6.0 percent ($p = 0.10$), a share that was already rising in ban states relative to protected states before the bans.

For the second step, I use a reweighting decomposition in the spirit of DiNardo, Fortin and Lemieux (1996), which reweights post-period pregnancies so that their pre-pregnancy characteristics match the pre-period and splits each change, differenced across ban and protected states, into a part due to composition and a part due to care conditional on baseline risk (Supplemental Appendix A8). Because it compares all pregnancies before the bans with all pregnancies after them, its totals differ from the stacked estimates, so I use it only to split each change into these two parts, and not to measure the size of either.

Composition accounts for roughly a quarter of the change in how pregnancies ended and in the length of pregnancy (Supplemental Appendix Table A14). For the health outcomes it is small and runs toward healthier pregnancies (Figure 6). For the adverse-outcome composite, composition moves the rate by −0.5 percent of the pre-ban treated mean (95 percent interval −3.4 to 2.3), while care conditional on baseline risk raises it by 4.7 percent (1.2 to 8.3). For severe maternal morbidity the composition component is −3.2 percent of the mean (−8.5 to 2.0), and it is negative for hypertensive disorders and preterm birth as well, so a change in who became pregnant would, if anything, mask rather than produce harm.

## VII. Discussion

The *Dobbs* decision ended a constitutional right for women that had stood for half a century, and many expected the bans that followed to harm maternal health broadly, by delaying the management of complications and ending access to terminations for medical reasons. However, two features of the pre-*Dobbs* environment, both documented in outside work, are consistent with effects this much smaller than predicted. First, abortion care in states that subsequently banned

abortion was already highly restricted, and those earlier restrictions had already caused harm, as restrictions on abortion providers raised hypertensive disorders of pregnancy (Gardner, 2024). Potentially, then, the marginal effect of the bans was small. Second, even as restrictions came rapidly and repeatedly in the years leading up to *Dobbs*, the national environment loosened in some ways, as telehealth provision of abortion expanded after the COVID-19 pandemic (Society of Family Planning, 2025). In line with this work, insurance-billed abortion was already rare in ban states before the bans and continued to be rare after them (Section IV.C). For insured women in ban states, abortion had left the insurance system before the bans.

After the bans, the abortions ban-state residents obtained took place elsewhere, at clinics across state lines, where the share of insured ban-state abortions obtained out of state rose from 27.4 to 53.4 percent (Odouard et al., 2024), and through telehealth providers mailing medication under shield laws, who provided nearly 15,000 abortions a month by June 2025 (Society of Family Planning, 2025). Against this background, I provide evidence that the bans changed less than the warnings assumed, likely because much of what a ban restricts had already been restricted.

How do these estimates compare with prior work? As Section IV.A notes, the bound on hypertensive disorders lies below the increase that targeted regulation of abortion providers produced (Gardner, 2024). That regulation raised travel distances to a provider, while the bans I study arrived in states that had restricted abortion for a decade, and one reading consistent with both results is that the earlier restrictions had already done in these states much of what a ban could do to maternal health.

To make the magnitudes of my estimates concrete, consider 10,000 insured pregnancies in a ban state. Before the bans, about 264 of them would have involved severe maternal morbidity. The point estimate adds about 4.6, and the upper end of the 95 percent interval allows about 41 more. About 2,080 of the same pregnancies would have had a hypertensive disorder, and the interval allows at most about 104 more. Even at the upper ends of these intervals, the added harm the data allow is modest next to how common these complications were in ban states before the bans.

Prior work found large increases in severe complications within narrow groups. The rise in sepsis among Texas women hospitalized with a second-trimester loss after SB8, described in Section I.A, is an increase of 55 percent (Presser et al., 2025), and among high-risk hospitalized pregnancies the law was associated with sustained increases in critical illness, sepsis, and infection, an association the same study does not detect across all hospitalized pregnancies (Chen et al., 2026). Because these groups are a small share of pregnancies, an effect of that size within them would move severe maternal morbidity among all insured pregnancies by far less than my bound. For the Texas rise in

sepsis, 1.6 percentage points among women hospitalized with a second-trimester loss, to add the 41 cases of severe maternal morbidity per 10,000 pregnancies at the upper end of my interval, about 25 percent of pregnancies would need to end in a second-trimester loss, more than the 16 percent of ban-state pregnancies with an observed ending that ended in a miscarriage before the bans.

These averages do not imply that the bans were harmless for every woman they reached. A pooled estimate spreads any harm across every insured pregnancy in a ban state. Spread instead over the 2.5 percent of pregnancy endings that are ectopic, or over the smaller share that end in a second-trimester loss, the same bound would allow the risk of severe morbidity in those groups to rise several times over. Estimated among losses directly, severe morbidity changes by 19.5 percent, with an interval that reaches 53.1 percent (Section IV.D). Estimated among pregnancies with a severe anomaly, early severe preeclampsia, or an ectopic pregnancy, its intervals reach 0.5, 10.8, and 1.8 percentage points (Section V). Because losses are a small share of pregnancies and carry a lower rate of severe morbidity than deliveries, a rise of that size among them would move the pooled rate by only a few percent, inside its interval. The deaths and near-deaths documented in reporting and clinician interviews (Presser, 2024; Seitz, 2024; Sabbath et al., 2024) can be consistent with the null aggregate estimates I provide. My estimates speak to how common such harm was across a wide population, not to whether it occurred at all.

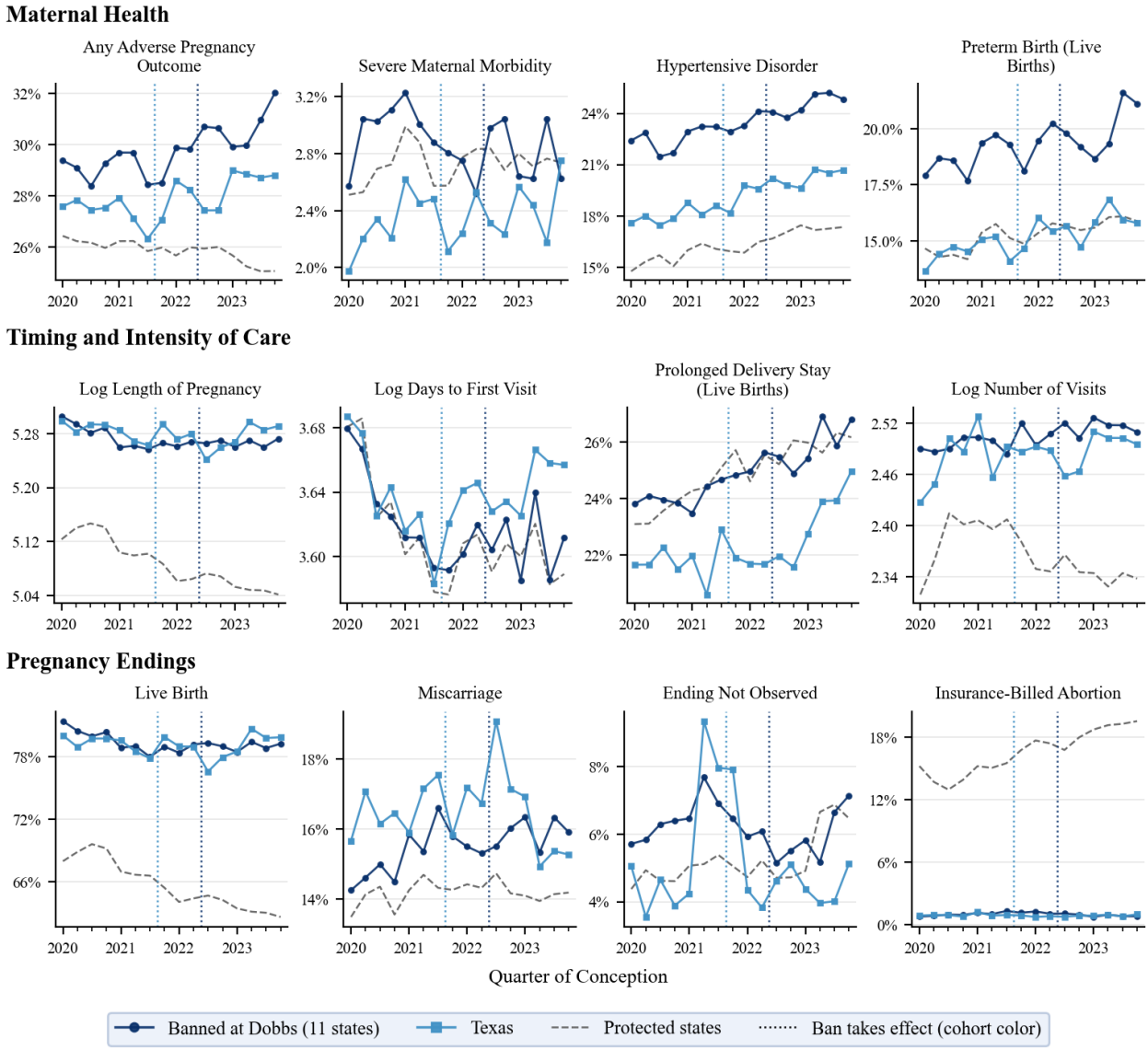
The results, taken in line with previous work, carry a stark lesson for policy makers and maternal health advocates. The debate over *Dobbs* has centered on the constitutional right to abortion, but for the women studied here, the restrictions that limited access came earlier. Restoring a federal right to abortion would lift the bans, but it would not by itself restore insurance coverage of abortion or the clinics those earlier restrictions closed. On its own, it would likely not meaningfully change the maternal health and care landscape for women in states that banned abortion.

Table 1—Characteristics of Pregnancies by State Policy and Period

| | Ban States | | Protected States | |
|---|---|---|---|---|
| | **Before** | **After** | **Before** | **After** |
| Pregnancies | 140,836 | 121,531 | 437,625 | 295,385 |
| **Demographics, % Unless Noted** | | | | |
| Age, mean (SD) | 29.01 (6.40) | 28.84 (6.82) | 30.35 (6.23) | 30.18 (6.57) |
| Under 25 | 26.7 | 31.0 | 19.3 | 22.6 |
| Medicaid | 42.4 | 37.1 | 53.4 | 56.3 |
| Commercial | 57.5 | 62.8 | 46.4 | 43.5 |
| White | 41.6 | 41.2 | 45.9 | 46.4 |
| Black | 19.2 | 16.2 | 14.0 | 14.4 |
| Asian | 1.9 | 1.8 | 3.9 | 3.7 |
| Hispanic | 8.7 | 14.0 | 23.8 | 26.5 |
| First observed pregnancy | 25.7 | 27.2 | 25.7 | 25.4 |
| **Health Before Pregnancy, % Unless Noted** | | | | |
| Hypertension, obesity, or substance use | 23.6 | 25.5 | 24.0 | 27.5 |
| Obstetric comorbidity score, mean (SD) | 8.74 (14.34) | 9.14 (14.78) | 8.37 (14.30) | 9.48 (15.27) |
| **Maternal Health, %** | | | | |
| Any adverse pregnancy outcome | 28.6 | 29.2 | 26.1 | 25.5 |
| *Adjusted for characteristics* | [29.4] | [30.1] | [25.7] | [25.1] |
| Severe maternal morbidity | 2.8 | 2.6 | 2.7 | 2.8 |
| *Adjusted for characteristics* | [2.6] | [2.4] | [2.4] | [2.4] |
| Hypertensive disorder | 21.3 | 22.1 | 15.7 | 17.2 |
| *Adjusted for characteristics* | [20.4] | [21.4] | [15.1] | [16.4] |
| Preterm birth (live births) | 17.3 | 17.6 | 14.9 | 15.9 |
| *Adjusted for characteristics* | [16.7] | [17.0] | [14.3] | [14.9] |
| **Timing and Intensity of Care, % Unless Noted** | | | | |
| Length of pregnancy, days, mean (SD) | 225.0 (83.6) | 224.2 (84.1) | 201.7 (96.8) | 193.9 (99.2) |
| *Adjusted for characteristics* | [226.7] | [226.6] | [204.3] | [198.3] |
| Days to first visit, mean (SD) | 46.0 (33.4) | 45.7 (33.1) | 45.3 (31.9) | 44.3 (31.1) |
| *Adjusted for characteristics* | [51.9] | [52.5] | [48.6] | [48.0] |
| Prolonged delivery stay (live births) | 23.3 | 24.2 | 24.3 | 25.9 |
| *Adjusted for characteristics* | [23.5] | [23.9] | [24.8] | [26.1] |
| Number of visits, mean (SD) | 16.2 (10.9) | 16.3 (10.8) | 16.3 (12.5) | 16.3 (13.1) |
| *Adjusted for characteristics* | [16.1] | [16.1] | [15.9] | [15.9] |
| **Pregnancy Endings, % of Observed Endings Unless Noted** | | | | |
| Live birth | 79.3 | 79.0 | 67.1 | 63.3 |
| *Adjusted for characteristics* | [80.0] | [80.0] | [68.2] | [65.4] |
| Miscarriage | 15.7 | 16.3 | 14.2 | 14.3 |
| *Adjusted for characteristics* | [14.1] | [14.2] | [12.8] | [12.6] |
| Ending not observed (percent of all pregnancies) | 6.4 | 5.7 | 4.9 | 5.8 |
| *Adjusted for characteristics* | [5.2] | [4.6] | [4.8] | [5.6] |
| Insurance-billed abortion | 1.2 | 0.9 | 15.2 | 18.6 |
| *Adjusted for characteristics* | [2.3] | [2.1] | [15.7] | [18.6] |

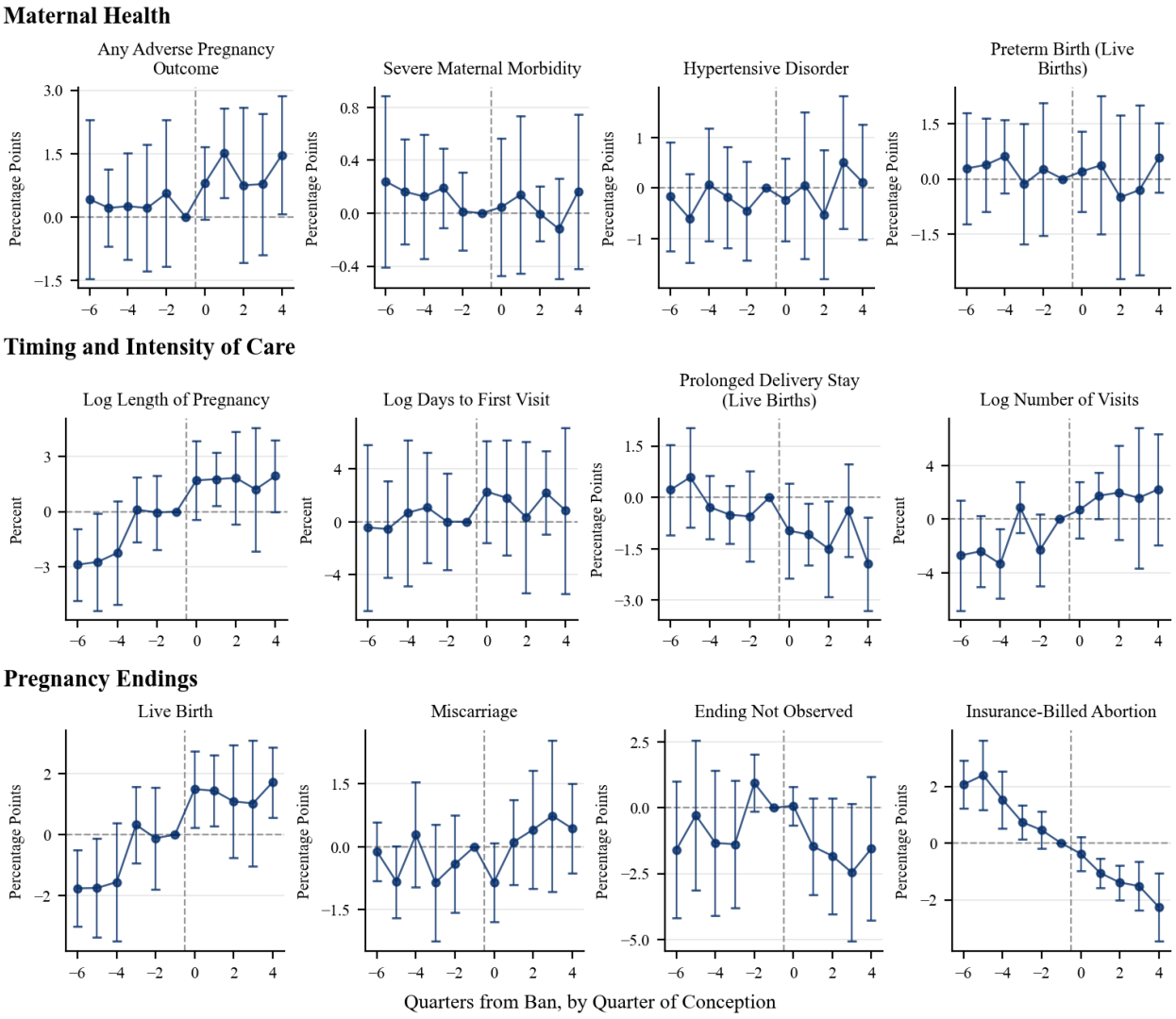
*Notes:* Pregnancies started January 2020 through February 2024 in the continuous-enrollment sample. Ban states enacted a strict abortion ban; protected states never did. Before and after refer to pregnancies that began before or after the ban took effect in the woman's state, and after begins in June 2022 for protected states. Shares are percentages; age, the obstetric comorbidity score, the length of pregnancy, days to first visit, and number of visits are means with standard deviations (SD) in parentheses. Preterm birth and a prolonged delivery stay, a delivery admission longer than the federal minimum of two days after a vaginal and four after a cesarean delivery (42 U.S.C. 300gg-25), are percentages of live births; live birth, miscarriage, and insurance-billed abortion are percentages of observed endings; ending not observed is a percentage of all pregnancies. Hypertension, obesity, or substance use is a diagnosis of any of the three in the year before pregnancy. Each bracketed row adjusts the outcome for the individual controls of the main specification (the residual from a regression on them, plus the overall mean), so gaps in that row are not due to those characteristics. Table A6 reports normalized differences.

Figure 1. Raw Means of the Pregnancy Outcomes by Quarter of Conception




*Notes:* Means by calendar quarter of conception in the continuous-enrollment sample for the eleven states that banned at *Dobbs* (circles; first treated in the third quarter of 2022), Texas (squares; fourth quarter of 2021), and protected states (dashed line). A dotted vertical line for each cohort falls between its last untreated and first treated quarter, the earlier one for Texas. West Virginia, whose quarterly counts are small, and Indiana and South Carolina, whose post-ban window runs past the data, are omitted (Supplemental Appendix Table A5). Live birth, miscarriage, and insurance-billed abortion are percentages of pregnancies with an observed ending, preterm birth and a prolonged delivery stay of live births, and the other shares of all pregnancies; the length of pregnancy, the days to the first visit, and the number of visits are in logs. Quarters after the fourth quarter of 2023 are omitted because their pregnancies cannot all be followed to their ending.

Figure 2. Event Studies for the Pregnancy Outcomes



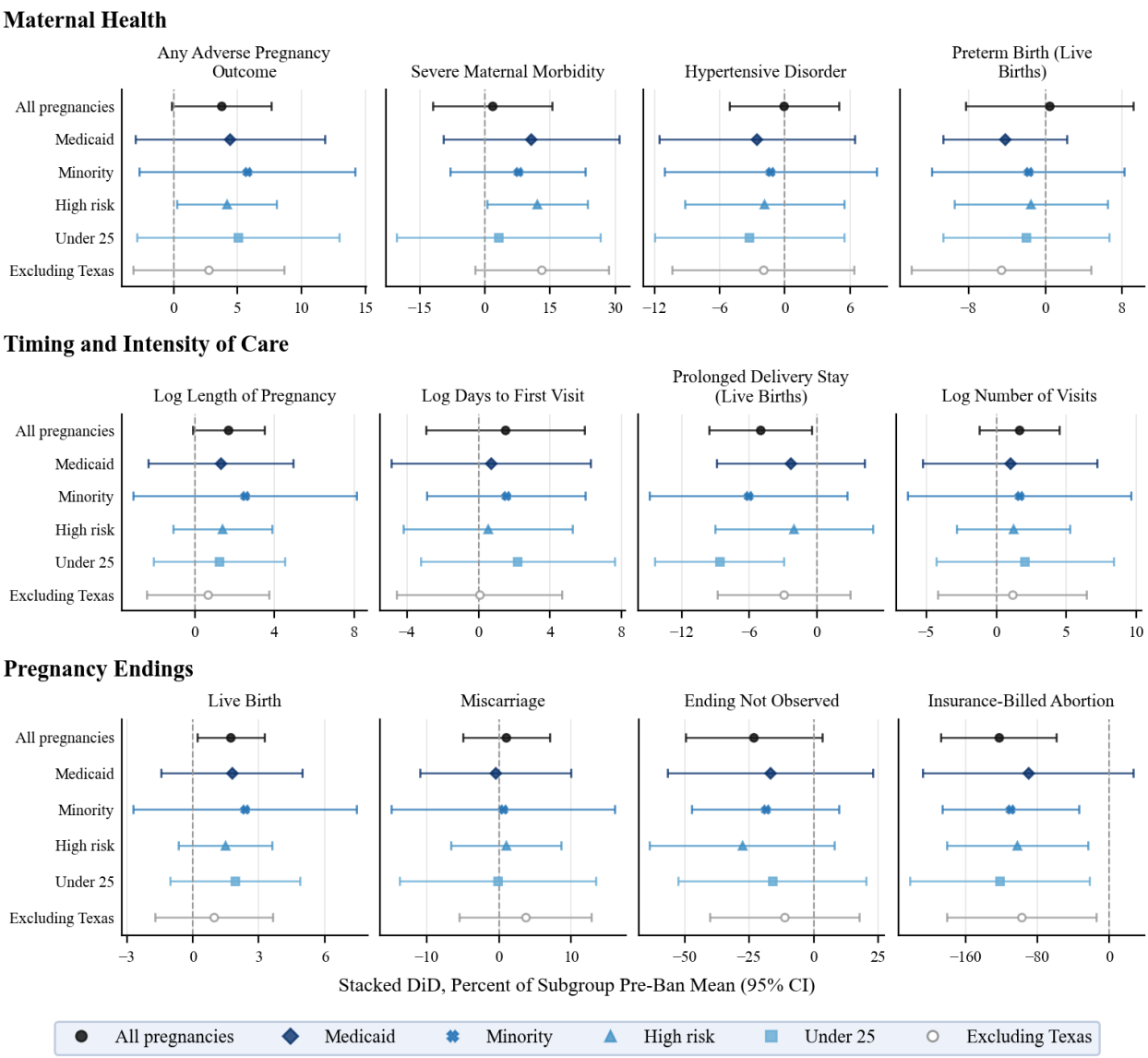

*Notes:* Stacked difference-in-differences event-study coefficients by quarter of conception relative to the ban, quarters −6 to 4, with 95 percent confidence intervals from standard errors clustered by state, in the Controls specification and the continuous-enrollment sample. Coefficients are in percentage points for shares and in percent (100 times the coefficient) for the outcomes in logs. Each ban cohort is compared only with states untreated through the end of its window, using the weights of Wing, Freedman and Hollingsworth (2024). Quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0. Table 2 reports pre-ban means and joint pre-trend tests.

Table 2—The Effect of Abortion Bans on the Pregnancy Outcomes

| Outcome | Pre-Ban Mean (1) | Controls | | | | Raw | |
|---|---|---|---|---|---|---|---|
| | | Estimate (SE) (2) | Percent of Mean (3) | 95% CI, Percent of Mean (4) | Pre-Trend $p$-Value (5) | Estimate (SE) (6) | Percent of Mean (7) |
| **Maternal Health** | | | | | | | |
| Any adverse pregnancy outcome | 0.285 | 0.011 (0.005) | 3.7 | [−0.2, 7.6] | 0.98 | 0.012 (0.005) | 4.1 |
| Severe maternal morbidity | 0.026 | 0.000 (0.002) | 1.7 | [−12.0, 15.4] | 0.86 | 0.000 (0.002) | 1.4 |
| Hypertensive disorder | 0.208 | 0.000 (0.005) | −0.1 | [−5.1, 5.0] | 0.48 | 0.000 (0.005) | 0.2 |
| Preterm birth (live births) | 0.171 | 0.001 (0.007) | 0.4 | [−8.3, 9.2] | 0.29 | 0.001 (0.007) | 0.4 |
| **Timing and Intensity of Care** | | | | | | | |
| Log length of pregnancy | 5.272 | 0.017 (0.009) | 1.7 | [−0.1, 3.5] | <0.001 | 0.019 (0.009) | 1.9 |
| Log days to first visit | 3.616 | 0.015 (0.022) | 1.5 | [−2.9, 5.9] | 0.12 | 0.015 (0.022) | 1.5 |
| Prolonged delivery stay (live births) | 0.233 | −0.012 (0.005) | −5.0 | [−9.6, −0.4] | 0.03 | −0.009 (0.005) | −3.9 |
| Log number of visits | 2.496 | 0.016 (0.014) | 1.6 | [−1.2, 4.5] | <0.001 | 0.021 (0.015) | 2.1 |
| **Pregnancy Endings** | | | | | | | |
| Live birth | 0.789 | 0.014 (0.006) | 1.7 | [0.2, 3.2] | <0.001 | 0.015 (0.006) | 1.9 |
| Miscarriage | 0.162 | 0.002 (0.005) | 1.0 | [−5.1, 7.0] | <0.001 | 0.001 (0.005) | 0.4 |
| Ending not observed | 0.062 | −0.014 (0.008) | −23.3 | [−49.7, 3.2] | 0.001 | −0.015 (0.008) | −24.2 |
| Insurance-billed abortion | 0.011 | −0.013 (0.003) | −123.6 | [−188.3, −58.9] | <0.001 | −0.013 (0.004) | −124.2 |

*Notes:* Event-study sample of 818,245 pregnancies in the continuous-enrollment sample (fewer for outcomes defined for observed endings or live births). Column 1 is the ban-state mean in event quarters −6 to −1. Columns 1, 2, and 6 are in each outcome's units (proportions or logs); preterm birth and a prolonged delivery stay (Table 1) are shares of live births. Under Controls, column 2 is the stacked difference-in-differences estimate with event-time and sub-experiment by state fixed effects, the weights of Wing, Freedman and Hollingsworth (2024), and the individual controls (age and its square, six race and ethnicity groups, Medicaid, first observed pregnancy, multiple gestation, and the comorbidity score of Leonard et al. (2020)), with its standard error (SE), clustered by state, in parentheses; column 3 is that estimate as a percent of the pre-ban mean (100 times the coefficient for outcomes in logs), column 4 its 95 percent confidence interval (CI) on the same scale, and column 5 the $p$-value of the joint test that the pre-period event-study coefficients are zero. Under Raw, columns 6 and 7 repeat columns 2 and 3 without the individual controls. The insurance-billed abortion estimate exceeds 100 percent of its mean because insured abortion rose in protected states (Figure 1).

Figure 3. Heterogeneity: Percent of Each Subgroup's Pre-Ban Mean



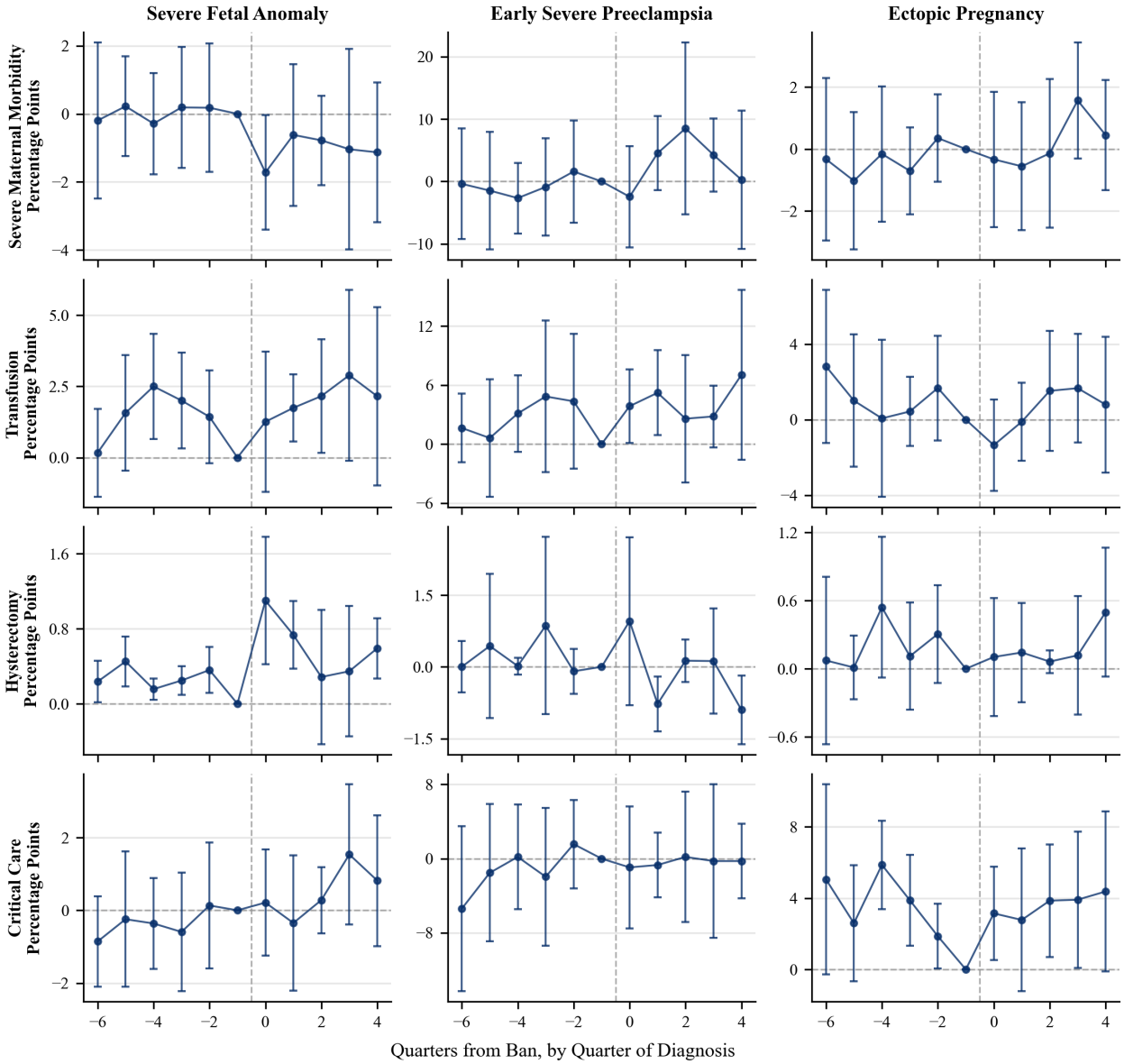

*Notes:* Each marker is a separate stacked difference-in-differences (DiD) regression in the named group, as a percent of that group's pre-ban mean (100 times the coefficient for outcomes in logs), with a 95 percent confidence interval (CI) from standard errors clustered by state, in the Controls specification and the continuous-enrollment sample. The all-pregnancies row repeats Table 2, and each panel has its own scale. Medicaid pregnancies have a Medicaid payer in the month the pregnancy started; minority women have a Black race code or a Hispanic ethnicity code; high-risk women have at least one condition in the obstetric comorbidity index before pregnancy; and excluding Texas removes the Texas cohort. The dashed vertical line marks zero, and Supplemental Appendix Table A9 reports the estimates.

Table 3—Complicated Pregnancies

| Outcome | Estimate (SE) (1) | 95% CI (2) | Pre-Trend $p$-Value (3) |
|---|---|---|---|
| **A. Share of All Pregnancies** | | | |
| Severe fetal anomaly | 0.33 (0.26) | [−0.19, 0.86] | 0.29 |
| Early severe preeclampsia | 0.15 (0.10) | [−0.06, 0.36] | 0.11 |
| Ectopic pregnancy | −0.15 (0.16) | [−0.48, 0.18] | 0.002 |
| **B. Within Severe Fetal Anomaly** | | | |
| Severe maternal morbidity | −1.1 (0.8) | [−2.7, 0.5] | 0.95 |
| Transfusion | 2.0 (0.8) | [0.3, 3.7] | <0.001 |
| Hysterectomy | 0.6 (0.1) | [0.3, 0.9] | <0.001 |
| Critical care | 0.5 (0.6) | [−0.8, 1.7] | 0.010 |
| **C. Within Early Severe Preeclampsia** | | | |
| Severe maternal morbidity | 2.9 (3.9) | [−5.0, 10.8] | 0.67 |
| Transfusion | 4.2 (1.7) | [0.7, 7.8] | 0.35 |
| Hysterectomy | 0.0 (0.4) | [−0.8, 0.7] | 0.88 |
| Critical care | −0.4 (2.2) | [−5.0, 4.2] | 0.21 |
| **D. Within Ectopic Pregnancy** | | | |
| Severe maternal morbidity | 0.2 (0.8) | [−1.4, 1.8] | 0.40 |
| Transfusion | 0.5 (1.2) | [−2.0, 3.1] | 0.16 |
| Hysterectomy | 0.2 (0.2) | [−0.1, 0.5] | 0.04 |
| Critical care | 3.6 (1.2) | [1.2, 6.0] | 0.001 |

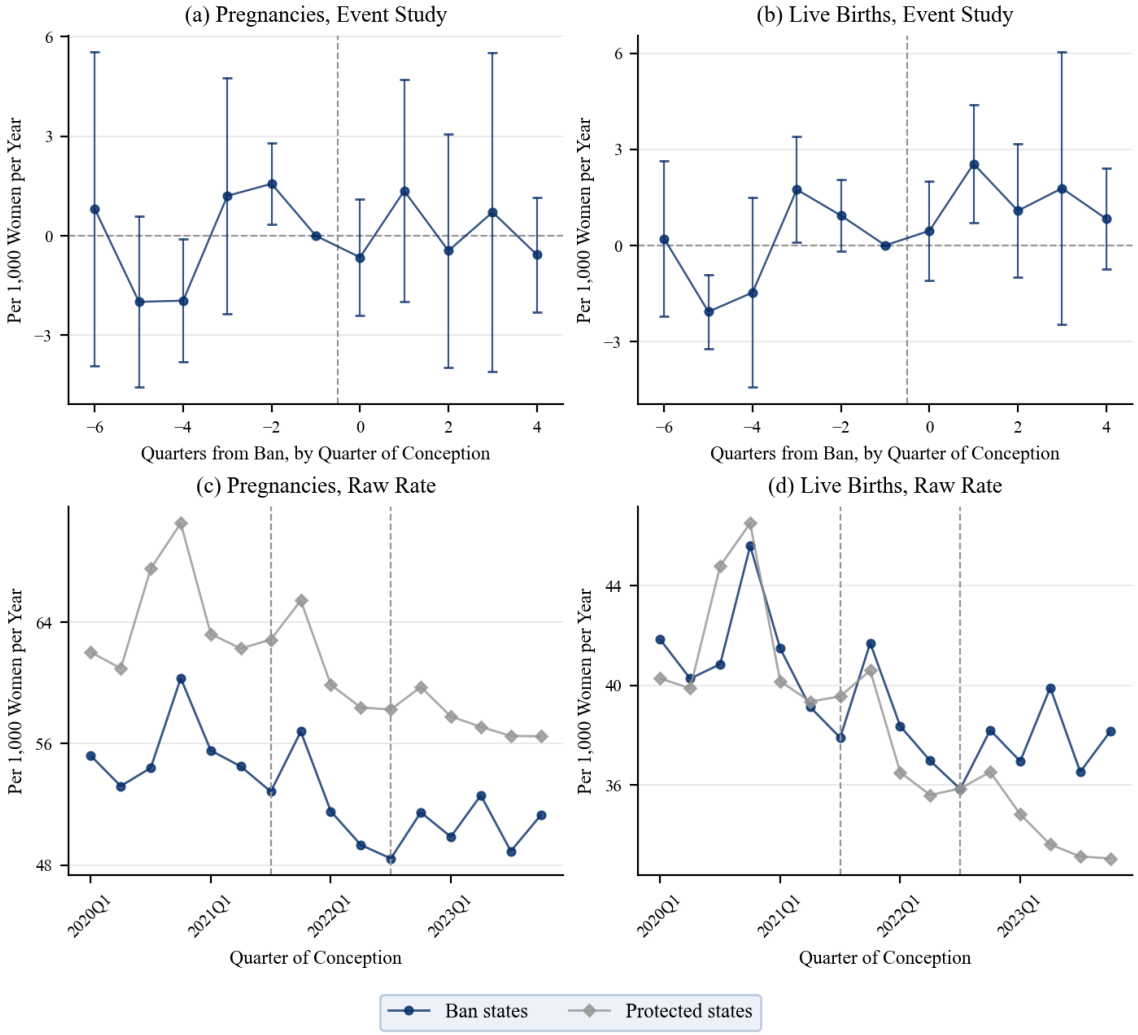
*Notes:* Stacked difference-in-differences estimates in percentage points, with standard errors, clustered by state, in parentheses and 95 percent confidence intervals in brackets, in the Controls specification and the continuous-enrollment sample. Panel A is estimated on all pregnancies by quarter of conception. Panels B to D are estimated within each group by quarter of its first diagnosis. Supplemental Appendix A12 defines each group and outcome. Severe maternal morbidity in Panels B to D differs from Table 2: it omits eclampsia, a diagnosis that places a pregnancy in the early severe preeclampsia group, and counts claims only from the first diagnosis through 42 days after the pregnancy ends. Each estimate compares the post-ban quarters with quarter −1. Column 3 is the $p$-value of the joint test that the pre-period event-study coefficients are zero.

Figure 4. Event Studies for Complicated Pregnancies



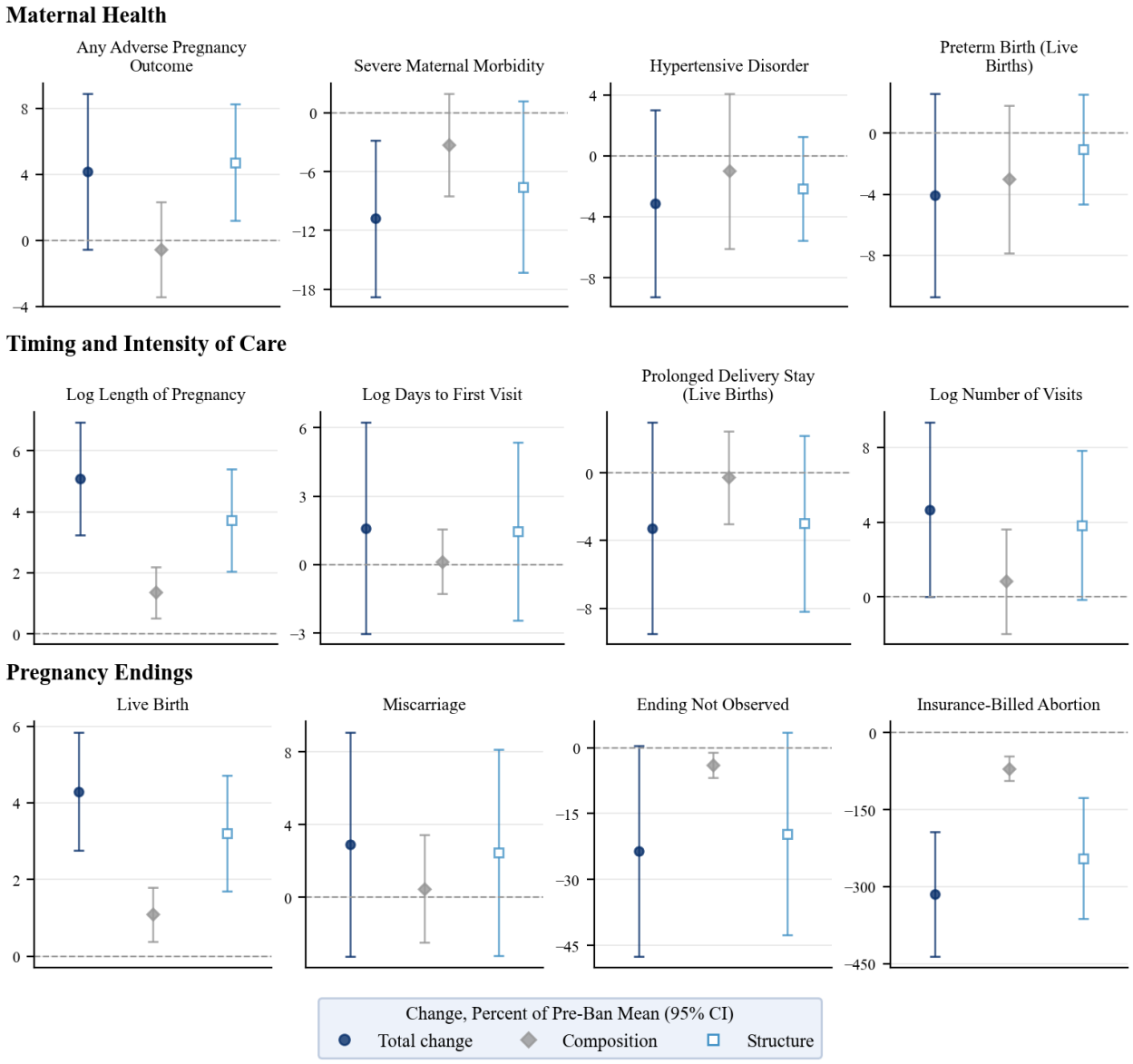

*Notes:* Stacked difference-in-differences event-study coefficients in percentage points by quarter of first diagnosis relative to the ban, quarters −6 to 4, with 95 percent confidence intervals from standard errors clustered by state, in the Controls specification and the continuous-enrollment sample. Quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0. Each panel has its own scale.

Figure 5. Fertility: Event Studies and Raw Rates




*Notes:* Panels (a) and (b) plot stacked difference-in-differences event-study coefficients for the pregnancy and live-birth rates on the panel of enrolled women aged 15 to 45 by state and quarter, with event-time and sub-experiment by state fixed effects, weights for enrolled women, and 95 percent confidence intervals from standard errors clustered by state; quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0. Panels (c) and (d) plot the raw rates by quarter of conception in ban states (circles) and protected states (diamonds) in the continuous-enrollment sample, monthly rates averaged to quarters; dashed lines mark the third quarters of 2021 (Texas Senate Bill 8) and 2022 (*Dobbs*). All rates are per 1,000 enrolled women per year, and Supplemental Appendix Figure A1 shows the episode-enrolled sample.

Figure 6. Decomposition of Outcome Changes into Composition and Care




*Notes:* Reweighting decomposition (Section VI.B) in the continuous-enrollment sample. The total change, the before-to-after change in ban states minus that in protected states, is split into a part due to pre-pregnancy characteristics (composition) and a part due to outcomes conditional on those characteristics (structure, the care component). Each is a percent of the ban-state pre-ban mean (100 times the change for outcomes in logs), with 95 percent confidence intervals from a block bootstrap over states with 200 draws. Supplemental Appendix Table A14 reports the estimates.

## REFERENCES


**Abraha, Hiluf Ebuy, Jeffrey Buzas, Marta Bornstein, and Nansi S. Boghossian.** 2026. "US Abortion Bans and Pregnancy-Associated Mortality." *JAMA Network Open*, 9(4): e264801.

**Ailes, Elizabeth C., Regina M. Simeone, April L. Dawson, Emily E. Petersen, and Suzanne M. Gilboa.** 2016. "Using Insurance Claims Data to Identify and Estimate Critical Periods in Pregnancy: An Application to Antidepressants." *Birth Defects Research Part A: Clinical and Molecular Teratology*, 106(11): 927–934.

**American College of Obstetricians and Gynecologists.** 2017. "Committee Opinion No. 700: Methods for Estimating the Due Date." *Obstetrics and Gynecology*, 129(5): e150–e154.

**American College of Obstetricians and Gynecologists.** 2018*a*. "ACOG Practice Bulletin No. 193: Tubal Ectopic Pregnancy." *Obstetrics and Gynecology*, 131(3): e91–e103.

**American College of Obstetricians and Gynecologists.** 2018*b*. "ACOG Practice Bulletin No. 200: Early Pregnancy Loss." *Obstetrics and Gynecology*, 132(5): e197–e207.

**American College of Obstetricians and Gynecologists.** 2022. "ACOG Statement on the Decision in *Dobbs v. Jackson*." https://www.acog.org/news/news-releases/2022/06/acog-statement-on-the-decision-in-dobbs-v-jackson, Accessed 2026-09-26.

**American College of Obstetricians and Gynecologists, and Society of Family Planning.** 2020. "Medication Abortion up to 70 Days of Gestation: ACOG Practice Bulletin No. 225." *Obstetrics and Gynecology*, 136(4): e31–e47.

**Arey, Whitney, Klaira Lerma, Anitra Beasley, Lorie Harper, Ghazaleh Moayedi, and Kari White.** 2022. "A Preview of the Dangerous Future of Abortion Bans: Texas Senate Bill 8." *New England Journal of Medicine*, 387(5): 388–390.

**Arey, Whitney, Klaira Lerma, Emma Carpenter, Ghazaleh Moayedi, Lorie Harper, Anitra Beasley, Tony Ogburn, and Kari White.** 2023. "Abortion Access and Medically Complex Pregnancies Before and After Texas Senate Bill 8." *Obstetrics and Gynecology*, 141(5): 995–1003.

**Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager.** 2021. "Synthetic Difference-in-Differences." *American Economic Review*, 111(12): 4088–4118.

**Bell, Suzanne O., Alexander M. Franks, Amy Ozinsky, Selena Anjur-Dietrich, Claire E. Margerison, Elizabeth A. Stuart, Avi Feller, and Alison Gemmill.** 2026. "Abortion Bans and Maternal, Pregnancy-Related, and Pregnancy-Associated Mortality in 14 US States, 2016–2023: Estimated Impacts amid Substantial Measurement Challenges." *American Journal of Public Health*, 116(6): 819–828.

**Bell, Suzanne O., Alexander M. Franks, David Arbour, Selena Anjur-Dietrich, Elizabeth A. Stuart, Eli Ben-Michael, Avi Feller, and Alison Gemmill.** 2025. "US Abortion Bans and Fertility." *JAMA*, 333(15): 1324–1332.

**Bell, Suzanne O., Elizabeth A. Stuart, and Alison Gemmill.** 2023. "Texas' 2021 Ban on Abortion in Early Pregnancy and Changes in Live Births." *JAMA*, 330(3): 281–282.

**Boghossian, Nansi S., Lucy T. Greenberg, Jeffrey S. Buzas, Jeannette Rogowski, Scott A. Lorch, Molly Passarella, George R. Saade, and Ciaran S. Phibbs.** 2024. "Severe Maternal Morbidity from Pregnancy through 1 Year Postpartum." *American Journal of Obstetrics & Gynecology MFM*, 6(7): 101385.

**Borusyak, Kirill, Xavier Jaravel, and Jann Spiess.** 2024. "Revisiting Event-Study Designs: Robust and Efficient

Estimation." *The Review of Economic Studies*, 91(6): 3253–3285.

**Callaway, Brantly, and Pedro H. C. Sant'Anna.** 2021. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics*, 225(2): 200–230.

**Caraher, Raymond.** 2024. "Do Abortion Bans Affect Reproductive and Infant Health? Evidence from Texas's 2021 Ban and Its Impact on Health Disparities." Political Economy Research Institute, University of Massachusetts Amherst Working Paper 606.

**Caraher, Raymond.** 2026. "Reproductive Injustice? A County-Level Analysis of the Impact of Abortion Restrictions on Abortion Rates." *American Journal of Health Economics*. Published online July 15, 2026.

**Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer.** 2019. "The Effect of Minimum Wages on Low-Wage Jobs." *Quarterly Journal of Economics*, 134(3): 1405–1454.

**Centers for Disease Control and Prevention.** 2024. "Severe Maternal Morbidity." https://www.cdc.gov/maternal-infant-health/php/severe-maternal-morbidity/index.html, Accessed 2026-09-07.

**Centers for Medicare & Medicaid Services.** 2023. "Identifying Pregnant and Postpartum Beneficiaries in Medicaid and CHIP Administrative Data: Technical Specifications." *Maternal and Infant Health (MIH) Technical Specifications and Reference Codes, Center for Medicaid and CHIP Services.* https://www.medicaid.gov/medicaid/data-and-systems/downloads/macbis/mih_techspecs.pdf.

**Centers for Medicare & Medicaid Services.** 2026. "NPPES Downloadable File." https://download.cms.gov/nppes/NPI_Files.html, Accessed 2026-09-26.

**Chen, Catherine, Deepshikha C. Ashana, Kevin Callison, Courtney Baker, Ralph Nicholas Burns, Jing Cao, Li Yuan, and Hayley B. Gershengorn.** 2026. "Critical Illness Outcomes of Hospitalized Pregnant Women Following a Texas Abortion Ban." *American Journal of Respiratory and Critical Care Medicine*, 212(8): 1730–1739.

**Chomistek, Andrea K., Kelesitse Phiri, Michael C. Doherty, Jenna F. Calderbank, Stephanie E. Chiuve, Brenda Hinman McIlroy, Michael C. Snabes, Cheryl Enger, and John D. Seeger.** 2023. "Development and Validation of ICD-10-CM-Based Algorithms for Date of Last Menstrual Period, Pregnancy Outcomes, and Infant Outcomes." *Drug Safety*, 46(2): 209–222.

**Clarke, Damian, and Hanna Mühlrad.** 2021. "Abortion Laws and Women's Health." *Journal of Health Economics*, 76: 102413. Corrigendum in Journal of Health Economics 80 (2021): 102535.

**Crump, Casey, Jan Sundquist, and Kristina Sundquist.** 2024. "Adverse Pregnancy Outcomes and Long-Term Mortality in Women." *JAMA Internal Medicine*, 184(6): 631–640.

**Curhan, Talia.** 2026. "State Bans on Abortion Throughout Pregnancy." *Guttmacher Institute.* https://www.guttmacher.org/state-policy/explore/state-policies-abortion-bans.

**Daw, Jamie R., Laura A. Hatfield, Katherine Swartz, and Benjamin D. Sommers.** 2017. "Women in the United States Experience High Rates of Coverage "Churn" in Months before and after Childbirth." *Health Affairs*, 36(4): 598–606.

**de Chaisemartin, Clément, and Xavier D'Haultfœuille.** 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9): 2964–2996.

**DeMillo, Andrew.** 2021. "Federal Judge Blocks Arkansas Law Banning Most Abortions." *Associated Press, via PBS NewsHour.* https://www.pbs.org/newshour/nation/federal-judge-blocks-arkansas-law-banning-most-abortions.

**Dench, Daniel L., Caitlin K. Myers, and Mayra Pineda-Torres.** 2025. "The Road Not Taken: How Driv-

ing Distance and Appointment Availability Shape the Effects of Abortion Bans." National Bureau of Economic Research Working Paper 33548.

**Dench, Daniel, Mayra Pineda-Torres, and Caitlin Myers.** 2024. "The Effects of Post-Dobbs Abortion Bans on Fertility." *Journal of Public Economics*, 234: 105124.

**Deshpande, Manasi, and Yue Li.** 2019. "Who Is Screened Out? Application Costs and the Targeting of Disability Programs." *American Economic Journal: Economic Policy*, 11(4): 213–248.

**DiNardo, John, Nicole M. Fortin, and Thomas Lemieux.** 1996. "Labor Market Institutions and the Distribution of Wages, 1973–1992: A Semiparametric Approach." *Econometrica*, 64(5): 1001–1044.

**Ellison, Jacqueline E., Brittany L. Brown-Podgorski, and Jake R. Morgan.** 2024. "Changes in Permanent Contraception Procedures among Young Adults Following the Dobbs Decision." *JAMA Health Forum*, 5(4): e240424.

**Farin, Sherajum Monira, Lauren Hoehn-Velasco, and Michael F. Pesko.** 2024. "The Impact of Legal Abortion on Maternal Mortality." *American Economic Journal: Economic Policy*, 16(3): 174–216.

**Felix, Mabel, Laurie Sobel, and Alina Salganicoff.** 2024. "A Review of Exceptions in State Abortion Bans: Implications for the Provision of Abortion Services." *KFF*. https://www.kff.org/womens-health-policy/a-review-of-exceptions-in-state-abortions-bans-implications-for-the-provision-of-abortion-services/.

**Felix, Mabel, Laurie Sobel, and Alina Salganicoff.** 2025. "Criminal Penalties for Physicians in State Abortion Bans." *KFF*.

**Finer, Lawrence B., Lori F. Frohwirth, Lindsay A. Dauphinee, Susheela Singh, and Ann M. Moore.** 2005. "Reasons U.S. Women Have Abortions: Quantitative and Qualitative Perspectives." *Perspectives on Sexual and Reproductive Health*, 37(3): 110–118.

**Fischer, Stefanie, Heather Royer, and Corey White.** 2018. "The Impacts of Reduced Access to Abortion and Family Planning Services on Abortions, Births, and Contraceptive Purchases." *Journal of Public Economics*, 167: 43–68.

**Frederiksen, Brittni, Usha Ranji, Ivette Gomez, and Alina Salganicoff.** 2023. "A National Survey of OBGYNs' Experiences After Dobbs." *KFF*.

**Freedman, Lori R., Uta Landy, and Jody Steinauer.** 2008. "When There's a Heartbeat: Miscarriage Management in Catholic-owned Hospitals." *American Journal of Public Health*, 98(10): 1774–1778.

**Gardner, Graham.** 2024. "The Maternal and Infant Health Consequences of Restricted Access to Abortion in the United States." *Journal of Health Economics*, 98: 102938.

**Gemmill, Alison, Alexander M. Franks, Selena Anjur-Dietrich, Amy Ozinsky, David Arbour, Elizabeth A. Stuart, Eli Ben-Michael, Avi Feller, and Suzanne O. Bell.** 2025. "US Abortion Bans and Infant Mortality." *JAMA*, 333(15): 1315–1323.

**Gemmill, Alison, Claire E. Margerison, Elizabeth A. Stuart, and Suzanne O. Bell.** 2024. "Infant Deaths after Texas' 2021 Ban on Abortion in Early Pregnancy." *JAMA Pediatrics*, 178(8): 784–791.

**Goodman-Bacon, Andrew.** 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics*, 225(2): 254–277.

**Grossman, Daniel, Carole Joffe, Shelly Kaller, Katrina Kimport, Elizabeth Kinsey, Klaira Lerma, Natalie Morris, and Kari White.** 2023. "Care Post-Roe: Documenting Cases of Poor-quality Care Since the

Dobbs Decision." Advancing New Standards in Reproductive Health and Population Research Center and Texas Policy Evaluation Project.

**Grossman, Tracy B., and Stephen T. Chasen.** 2020. "Abortion for Fetal Genetic Abnormalities: Type of Abnormality and Gestational Age at Diagnosis." *AJP Reports*, 10(1): e87–e92.

**Guttmacher Institute.** 2021. "Targeted Regulation of Abortion Providers (as of November 1, 2021)." *Guttmacher Institute, State Laws and Policies. Archived January 2, 2022.* http://web.archive.org/web/20220102054820/https://www.guttmacher.org/state-policy/explore/targeted-regulation-abortion-providers.

**Guttmacher Institute.** 2022. "An Overview of Abortion Laws (as of January 1, 2022)." *Guttmacher Institute, State Laws and Policies. Archived January 7, 2022.* http://web.archive.org/web/20220107211004/https://www.guttmacher.org/state-policy/explore/overview-abortion-laws.

**Guttmacher Institute.** 2023. "The High Toll of US Abortion Bans: Nearly One in Five Patients Now Traveling Out of State for Abortion Care." https://www.guttmacher.org/2023/12/high-toll-us-abortion-bans-nearly-one-five-patients-now-traveling-out-state-abortion-care.

**Harris, Lisa H.** 2022. "Navigating Loss of Abortion Services: A Large Academic Medical Center Prepares for the Overturn of Roe v. Wade." *New England Journal of Medicine*, 386(22): 2061–2064.

**Henshaw, Stanley K., Theodore J. Joyce, Amanda Dennis, Lawrence B. Finer, and Kelly Blanchard.** 2009. "Restrictions on Medicaid Funding for Abortions: A Literature Review." Guttmacher Institute, New York.

**Hill, Latoya, Samantha Artiga, Usha Ranji, Ivette Gomez, and Nambi Ndugga.** 2024. "What Are the Implications of the Dobbs Ruling for Racial Disparities?" *KFF.* https://www.kff.org/womens-health-policy/what-are-the-implications-of-the-dobbs-ruling-for-racial-disparities/.

**Hoyert, Donna L.** 2026. "Maternal Mortality Rates in the United States, 2024." National Center for Health Statistics NCHS Health E-Stat 113.

**Imbens, Guido W., and Jeffrey M. Wooldridge.** 2009. "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature*, 47(1): 5–86.

**Jerman, Jenna, Rachel K. Jones, and Tsuyoshi Onda.** 2016. "Characteristics of U.S. Abortion Patients in 2014 and Changes since 2008." Guttmacher Institute Report.

**Landay, Sophia, Emily Newton-Hoe, Subasri Narasimhan, Anitra Beasley, Jessica Adkins, and Sara Neill.** 2026. "Abortion Bans and Pregnancy-Related Care Across Physician Specialties: A Qualitative Study." *JAMA Network Open*, 9(6): e2619644.

**Leonard, Stephanie A., Chris J. Kennedy, Suzan L. Carmichael, Deirdre J. Lyell, and Elliott K. Main.** 2020. "An Expanded Obstetric Comorbidity Scoring System for Predicting Severe Maternal Morbidity." *Obstetrics and Gynecology*, 136(3): 440–449.

**Leonard, Stephanie A., Elliott K. Main, Karen A. Scott, Jochen Profit, and Suzan L. Carmichael.** 2019. "Racial and Ethnic Disparities in Severe Maternal Morbidity Prevalence and Trends." *Annals of Epidemiology*, 33: 30–36.

**Levine, Phillip B., Amy B. Trainor, and David J. Zimmerman.** 1996. "The Effect of Medicaid Abortion Funding Restrictions on Abortions, Pregnancies and Births." *Journal of Health Economics*, 15(5): 555–578.

**Lift Louisiana, Physicians for Human Rights, RH Impact, and Center for Reproductive Rights.** 2024. "Criminalized Care: How Louisiana's Abortion Bans Endanger Patients and Clinicians." *Joint report of Lift*

*Louisiana, Physicians for Human Rights, Reproductive Health Impact, and Center for Reproductive Rights.*

**Lindo, Jason M., and Mayra Pineda-Torres.** 2021. "New Evidence on the Effects of Mandatory Waiting Periods for Abortion." *Journal of Health Economics*, 80: 102533.

**Lindo, Jason M., Caitlin Knowles Myers, Andrea Schlosser, and Scott Cunningham.** 2020. "How Far Is Too Far? New Evidence on Abortion Clinic Closures, Access, and Abortions." *Journal of Human Resources*, 55(4): 1137–1160.

**Lisonkova, Sarka, Jayson Potts, Giulia M. Muraca, Neda Razaz, Yasser Sabr, Wee-Shian Chan, and Michael S. Kramer.** 2017. "Maternal Age and Severe Maternal Morbidity: A Population-Based Retrospective Cohort Study." *PLOS Medicine*, 14(5): e1002307.

**MacDonald, Andrea, Hayley B. Gershengorn, and Deepshikha Charan Ashana.** 2022. "The Challenge of Emergency Abortion Care Following the Dobbs Ruling." *JAMA*, 328(17): 1691–1692.

**Matcho, Amy, Patrick Ryan, Daniel Fife, Dina Gifkins, Chris Knoll, and Andrew Friedman.** 2018. "Inferring Pregnancy Episodes and Outcomes within a Network of Observational Databases." *PLOS ONE*, 13(2): e0192033.

**Medicaid and CHIP Payment and Access Commission.** 2019. "Managed Care Overview." *MACPAC.* https://www.macpac.gov/subtopic/managed-care-overview/.

**Miller, Sarah, Laura R. Wherry, and Diana Greene Foster.** 2023. "The Economic Consequences of Being Denied an Abortion." *American Economic Journal: Economic Policy*, 15(1): 394–437.

**Moll, Keran, Hui Lee Wong, Kathryn Fingar, Shayan Hobbi, Minya Sheng, Timothy A. Burrell, Linda O. Eckert, Flor M. Munoz, Bethany Baer, Azadeh Shoaibi, and Steven Anderson.** 2021. "Validating Claims-Based Algorithms Determining Pregnancy Outcomes and Gestational Age Using a Linked Claims-Electronic Medical Record Database." *Drug Safety*, 44(11): 1151–1164.

**Myers, Caitlin.** 2024. "Forecasts for a Post-Roe America: The Effects of Increased Travel Distance on Abortions and Births." *Journal of Policy Analysis and Management*, 43(1): 39–62.

**Myers, Caitlin, and Daniel Ladd.** 2020. "Did Parental Involvement Laws Grow Teeth? The Effects of State Restrictions on Minors' Access to Abortion." *Journal of Health Economics*, 71: 102302.

**Nagle, Amanda, Goleen Samari, Lauren Thaxton, and Alison Gemmill.** 2025. "Blood Transfusions for Pregnancy Loss in Texas Before and After Abortion Bans, 2017–2023." *American Journal of Public Health*, 115(11): 1887–1894.

**Nambiar, Anjali, Elaine L. Duryea, Lindsay R. Thiele, Patricia Santiago-Munoz, David B. Nelson, Catherine Y. Spong, and Christina C. Baker.** 2025. "Maternal Morbidity with Expectant Management of Life-Limiting Fetal Conditions." *JAMA Network Open*, 8(7): e2521883.

**Nash, Elizabeth.** 2021. "State Policy Trends 2021: The Worst Year for Abortion Rights in Almost Half a Century." *Guttmacher Institute.* https://www.guttmacher.org/article/2021/12/state-policy-trends-2021-worst-year-abortion-rights-almost-half-century.

**Nash, Elizabeth, and Isabel Guarnieri.** 2022. "13 States Have Abortion Trigger Bans: Here's What Happens When Roe Is Overturned." *Guttmacher Institute.* https://www.guttmacher.org/article/2022/06/13-states-have-abortion-trigger-bans-heres-what-happens-when-roe-overturned.

**Nash, Elizabeth, Lizamarie Mohammed, Olivia Cappello, and Sophia Naide.** 2019. "State Policy Trends

2019: A Wave of Abortion Bans, but Some States Are Fighting Back." *Guttmacher Institute.* https://www.guttmacher.org/article/2019/12/state-policy-trends-2019-wave-abortion-bans-some-states-are-fighting-back.

**Nuss, Emily, Mari Iwasaki, Lindsay Robbins, Peggy Ye, Misa Hayasaka, George Saade, and Tetsuya Kawakita.** 2025. "Maternal Mortality According to State Abortion Legislative Climate Following the US Supreme Court's Dobbs v. Jackson Women's Health Organization Ruling." *Pregnancy*, 1(6): e70128.

**Odouard, Ilina C., Jenny S. Guadamuz, Apurba Chakraborty, G. Caleb Alexander, and Dima M. Qato.** 2024. "Induced Abortion and Out-of-State Travel among Insured Women before and after the Dobbs v. Jackson Women's Health Organization Decision." *O&G Open*, 1(2): 16.

**Osterman, Michelle J. K., Brady E. Hamilton, Joyce A. Martin, Anne K. Driscoll, and Claudia P. Valenzuela.** 2025. "Births: Final Data for 2023." National Center for Health Statistics National Vital Statistics Reports 74(1).

**Pop-Eleches, Cristian.** 2006. "The Impact of an Abortion Ban on Socioeconomic Outcomes of Children: Evidence from Romania." *Journal of Political Economy*, 114(4): 744–773.

**Presser, Lizzie.** 2024. "A Third Woman Has Died under Texas' Abortion Ban as Doctors Reach for Riskier Miscarriage Treatments." *The Texas Tribune.* https://www.texastribune.org/2024/11/27/texas-abortion-death-porsha-ngumezi/.

**Presser, Lizzie, Andrea Suozzo, Sophie Chou, and Kavitha Surana.** 2025. "Texas Banned Abortion, Then Sepsis Rates Soared." *ProPublica, February 20.* https://www.propublica.org/article/texas-abortion-ban-sepsis-maternal-mortality-analysis.

**Ralph, Lauren J., Eleanor Bimla Schwarz, Daniel Grossman, and Diana Greene Foster.** 2019. "Self-Reported Physical Health of Women Who Did and Did Not Terminate Pregnancy after Seeking Abortion Services." *Annals of Internal Medicine*, 171(4): 238–247.

**Ranji, Usha, Alina Salganicoff, Jennifer Tolbert, Brittni Frederiksen, and Ivette Gomez.** 2025. "5 Key Facts About Medicaid and Pregnancy." *KFF.*

**Roberts, Sarah C. M., Heather Gould, Katrina Kimport, Tracy A. Weitz, and Diana Greene Foster.** 2014. "Out-of-Pocket Costs and Insurance Coverage for Abortion in the United States." *Women's Health Issues*, 24(2): e211–e218.

**Rodriguez, Maria I., Megan Fuerst, and Kaitlin Schrote.** 2026. "Management of Spontaneous Abortion among Commercially Insured Individuals in the United States after Dobbs v Jackson." *JAMA*, 336(1): 48–55.

**Rossen, Lauren M., Brady E. Hamilton, Joyce C. Abma, Elizabeth C. W. Gregory, Vladislav Beresovsky, Adriana V. Resendez, Anjani Chandra, and Joyce A. Martin.** 2023. "Updated Methodology to Estimate Overall and Unintended Pregnancy Rates in the United States." National Center for Health Statistics Vital and Health Statistics Series 2, No. 201.

**Sabbath, Erika L., Samantha M. McKetchnie, Kavita S. Arora, and Mara Buchbinder.** 2024. "US Obstetrician-Gynecologists' Perceived Impacts of Post–Dobbs v Jackson State Abortion Bans." *JAMA Network Open*, 7(1): e2352109.

**Salganicoff, Alina, Laurie Sobel, Ivette Gomez, and Amrutha Ramaswamy.** 2024. "The Hyde Amendment and Coverage for Abortion Services under Medicaid in the Post-Roe Era." *KFF Issue Brief.* https://www.kff.org/womens-health-policy/the-hyde-amendment-and-coverage-for-abortion-services-und

er-medicaid-in-the-post-roe-era/.

**Sarayani, Amir, Xi Wang, Thuy Nhu Thai, Yasser Albogami, Nakyung Jeon, and Almut G. Winterstein.** 2020. "Impact of the Transition from ICD-9-CM to ICD-10-CM on the Identification of Pregnancy Episodes in US Health Insurance Claims Data." *Clinical Epidemiology*, 12: 1129–1138.

**Seitz, Amanda.** 2024. "After Dobbs, ERs Turned Away Dozens of Pregnant Women." *Associated Press, via Mississippi Free Press.*

**Simmons-Duffin, Selena.** 2022. "Doctors Who Want to Defy Abortion Laws Say It's Too Risky." *NPR.*

**Singh, Parvati, and Maria F. Gallo.** 2024. "National Trends in Infant Mortality in the US After Dobbs." *JAMA Pediatrics*, 178(12): 1364–1366.

***SisterSong v. Governor of Georgia.*** 2022. *SisterSong Women of Color Reproductive Justice Collective v. Governor of the State of Georgia, 40 F.4th 1320 (11th Cir. 2022).*

**Smith, Mikaela H., Zoe Muzyczka, Payal Chakraborty, Elaina Johns-Wolfe, Jenny Higgins, Danielle Bessett, and Alison H. Norris.** 2022. "Abortion Travel within the United States: An Observational Study of Cross-State Movement to Obtain Abortion Care in 2017." *The Lancet Regional Health - Americas*, 10: 100214.

**Society of Family Planning.** 2025. "#WeCount Report: April 2022 through June 2025." https://doi.org/10.46621/750591yxmcwh.

**Stevenson, Amanda Jean, and Leslie Root.** 2024. "Trends in Maternal Death Post-Dobbs v Jackson Women's Health." *JAMA Network Open*, 7(8): e2430035.

**Sun, Liyang, and Sarah Abraham.** 2021. "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects." *Journal of Econometrics*, 225(2): 175–199.

**Thompson, Terri-Ann.** 2019. "Insured but Still Uncovered: A Review of Private Insurance Coverage of Abortion Care." *Ibis Reproductive Health.* https://www.ibisreproductivehealth.org/publications/insured-still-uncovered-review-private-insurance-coverage-abortion-care.

**Tolbert, Jennifer, and Meghana Ammula.** 2023. "10 Things to Know About the Unwinding of the Medicaid Continuous Enrollment Provision." *KFF Issue Brief.*

**Venator, Joanna, and Jason Fletcher.** 2021. "Undue Burden beyond Texas: An Analysis of Abortion Clinic Closures, Births, and Abortions in Wisconsin." *Journal of Policy Analysis and Management*, 40(3): 774–813.

***Whole Woman's Health v. Jackson.*** 2021. *Order denying application for injunctive relief, No. 21A24, September 1, 2021 (Supreme Court of the United States).* https://www.supremecourt.gov/opinions/20pdf/21a24_8759.pdf.

**Wing, Coady, Seth M. Freedman, and Alex Hollingsworth.** 2024. "Stacked Difference-in-Differences." National Bureau of Economic Research Working Paper 32054.

**Xu, Xiao, Ling Chen, Vrunda B. Desai, Cary P. Gross, Craig Evan Pollack, Peter E. Schwartz, and Jason D. Wright.** 2024. "Tubal Sterilization Rates by State Abortion Laws after the Dobbs Decision." *JAMA*, 332(14): 1204–1206.

# Supplemental Appendix

# It Was Already Dobbs: Abortion Bans and Maternal Health

## Appendix

This appendix supports the main text. Section A1 details abortion policy before *Dobbs*. Section A2 documents the timing of the bans, how each state is classified, and the legal and policy sources. Section A3 describes how pregnancy episodes are built and dated. Section A4 describes the samples: their construction and pre-period balance. Section A5 compares the continuous-enrollment sample with the episode-enrolled sample. Section A6 reports the subgroup estimates. Section A8 reports the composition of the pregnant population and the decomposition by composition and care. Section A9 documents the stacked design and compares it with two-way fixed effects, Section A11 presents the close-to-care design, and Section A13 defines the outcomes.

### *A1. Abortion Policy Before Dobbs*

Section I.B summarizes this history. By the start of 2022, every one of the fifteen states that would later ban abortion except Tennessee, whose law was enjoined, made women wait between counseling and the procedure, for 18 to 72 hours (Guttmacher Institute, 2022). The targeted regulations of abortion providers that eleven of them enforced often hold clinics to the licensing standards of ambulatory surgical centers (Guttmacher Institute, 2021*b*), and the pace of new restrictions kept rising in the years leading up to *Dobbs* (Nash, 2021). Some of these rules already carried penalties for physicians. In Texas, for example, performing an abortion without the sonogram and information the state required had been a criminal offense since 2011 (Texas State Law Library, 2026*a*,*b*).

Insurance coverage was limited as well. Most of the fifteen states limited Medicaid coverage of abortion to cases of life endangerment, rape, or incest, and South Dakota to life endangerment alone (Guttmacher Institute, 2022). Beyond the fourteen that restricted abortion coverage in plans sold on the health insurance exchanges, six, Idaho, Indiana, Kentucky, Missouri, Oklahoma, and Texas, restricted it in every private plan written in the state (Guttmacher Institute, 2021*a*). In 2020 a patient paying out of pocket faced a median charge of $560 for a medication abortion and $575 for a first-trimester procedural abortion (Upadhyay et al., 2022).

Several states had seen their bans enjoined before *Dobbs*. Texas's law was the only one of the six-week bans enacted since 2013 then in effect, the others having been blocked by courts (Nash, 2021). *Dobbs* itself arose from a challenge to Mississippi's 15-week ban brought by the state's only abortion clinic (*Dobbs v. Jackson Women's Health Organization*, 2022), and after the decision state courts blocked the Kentucky and Louisiana bans for much of July 2022 (Table A2).

### *A2. Ban Timing, State Classification, and Policy Sources*

#### Policy sources

Table A1 lists the statute behind each ban. The *Dobbs* decision (*Dobbs v. Jackson Women's Health Organization*, 2022) overturned *Roe v. Wade* (*Roe v. Wade*, 1973). In Texas, SB8 (Hughes, 2021) took effect on September 1, 2021, and because private lawsuits rather than state officials enforced it, there was no official a federal court could enjoin to stop it (*Whole Woman's Health v. Jackson*, 2021). The state's trigger law (Texas Legislature, 2021) took effect on August 25, 2022 (Texas State Law Library, 2026*b*). After *Dobbs*, federal courts allowed the previously enjoined bans of Alabama (*Robinson v. Marshall*, 2022) and Georgia (*SisterSong v. Governor of Georgia*, 2022) to take effect. The author of the Texas law and Kentucky Senate Republicans have each described the reach of their state's health exceptions (Hughes, 2024; Kentucky Senate GOP, 2025), and reporting on Kate Cox's case in Texas documents women denied terminations despite serious health risks (Klibanoff, 2023; Goldhill, 2023). Further investigative reporting documents pregnant women in Texas who died after emergency care was delayed (Surana and Presser, 2024; Surana and Jaramillo, 2024). The Texas sepsis figures in Section I.A come from hospital discharge records analyzed by Presser et al. (2025), whose method is described in Suozzo, Chou and Presser (2025). Among protected states, Illinois and California widened protections and coverage after *Dobbs* (Office of Governor JB Pritzker, 2023; Office of Governor Gavin Newsom, 2023).

Table A1—State Abortion Policy Classification

| State | Ban in Force | Law | First Treated Quarter |
|---|---|---|---|
| *Ban States (N = 15)* | | | |
| Texas | September 1, 2021 | Six-week ban (SB8) | Fourth quarter of 2021 |
| Alabama | June 24, 2022 | Total ban (HB 314, 2019) | Third quarter of 2022 |
| Arkansas | June 24, 2022 | Trigger ban | Third quarter of 2022 |
| Georgia | July 20, 2022 | Six-week ban (HB 481) | Third quarter of 2022 |
| Idaho | August 12, 2022 | Six-week ban; trigger ban August 25 | Third quarter of 2022 |
| Kentucky | June 24, 2022 | Trigger ban | Third quarter of 2022 |
| Louisiana | June 24, 2022 | Trigger ban | Third quarter of 2022 |
| Mississippi | July 7, 2022 | Trigger ban | Third quarter of 2022 |
| Missouri | June 24, 2022 | Trigger ban | Third quarter of 2022 |
| Oklahoma | May 3, 2022 | Civil bans; trigger ban June 24 | Third quarter of 2022 |
| South Dakota | June 24, 2022 | Trigger ban | Third quarter of 2022 |
| Tennessee | June 28, 2022 | Six-week ban; total ban August 25 | Third quarter of 2022 |
| West Virginia | September 16, 2022 | Total ban (HB 302) | Fourth quarter of 2022 |
| Indiana | August 21, 2023 | Total ban (SEA 1) | Fourth quarter of 2023 |
| South Carolina | August 23, 2023 | Six-week ban (S. 474) | Fourth quarter of 2023 |

*Protected States and DC (N = 25)*
Alaska, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Kansas, Maine, Maryland, Massachusetts, Michigan, Minnesota, Montana, Nevada, New Hampshire, New Jersey, New Mexico, New York, Oregon, Rhode Island, Vermont, Virginia, Washington

*Excluded States (N = 11)*
Arizona, Florida, Iowa, Nebraska, North Carolina, North Dakota, Ohio, Pennsylvania, Utah, Wisconsin, Wyoming

*Notes:* The date is the first date a ban at six weeks of gestation or earlier was in force. The analysis gives every state in a cohort the cohort's first fully treated quarter (last column). Table A2 gives the reasons for each state whose legal date differs from its cohort's and for each excluded state. Protected states kept abortion legal through at least 22 weeks of gestation throughout the window. Excluded states had bans that were enjoined, partial, or changing during the window. Classification follows Dench, Pineda-Torres and Myers (2024). Sources: Nash and Guarnieri (2022), Curhan (2026), KFF, the Center for Reproductive Rights, state statutes, and court orders.

Table A2—Classification Decisions for States with Contested Timing or Status

| State | Group and Timing in the Analysis | Reason |
|---|---|---|
| *Ban States* | | |
| Texas | Ban; first treated in the fourth quarter of 2021 | SB8 took effect September 1, 2021, and was never enjoined, because its private enforcement left no official to sue; clinics stopped providing abortion after about six weeks that day. The August 2022 trigger ban extended a ban already in force on the ground. |
| Oklahoma | Ban, *Dobbs* cohort; third quarter of 2022 | Civilly enforced bans took effect in May 2022, weeks before *Dobbs*, but the ban regime was incomplete until then: enforcement rested on private suits, and the criminal trigger ban took effect only with *Dobbs*. Coding Oklahoma with the *Dobbs* cohort places May and June 2022 in its reference quarter, so any early effect raises the reference level and pulls its estimate toward zero rather than away from it. |
| Georgia, Idaho, Mississippi | Ban, *Dobbs* cohort; third quarter of 2022 | Each ban took effect between July 7 and August 25, 2022, inside the cohort's first post quarter. That quarter is only partly treated for these states, which attenuates the estimate at event time 0 (Section III); every later quarter is fully treated, apart from November 15 to 23, 2022, when a state court blocked Georgia's ban. |
| Kentucky, Louisiana | Ban, *Dobbs* cohort; third quarter of 2022 | State courts blocked the trigger bans for parts of July 2022, but each ban was in force from late June, again from August 2022, and for the rest of the window. The interruptions fall inside the partly treated first post quarter. |
| West Virginia | Ban, own cohort; fourth quarter of 2022 | Providers stopped under the 1849 statute from June 24 until a court enjoined it on July 18, 2022, and resumed until the total ban (HB 302) took effect September 16, 2022. The analysis dates the ban from the statute that remained in force; the three-week pause falls in West Virginia's reference quarter and, if it mattered, biases its estimate toward zero. |
| Indiana | Ban from the fourth quarter of 2023; not in the stacked pregnancy estimates | SEA 1 was in force for one week in September 2022 and then enjoined, and abortion remained available until August 21, 2023. Indiana has no complete post-ban window in the pregnancy data, so it does not enter the stacked pregnancy estimates. |
| South Carolina | Ban from the fourth quarter of 2023; not in the stacked pregnancy estimates | A six-week ban was in force from June 27 to August 17, 2022, before the state supreme court blocked it; abortion was then legal to about 22 weeks until August 23, 2023. South Carolina has no complete post-ban window in the pregnancy data, so it does not enter the stacked pregnancy estimates. |
| *Protected States* | | |
| Kansas | Protected | Abortion is legal to 22 weeks, before viability, but the state constitution protects it and voters rejected an amendment to remove that protection in August 2022. The 22-week limit binds fewer than 1 percent of abortions (Kortsmit et al., 2023), and more than three-quarters of abortions in Kansas in 2023 were to nonresidents, most from neighboring ban states (Conlon, 2024). |
| Michigan | Protected | The 1931 ban was enjoined on May 17, 2022, before *Dobbs*, and was never enforced afterward; Proposal 3 added the right to the state constitution in November 2022. |
| *Excluded States* | | |
| Wisconsin | Excluded | Providers stopped under the 1849 statute from June 2022, a ban on the ground, but they resumed in September 2023 after a court ruling. Treating Wisconsin as a ban state would place a reversal inside its post-period, and treating it as protected would place fifteen months of no access inside a control. |
| Ohio, Utah, Arizona | Excluded | Each had a near-total or six-week ban in force for days to months in 2022 and a gestational limit of 15 to 18 weeks, or a later reversal, for the rest of the window, so neither group describes it. |
| Florida, Nebraska, North Carolina, Iowa | Excluded | Each moved from legal abortion to a 15-week, 12-week, or six-week limit during the window, a partial restriction that changed within the period rather than a ban or protected access throughout. |
| North Dakota | Excluded | The trigger ban was blocked by a state court, but the state's only clinic moved to Minnesota in August 2022, so legal status and access on the ground diverged; a near-total ban (SB 2150) then took effect on April 24, 2023. |
| Wyoming | Excluded | The trigger ban was in effect for a day in July 2022 and then enjoined, with further bans litigated through November 2024. |
| Pennsylvania | Excluded | Abortion was legal to 24 weeks throughout, but the state kept one of the most restrictive regimes among states where it remained legal: a 24-hour waiting period with state-directed counseling, parental consent for minors, no public coverage beyond the federal Hyde exceptions, and a requirement that clinics meet ambulatory surgical facility standards, a targeted regulation of abortion providers. Placing it among the protected states would put a restrictive regime in the comparison group, which would bias the estimates toward zero. |

*Notes:* The classification in Table A1 is fixed in the analysis code. This table gives, for each state whose placement is not obvious from its statute, the reason for its group and the direction in which any misplacement would bias the estimates. Where a ban's legal status and access on the ground diverged, the analysis follows the statute in force for the rest of the window and notes the gap here. Sources as in Table A1.

## *A3. Constructing and Dating Pregnancy Episodes*

### Endings

To identify the ending of a pregnancy, I identify every claim carrying a diagnosis, procedure, or diagnosis-related-group code for a live birth, stillbirth, molar pregnancy, ectopic pregnancy, induced abortion, or spontaneous abortion, each as a separate ending type. Codes that indicate the ending of a pregnancy may span several days, for example several days of labor and the postpartum care that follows, so I link ending codes that fall within 30 days of the previous one into a single ending, dated at the earliest claim. Where an ending carries codes for more than one type, I apply the hierarchy that Moll et al. (2021) validated against physician-adjudicated records, which orders endings by how reliably each is coded, from live birth, stillbirth, molar pregnancy, ectopic pregnancy, and induced abortion to spontaneous abortion (miscarriage). Because the abortion codes include the billing codes for mifepristone and misoprostol, which also treat miscarriage, and the hierarchy ranks induced abortion above miscarriage, a miscarriage managed with these medications can be counted as an abortion, so the classification errs toward counting too many abortions rather than too few. Additionally, because women may use the health care and insurance systems intermittently throughout pregnancy, especially for an early miscarriage or an abortion paid outside insurance, some endings imply a pregnancy that began implausibly soon after the woman's previous one ended. To account for this, I treat an ending whose estimated start falls within the recovery period after her previous ending, 24 weeks after a birth or stillbirth and 60 days after any other ending, as part of the earlier pregnancy, and I exclude it.

### Pregnancy start

I date each pregnancy from the first day of the estimated last menstrual period, the clinical convention from which gestational age is counted (American College of Obstetricians and Gynecologists, 2017), and call this date the pregnancy start. I largely rely on the Z3A diagnosis code, which records the gestational week at a visit (American Academy of Professional Coders, 2024).[1] For each Z3A code in the 300 days before the ending, I subtract the recorded weeks from the date of service, and I take the median of these estimates, assigning four weeks to codes below eight weeks, as the validated algorithm does (Chomistek et al., 2023). In my analysis sample, Z3A codes date 87 percent of ban-state and 75 percent of protected-state pregnancies (Table A3). Against last-period dates that an epidemiologist adjudicated from medical records, this method has a median absolute

[1] Where an embryo transfer is observed, I set the start 14 days before it.

error of four days, with 65 percent of estimates within a week (Chomistek et al., 2023). However, its accuracy varies by ending. For example, 89.7 percent of premature deliveries fall within seven days, but only 34.3 percent of spontaneous abortions do, because early losses often end before the first visit at which a Z3A code is recorded. These facts give the first of two reasons I measure event time in quarters rather than months (Section III), as miscarriages alone end about 16 percent of ban-state pregnancies with an observed ending before the bans, so monthly event time would add noise from dating error. Second, pooling to quarters estimates each event-time coefficient on more pregnancies, at the cost of making changes within a quarter harder to detect.

I date episodes without a Z3A code from the earliest test performed at a known gestational age, adapting the priority order in Moll et al. (2021).[2] Failing all of these, I assign the ending-type defaults validated by Chomistek et al. (2023) (39 weeks for a singleton live birth, 36 for a multiple birth, 28 for a stillbirth, 10 for an abortion of either type, and 8 for ectopic and molar pregnancies). Table A3 reports the share of episodes dated by each source.

### Pregnancies without an observed ending

Some pregnancies carry pregnancy care but no ending code. In my main analysis sample, they are 6.2 percent of ban-state pregnancies conceived in the six quarters before each state's ban (Section II.D). To account for these pregnancies, I build episodes with no observed ending from pregnancy-indicating claims (for example, prenatal visits, gestational-age codes, and diagnoses of adverse pregnancy outcomes) that fall outside every other observed pregnancy for a woman,[3] as Sarayani et al. (2020) do for pregnancies with unknown outcomes. I group these claims into clusters, starting a new cluster whenever more than 60 days separate two claims, similar to the two-month separation between pregnancies that Ailes et al. (2016) require, and a cluster with claims on at least two dates becomes an episode. Because nothing in the claims marks how such a pregnancy ended, I end the episode on the date of its last pregnancy-indicating claim. I date its start from its latest Z3A code or, if it has none, eight weeks before its first visit, close to the median time from the last menstrual period to the first pregnancy-care claim among deliveries in insurance claims (Sarayani et al., 2020). Finally, a cluster whose last claim falls within 200 days before an observed ending is likely early care for that same pregnancy rather than a separate one, so I exclude it.[4]

[2]The order is nuchal translucency ultrasound, cell-free DNA screening, and maternal serum screening, then the detailed fetal anatomy ultrasound and glucose tolerance testing. I exclude hCG tests from the serum-screening tier, because they are drawn early in pregnancy to work up a suspected ectopic pregnancy or loss (American College of Obstetricians and Gynecologists, 2018*a*,*b*) rather than at the gestational age that tier assumes.

[3]An observed pregnancy runs from 90 days before its start through its 42-day postpartum window.

[4]This 200-day window is comparable to the 168 to 182 days that Matcho et al. (2018) require between an earlier outcome and a live birth.

Table A3—Source of the Pregnancy Start Date

| Dating Source | Ban States, % | Protected States, % |
|---|---|---|
| Gestational-week diagnosis code (Z3A) | 87.051 | 75.345 |
| Ending-type default | 9.112 | 19.579 |
| First prenatal visit default | 2.804 | 2.862 |
| Embryo transfer | 0.711 | 1.647 |
| Cell-free DNA screening | 0.175 | 0.205 |
| Glucose tolerance test | 0.063 | 0.115 |
| Ending-type default, multiple birth | 0.028 | 0.048 |
| Maternal serum screening | 0.023 | 0.068 |
| Nuchal translucency ultrasound | 0.021 | 0.102 |
| Fetal anatomy ultrasound | 0.012 | 0.029 |

*Notes:* Percent of pregnancy episodes in the continuous-enrollment sample dated by each source, under the priority order in Appendix A3; rows are sorted by the ban-state share.

### *A4. Sample Construction and Descriptives*

#### Additional restrictions

The pregnancy episode construction described in Section II.C and Appendix A3 yields 10,635,041 pregnancies for women aged 15 to 45, 1,848,589 of them to continuously enrolled women. Among those, I keep pregnancies in the 15 ban states and 25 protected jurisdictions (1,576,819) and pregnancies that started in 2020 or later (1,135,994), because some pregnancies observed in my window started in 2019 and continued into 2020. I require the pregnancy to start no later than February 2024 so that a typical 40-week pregnancy can be followed to its ending (996,163). I exclude observed endings with an implied gestation shorter than 28 or longer than 315 days, about 0.1 percent of the remaining pregnancies, which likely reflect coding or dating errors. The continuous-enrollment sample contains 995,377 pregnancies. Table A4 reports counts after each step.

#### The pre-pregnancy year

I exclude pregnancies that start in 2019 (Section II.D). For pregnancies that start in 2020, part of the pre-pregnancy year falls in 2019, before the enrollment requirement begins, so their baseline health measures draw on whatever 2019 claims the woman has. Of these 2020 pregnancies, 81 percent were also enrolled for the whole year before the pregnancy, from 72 percent of those that started in the first quarter to 91 percent in the fourth. The share is similar in ban states (83 percent) and protected states (81 percent), so a shorter look-back does not fall more on one side of the comparison of baseline health.

### Provider locations

Claims locate care only through the registered address of the provider's NPI, which is why in my core analysis I assign treatment by the woman's state of residence in the enrollment file. Of all claim lines, 25.5 percent name no provider, and 24.3 percent are billed under an organizational NPI, whose registered address may be a billing office or a health system's headquarters rather than the place of care.

### Pre-pregnancy comorbidity score

The conditions of the obstetric comorbidity index (Section II.D) include pulmonary hypertension, cardiac and renal disease, chronic hypertension, pre-existing diabetes, autoimmune and bleeding disorders, anemia, asthma, and mental health and substance use disorders, each measured in the year before conception. I omit placenta accreta, which is diagnosed during pregnancy. The score weights each pre-pregnancy condition by the points Leonard et al. (2020) estimated for severe maternal morbidity (Table A21). I also report the number of conditions and an indicator for any condition, which do not depend on the weights (Section VI.B).

Table A4—Sample Construction

| **Restriction** | **Continuous Enrollment** | **Episode Enrollment** |
|---|---|---|
| All pregnancy episodes meeting the enrollment requirement | 1,848,589 | 3,021,280 |
| Ban or protected state at pregnancy start | 1,576,819 | 2,495,494 |
| Pregnancy start in 2020 or later* | 1,135,994 | 2,495,494 |
| Pregnancy start on or before February 2024 (40 weeks observable) | 996,163 | 2,418,056 |
| Exclude ended episodes with implausible gestation | 995,377 | 2,416,491 |
| **Analysis sample** | **995,377** | **2,416,491** |
| Ban states | 262,367 | 798,269 |
| Protected states | 733,010 | 1,618,222 |
| **Event-study sample†** | **818,245** | **2,000,319** |
| Ban states, in own cohort's window | 155,502 | 485,714 |
| Protected states | 662,743 | 1,514,605 |

*Notes:* Pregnancy episodes remaining after each restriction in the continuous-enrollment and episode-enrolled samples (Section II.D and Appendix A5). *This step excludes no episode-enrolled pregnancy. That sample requires enrollment from twelve months before the pregnancy start, and the enrollment data begin in January 2019, so no pregnancy that started in 2019 can qualify; every episode-enrolled pregnancy already starts in 2020 or later. †Distinct pregnancies in each sample used by the event studies and pooled estimates (Section III): the ban-state pregnancies conceived in their own cohort's eleven-quarter window (Table A5, quarters −6 to 4, excluding Indiana and South Carolina) and the protected-state pregnancies conceived from the second quarter of 2020 through the fourth quarter of 2023. Because a control pregnancy enters every sub-experiment whose window covers it, the stacked regression has more observations than these counts (1,607,569 in the continuous-enrollment sample for outcomes measured on all pregnancies).

Table A5—Pregnancies in Each Ban Cohort by Quarter Relative to the Ban

| Quarter from Ban | Banned at *Dobbs* | Texas | West Virginia | Indiana and South Carolina | All Ban States |
|---|---|---|---|---|---|
| First treated quarter | 2022Q3 | 2021Q4 | 2022Q4 | 2023Q4 | |
| **Pregnancies Conceived in the Quarter** | | | | | |
| −6 | 7,376 | 6,484 | 241 | 1,555 | 15,656 |
| −5 | 7,050 | 6,798 | 226 | 1,571 | 15,645 |
| −4 | 6,810 | 7,517 | 238 | 1,625 | 16,190 |
| −3 | 7,588 | 7,138 | 211 | 1,590 | 16,527 |
| −2 | 7,050 | 7,176 | 253 | 1,643 | 16,122 |
| −1 | 6,900 | 6,929 | 217 | 1,481 | 15,527 |
| 0 | 6,706 | 7,185 | 262 | 1,539 | 15,692 |
| 1 | 7,231 | 6,781 | 236 | 1,001 | 15,249 |
| 2 | 7,076 | 6,248 | 239 | – | 13,563 |
| 3 | 7,392 | 6,187 | 198 | – | 13,777 |
| 4 | 6,866 | 6,485 | 208 | – | 13,559 |
| **Total, quarters −6 to 4** | **78,045** | **74,928** | **2,529** | **12,005** | **167,507** |
| **Total, every quarter observed** | **118,684** | **113,751** | **3,854** | **26,078** | **262,367** |

*Notes:* Ban-state pregnancies in the continuous-enrollment sample by quarter of conception relative to each cohort's first fully treated quarter, the series behind Figure 1. The *Dobbs* cohort is Alabama, Arkansas, Georgia, Idaho, Kentucky, Louisiana, Mississippi, Missouri, Oklahoma, South Dakota, and Tennessee. – marks a quarter whose pregnancies would be conceived after the data end, which is also why Indiana and South Carolina are not a sub-experiment of the stacked design.

Table A6—Pre-Period Balance Between Ban and Protected States

| | Ban States Mean | Protected States Mean | Normalized Difference |
|---|---|---|---|
| **Characteristics Fixed Before Pregnancy** | | | |
| Age (years) | 29.014 | 30.352 | −0.21 |
| Under 25 | 0.267 | 0.193 | 0.18 |
| Race: White | 0.416 | 0.459 | −0.09 |
| Race: Black | 0.192 | 0.140 | 0.14 |
| Race: Asian | 0.019 | 0.039 | −0.12 |
| Race: unknown | 0.334 | 0.215 | 0.27 |
| Ethnicity: Hispanic | 0.087 | 0.238 | −0.42 |
| Ethnicity: unknown | 0.406 | 0.260 | 0.31 |
| Medicaid | 0.424 | 0.534 | −0.22 |
| Commercial insurance | 0.575 | 0.464 | 0.22 |
| First observed pregnancy | 0.257 | 0.257 | 0.00 |
| Multiple gestation | 0.020 | 0.018 | 0.01 |
| Obstetric comorbidity score | 8.744 | 8.372 | 0.03 |
| Number of comorbid conditions | 0.776 | 0.719 | 0.05 |
| Any comorbid condition | 0.437 | 0.420 | 0.03 |
| Pre-existing hypertension | 0.067 | 0.051 | 0.07 |
| Pre-existing obesity | 0.169 | 0.180 | −0.03 |
| Pre-existing drug or alcohol use disorder | 0.048 | 0.049 | −0.01 |
| Any of hypertension, obesity, or substance use | 0.236 | 0.240 | −0.01 |
| Pre-existing pulmonary hypertension | 0.000 | 0.001 | −0.01 |
| **Maternal Health** | | | |
| Any adverse pregnancy outcome | 0.286 | 0.261 | 0.06 |
| Severe maternal morbidity | 0.028 | 0.027 | 0.01 |
| Hypertensive disorder | 0.213 | 0.157 | 0.14 |
| Preterm birth (live births) | 0.173 | 0.149 | 0.06 |
| **Timing and Intensity of Care** | | | |
| Length of pregnancy, days | 225.000 | 201.728 | 0.26 |
| Days to first visit | 45.992 | 45.261 | 0.02 |
| Prolonged delivery stay (live births) | 0.233 | 0.243 | −0.02 |
| Number of visits | 16.203 | 16.316 | −0.01 |
| **Pregnancy Endings** | | | |
| Live birth | 0.793 | 0.671 | 0.28 |
| Miscarriage | 0.157 | 0.142 | 0.04 |
| Ending not observed | 0.064 | 0.049 | 0.06 |
| Insurance-billed abortion | 0.012 | 0.152 | −0.53 |

*Notes:* Pre-ban means in the continuous-enrollment sample, as proportions except for age, the comorbidity score and count, and the timing and intensity outcomes (in days and visits), and normalized differences (the difference in means divided by the square root of the average of the two variances). Live birth, miscarriage, insurance-billed abortion, the length of pregnancy, and the days to the first visit are among pregnancies with an observed ending, preterm birth and a prolonged delivery stay among live births, and the other rows among all pregnancies. Normalized differences above 0.25 in absolute value, for Hispanic ethnicity, unknown race and ethnicity, the length of pregnancy, live birth, and insurance-billed abortion, are level differences that the state fixed effects absorb.

Table A7—The Continuous-Enrollment Sample and U.S. Births

| | Continuous-Enrollment Sample | U.S. Births, 2023 |
|---|---|---|
| **Pregnancies, %** | | |
| Mean age of mother, years | 29.9 | 29.6 |
| Medicaid | 50.7 | 41.5 |
| Commercial or private insurance | 49.1 | 51.0 |
| Mother under 25 | 22.7 | 21.1 |
| White | 44.9 | 49.7 |
| Black | 15.1 | 13.7 |
| Asian | 3.3 | 6.0 |
| Hispanic | 21.3 | 26.3 |
| Race unknown | 24.4 | – |
| **Live Births, %** | | |
| Cesarean delivery | 31.0 | 32.3 |
| Gestational diabetes | 11.4 | 8.3 |

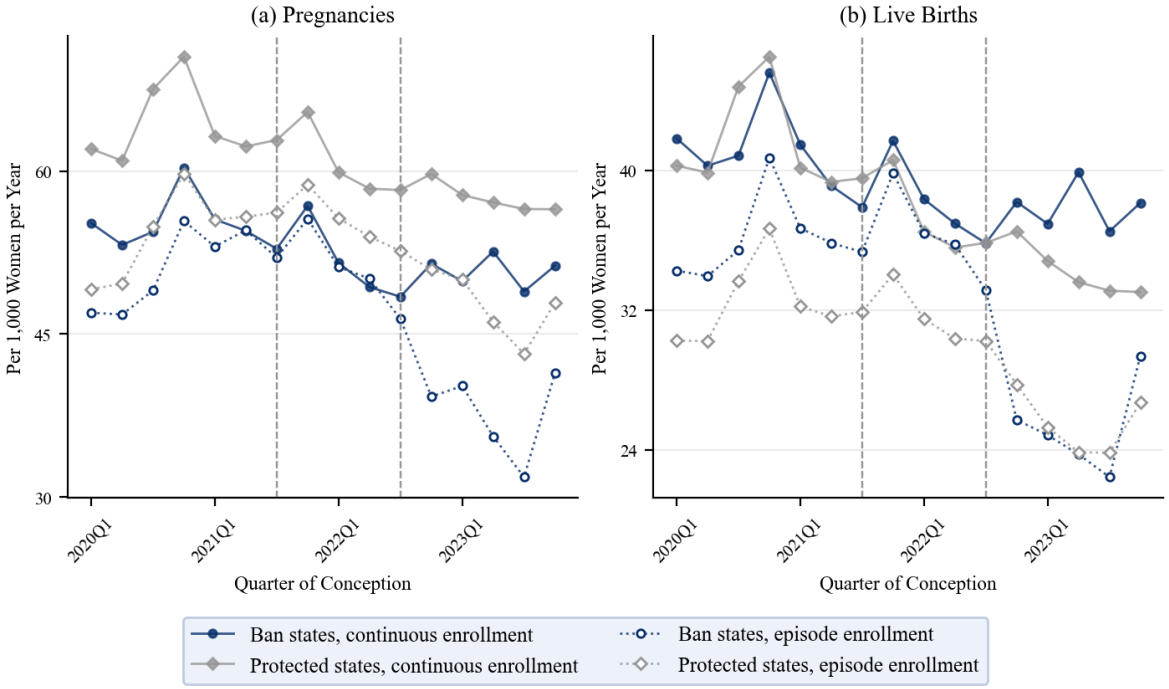
*Notes:* Column 1 pools ban and protected states in the continuous-enrollment sample for pregnancies started January 2020 to February 2024, weighting by pregnancies; its upper rows are percentages of all pregnancies and its lower rows percentages of live births. Column 2 is every U.S. birth in 2023 from the final natality data (Osterman et al., 2025), with mean age from Brown et al. (2025); race and Hispanic origin refer to non-Hispanic single-race mothers and Hispanic mothers of any race, and payer is the source of payment for the delivery. Mean age is in years, and – marks a row with no counterpart in the natality data. Race is unknown for many pregnancies in the claims (the race unknown row), so the race rows are not strictly comparable, and birth certificates miss many complications that claims record (Bell, Anjur-Dietrich and Gemmill, 2026), so the gestational diabetes rows also differ in how they are measured.

### *A5. Comparison with the Episode-Enrolled Sample*

The episode-enrolled sample requires enrollment from twelve months before the pregnancy start through three months after the ending, so it keeps women who move between insurers or plans outside their pregnancies; it contains 2,416,491 pregnancies. The main text does not use it, because its composition changes over the period for reasons the bans likely did not cause. First, insurers enter and leave the data, and because plans can be regional, each entry or exit can shift who is in the sample. Second, its requirement of coverage through three months after each pregnancy ends means that its membership after mid-2022 is set by the end of the pandemic-era Medicaid continuous-enrollment provision on March 31, 2023, after which states began removing women from Medicaid (Tolbert and Ammula, 2023), and in this sample the resulting loss of members is concentrated in ban states. Between conceptions in the second quarter of 2022 and the third quarter of 2023, the number of episode-enrolled pregnancies changes by −42 percent in ban states and −22 percent in protected states, and the Medicaid share of ban-state pregnancies in that sample falls from 61.5 to 45.9 percent, while the protected-state share holds near 60.4 percent. Over the same quarters the continuous-enrollment sample changes by 2 and −2 percent. Figure A1 shows the birth and pregnancy rates in the episode-enrolled sample, where the ban-state birth rate falls to the protected-state rate after 2022, as the end of Medicaid continuous enrollment would predict

in a sample whose membership it sets. Table A8 reports the difference-in-differences estimates in both samples under the Controls specification, and Figure A2 overlays their event studies. The two samples are qualitatively similar and lead to the same conclusions.

Figure A1. Pregnancy and Birth Rates in the Two Enrollment Samples



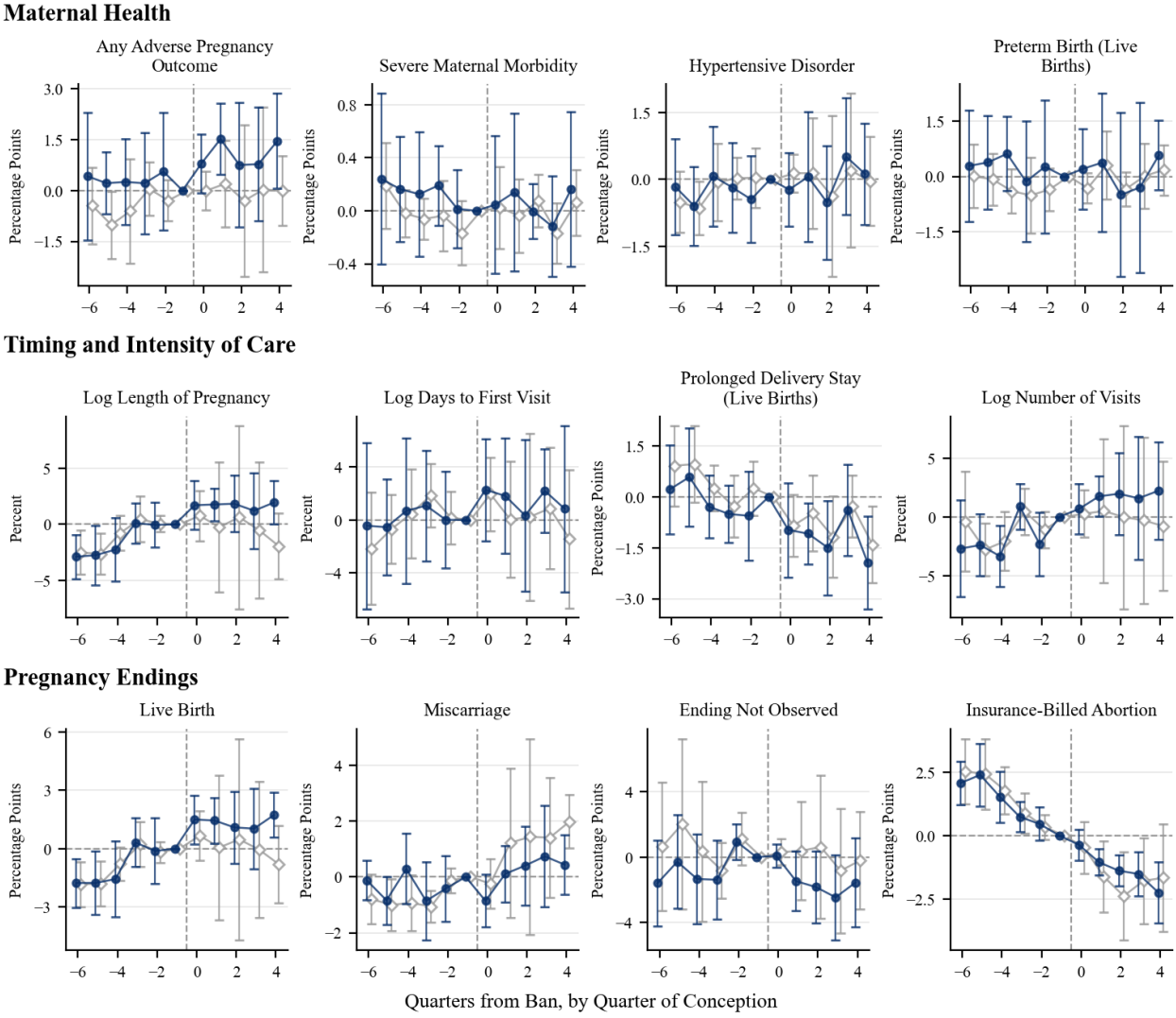

*Notes:* Pregnancies and live births per 1,000 enrolled women aged 15 to 45 per year, by quarter of conception, in ban and protected states, in the continuous-enrollment sample (solid lines, filled markers, as in Figure 5) and the episode-enrolled sample (dotted lines, hollow markers). Monthly rates are averaged to quarters. Ban states are circles and protected states diamonds. Dashed lines mark the third quarters of 2021 (Texas Senate Bill 8) and 2022 (*Dobbs*).

Table A8—Estimates by Enrollment Sample

| Outcome | Pre-Ban Mean (1) | Episode-Enrolled Sample: Estimate (SE) (2) | Episode-Enrolled Sample: Percent of Mean (3) | Episode-Enrolled Sample: 95% CI, Percent of Mean (4) | Continuous-Enrollment Sample: Estimate (SE) (5) | Continuous-Enrollment Sample: Percent of Mean (6) |
|---|---|---|---|---|---|---|
| **Maternal Health** | | | | | | |
| Any adverse pregnancy outcome | 0.278 | 0.000 (0.007) | −0.1 | [−4.8, 4.7] | 0.011 (0.005) | 3.7 |
| Severe maternal morbidity | 0.027 | 0.000 (0.001) | −0.3 | [−8.5, 7.9] | 0.000 (0.002) | 1.7 |
| Hypertensive disorder | 0.202 | 0.000 (0.005) | 0.1 | [−5.2, 5.4] | 0.000 (0.005) | −0.1 |
| Preterm birth (live births) | 0.182 | 0.000 (0.002) | −0.3 | [−2.8, 2.2] | 0.001 (0.007) | 0.4 |
| **Timing and Intensity of Care** | | | | | | |
| Log length of pregnancy | 5.242 | −0.002 (0.022) | −0.2 | [−4.6, 4.3] | 0.017 (0.009) | 1.7 |
| Log days to first visit | 3.676 | 0.004 (0.021) | 0.4 | [−3.9, 4.8] | 0.015 (0.022) | 1.5 |
| Prolonged delivery stay (live births) | 0.246 | −0.008 (0.005) | −3.3 | [−7.2, 0.5] | −0.012 (0.005) | −5.0 |
| Log number of visits | 2.397 | 0.000 (0.022) | 0.0 | [−4.5, 4.5] | 0.016 (0.014) | 1.6 |
| **Pregnancy Endings** | | | | | | |
| Live birth | 0.772 | 0.001 (0.014) | 0.2 | [−3.4, 3.7] | 0.014 (0.006) | 1.7 |
| Miscarriage | 0.170 | 0.011 (0.009) | 6.3 | [−4.0, 16.6] | 0.002 (0.005) | 1.0 |
| Ending not observed | 0.097 | 0.001 (0.014) | 1.0 | [−27.7, 29.7] | −0.014 (0.008) | −23.3 |
| Insurance-billed abortion | 0.012 | −0.015 (0.008) | −123.9 | [−246.3, −1.6] | −0.013 (0.003) | −123.6 |

*Notes:* Stacked difference-in-differences estimates in the Controls specification, with standard errors (SE), clustered by state, in parentheses. Columns 1 to 4 use the episode-enrolled sample (Appendix A5). Column 1 is its ban-state mean in event quarters −6 to −1; columns 1, 2, and 5 are in each outcome's units. Column 3 is the estimate as a percent of the pre-ban mean (100 times the coefficient for outcomes in logs), and column 4 its 95 percent confidence interval (CI). Columns 5 and 6 repeat columns 2 and 3 of Table 2.

Figure A2. Event Studies, Continuous-Enrollment and Episode-Enrolled Samples




*Notes:* Stacked event-study coefficients in the continuous-enrollment sample (filled markers, as in Figure 2) and the episode-enrolled sample (hollow markers), in the Controls specification, with 95 percent confidence intervals from standard errors clustered by state. Quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0.

*A6. Subgroup Estimates*

Table A9—Subgroup Estimates for the Pregnancy Outcomes

| Outcome | All Pregnancies (1) | Medicaid (2) | Minority (3) | High Risk (4) | Under 25 (5) | Excluding Texas (6) |
|---|---|---|---|---|---|---|
| **Maternal Health** | | | | | | |
| Any adverse pregnancy outcome | 3.7 (1.9) | 4.4 (3.7) | 5.7 (4.2) | 4.1 (1.9) | 5.0 (3.9) | 2.7 (2.9) |
| | [−0.2, 7.6] | [−3.0, 11.8] | [−2.7, 14.2] | [0.2, 8.0] | [−2.9, 13.0] | [−3.2, 8.7] |
| Pre-ban mean | 0.285 | 0.322 | 0.313 | 0.315 | 0.286 | 0.295 |
| Severe maternal morbidity | 1.7 (6.8) | 10.7 (10.0) | 7.6 (7.7) | 12.1 (5.7) | 3.2 (11.6) | 13.1 (7.6) |
| | [−12.0, 15.4] | [−9.5, 30.9] | [−8.0, 23.2] | [0.5, 23.7] | [−20.2, 26.6] | [−2.3, 28.6] |
| Pre-ban mean | 0.026 | 0.035 | 0.036 | 0.037 | 0.026 | 0.029 |
| Hypertensive disorder | −0.1 (2.5) | −2.6 (4.4) | −1.3 (4.8) | −1.9 (3.6) | −3.3 (4.3) | −2.0 (4.1) |
| | [−5.1, 5.0] | [−11.6, 6.4] | [−11.1, 8.5] | [−9.2, 5.4] | [−12.0, 5.4] | [−10.4, 6.4] |
| Pre-ban mean | 0.208 | 0.247 | 0.238 | 0.264 | 0.203 | 0.234 |
| Preterm birth (live births) | 0.4 (4.3) | −4.2 (3.2) | −1.8 (4.9) | −1.5 (3.9) | −2.0 (4.3) | −4.6 (4.6) |
| | [−8.3, 9.2] | [−10.7, 2.3] | [−11.9, 8.2] | [−9.5, 6.5] | [−10.7, 6.7] | [−14.0, 4.8] |
| Pre-ban mean | 0.171 | 0.223 | 0.218 | 0.220 | 0.172 | 0.195 |
| **Timing and Intensity of Care** | | | | | | |
| Log length of pregnancy | 1.7 (0.9) | 1.3 (1.8) | 2.5 (2.8) | 1.4 (1.2) | 1.2 (1.6) | 0.7 (1.5) |
| | [−0.1, 3.5] | [−2.3, 4.9] | [−3.1, 8.1] | [−1.1, 3.9] | [−2.1, 4.5] | [−2.4, 3.7] |
| Pre-ban mean | 5.272 | 5.255 | 5.223 | 5.234 | 5.315 | 5.262 |
| Log days to first visit | 1.5 (2.2) | 0.7 (2.7) | 1.5 (2.2) | 0.5 (2.3) | 2.2 (2.7) | 0.0 (2.3) |
| | [−2.9, 5.9] | [−4.9, 6.2] | [−2.9, 6.0] | [−4.2, 5.2] | [−3.2, 7.6] | [−4.6, 4.7] |
| Pre-ban mean | 3.616 | 3.616 | 3.643 | 3.490 | 3.635 | 3.604 |
| Prolonged delivery stay (live births) | −5.0 (2.3) | −2.4 (3.2) | −6.1 (4.3) | −2.0 (3.4) | −8.7 (2.8) | −2.9 (2.9) |
| | [−9.6, −0.4] | [−8.9, 4.2] | [−14.9, 2.7] | [−9.0, 5.0] | [−14.4, −2.9] | [−8.8, 3.0] |
| Pre-ban mean | 0.233 | 0.261 | 0.265 | 0.246 | 0.303 | 0.247 |
| Log number of visits | 1.6 (1.4) | 1.0 (3.1) | 1.6 (3.9) | 1.2 (2.0) | 2.0 (3.1) | 1.1 (2.6) |
| | [−1.2, 4.5] | [−5.2, 7.2] | [−6.3, 9.6] | [−2.8, 5.2] | [−4.3, 8.4] | [−4.2, 6.4] |
| Pre-ban mean | 2.496 | 2.566 | 2.465 | 2.621 | 2.535 | 2.505 |
| **Pregnancy Endings** | | | | | | |
| Live birth | 1.7 (0.8) | 1.8 (1.6) | 2.4 (2.5) | 1.5 (1.1) | 1.9 (1.5) | 0.9 (1.3) |
| | [0.2, 3.2] | [−1.4, 5.0] | [−2.7, 7.4] | [−0.7, 3.6] | [−1.0, 4.9] | [−1.7, 3.6] |
| Pre-ban mean | 0.789 | 0.785 | 0.763 | 0.771 | 0.819 | 0.787 |
| Miscarriage | 1.0 (3.0) | −0.5 (5.2) | 0.6 (7.6) | 0.9 (3.8) | −0.2 (6.7) | 3.6 (4.5) |
| | [−5.1, 7.0] | [−11.0, 10.0] | [−14.9, 16.0] | [−6.7, 8.6] | [−13.8, 13.5] | [−5.5, 12.8] |
| Pre-ban mean | 0.162 | 0.155 | 0.171 | 0.172 | 0.133 | 0.157 |
| Ending not observed | −23.3 (13.1) | −16.9 (19.7) | −18.7 (14.0) | −27.8 (17.7) | −16.1 (18.1) | −11.3 (14.3) |
| | [−49.7, 3.2] | [−56.8, 23.1] | [−47.2, 9.8] | [−63.6, 8.1] | [−52.7, 20.5] | [−40.3, 17.6] |
| Pre-ban mean | 0.062 | 0.067 | 0.071 | 0.076 | 0.055 | 0.067 |
| Insurance-billed abortion | −123.6 (31.9) | −90.7 (58.0) | −110.2 (37.5) | −102.6 (39.0) | −122.6 (49.6) | −97.7 (41.2) |
| | [−188.3, −58.9] | [−208.4, 26.9] | [−186.4, −34.1] | [−181.7, −23.6] | [−223.0, −22.2] | [−181.3, −14.1] |
| Pre-ban mean | 0.011 | 0.011 | 0.015 | 0.011 | 0.012 | 0.012 |

*Notes:* Stacked difference-in-differences estimates in the Controls specification and the continuous-enrollment sample, each as a percent of the subgroup's pre-ban mean (100 times the coefficient for outcomes in logs), with the standard error, clustered by state, in parentheses, the 95 percent confidence interval in brackets, and the group's ban-state mean in event quarters −6 to −1, in the outcome's units, beneath. The All Pregnancies column repeats Table 2, and the subgroups are defined as in Figure 3.

Table A10—Maternal Health and Care within Live Births and within Losses

| Outcome | Pre-Ban Mean (1) | Estimate (SE) (2) | Percent of Mean (3) | 95% CI, Percent of Mean (4) |
|---|---|---|---|---|
| **A. Live Births** | | | | |
| Any adverse pregnancy outcome | 0.369 | 0.003 (0.005) | 0.8 | [−2.0, 3.6] |
| Severe maternal morbidity | 0.030 | −0.002 (0.003) | −5.8 | [−23.8, 12.1] |
| Hypertensive disorder | 0.262 | −0.008 (0.004) | −3.0 | [−6.1, 0.2] |
| Preterm birth (live births) | 0.171 | 0.001 (0.007) | 0.4 | [−8.3, 9.2] |
| Log length of pregnancy | 5.579 | −0.002 (0.001) | −0.2 | [−0.5, 0.1] |
| Log days to first visit | 3.653 | 0.017 (0.024) | 1.7 | [−3.2, 6.6] |
| Prolonged delivery stay (live births) | 0.233 | −0.012 (0.005) | −5.0 | [−9.6, −0.4] |
| Log number of visits | 2.864 | −0.016 (0.012) | −1.6 | [−4.0, 0.7] |
| **B. Losses** | | | | |
| Any adverse pregnancy outcome | 0.022 | 0.001 (0.005) | 4.0 | [−39.5, 47.6] |
| Severe maternal morbidity | 0.014 | 0.003 (0.002) | 19.5 | [−14.2, 53.1] |
| Hypertensive disorder | 0.035 | −0.006 (0.002) | −17.7 | [−31.9, −3.5] |
| Log length of pregnancy | 4.113 | 0.012 (0.007) | 1.2 | [−0.2, 2.5] |
| Log days to first visit | 3.464 | 0.026 (0.014) | 2.6 | [−0.2, 5.4] |
| Log number of visits | 1.338 | −0.029 (0.016) | −2.9 | [−6.0, 0.2] |

*Notes:* Stacked difference-in-differences estimates in the Controls specification and the continuous-enrollment sample, among pregnancies that ended in a live birth (Panel A) and among pregnancies that ended in a miscarriage, stillbirth, or ectopic pregnancy (Panel B). Column 1 is the group's ban-state mean in event quarters −6 to −1 and column 2 the estimate, both in each outcome's units, with its standard error (SE), clustered by state, in parentheses. Column 3 is the estimate as a percent of that mean (100 times the coefficient for outcomes in logs) and column 4 its 95 percent confidence interval (CI). Preterm birth and a prolonged delivery stay are defined only for live births, and the ending outcomes are not estimated, because each group fixes the ending. Because the bans changed how pregnancies ended, these estimates condition on an outcome of the bans, so they describe how the pooled estimates divide across endings rather than identify effects within each.

### *A7. Components of the Adverse-Outcome Composite*

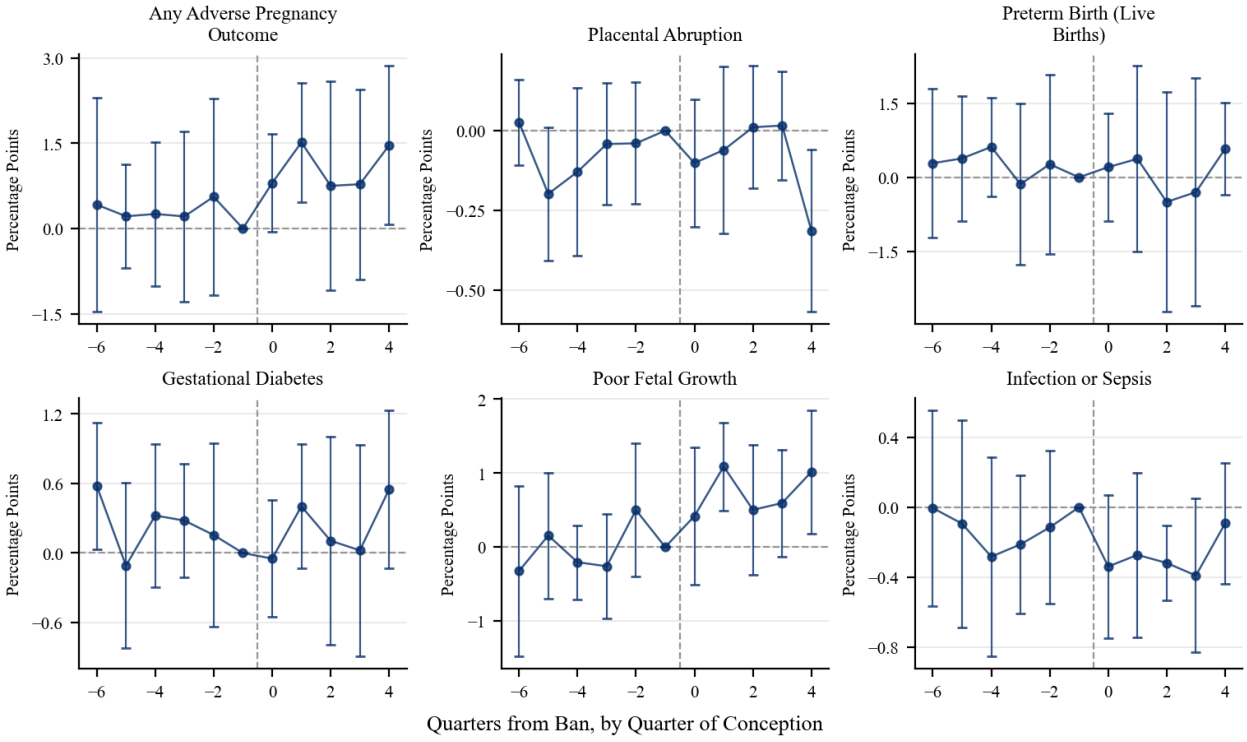
The adverse-outcome composite equals one if a pregnancy has any of five diagnoses (Section II.E). Table A11 estimates each on its own and Figure A3 shows their event studies. Hypertensive disorders, reported in Table 2, are not part of the composite.

Table A11—The Adverse-Outcome Composite and Its Components

| Outcome | Pre-Ban Mean (1) | Controls Estimate (SE) (2) | Controls Percent of Mean (3) | Controls 95% CI, Percent of Mean (4) | Raw Estimate (SE) (5) | Raw Percent of Mean (6) |
|---|---|---|---|---|---|---|
| **Composite** | | | | | | |
| Any adverse pregnancy outcome | 0.285 | 0.011 (0.005) | 3.7 | [−0.2, 7.6] | 0.012 (0.005) | 4.1 |
| **Components** | | | | | | |
| Placental abruption | 0.0117 | −0.0009 (0.0008) | −7.6 | [−22.2, 6.9] | −0.0009 (0.0008) | −7.5 |
| Preterm birth (live births) | 0.171 | 0.001 (0.007) | 0.4 | [−8.3, 9.2] | 0.001 (0.007) | 0.4 |
| Gestational diabetes | 0.094 | 0.002 (0.003) | 2.2 | [−3.7, 8.1] | 0.002 (0.003) | 1.6 |
| Poor fetal growth | 0.101 | 0.007 (0.003) | 7.1 | [1.2, 13.1] | 0.008 (0.003) | 8.0 |
| Infection or sepsis | 0.0240 | −0.0028 (0.0011) | −11.8 | [−21.5, −2.1] | −0.0025 (0.0011) | −10.4 |

*Notes:* Stacked difference-in-differences estimates in the continuous-enrollment sample, with fixed effects and weights as in Table 2 and laid out as that table without its pre-trend test. The composite equals one if a pregnancy has any of the five components below it. Preterm birth is measured among live births. Poor fetal growth is diagnosed only in pregnancies that continue, and in one validation study about a third of claims diagnoses of a small-for-gestational-age infant were confirmed in medical records (Chomistek et al., 2023).

Figure A3. Event Studies for the Adverse-Outcome Composite and Its Components




*Notes:* Stacked difference-in-differences event-study coefficients by quarter of conception relative to the ban, with 95 percent confidence intervals from standard errors clustered by state, in the Controls specification and the continuous-enrollment sample, for the composite (first panel, as in Figure 2) and its five components. Coefficients are in percentage points (of live births for preterm birth). Quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0.

Table A12—Fertility: Pregnancy and Live-Birth Rates among Enrolled Women

| Group | Pre-Ban Mean (1) | State Fixed Effects | | | Raw | |
|---|---|---|---|---|---|---|
| | | Estimate (SE) (2) | Percent of Mean (3) | 95% CI, Percent of Mean (4) | Estimate (SE) (5) | Percent of Mean (6) |
| **Pregnancy Rate** | | | | | | |
| All | 52.09 | 0.05 (1.11) | 0.1 | [−4.2, 4.4] | −0.44 (1.29) | −0.8 |
| Medicaid | 67.82 | 0.82 (1.31) | 1.2 | [−2.7, 5.1] | −0.60 (2.07) | −0.9 |
| Minority | 63.12 | −1.37 (1.33) | −2.2 | [−6.4, 2.1] | −2.69 (1.65) | −4.3 |
| High risk[a] | 53.17 | −0.37 (1.09) | −0.7 | [−4.8, 3.5] | −1.16 (1.39) | −2.2 |
| Under 25 | 41.06 | 1.02 (0.82) | 2.5 | [−1.6, 6.5] | −0.14 (0.70) | −0.4 |
| Excluding Texas | 52.30 | 0.88 (0.63) | 1.7 | [−0.7, 4.1] | 0.96 (1.03) | 1.8 |
| **Live-Birth Rate** | | | | | | |
| All | 38.50 | 1.31 (0.75) | 3.4 | [−0.5, 7.3] | 0.94 (0.84) | 2.4 |
| Medicaid | 49.04 | 2.38 (1.44) | 4.8 | [−1.1, 10.8] | 1.16 (1.84) | 2.4 |
| Minority | 43.73 | 1.03 (1.09) | 2.4 | [−2.7, 7.4] | 0.05 (1.26) | 0.1 |
| High risk[a] | 38.11 | 1.31 (0.72) | 3.4 | [−0.4, 7.3] | 0.64 (0.86) | 1.7 |
| Under 25 | 31.57 | 1.70 (0.83) | 5.4 | [0.0, 10.7] | 0.76 (0.72) | 2.4 |
| Excluding Texas | 38.62 | 1.28 (0.92) | 3.3 | [−1.5, 8.1] | 1.34 (1.08) | 3.5 |

*Notes:* Stacked difference-in-differences (DiD) estimates on the panel of enrolled women aged 15 to 45 by state and quarter, weighted by enrolled women, with standard errors (SE), clustered by state, in parentheses. State Fixed Effects has event-time and sub-experiment by state fixed effects; Raw has event-time fixed effects and a treated-state indicator. Rates are pregnancies or live births per 1,000 enrolled women per year (conceptions in the state and quarter divided by woman-months of enrollment, times 12,000), so they do not condition on who becomes pregnant. Column 1 is the mean in treated states before the ban; columns 1, 2, and 5 are in rates. Column 3 is the estimate as a percent of that mean, and column 4 its 95 percent confidence interval (CI). Unlike the pregnancy estimates, this stack keeps Indiana and South Carolina as a fourth sub-experiment, because rates need no pregnancy to be followed to its ending, so it has 40 clusters. Minority women have a Black race code or a Hispanic ethnicity code. [a]High-risk women have any condition of the obstetric comorbidity index in the preceding 12 months.

### *A8. Composition of the Pregnant Population*

The decomposition in Section VI.B is a reweighting decomposition in the spirit of DiNardo, Fortin and Lemieux (1996). Within each group of states, I estimate the probability $\hat{p}$ that a pregnancy is observed in the post-period as a function of its pre-pregnancy characteristics $X$ (the comorbidity conditions and other conditions measured in the year before conception, age, race and ethnicity, payer and plan type, and first observed pregnancy), and reweight post-period pregnancies by $(1-\hat{p})/\hat{p}$ so that their baseline risk matches the pre-period. The difference between the observed and reweighted post-period means is the part of the change due to composition, and the difference between the reweighted post-period mean and the pre-period mean is the part due to care conditional on baseline risk. I difference each part across ban and protected states, with the protected-state post-period beginning in June 2022, and compute standard errors by a block bootstrap over states with 200 draws. Because the decomposition compares all pregnancies before the bans with all pregnancies after them, rather than each cohort with its controls around its own ban, its totals differ from the stacked estimates. For severe maternal morbidity, for example, its total is −10.8 percent of the mean, against a stacked estimate of 1.7 percent. I therefore use it only to

split each change into a part due to composition and a part due to care, not to measure either.

Table A13—Composition of the Pregnant Population by Pre-Pregnancy Health

| Characteristic | Pre-Ban Mean (1) | Raw | | |
|---|---|---|---|---|
| | | Estimate (SE) (2) | Percent of Mean (3) | 95% CI, Percent of Mean (4) |
| Obstetric comorbidity score | 8.387 | −0.164 (0.167) | −1.9 | [−6.0, 2.1] |
| Number of obstetric comorbidities | 0.741 | −0.007 (0.014) | −0.9 | [−4.8, 2.9] |
| Any obstetric comorbidity | 0.423 | −0.005 (0.003) | −1.1 | [−2.6, 0.5] |

*Notes:* Stacked difference-in-differences (DiD) estimates in the continuous-enrollment sample for pre-pregnancy health measures, with standard errors (SE), clustered by state, in parentheses. The columns are as under Controls in Table 2 without the pre-trend test, but the estimates use the Raw specification because the comorbidity score is itself a control. A ban cannot change these measures for any one woman, so a nonzero estimate means that the set of women who became pregnant changed. Unlike the decomposition in Supplemental Appendix Table A14, which asks how much that change moved each outcome, this table asks whether the pregnant population changed at all. The comorbidity score weights conditions by the points of Leonard et al. (2020), as in the regression controls and Table 1. The Medicaid and under-25 shares are in Section VI.B.

Table A14—Decomposition of Outcome Changes into Composition and Care

| Outcome | Pre-Ban Mean (1) | Change, Percent of Pre-Ban Mean | | |
|---|---|---|---|---|
| | | Total (2) | Composition (3) | Structure (4) |
| **Maternal Health** | | | | |
| Any adverse pregnancy outcome | 0.286 | 4.2 (2.4) | −0.5 (1.5) | 4.7 (1.8) |
| Severe maternal morbidity | 0.028 | −10.8 (4.1) | −3.2 (2.7) | −7.5 (4.5) |
| Hypertensive disorder | 0.213 | −3.1 (3.1) | −1.0 (2.6) | −2.1 (1.7) |
| Preterm birth (live births) | 0.173 | −4.1 (3.4) | −3.0 (2.5) | −1.0 (1.8) |
| **Timing and Intensity of Care** | | | | |
| Log length of pregnancy | 5.276 | 5.1 (0.9) | 1.4 (0.4) | 3.7 (0.9) |
| Log days to first visit | 3.637 | 1.6 (2.4) | 0.1 (0.7) | 1.5 (2.0) |
| Prolonged delivery stay (live births) | 0.233 | −3.3 (3.2) | −0.3 (1.4) | −3.0 (2.6) |
| Log number of visits | 2.491 | 4.7 (2.4) | 0.8 (1.4) | 3.8 (2.0) |
| **Pregnancy Endings** | | | | |
| Live birth | 0.793 | 4.3 (0.8) | 1.1 (0.4) | 3.2 (0.8) |
| Miscarriage | 0.157 | 2.9 (3.1) | 0.5 (1.5) | 2.4 (2.9) |
| Ending not observed | 0.064 | −23.6 (12.2) | −4.0 (1.5) | −19.6 (11.7) |
| Insurance-billed abortion | 0.012 | −314.2 (62.0) | −69.8 (12.1) | −244.4 (60.0) |

*Notes:* Reweighting decomposition (Section VI.B) in the continuous-enrollment sample, as plotted in Figure 6. Column 1 covers every pre-ban pregnancy, as in Table 1, rather than event quarters −6 to −1. The total change, the before-to-after change in ban states minus that in protected states, is split into composition (pre-pregnancy characteristics) and structure (outcomes conditional on those characteristics, the care component). Each is a percent of the ban-state pre-ban mean (100 times the change for outcomes in logs), with block-bootstrap standard errors over states (200 draws) in parentheses.

### *A9. The Stacked Design and Two-Way Fixed Effects*

The event-study sample is the continuous-enrollment pregnancies conceived in the eleven quarters around each cohort's ban, from the second quarter of 2020 through the fourth quarter of 2022 for

Texas, from the first quarter of 2021 through the third quarter of 2023 for the *Dobbs* cohort, and from the second quarter of 2021 through the fourth quarter of 2023 for West Virginia. Table A4 reports the number of distinct pregnancies in this event study sample, 818,245, and Table A15 the pregnancies in each sub-experiment. Because a control pregnancy enters every sub-experiment whose window covers it, the stacked regression has more observations than distinct pregnancies.

Table A15—Sub-Experiments in the Stacked Design

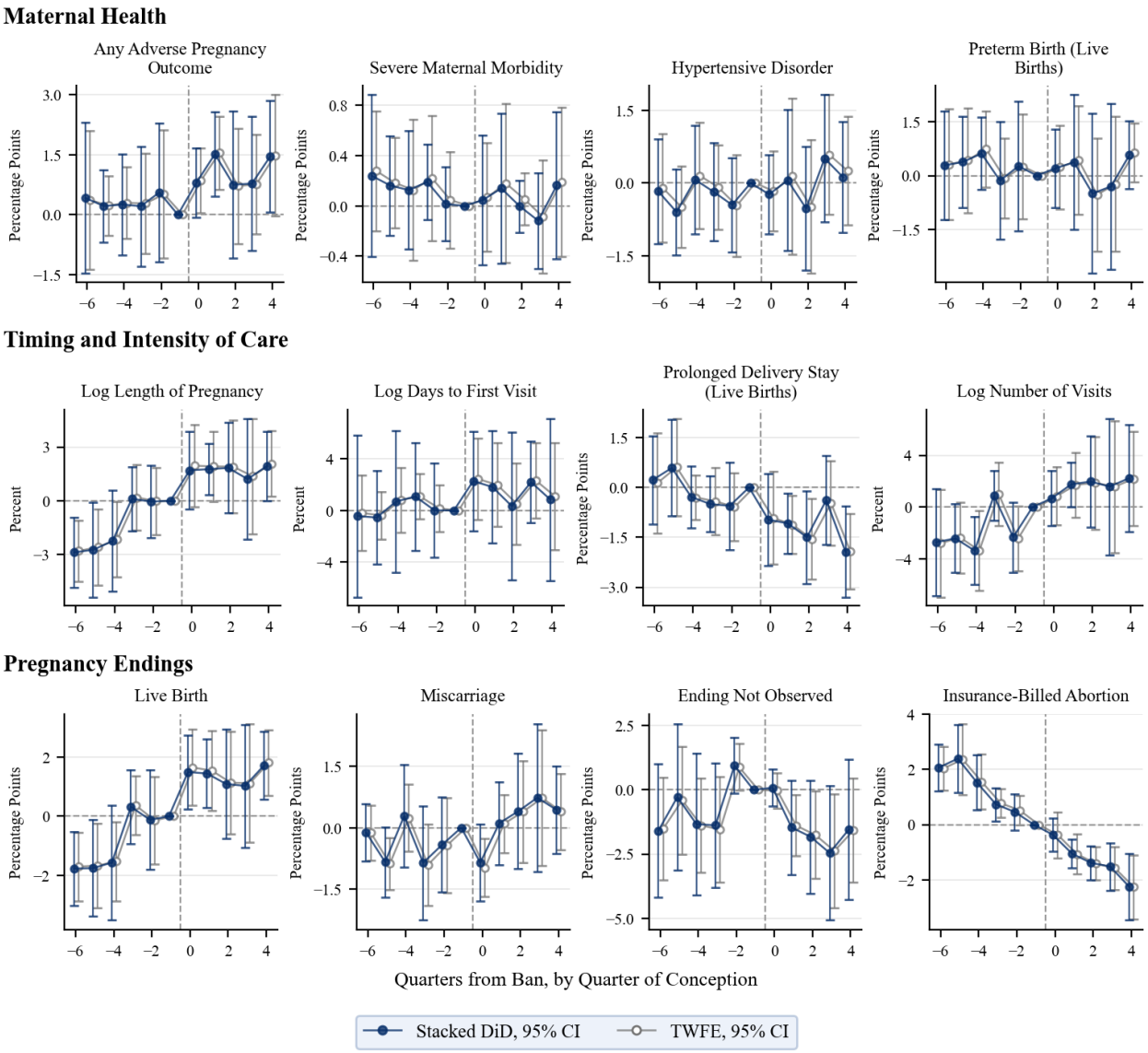
| First Treated Quarter | Treated States | Treated Pregnancies | Control Pregnancies | Share of Treated Pregnancies, % |
|---|---|---|---|---|
| 2021Q4 | TX | 74,928 | 493,967 | 48.2 |
| 2022Q3 | AL, AR, GA, ID, KY, LA, MO, MS, OK, SD, TN | 78,045 | 480,762 | 50.2 |
| 2022Q4 | WV | 2,529 | 477,338 | 1.6 |

*Notes:* One row per ban cohort, defined by its first fully treated quarter, in the continuous-enrollment sample. Each sub-experiment pairs the cohort with the protected states over its event window, quarters −6 to 4. Treated pregnancies are the cohort's pregnancies conceived in the window, and control pregnancies are the protected-state pregnancies conceived in it, so a protected-state pregnancy is counted in every sub-experiment whose window covers it. The last column is each cohort's share of the treated pregnancies in the stacked design. The window of Indiana and South Carolina runs past the data, so they are not a sub-experiment (Table A5).

### Reference quarter

The pooled estimates in Table 2 compare the post-ban quarters with quarter −1 alone, because the pre-period interactions stay in the pooled regression (Section III). Pregnancies conceived in quarter −1 were still early when the bans took effect, and one quarter is a noisy baseline, so Table A16 repeats the estimates against quarter −2. The post-ban quarters are the same, so each estimate moves only by the difference between the two reference quarters. The suggestion of harm in the composite of adverse pregnancy outcomes weakens further. It falls from 3.7 to 1.8 percent of the pre-ban mean ($p = 0.40$), with an interval that rules out increases larger than 5.9 percent, and its pre-period coefficients do not jointly differ from zero ($p = 0.98$). Severe maternal morbidity changes by 1.2 percent, with an interval that rules out increases larger than 12.1 percent, and preterm birth by −1.1 percent. Hypertensive disorders move the other way, to 2.1 percent ($p = 0.06$), and the fall in prolonged delivery stays shrinks to 2.6 percent ($p = 0.21$). The number of visits rises 4.0 percent ($p = 0.001$), although its pre-period coefficients jointly differ from zero.

Figure A4. Event Studies, Stacked and Two-Way Fixed Effects




*Notes:* Stacked difference-in-differences event-study coefficients from Figure 2 (filled markers) and two-way fixed effects (TWFE) coefficients for the same outcome (hollow markers), in the Controls specification and the continuous-enrollment sample, with 95 percent confidence intervals from standard errors clustered by state. The TWFE regression uses the pregnancies of the stacked design, with state and quarter-of-conception fixed effects and the Controls covariates, each pregnancy entering once and unweighted. Quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0.

Table A16—Stacked Estimates by Reference Quarter

| Outcome | Pre-Ban Mean (1) | Reference Quarter −2 | | | Reference Quarter −1 | |
|---|---|---|---|---|---|---|
| | | Estimate (SE) (2) | Percent of Mean (3) | 95% CI, Percent of Mean (4) | Estimate (SE) (5) | Percent of Mean (6) |
| **Maternal Health** | | | | | | |
| Any adverse pregnancy outcome | 0.285 | 0.005 (0.006) | 1.8 | [−2.4, 5.9] | 0.011 (0.005) | 3.7 |
| Severe maternal morbidity | 0.026 | 0.000 (0.001) | 1.2 | [−9.7, 12.1] | 0.000 (0.002) | 1.7 |
| Hypertensive disorder | 0.208 | 0.004 (0.002) | 2.1 | [−0.1, 4.2] | 0.000 (0.005) | −0.1 |
| Preterm birth (live births) | 0.171 | −0.002 (0.003) | −1.1 | [−5.1, 2.9] | 0.001 (0.007) | 0.4 |
| **Timing and Intensity of Care** | | | | | | |
| Log length of pregnancy | 5.272 | 0.017 (0.006) | 1.7 | [0.5, 3.0] | 0.017 (0.009) | 1.7 |
| Log days to first visit | 3.616 | 0.015 (0.016) | 1.5 | [−1.7, 4.7] | 0.015 (0.022) | 1.5 |
| Prolonged delivery stay (live births) | 0.233 | −0.006 (0.005) | −2.6 | [−6.8, 1.6] | −0.012 (0.005) | −5.0 |
| Log number of visits | 2.496 | 0.040 (0.011) | 4.0 | [1.7, 6.2] | 0.016 (0.014) | 1.6 |
| **Pregnancy Endings** | | | | | | |
| Live birth | 0.789 | 0.015 (0.004) | 1.9 | [0.8, 3.0] | 0.014 (0.006) | 1.7 |
| Miscarriage | 0.162 | 0.006 (0.005) | 3.6 | [−2.9, 10.0] | 0.002 (0.005) | 1.0 |
| Ending not observed | 0.062 | −0.024 (0.012) | −38.6 | [−76.5, −0.7] | −0.014 (0.008) | −23.3 |
| Insurance-billed abortion | 0.011 | −0.018 (0.005) | −166.9 | [−271.3, −62.5] | −0.013 (0.003) | −123.6 |

*Notes:* Stacked difference-in-differences estimates in the Controls specification and the continuous-enrollment sample, laid out as Table 2 without its pre-trend test, with standard errors (SE), clustered by state, in parentheses. Columns 2 to 4 use quarter −2 as the reference, and columns 5 and 6 repeat columns 2 and 3 of Table 2, with quarter −1 as the reference. The post-ban quarters are the same in both.

### *A10. Synthetic Difference-in-Differences*

If protected states are a poor comparison for ban states, the stacked estimates could reflect differences in trends rather than the bans. Synthetic difference-in-differences (Arkhangelsky et al., 2021) addresses this by choosing weights on protected states and on pre-ban quarters so that the weighted protected-state path matches the ban states' path before the bans. I estimate it on state by conception-quarter means, with 38 states, 13 of them treated, and 16 quarters, and placebo standard errors (Table A17). Insured abortion falls 213.9 percent of the pre-ban mean ($p = 0.01$), and no maternal health estimate points toward harm. Severe maternal morbidity changes by −4.3 percent (95 percent confidence interval −32.6 to 24.0 percent) and the adverse-outcome composite by 2.0 percent (−7.7 to 11.7 percent). Because it uses state means rather than pregnancies, its intervals are wider than the stacked ones, and it does not distinguish the rises in live births and in the composite from zero.

Table A17—Stacked and Synthetic Difference-in-Differences Estimates

| | | Synthetic DiD, Controls | | | Synthetic DiD, Raw | | Stacked DiD, |
|---|---|---|---|---|---|---|---|
| **Outcome** | **Pre-Ban Mean (1)** | **Estimate (SE) (2)** | **Percent of Mean (3)** | **95% CI, Percent of Mean (4)** | **Estimate (SE) (5)** | **Percent of Mean (6)** | **Controls, Percent of Mean (7)** |
| **Maternal Health** | | | | | | | |
| Any adverse pregnancy outcome | 0.285 | 0.006 (0.014) | 2.0 | [−7.7, 11.7] | 0.007 (0.014) | 2.4 | 3.7 |
| Severe maternal morbidity | 0.026 | −0.001 (0.004) | −4.3 | [−32.6, 24.0] | −0.001 (0.004) | −4.1 | 1.7 |
| Hypertensive disorder | 0.208 | 0.001 (0.011) | 0.3 | [−9.9, 10.6] | 0.002 (0.011) | 1.0 | −0.1 |
| Preterm birth (live births) | 0.171 | −0.012 (0.013) | −7.1 | [−21.5, 7.4] | −0.008 (0.011) | −4.9 | 0.4 |
| **Timing and Intensity of Care** | | | | | | | |
| Log length of pregnancy | 5.272 | 0.009 (0.019) | 0.9 | [−2.9, 4.7] | 0.011 (0.020) | 1.1 | 1.7 |
| Log days to first visit | 3.616 | 0.011 (0.026) | 1.1 | [−4.0, 6.2] | 0.011 (0.026) | 1.1 | 1.5 |
| Prolonged delivery stay (live births) | 0.233 | −0.019 (0.022) | −8.0 | [−26.5, 10.6] | −0.017 (0.023) | −7.2 | −5.0 |
| Log number of visits | 2.496 | 0.011 (0.034) | 1.1 | [−5.5, 7.6] | 0.002 (0.032) | 0.2 | 1.6 |
| **Pregnancy Endings** | | | | | | | |
| Live birth | 0.789 | 0.005 (0.013) | 0.6 | [−2.5, 3.7] | 0.010 (0.015) | 1.2 | 1.7 |
| Miscarriage | 0.162 | 0.021 (0.013) | 13.1 | [−2.4, 28.7] | 0.017 (0.012) | 10.2 | 1.0 |
| Ending not observed | 0.062 | −0.009 (0.018) | −13.9 | [−70.0, 42.1] | −0.008 (0.018) | −12.5 | −23.3 |
| Insurance-billed abortion | 0.011 | −0.023 (0.009) | −213.9 | [−382.4, −45.4] | −0.023 (0.009) | −217.4 | −123.6 |

*Notes:* Column 1 is the ban-state mean in event quarters −6 to −1 of the stacked design, which scales every percent (100 times the coefficient for outcomes in logs). Columns 2 to 6 are synthetic difference-in-differences (DiD) estimates (Arkhangelsky et al., 2021) on state by quarter-of-conception means in the continuous-enrollment sample, conceptions from the first quarter of 2020 through the fourth quarter of 2023, excluding Indiana and South Carolina. Columns 2 to 4 average the outcome net of the Controls covariates, fitted on pregnancies with state and quarter fixed effects, and columns 5 and 6 the outcome itself. Standard errors (SE) come from 200 placebo replications, and the 95 percent confidence interval (CI) in column 4 uses the normal critical value. Synthetic DiD has no pre-trend test. Column 7 repeats column 3 of Table 2.

### *A11. Close-to-Care Zip Code Design*

As a check that does not rely on protected states, I implement a close-to-care zip-code design that restricts the sample to pregnancies in ban-state zip codes and compares women by their distance to emergency care in protected states. A pregnancy is close to care, the untreated group, when the

centroid of its assigned zip code lies within 50 miles of the zip-code centroid of the nearest general acute care hospital or emergency medicine provider (an organizational, Type 2, NPI in NPPES) in a protected state. Restrictions on abortion care likely bind less for women who live closer to care outside their state, because crossing the border for care costs them less, especially in an emergency; pregnancies farther than 50 miles from it form the treated group.

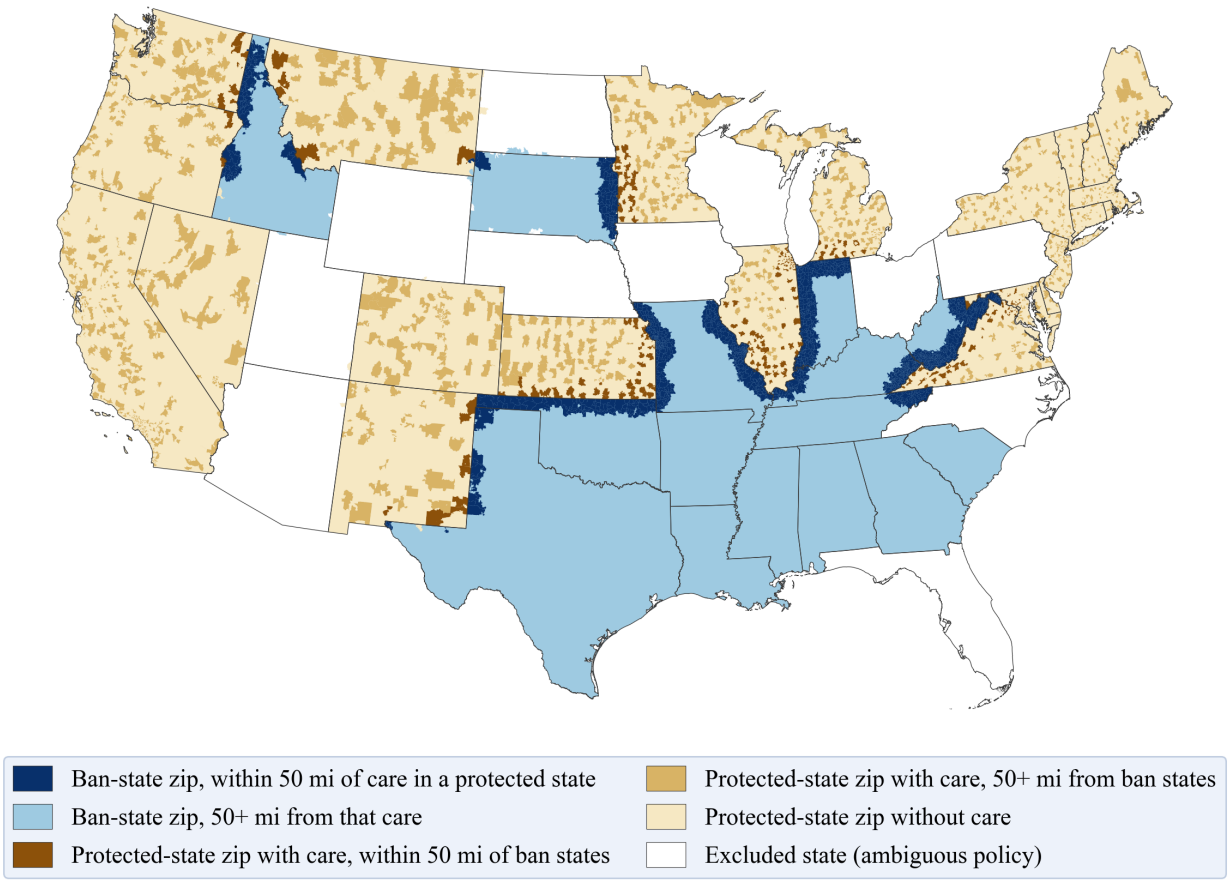
The first step is assigning pregnancies to zip codes, because the data do not record a woman's five-digit zip code. For each pregnancy I take the modal practice zip code, from NPPES, of the individual (Type 1) providers the woman saw in the 365 days before her pregnancy started, and use it as her assigned zip code. Assigning the zip code pregnancy by pregnancy rather than once per woman avoids a stale location for women who move between pregnancies, and using individual providers avoids placing a woman at a hospital system's headquarters. Treatment timing and the state fixed effects follow the state of the assigned zip code, and standard errors are clustered by assigned zip code, the unit at which treatment is assigned. The design only works if the assigned zip code is a reasonable proxy for residence. Table A18 reports placement diagnostics by sub-sample. In every sub-sample, a majority of the visits with a provider zip code fall in the modal zip code, and most fall within its three-digit zip area, so the assigned zip code likely describes where the woman gets care. In the border file as a whole, before the continuous-enrollment restriction, 12.1 percent of pregnancies lie within 50 miles of protected-state care, and the mean distance from the assigned zip code to that care is 248 miles. The assigned zip code is not an exact residence but a stable location that supports a close-to-care versus interior contrast.

Figure A5 maps the close-to-care classification across all lower-48 zip codes. Ban-state zip codes within 50 miles of protected-state care identify the close-to-care (untreated) group, ban-state zip codes farther than 50 miles form the treated group, and protected-state zip codes near the ban states form the natural set of substitution destinations. Zip codes in states whose abortion policy was ambiguous during the analysis window are white and excluded from both groups. The continuous-enrollment border sample contains 235,305 pregnancies, and the border file as a whole spans 5,462 assigned ban-state zip codes. Sample descriptives for the close-to-care groups appear in Table A19.

Table A20 reports the close-to-care estimates and Figure A6 their event studies. The design is less precise than the main one, and no maternal health estimate points toward harm. The adverse-outcome composite changes by −11.1 percent far from protected-state care relative to near it, severe maternal morbidity by −30.8 percent, and preterm birth by −4.2 percent, none distinguishable from zero. Insured abortion, already rare on both sides of the 50-mile line, changes by 2.9 percent, with an interval from −124.2 to 130.0 percent. Only the share of pregnancies with no observed ending

moves clearly, falling 44.3 percent ($p = 0.02$). These estimates are consistent with the main results in a design that does not rely on protected states, although their intervals are too wide to rule out effects that the main design rules out.

Figure A5. Close-to-Care Classification of Lower-48 Zip Codes




*Notes:* Each polygon is a 2020 Census ZIP Code Tabulation Area (ZCTA), with states classified as in Table A1 and excluded states in white. In ban states, ZCTAs within 50 miles of a ZCTA with observed emergency care (a general acute care hospital or emergency medicine provider) in a protected state are navy and the rest light blue, the close-to-care classification, drawn here from each ZCTA's interior point rather than the centroid the estimation uses. In protected states, ZCTAs with observed care within 50 miles of a ban-state ZCTA are dark brown, those with care farther away are tan, and those without observed care are cream. Distances use the Conus Albers equal-area projection (EPSG 5070). As the data-use terms require, the map shows every eligible zip code, not the zip codes where pregnancies are observed.

Table A18—Zip-Code Placement Diagnostics

| | Within 50 Miles of Protected Care | Beyond 50 Miles, by Ban Cohort | | |
|---|---|---|---|---|
| | | Texas | *Dobbs* Cohort | Late Adopters |
| Visits in the year before pregnancy | 21.09 | 17.26 | 22.11 | 20.01 |
| in the modal zip code | 7.20 | 5.94 | 7.03 | 6.88 |
| in the second most common zip code | 3.04 | 2.64 | 3.01 | 2.91 |
| in the modal three-digit zip area | 9.02 | 8.10 | 9.42 | 9.11 |
| with no provider zip code | 7.54 | 5.81 | 8.42 | 7.11 |
| Unique zip codes visited | 4.41 | 4.21 | 4.66 | 4.25 |
| **Share of visits with a provider zip in the modal zip code** | **0.53** | **0.52** | **0.51** | **0.53** |
| in the second most common zip code | 0.22 | 0.23 | 0.22 | 0.23 |
| in the modal three-digit zip area | 0.67 | 0.71 | 0.69 | 0.71 |
| Pregnancies | 28,285 | 95,685 | 94,265 | 17,070 |

*Notes:* Means across women in the year before their first observed pregnancy, in the continuous-enrollment sample. Column 1 is the close-to-care sample, pregnancies whose assigned ban-state zip code lies within 50 miles of protected-state emergency care (Section A11); the other columns are assigned zip codes farther away, by ban cohort, with the late adopters West Virginia, Indiana, and South Carolina. A visit is located by the NPPES practice zip code of its rendering NPI, and the modal three-digit zip area counts visits to any zip code with the same first three digits.

Table A19—Close-to-Care Sample Characteristics

| | Beyond 50 Miles | | Within 50 Miles | |
|---|---|---|---|---|
| | **Before** | **After** | **Before** | **After** |
| Pregnancies | 107,453 | 99,567 | 18,678 | 9,607 |
| **Demographics, % Unless Noted** | | | | |
| Age, mean (SD) | 29.10 (6.40) | 28.99 (6.83) | 28.30 (6.41) | 27.85 (6.71) |
| Under 25 | 26.1 | 30.0 | 31.4 | 36.6 |
| Medicaid | 42.2 | 36.1 | 53.6 | 55.2 |
| Commercial | 57.7 | 63.8 | 46.3 | 44.8 |
| White | 41.6 | 41.1 | 52.6 | 57.6 |
| Black | 20.0 | 16.5 | 14.9 | 11.6 |
| Asian | 1.9 | 1.9 | 0.9 | 0.8 |
| Hispanic | 9.3 | 14.4 | 9.3 | 12.8 |
| First observed pregnancy | 25.7 | 26.8 | 25.5 | 29.5 |
| **Health Before Pregnancy, % Unless Noted** | | | | |
| Hypertension, obesity, or substance use | 25.8 | 27.4 | 28.0 | 33.2 |
| Obstetric comorbidity score, mean (SD) | 9.49 (14.68) | 9.84 (15.14) | 10.63 (15.68) | 11.85 (16.42) |
| **Maternal Health, %** | | | | |
| Any adverse pregnancy outcome | 28.7 | 29.2 | 28.4 | 28.8 |
| *Adjusted for characteristics* | [28.9] | [29.5] | [28.4] | [28.4] |
| Severe maternal morbidity | 2.8 | 2.6 | 2.9 | 3.0 |
| *Adjusted for characteristics* | [2.8] | [2.6] | [2.8] | [2.8] |
| Hypertensive disorder | 21.6 | 22.1 | 21.1 | 23.6 |
| *Adjusted for characteristics* | [21.3] | [22.1] | [20.8] | [22.9] |
| Preterm birth (live births) | 17.7 | 17.6 | 17.7 | 19.2 |
| *Adjusted for characteristics* | [17.8] | [17.9] | [17.6] | [19.0] |
| **Timing and Intensity of Care, % Unless Noted** | | | | |
| Length of pregnancy, days, mean (SD) | 223.9 (84.2) | 223.2 (84.8) | 221.4 (86.2) | 222.7 (84.9) |
| *Adjusted for characteristics* | [225.1] | [224.9] | [221.4] | [221.1] |
| Days to first visit, mean (SD) | 43.2 (29.4) | 43.0 (29.1) | 44.1 (32.0) | 42.9 (30.0) |
| *Adjusted for characteristics* | [44.6] | [44.4] | [44.9] | [43.8] |
| Prolonged delivery stay (live births) | 23.5 | 24.1 | 23.2 | 23.5 |
| *Adjusted for characteristics* | [23.8] | [24.0] | [23.5] | [23.0] |
| Number of visits, mean (SD) | 16.6 (11.0) | 16.7 (10.9) | 17.1 (11.6) | 18.0 (11.8) |
| *Adjusted for characteristics* | [16.6] | [16.8] | [16.8] | [17.3] |
| **Pregnancy Endings, % of Observed Endings Unless Noted** | | | | |
| Live birth | 78.8 | 78.5 | 77.5 | 78.1 |
| *Adjusted for characteristics* | [79.5] | [79.4] | [77.4] | [77.3] |
| Miscarriage | 16.0 | 16.6 | 15.5 | 15.4 |
| *Adjusted for characteristics* | [15.3] | [15.5] | [15.3] | [15.8] |
| Ending not observed (percent of all pregnancies) | 6.2 | 5.6 | 6.7 | 6.9 |
| *Adjusted for characteristics* | [6.3] | [6.1] | [6.8] | [7.1] |
| Insurance-billed abortion | 1.3 | 1.1 | 3.5 | 2.7 |
| *Adjusted for characteristics* | [1.3] | [1.2] | [3.6] | [2.9] |

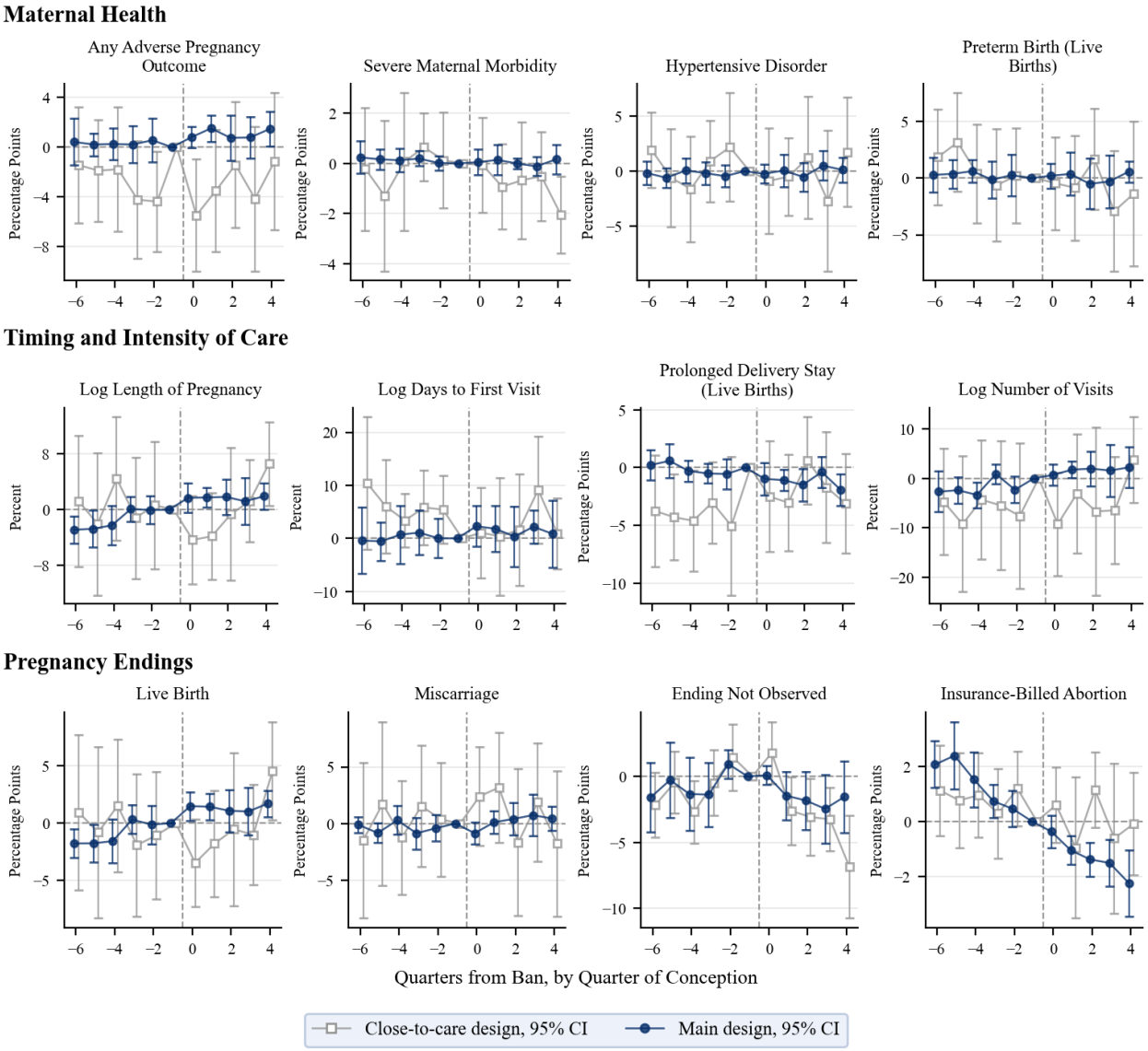
*Notes:* Pregnancies in ban-state zip codes in the continuous-enrollment sample, laid out as Table 1. Beyond 50 miles are assigned zip codes farther than 50 miles from protected-state care (the treated group in Table A20); within 50 miles are the close-to-care zip codes. Before and after are relative to the ban in each pregnancy's state.

Table A20—Close-to-Care Design Estimates

| Outcome | Pre-Ban Mean (1) | Controls | | | Raw | |
|---|---|---|---|---|---|---|
| | | Estimate (SE) (2) | Percent of Mean (3) | 95% CI, Percent of Mean (4) | Estimate (SE) (5) | Percent of Mean (6) |
| **Maternal Health** | | | | | | |
| Any adverse pregnancy outcome | 0.285 | −0.032 (0.019) | −11.1 | [−24.4, 2.3] | −0.036 (0.020) | −12.5 |
| Severe maternal morbidity | 0.027 | −0.008 (0.008) | −30.8 | [−86.4, 24.8] | −0.009 (0.008) | −33.2 |
| Hypertensive disorder | 0.210 | −0.002 (0.021) | −1.1 | [−20.5, 18.3] | −0.005 (0.022) | −2.3 |
| Preterm birth (live births) | 0.174 | −0.007 (0.018) | −4.2 | [−24.7, 16.2] | −0.005 (0.019) | −3.0 |
| **Timing and Intensity of Care** | | | | | | |
| Log length of pregnancy | 5.265 | −0.004 (0.027) | −0.4 | [−5.6, 4.8] | −0.007 (0.031) | −0.7 |
| Log days to first visit | 3.573 | 0.027 (0.042) | 2.7 | [−5.5, 10.8] | 0.022 (0.041) | 2.2 |
| Prolonged delivery stay (live births) | 0.230 | −0.019 (0.017) | −8.2 | [−22.6, 6.1] | −0.018 (0.018) | −7.7 |
| Log number of visits | 2.522 | −0.045 (0.051) | −4.5 | [−14.5, 5.6] | −0.053 (0.051) | −5.3 |
| **Pregnancy Endings** | | | | | | |
| Live birth | 0.785 | −0.006 (0.019) | −0.7 | [−5.6, 4.1] | −0.008 (0.022) | −1.0 |
| Miscarriage | 0.165 | 0.009 (0.024) | 5.2 | [−23.7, 34.1] | 0.010 (0.027) | 6.1 |
| Ending not observed | 0.062 | −0.027 (0.012) | −44.3 | [−81.5, −7.1] | −0.028 (0.012) | −44.6 |
| Insurance-billed abortion | 0.012 | 0.000 (0.008) | 2.9 | [−124.2, 130.0] | 0.001 (0.008) | 4.8 |

*Notes:* Pregnancies in ban-state zip codes in the continuous-enrollment sample. Treated pregnancies are in assigned zip codes farther than 50 miles from protected-state emergency care (Section A11), and comparison pregnancies are within 50 miles. Column 1 is the treated mean in event quarters −6 to −1 in each outcome's units. Column 2 is the stacked difference-in-differences (DiD) estimate, comparing each cohort's treated zip codes only with close-to-care zip codes in states banned on the same date, with the Controls covariates, the weights of Wing, Freedman and Hollingsworth (2024), and standard errors (SE), clustered by assigned zip code, in parentheses. Column 3 is the estimate as a percent of the pre-ban mean (100 times the coefficient for outcomes in logs), and column 4 its 95 percent confidence interval (CI). Columns 5 and 6 drop the covariates.

Figure A6. Event Studies, Close-to-Care and Main Designs




*Notes:* Stacked event-study coefficients of the close-to-care design (hollow markers), comparing ban-state zip codes farther than 50 miles from protected-state care with close-to-care zip codes in states whose ban took effect on the same date, beside the main design's coefficients from Figure 2 (filled markers), in the Controls specification and the continuous-enrollment sample. The 95 percent confidence intervals use standard errors clustered by assigned zip code in the close-to-care design and by state in the main design. Quarter −1 is the reference period, and the dashed vertical line falls between quarters −1 and 0.

### *A12. Complicated Pregnancies*

Section V studies three groups, each dated by the quarter of its first diagnosis through the first quarter of 2024. *Severe fetal anomaly* is a first ICD-10-CM O35.0 or O35.1 code, for central nervous system malformation or chromosomal abnormality in the fetus. Both prefixes match the codes before and after the October 2022 expansion of O35, so the definition does not change at that date,

and it includes non-lethal conditions such as spina bifida and trisomy 21. *Early severe preeclampsia* is a first O14.1, O14.2, or O15 code before 34 weeks of gestation. *Ectopic pregnancy* is an ectopic ending, dated from its first O00 code. Each outcome runs from the diagnosis through 42 days after the ending. Severe maternal morbidity uses the 16 diagnosis indicators of Appendix A13, less eclampsia, an index code for preeclampsia. Transfusion and hysterectomy build on the procedure indicators of the same definition, and hysterectomy adds CPT 58150 to 58573 and 59525. Critical care is CPT 99291 or 99292. A cell is estimated only with at least 30 distinct treated and 30 distinct control women. The share of all pregnancies in each group in Panel A of Table 3 is estimated on the pregnancy sample of Table 2, by quarter of conception.

### *A13. Outcome Definitions*

Severe maternal morbidity uses the 16 diagnosis indicators of the definition of the Centers for Disease Control and Prevention (Section II.E). The definition adds five procedure indicators: transfusion, hysterectomy, ventilation, temporary tracheostomy, and conversion of cardiac rhythm. I do not use them, because procedure codes are recorded mainly on facility claims and their completeness varies by payer and setting, and because transfusion, the most common of them, is also the most common false positive in chart validation (Main et al., 2016). Preterm birth is measured from gestational age at delivery for pregnancies dated by Z3A codes or embryo transfer (Leonard et al., 2020, Appendix 2), and otherwise from the preterm-labor diagnosis. Insurance-billed abortion counts an ending with any abortion-related code. The codes are ICD-10-CM Z33.2 (encounter for elective termination), O04, and O07; CPT 59840, 59841, 59850 to 59852, and 59855 to 59857; ICD-10-PCS 10A0; and HCPCS S0190, S0191, S0199, S2260, and S2265 to S2267.

Table A21—Outcome Construction

| Outcome | Definition | Codes | Form |
|---|---|---|---|
| **Pregnancy Endings** | | | |
| Live birth | Ending type after the validated hierarchy (live birth, stillbirth, molar, ectopic, induced abortion, miscarriage), among observed endings. | CM Z37.0, Z37.2, Z37.3, Z37.5, Z37.6, O80, O82; PCS 10D00Z0–10D00Z2, 10D07Z3–10D07Z8, 10E0XZZ; CPT 59400–59622 (delivery); DRG 765–768, 774, 775, 783–788, 796–798, 805–807 | Indicator |
| Miscarriage | Ending type after the same hierarchy, among observed endings. | CM O02.0, O02.1, O02.8, O02.9, O03; CPT 59812, 59820, 59821; PCS 10D17Z; DRG 770, 779 | Indicator |
| Ending not observed | Two or more dates with prenatal, adverse-outcome, or Z3A codes, no gap over 60 days, and no ending code. | CM Z34, Z36, O09, Z3A; CPT 59425, 59426, 76801–76817, 59000–59015; adverse-outcome codes below | Indicator |
| Insurance-billed abortion | Ending type after the same hierarchy, among observed endings. | CM Z33.2, O04, O07; CPT 59840, 59841, 59850–59852, 59855–59857; PCS 10A0; HCPCS S0190, S0191, S0199, S2260, S2265–S2267 | Indicator |
| **Maternal Health** (window: pregnancy start through 42 days after the ending for SMM; pregnancy otherwise) | | | |
| Any adverse pregnancy outcome | Any of infection or sepsis, preterm birth, gestational diabetes, poor fetal growth, or placental abruption. Preterm birth: live births under 259 days from start to delivery if dated by Z3A codes or embryo transfer, otherwise a preterm-delivery diagnosis. | Infection CM A40, A41, A32.7, R65.20, O85, O86, O41.12, T80.211A, T81.4XXA, T81.44; preterm dating CM Z3A, CPT 58974, 58976, 89258, diagnosis CM O60.1, O60.2; gestational diabetes CM O24.4; fetal growth restriction CM O36.5; abruption CM O45 | Indicator |
| Severe maternal morbidity | Any of the 16 CDC diagnosis indicators on any claim; the five procedure indicators (transfusion, hysterectomy, ventilation, temporary tracheostomy, conversion of cardiac rhythm) are not used. | Leonard et al. (2020, Appendix 1) | Indicator |
| Hypertensive disorder | Diagnosis. | CM O10–O16 | Indicator |
| Preterm birth | Live births only: under 259 days from start to delivery if dated by Z3A codes or embryo transfer, otherwise a preterm-delivery diagnosis. | CM Z3A; CPT 58974, 58976, 89258; CM O60.1, O60.2 | Indicator |
| **Timing and Intensity of Care** (window: pregnancy) | | | |
| Log length of pregnancy | Pregnancy start to ending, among observed endings of 28 to 315 days. | Start dated from CM Z3A, embryo transfer, or screening tests | ln |
| Log days to first visit | Pregnancy start to the first claim of any kind, among observed endings; values under 10 days set to missing. | All claims in the window | Wins. p99; ln |
| Prolonged delivery stay | Live birth whose delivery admission lasts more than 2 days after a vaginal delivery or more than 4 after a cesarean, the federal minimum stays (42 U.S.C. 300gg-25) | Delivery admission, delivery-mode codes | Live births |
| Log number of visits | Days with any claim. | All claims in the window. | Wins. p99; ln |

Table A21—Outcome Construction (Continued)

| Outcome | Definition | Codes | Form |
|---|---|---|---|
| **Controls and Subgroups** (window: 365 days before the pregnancy start unless noted) | | | |
| Obstetric comorbidity score | Leonard conditions present before pregnancy, weighted by the severe maternal morbidity points of Leonard et al. (2020): pulmonary hypertension 50; chronic renal disease 38; bleeding disorder 34; cardiac disease 31; HIV 30; anemia 20; gastrointestinal disease 12; asthma 11; substance use, autoimmune disease, chronic hypertension 10; pre-existing diabetes, neuromuscular disease 9; mental health 7; thyrotoxicosis 6; BMI 40 or more 5; prior cesarean 4; bariatric surgery 0. High risk: any of these conditions. | Leonard et al. (2020) list, 18 conditions, e.g. chronic hypertension CM I10, O10, O11; pre-existing diabetes CM E08–E13, O24.0, O24.1, O24.3, O24.8, O24.9, Z79.4 | Score; indicator |
| Under 25; Medicaid | Enrollment-file age under 25; Medicaid payer group, each in the enrollment month nearest the pregnancy start. | Enrollment file | Indicator |
| Minority | A Black race code or a Hispanic ethnicity code, each counted once. | Enrollment file | Indicator |

*Notes:* CM denotes ICD-10-CM diagnosis codes and PCS ICD-10-PCS procedure codes; CPT, Current Procedural Terminology; HCPCS, Healthcare Common Procedure Coding System Level II; DRG, Medicare Severity Diagnosis Related Group. Codes match by prefix, so a three-character code covers every code that begins with it. Pregnancy and ending codes draw on the Maternal and Infant Health code lists of Centers for Medicare & Medicaid Services (2023), the mapping of Ailes et al. (2016), and the Z3A codes of American Academy of Professional Coders (2024).

## REFERENCES


**Ailes, Elizabeth C., Regina M. Simeone, April L. Dawson, Emily E. Petersen, and Suzanne M. Gilboa.** 2016. "Using Insurance Claims Data to Identify and Estimate Critical Periods in Pregnancy: An Application to Antidepressants." *Birth Defects Research Part A: Clinical and Molecular Teratology*, 106(11): 927–934.

**American Academy of Professional Coders.** 2024. "ICD-10 Code for Weeks of Gestation." *Codify by AAPC.* https://www.aapc.com/codes/icd-10-codes/Z3A.

**American College of Obstetricians and Gynecologists.** 2017. "Committee Opinion No. 700: Methods for Estimating the Due Date." *Obstetrics and Gynecology*, 129(5): e150–e154.

**American College of Obstetricians and Gynecologists.** 2018*a*. "ACOG Practice Bulletin No. 193: Tubal Ectopic Pregnancy." *Obstetrics and Gynecology*, 131(3): e91–e103.

**American College of Obstetricians and Gynecologists.** 2018*b*. "ACOG Practice Bulletin No. 200: Early Pregnancy Loss." *Obstetrics and Gynecology*, 132(5): e197–e207.

**Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager.** 2021. "Synthetic Difference-in-Differences." *American Economic Review*, 111(12): 4088–4118.

**Bell, Suzanne O., Selena Anjur-Dietrich, and Alison Gemmill.** 2026. "Birth Certificate Measures of Maternal Morbidity and Their Limits in Abortion Policy Research." *American Journal of Epidemiology*, kwag159.

**Brown, Andrea D., Brady E. Hamilton, Dmitry M. Kissin, and Joyce A. Martin.** 2025. "Trends in Mean Age of Mothers: United States, 2016–2023." *National Vital Statistics Reports*, 74(9).

**Centers for Medicare & Medicaid Services.** 2023. "Identifying Pregnant and Postpartum Beneficiaries in Medicaid and CHIP Administrative Data: Technical Specifications." *Maternal and Infant Health (MIH) Technical Specifications and Reference Codes, Center for Medicaid and CHIP Services.* https://www.medicaid.gov/medicaid/data-and-systems/downloads/macbis/mih_techspecs.pdf.

**Chomistek, Andrea K., Kelesitse Phiri, Michael C. Doherty, Jenna F. Calderbank, Stephanie E. Chiuve, Brenda Hinman McIlroy, Michael C. Snabes, Cheryl Enger, and John D. Seeger.** 2023. "Development and Validation of ICD-10-CM-Based Algorithms for Date of Last Menstrual Period, Pregnancy Outcomes, and Infant Outcomes." *Drug Safety*, 46(2): 209–222.

**Conlon, Rose.** 2024. "Kansas Provided More Abortions in 2023 than Ever Before, Mostly for Out-of-State Patients." *KCUR.* https://www.kcur.org/2024-12-13/kansas-provided-more-abortions-in-2023-than-ever-before-mostly-for-out-of-state-patients.

**Curhan, Talia.** 2026. "State Bans on Abortion Throughout Pregnancy." *Guttmacher Institute.* https://www.guttmacher.org/state-policy/explore/state-policies-abortion-bans.

**Dench, Daniel, Mayra Pineda-Torres, and Caitlin Myers.** 2024. "The Effects of Post-Dobbs Abortion Bans on Fertility." *Journal of Public Economics*, 234: 105124.

**DiNardo, John, Nicole M. Fortin, and Thomas Lemieux.** 1996. "Labor Market Institutions and the Distribution of Wages, 1973–1992: A Semiparametric Approach." *Econometrica*, 64(5): 1001–1044.

***Dobbs v. Jackson Women's Health Organization.*** 2022. *597 U.S. 215 (Supreme Court of the United States).*

**Goldhill, Olivia.** 2023. "Kate Cox Is One of Hundreds in Texas Denied Abortions despite Serious Health Risks, Data Show." *STAT.*

**Guttmacher Institute.** 2021*a*. "Regulating Insurance Coverage of Abortion (as of November 1, 2021)." *Guttmacher*

*Institute, State Laws and Policies. Archived December 16, 2021.* http://web.archive.org/web/20211216042906/https://www.guttmacher.org/state-policy/explore/regulating-insurance-coverage-abortion.

**Guttmacher Institute.** 2021*b*. "Targeted Regulation of Abortion Providers (as of November 1, 2021)." *Guttmacher Institute, State Laws and Policies. Archived January 2, 2022.* http://web.archive.org/web/20220102054820/https://www.guttmacher.org/state-policy/explore/targeted-regulation-abortion-providers.

**Guttmacher Institute.** 2022. "An Overview of Abortion Laws (as of January 1, 2022)." *Guttmacher Institute, State Laws and Policies. Archived January 7, 2022.* http://web.archive.org/web/20220107211004/https://www.guttmacher.org/state-policy/explore/overview-abortion-laws.

**Hughes, Bryan.** 2021. "Texas Heartbeat Act." *Texas Senate Bill 8, 87th Legislature, Regular Session*.

**Hughes, Bryan.** 2024. "I Wrote Texas' Abortion Law. It's Clear about Medical Emergencies." *Houston Chronicle.* https://www.houstonchronicle.com/opinion/outlook/article/texas-abortion-medical-emergencies-19900172.php.

**Kentucky Senate GOP.** 2025. "Senate Advances Clarifying Language in House Bill 90 to Protect Women, Doctors." https://kysenaterepublicans.com/press-releases/2025/3/26/senate-advances-clarifying-language-in-house-bill-90-to-protect-women-doctors.

**Klibanoff, Eleanor.** 2023. "Kate Cox's Case Reveals How Far Texas Intends to Go to Enforce Abortion Laws." *The Texas Tribune.* https://www.texastribune.org/2023/12/13/texas-abortion-lawsuit/.

**Kortsmit, Katherine, Antoinette T. Nguyen, Michele G. Mandel, Lisa M. Hollier, Stephanie Ramer, Jessica Rodenhizer, and Maura K. Whiteman.** 2023. "Abortion Surveillance: United States, 2021." *MMWR Surveillance Summaries*, 72(9): 1–29.

**Leonard, Stephanie A., Chris J. Kennedy, Suzan L. Carmichael, Deirdre J. Lyell, and Elliott K. Main.** 2020. "An Expanded Obstetric Comorbidity Scoring System for Predicting Severe Maternal Morbidity." *Obstetrics and Gynecology*, 136(3): 440–449.

**Main, Elliott K., Anisha Abreo, Jennifer McNulty, William Gilbert, Colleen McNally, Debra Poeltler, Katarina Lanner-Cusin, Douglas Fenton, Theresa Gipps, Kathryn Melsop, Naomi Greene, Jeffrey B. Gould, and Sarah Kilpatrick.** 2016. "Measuring Severe Maternal Morbidity: Validation of Potential Measures." *American Journal of Obstetrics and Gynecology*, 214(5): 643.e1–643.e10.

**Matcho, Amy, Patrick Ryan, Daniel Fife, Dina Gifkins, Chris Knoll, and Andrew Friedman.** 2018. "Inferring Pregnancy Episodes and Outcomes within a Network of Observational Databases." *PLOS ONE*, 13(2): e0192033.

**Moll, Keran, Hui Lee Wong, Kathryn Fingar, Shayan Hobbi, Minya Sheng, Timothy A. Burrell, Linda O. Eckert, Flor M. Munoz, Bethany Baer, Azadeh Shoaibi, and Steven Anderson.** 2021. "Validating Claims-Based Algorithms Determining Pregnancy Outcomes and Gestational Age Using a Linked Claims-Electronic Medical Record Database." *Drug Safety*, 44(11): 1151–1164.

**Nash, Elizabeth.** 2021. "State Policy Trends 2021: The Worst Year for Abortion Rights in Almost Half a Century." *Guttmacher Institute.* https://www.guttmacher.org/article/2021/12/state-policy-trends-2021-worst-year-abortion-rights-almost-half-century.

**Nash, Elizabeth, and Isabel Guarnieri.** 2022. "13 States Have Abortion Trigger Bans: Here's What Happens When Roe Is Overturned." *Guttmacher Institute.* https://www.guttmacher.org/article/2022/06/13-states-have-abortion-trigger-bans-heres-what-happens-when-roe-overturned.

**Office of Governor Gavin Newsom.** 2023. "California Expands Access and Protections for Reproductive Health Care." *Press release, Office of the Governor of California.*

**Office of Governor JB Pritzker.** 2023. "Gov. Pritzker Signs Sweeping Reproductive Rights Protections Into Law." *Press release, State of Illinois.* https://www.illinois.gov/news/release.html?releaseid=25906.

**Osterman, Michelle J. K., Brady E. Hamilton, Joyce A. Martin, Anne K. Driscoll, and Claudia P. Valenzuela.** 2025. "Births: Final Data for 2023." National Center for Health Statistics National Vital Statistics Reports 74(1).

**Presser, Lizzie, Andrea Suozzo, Sophie Chou, and Kavitha Surana.** 2025. "Texas Banned Abortion, Then Sepsis Rates Soared." *ProPublica, February 20.* https://www.propublica.org/article/texas-abortion-ban-sepsis-maternal-mortality-analysis.

***Robinson v. Marshall.*** 2022. *Order dissolving the preliminary injunction against Alabama HB 314, No. 2:19-cv-365-MHT (M.D. Ala. June 24, 2022).*

***Roe v. Wade.*** 1973. *410 U.S. 113 (Supreme Court of the United States).*

**Sarayani, Amir, Xi Wang, Thuy Nhu Thai, Yasser Albogami, Nakyung Jeon, and Almut G. Winterstein.** 2020. "Impact of the Transition from ICD-9-CM to ICD-10-CM on the Identification of Pregnancy Episodes in US Health Insurance Claims Data." *Clinical Epidemiology*, 12: 1129–1138.

***SisterSong v. Governor of Georgia.*** 2022. *SisterSong Women of Color Reproductive Justice Collective v. Governor of the State of Georgia, 40 F.4th 1320 (11th Cir. 2022).*

**Suozzo, Andrea, Sophie Chou, and Lizzie Presser.** 2025. "Texas Won't Study How Its Abortion Ban Impacts Women, So We Did." *ProPublica methodology note.* https://www.propublica.org/article/texas-maternal-mortality-analysis-methodology.

**Surana, Kavitha, and Cassandra Jaramillo.** 2024. "A Texas Woman Died After Waiting 40 Hours for Miscarriage Care." *ProPublica.* https://www.propublica.org/article/josseli-barnica-death-miscarriage-texas-abortion-ban.

**Surana, Kavitha, and Lizzie Presser.** 2024. "A Pregnant Teenager Died After Trying to Get Care in Three Visits to Texas Emergency Rooms." *ProPublica.* https://www.propublica.org/article/nevaeh-crain-death-texas-abortion-ban-emtala.

**Texas Legislature.** 2021. "House Bill 1280, Human Life Protection Act of 2021." *87th Legislature, Regular Session.*

**Texas State Law Library.** 2026*a*. "Abortion Laws: Criminal Penalties; Civil Penalties." *Texas State Law Library research guide.* https://guides.sll.texas.gov/abortion-laws/criminal-penalties; https://guides.sll.texas.gov/abortion-laws/civil-penalties.

**Texas State Law Library.** 2026*b*. "Timeline of Texas Abortion Laws." *Texas State Law Library research guide.* https://guides.sll.texas.gov/abortion-laws/history-of-abortion-laws.

**Tolbert, Jennifer, and Meghana Ammula.** 2023. "10 Things to Know About the Unwinding of the Medicaid Continuous Enrollment Provision." *KFF Issue Brief.*

**Upadhyay, Ushma D., Chris Ahlbach, Shelly Kaller, Clara Cook, and Isabel Muñoz.** 2022. "Trends in Self-Pay Charges and Insurance Acceptance for Abortion in the United States, 2017–20." *Health Affairs*, 41(4): 507–515.

***Whole Woman's Health v. Jackson.*** 2021. *595 U.S. 30 (Supreme Court of the United States).*

**Wing, Coady, Seth M. Freedman, and Alex Hollingsworth.** 2024. "Stacked Difference-in-Differences." Na-

tional Bureau of Economic Research Working Paper 32054.