The Impact of Abortion Bans on the Physician Labor Market: Compositional and Pipeline Effects*

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Abstract

I study the physician labor market after *Dobbs*, which allowed states to enforce abortion bans. I link quarterly obstetrician-gynecologist (OBGYN) locations to political contributions and use synthetic difference-in-differences to compare provider supply and migration between total-ban and abortion-protecting states. I find no aggregate supply decline. However, this stability masks compositional changes: relative per-capita supply falls for female, early-career, top-ranked, and Democratic-donating providers, while male and unranked supply increases. These shifts are more consistent with workforce entry and exit than migration by existing providers. Together, *Dobbs* polarized the U.S. OBGYN workforce, with implications for reproductive-care access and future supply.

JEL Codes: I11, I18, J44, J61
Keywords: abortion policy, physician labor markets, political sorting

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1 Introduction

With the *Dobbs v. Jackson Women's Health Organization* decision on June 24, 2022, the Supreme Court overturned precedent established in *Roe v. Wade (1973)* and *Planned Parenthood v. Casey (1992)* that had protected the right to abortion before fetal viability. The new ruling established that the U.S. Constitution does not confer a right to abortion and sent the regulation of abortion back to state legislatures.

This decision led to sharp policy changes across the United States: Within months, bans were enforced in multiple states while other states expanded protections. While the decision immediately impacted patients, it also delivered a shock to the supply side of the market, generating drastic cross-state differences in legal risk and scope of practice for clinicians. In this paper, I ask: How did physician location decisions change in response to abortion bans, and to what extent are those changes shaped by providers' political party affiliation, gender, career stage, and training background?

This question matters because physician availability is crucial in determining patient outcomes such as wait times, travel distances, and the feasibility of time-sensitive reproductive care. Within the physician labor market, I focus on obstetricians and gynecologists (OBGYNs), physicians who specialize in pregnancy, childbirth, and reproductive-system care. Losses of OBGYNs reverberate well beyond abortion provision: just over 10 percent of OBGYNs perform abortions (Stulberg et al., 2011; Desai et al., 2018). OBGYNs more often provide routine prenatal and postpartum care, manage miscarriages, and provide contraception and cancer screening (American Medical Association, 2024). Even when OBGYNs do not provide abortions, they are exposed to abortion policy through legal uncertainty and constraints surrounding the treatment of closely related conditions such as miscarriage and ectopic pregnancy. Among women with a primary care provider (PCP), close to a quarter rely on an OBGYN as their primary clinician (Mazzoni et al., 2017), so any reduction in OBGYN supply could diminish access to essential healthcare services of all kinds.[1]

Because physician supply is a key input into access, the impact of *Dobbs* is not only a question about patient outcomes, but also a labor-market question about where highly skilled clinicians choose to practice. I exploit post-*Dobbs* abortion bans as a shock to the labor market and compare migration flows, state-level stocks of reproductive care providers, and workforce composition in states that implemented total bans with those in states that protected abortion access.

To answer this question, I use quarterly data from the National Plan and Provider Enumeration System (NPPES) to build a physician-level panel that tracks practice addresses from 2018–2025. I estimate synthetic difference-in-differences models comparing pre- and post-ban changes in OBGYN stocks per 100K reproductive-age females and interstate migration in total-ban (treated) states with a weighted counterfactual constructed from abortion-protecting (comparison) states. I report

[1] The issue of access to OBGYN care is especially acute in rural areas, which had reproductive care shortages before *Dobbs* (Hung et al., 2017; Health Resources and Services Administration, 2025) and are largely located in states that implemented total bans on abortion care. Even modest declines in local OBGYN supply could force rural patients to travel sizable distances for care, leading to delayed or forgone reproductive health services (Myers, 2024; Myers et al., 2025).

estimates overall and by partisan affiliation, gender, career stage, and training background.

I begin with aggregate provider supply, investigating whether abortion bans led to differences in provider stocks per capita between states with abortion bans and states with abortion protections. I find no evidence of aggregate changes in the density of reproductive care providers, and 95% confidence intervals for the average post-ban estimate rule out a greater than 1.64 percent decline in the density of providers in states that implemented a total ban on abortion. In short, abortion bans did not reduce the overall supply of reproductive care physicians in states with total bans relative to those that protected access, at least in the short term.

While the overall supply remained constant, the salience of abortion bans for location decisions is likely to differ by physician characteristics, with gender, career stage, training background, and political alignment jointly shaping where physicians choose to practice. For example, given their own reproductive health and childbearing considerations, female physicians may face greater personal costs from reduced access to reproductive services than their male counterparts. Career stage also shapes these decisions. Early-career physicians often have weaker community ties and greater mobility, making them more responsive to policy changes than established providers. Physicians trained at highly ranked medical schools may also face different labor-market opportunities. In particular, these physicians may enjoy broader outside options and a greater ability to sort toward preferred practice environments.[2] Finally, political identity, a dimension my data are uniquely equipped to capture, is a likely driver of location choice. Democratic physicians, whose political commitments more often align with broad reproductive rights,[3] are likely to experience greater disutility from practicing in states that ban abortion. Because younger physicians are more likely to identify as Democrats, political alignment may partly account for greater responsiveness among early-career physicians.[4] In contrast, Republican physicians may be less deterred or view such environments more favorably. Together, these mechanisms imply that small net changes can mask opposing changes across provider subgroups.

These compositional shifts matter: Existing research has found that physicians' political identity, gender, and training background influence the services they provide and the costs they incur. Prior work shows that Republicans tend to adhere less closely to clinical guidelines than do Democrats and spend 6 percent more per patient, even conditional on patient composition, specialty, and location (Kim, 2025). Partisan differences in treatment intensity and style are particularly pronounced on polarized issues like abortion and the COVID-19 vaccine (Hersh and Goldenberg, 2016; Levin et al., 2023). Research also shows that patients have strong preferences about a provider's gender: Over half of patients report preferring a female OBGYN, while fewer than one in ten prefer a male physician (Tobler et al., 2016). Beyond patient preferences, Cabral and Dillender (2024)

[2]Relatedly, Gottlieb et al. (2025) document that graduates of top-five medical schools are disproportionately represented in higher-paying specialties, consistent with a broader association between training background and physicians' career paths.

[3]As of 2024, close to 60 percent of Republicans say abortion should be illegal in most or all cases, whereas 85 percent of Democrats express that abortion should be legal in all or most cases (Pew Research Center, 2025).

[4]Among OBGYNs enumerated in 2020 or later who make political donations and can be classified by major party, over 90 percent donate primarily to Democrats.

show that gender concordance yields more favorable medical evaluations for female patients. Similarly, observable training credentials predict subsequent practice behavior and resource utilization. While medical school rank is not a direct measure of clinical quality, prior work shows that training background is associated with practice style and resource use. Physicians from higher-ranked medical schools prescribe fewer opioids (Schnell and Currie, 2018). Related evidence finds that higher-ranked providers deliver care associated with lower costs, with no detectable differences in health outcomes (Doyle et al., 2010; Tsugawa et al., 2018). As a result, even if aggregate provider counts are stable, post-*Dobbs* reallocation may alter access, practice style, and patient experience in ways that are economically meaningful. To the degree that the quality of match between a patient and provider is altered by these shifts, compositional changes may have implications for patient welfare. The direction of this effect is ambiguous: some patients may lose access to preferred provider demographics, while sorting may also increase concordance between patients and physicians.

Although I find little evidence of aggregate changes in provider density, this result masks substantial heterogeneity in physician location decisions along demographic and political lines. To capture political heterogeneity, a dimension not available in standard physician data, I link the NPPES panel to the Database on Ideology, Money in Politics, and Elections, henceforth DIME (Bonica, 2024). This linkage provides a unique opportunity to study how location choices differ between physicians who donate to Democratic and Republican political candidates (henceforth, "Democratic" and "Republican" physicians). Because physician location choices happen slowly, the gap develops gradually: in quarters 7 to 10 after a ban, the estimated shortfall in Democratic OBGYN density is 0.808 physicians per 100,000 reproductive-age females, corresponding to an approximate 8.55 percent shortfall relative to baseline, and larger than the average post-ban estimate. In contrast, Republican provider stocks remain stable across the sample period.

Moving beyond partisan composition, I next examine heterogeneity by gender and career stage. In the later post-*Dobbs* period, male OBGYN density in banned states increases by 0.528 physicians per 100,000 reproductive-age females, or 1.89 percent relative to its pre-*Dobbs* baseline. In contrast, female OBGYN density declines by 1.32 physicians per 100,000 reproductive-age females, or 4.31 percent relative to baseline.

Over the same period, early-career OBGYN density declines by 0.985 physicians per 100,000 reproductive-age females, or 21.9 percent relative to baseline. The physicians most affected by post-*Dobbs* bans on abortion care are also those driving entry into the field. In recent years, the supply of new OBGYNs has become overwhelmingly Democratic and female. Because these groups now comprise most of the pipeline, persistent differences in where they begin practicing could accumulate as older cohorts retire.[5]

Alongside these demographic changes, the medical-school backgrounds represented in the OBGYN workforce also shift. To examine this margin, I link the NPPES panel to the National Download-

[5]Research in higher education shows that undergraduate applications declined in response to state abortion bans, especially for females and those in liberal counties (Batel Kane, 2025).

able File (NDF), which identifies the medical school attended for roughly 77 percent of the main analysis sample. I then construct a crosswalk between the schools reported in the NDF and the U.S. News and World Report's "Best Medical Schools: Research" ranking, following Schnell and Currie (2018). While the overall density of this group remains stable, the estimated shortfall in top-50-trained OBGYN density is 0.771 physicians per 100,000 reproductive-age females, or 9.03 percent relative to baseline. In contrast, the density of OBGYNs from named but unranked medical schools rises by 0.595 physicians per 100,000 reproductive-age females, or 3.55 percent relative to baseline. The training-background results provide another dimension in which aggregate headcounts mask changes in provider characteristics in banned and protected states.[6]

Having established that abortion bans did not reduce overall provider supply but did alter the composition of the workforce, I next study migration flows among incumbent physicians. Because stock measures also reflect entry and exit, flow-based migration responses need not mechanically align with aggregate supply changes. I separate the migration analysis into two margins: whether incumbent physicians leave a state (the extensive margin) and, conditional on moving, where they relocate (the destination-choice margin).

I find no evidence that abortion bans changed incumbent out-migration in the aggregate. However, the share of Democratic providers leaving banned states increases by 1.06 percentage points relative to protected states, or a 39.5 percent increase relative to the baseline out-migration rate of 2.69 percent. For Republican providers, I find no statistically significant change in out-migration. I also find no significant differential attrition by gender, career stage, or training background, nor do I observe a disproportionate increase in out-migration from rural areas. Conditional on migrating, physicians' destination choices are descriptively unchanged across provider characteristics.

To reconcile largely null incumbent-migration results with sizable stock changes, I use a stock-flow decomposition that separates incumbent migration from entry into and exit from the observed workforce. I find that incumbent migration is an important component of the Democratic stock decline, while changes in entry into and exit from the observed OBGYN workforce play a larger role in the female, early-career, and top-50 declines. The male and named-unranked increases appear to reflect a mix of the two margins, although the individual components are imprecisely estimated. I corroborate this evidence by implementing sample restrictions to exclude providers missing at either the beginning or end of the panel, and find that the effects for subgroups with large entry-exit components are concentrated among new entrants rather than exits.

My findings build on a growing literature that provides early evidence on changes in the OBGYN and broader reproductive-care workforce. Zhu et al. (2025a) compare 14 states with abortion restrictions in place before *Dobbs* to 12 states that passed restrictions after the decision, finding a 4.2 percent decline in reproductive care practitioners per 100K reproductive-age females.[7] Strasser

[6] The late-period point estimate also implies a shortfall of 0.345 rural OBGYNs per 100,000 reproductive-age females, or 3.17 percent relative to baseline. However, the estimate is imprecise and the specification has weaker pre-period fit, so I interpret it cautiously.

[7] Staiger et al. (2025b) question the estimate's robustness to measurement and design choices; see Zhu et al. (2025b) for a response.

et al. (2024) take a slightly different approach, comparing states with a total or 6-week ban to those without, before and after *Dobbs*. They find statistically insignificant changes in new (post-residency) and existing OBGYN Medicare Provider enrollment per population in restrictive states, relative to less restrictive states. Staiger et al. (2025a) take a descriptive cohort approach, and find no significant changes in the level or migration of OBGYNs across state policy environments. These nationwide results are consistent with Andersen and Grace (2026), who find no differential out-migration of reproductive-health physicians in response to pre-*Dobbs* SB-8 enforcement in Texas. Taken together, these studies suggest that by 2024, aggregate OBGYN supply showed little change across states. These short-run analyses provide valuable early evidence but leave open the full extent of changes in workforce composition. As a result, policymakers could infer that abortion bans had limited labor-market impact, even if underlying compositional shifts were already underway.

My research extends this work in three ways. First, I examine the extent to which practice location decisions in response to abortion bans are shaped by physician characteristics—namely, their gender, partisan identity, career stage, and training background. Second, I study new outcomes by decomposing migration into extensive and destination choice margins and by isolating effects on the early-career pipeline, which sheds light on the mechanisms driving longer-run changes in provider supply. Third, I extend the analysis along two dimensions: I incorporate an additional year of post-*Dobbs* data, which is important because location decisions often take time to manifest; and I implement a synthetic difference-in-differences (SDID) design, which improves identification by constructing a data-driven counterfactual that accommodates differing pre-treatment trajectories.

I also contribute to the broader literature on physician location choice. This work, often motivated by healthcare shortages, typically categorizes determinants into financial and non-financial incentives. The financial literature emphasizes the role of scholarships and loan forgiveness programs (Holmes, 2005; Falcettoni, 2018; Kulka and McWeeny, 2019; Ghosh, 2024). Non-financial mechanisms include immigration incentives like visa waivers (Braga et al., 2024), interstate licensing frictions (Johnson and Kleiner, 2020), Health Professional Shortage Area designations (Khoury et al., 2025), malpractice premiums and damage caps (Chou and Lo Sasso, 2009), and Medicaid expansions that alter local demand (Huh, 2021; Huh and Lin, 2025). Most recently, McNamara and Pineda-Torres (2025) examine hospital-level residency subsidies, highlighting the dominance of the training pipeline in determining long-term supply. Closely related is the literature documenting location "stickiness" in medicine, where original assignment and training locations are inputs of long-run location choice (Fadlon et al., 2020; Costa et al., 2024). Khoury et al. (2025) further note that early-career physicians are the most responsive to incentives, making the training window a critical period for policy intervention. I add to the existing literature by evaluating a novel driver of physician location choice—a joint shock to the personal and professional environment. I show that bans on abortion care shape the composition of physicians practicing across states and, crucially, the decision about where to begin practicing. Because location choice is historically sticky, these initial diversions of the early-career pipeline suggest that the long-run effects may be asymmetric and persistent.

The evidence indicates that abortion bans are already reshaping the composition and future pipeline of OBGYNs, with implications for long-run access and patient-provider matching even in the absence of aggregate shortages. The remainder of the paper proceeds as follows. Section 2 provides background. Section 3 outlines the data and provides some descriptive statistics. I present the empirical strategy in Section 4, and present results in Section 5. I show robustness in Section 6. Finally, Section 7 concludes.

2 Background

The *Dobbs* decision has significantly increased legal uncertainty for physicians providing reproductive care. Providers in states with abortion bans now face direct legal threats, including potential criminal charges and uncertainty about the legality of care in medically complex scenarios (Sabbath et al., 2024; Felix et al., 2025). Further, healthcare workers face harassment and threats of bodily harm in this sector (National Abortion Federation, 2025). It is unlikely that the effects will be isolated to abortion providers. Instead, the effects are likely to influence decision-making among providers treating related conditions such as miscarriage or ectopic pregnancy, or providing contraception (Strasser et al., 2022).

Early evidence points to substantial impacts on physician mental health and ability to practice effectively in restrictive states. A 2023 Kaiser Family Foundation survey documents increased stress and uncertainty among OBGYNs following the *Dobbs* decision,[8] with four in ten providers in states with abortion bans reporting constraints on their ability to manage miscarriages and pregnancy emergencies (Frederiksen et al., 2023). These reports have been supported by anecdotal news coverage, which documents cases of providers closing clinics or relocating to states with less restrictive reproductive care environments (Noor, 2022; Jarvis, 2023; Gay Stolberg, 2023; Weiner, 2023; Taladrid, 2024), though there is also evidence of providers moving to states with restrictions to support those in need (Ungar, 2023).

Related work finds that the number of residency applicants in states that implemented total abortion bans or restrictions has fallen (Hammoud et al., 2024; Orgera and Grover, 2024; Ganguly et al., 2026). Nearly all residency slots were eventually filled, indicating that policy changes may have influenced residency preferences but not the number of new residents in banned states. Even without a change in residency counts, these shifts may affect both who trains in these states and the scope of training they receive. Applicants willing to enter programs in banned states may differ in ideology, gender, or career goals from those who avoid them, changing the composition of future providers. In addition, residents in banned states face reduced exposure to abortion and family-planning procedures, as many programs have lost in-state training opportunities and must rely on limited out-of-state rotations, constraining the breadth of clinical experience (Vinekar et al., 2024). As a result, even if total residency counts remain stable, the skills and characteristics of new physicians emerging from these programs may diverge from those trained in protected states.

[8] Sixty-one percent of OBGYNs practicing in states with abortion bans reported significant concerns about legal risks in the context of patient care.

This divergence is particularly salient with respect to gender. Unlike other specialists, female OBGYNs in ban states face a dual burden: They encounter professional legal liability while simultaneously losing personal access to the reproductive care they are trained to provide. Given that a large share of the early-career workforce is female and of reproductive age, abortion bans represent a direct shock to their own health security–a non-pecuniary disamenity that their male colleagues do not personally experience, notwithstanding indirect risks to partners or family members.

Given the politically salient nature of abortion and reproductive care, a natural hypothesis is that the partisan identity of physicians may play a key role in their location choice post-*Dobbs*. Motyl et al. (2014) describe "ideological migration," in which individuals sort across space to align their social and political environments with their beliefs.[9] For reproductive-care specialists, abortion bans introduce two related forces: (i) direct regulatory frictions that affect day-to-day practice and liability, and (ii) disutility from working in a legal environment that conflicts with one's political beliefs, reflecting the type of belonging motive identified by Motyl and co-authors. If these costs differ by political affiliation, abortion bans may differentially affect Democratic and Republican physicians' decisions to practice in a state. In this setting, even a near-zero net change in OBGYN counts could be consistent with substantial changes in partisan composition.

This partisan sorting matters because political affiliation has been shown to influence clinical decision-making on politicized issues like abortion care, marijuana, and firearms (Hersh and Goldenberg, 2016). Bonica et al. (2014) provide early evidence that physicians themselves have become increasingly polarized along partisan lines, a shift that likely reinforces these divergences in practice patterns. Recent work by Kim (2025) documents that Republican physicians spend approximately 70 dollars more per patient per year compared to their Democratic counterparts, and roughly six percent more after controlling for specialty, location, and patient condition. This spending gap is more pronounced for elective procedures. Given that Kim (2025)'s results are for all procedures, and political polarization among physicians continues to rise, these practice variations may translate into the type and intensity of reproductive care provided by Republican and Democratic physicians. These differences matter because partisan sorting across states can lead to systematic differences in clinical practice, affecting the availability of certain procedures, the content of counseling, and the intensity of treatment patients receive.

3 Data and Descriptive Statistics

3.1 Physician Registry

In order to track changes in physician location and composition before and after abortion bans, I use data from the National Plan and Provider Enumeration System (NPPES). The NPPES is a publicly available registry of all healthcare providers in the United States, maintained by the Centers for Medicare & Medicaid Services (CMS). It provides a unique identifier for each provider

[9] Consistent with the salience of abortion policy in labor markets more broadly, recent work by Adrjan et al. (2026) shows that firms' public responses to the *Dobbs* ruling altered job applications and employee satisfaction along gender and ideological lines.

(NPI) along with their name, practice location, gender, taxonomy, license information, and date of enrollment. Because physicians must maintain an active and accurate address with their profile to bill public and private insurers, practice addresses are regularly updated, making the data well-suited for studying migration over time.

The analysis relies on quarterly snapshots of the NPPES from April 2018 through April 2025.[10] The quarterly frequency allows me to capture moves that would be obscured in annual data and to observe provider responses in periods closely surrounding major abortion policy changes after *Dobbs*. Relative to alternative physician location data such as the SK&A Healthcare Database and the American Medical Association Masterfile, NPPES offers more complete and timely coverage of the provider workforce (DesRoches et al., 2015).

I also augment NPPES with the CMS Doctors and Clinicians National Downloadable File (NDF), which reports physicians' medical schools for a Medicare-visible subset of providers. I follow Schnell and Currie (2018) and classify medical schools using the average *U.S. News & World Report* research ranking from 2010 to 2017. This provides a proxy for the observable human capital of physicians, as measured by their educational background.

In Appendix Table A1, I outline the taxonomy codes I utilize to define my sample. Throughout the analysis, I take those in Panel A (obstetrics-gynecology physicians) and call them "OBGYNs." In robustness, I use providers with a general surgery specialty or primary care specialty.[11] Each provider in the NPPES panel can list multiple taxonomies per observation, but designates a unique primary taxonomy. For each quarterly NPPES snapshot from 2018 to 2025, I first flag providers whose primary taxonomy matches any code in Appendix Table A1 or Appendix Table A2. I aggregate these observations into a master roster of unique NPIs that meet this criterion in at least one quarter. I then retain all quarterly records for these NPIs, regardless of their primary taxonomy in a given period, which preserves the full longitudinal history of every provider ever classified under a focal specialty.[12]

Outcomes are defined using providers' primary taxonomy in the current quarter, so an NPI contributes to a given specialty group only in periods in which its primary taxonomy corresponds to that specialty. Because each provider has a single primary designation in each quarter, providers may contribute to different specialty groups over time if their taxonomy changes, but an NPI can appear in at most one specialty group within a given quarter.

3.2 Rural-Urban Commuting Area (RUCA) Codes

I classify provider-quarter rurality using 2020 RUCA codes from the U.S. Department of Agriculture. For each provider-quarter, I assign RUCA codes based on the ZIP code of each provider's

[10]Specifically, I use snapshots from January, April, July, and October of each year, beginning with April 2018 and concluding with April 2025.

[11]See Appendix Table A2 for the full list of these taxonomies.

[12]Specialty reclassification is unlikely to explain the results. Among ever-observed OBGYNs who remain in the panel, quarter-to-quarter transitions from OBGYN to a non-OBGYN primary taxonomy occur in 1,190 of 1,426,110 provider-quarters, or 0.083 percent. At the provider level, 1,182 of 57,836 OBGYNs are ever observed making such a transition.

primary practice location reported in the NPPES and classify providers as rural in a given quarter if their practice ZIP corresponds to a RUCA designation of 4 or higher, denoting a rural or nonmetropolitan area.

3.3 Database on Ideology, Money in Politics, and Elections (DIME)

I identify the partisan identity of physicians using the Database on Ideology, Money in Politics, and Elections: Public Version 4.0 (Bonica, 2024). DIME is a comprehensive database of individual-level campaign contributions in the United States from 1980 to the present. It traces contributors across places and over time. Most importantly, DIME records the occupation of contributors, providing the opportunity to identify physicians in a way that is infeasible with other political data sources such as voter registration records. The data also record the political party of the recipient, allowing me to calculate the share of donations to each party for each contributor over time. This structure makes DIME well suited to measuring the partisan composition of the physician workforce.

To link medical providers to political contributions, I merge the physician panel from NPPES to the DIME database following Bonica et al. (2014) and Kim (2025). For each physician in the NPI data, I search for a self-identified physician in DIME with the same first name, last name, state, and hospital referral region (HRR). This approach yields the majority of matches. Following previous work, I supplement these exact matches with a set of "likely" matches for physicians without an exact counterpart in the same HRR but who appear in neighboring regions or states. Overall, I match 39.7 percent of OBGYNs; of these, 84.1 percent are exact matches, and the remaining 15.9 percent are "likely" matches. The match rate is higher than that in Kim (2025), who matches about 28 percent of all MD/DOs, but my analysis extends through the particularly salient 2020 and 2024 presidential election cycles, while the other analysis concludes with 2018 donations.

For each physician, I assign a static partisan affiliation based on the party to which they have donated a larger share of contributions up to the 2024 election cycle, using all available contributions from 1980 through 2024. Only 4.6 percent of matched OBGYNs switch their dominant party affiliation over the full period,[13] suggesting that this measure captures stable partisan alignment rather than short-run variation in donation behavior. Results are robust to omitting contributors who switch parties. Providers whose contributions are split evenly across parties, directed exclusively to non-major parties, or made solely to non-partisan organizations are classified as undecided.[14]

3.4 Treatment Definition

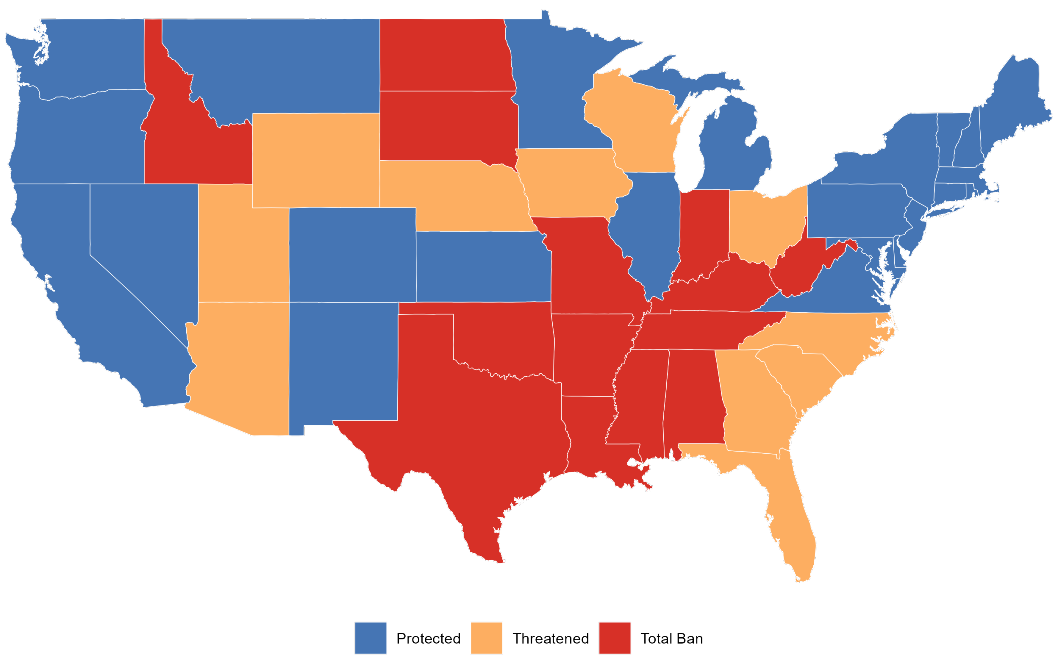
To capture the staggered nature of state-level policy changes following *Dobbs* and establish a clear framework for the empirical analysis, I classify states based on their post-*Dobbs* abortion policy environments and identify the timing of when each total ban took effect.

[13]This measure (along with the match rate above) is constructed from an April 2022 pre-*Dobbs* sample snapshot, following the other summary statistics.

[14]Approximately 49.9 percent of OBGYNs are classified as Democrat, another 26.6 percent as Republican, and the remaining 23.6 percent as undecided.

I define the treatment with three classifications in Figure 1,[15] which identifies states based on abortion access as "protected," "threatened," or "total ban." This classification is a snapshot of the policy status as of August 2024, and is gathered from the New York Times ongoing classification (McCann and Walker, 2022). It is reinforced by classifications from Center for Reproductive Rights (2025) and Guttmacher Institute (2025). In defining the treatment in this way, I follow prior work (Dench et al., 2024; Dench et al., 2025; Myers et al., 2025).[16] Most states in the "total ban" category had trigger laws that put abortion bans in place immediately after the *Dobbs* decision. States in the "threatened" category either enforce gestational bans or are deemed likely to enforce restrictions in the future. Finally, "protected" states did not enforce an abortion ban and do not seem likely to do so in the future. Unless otherwise specified, the treated group is composed of state-level outcomes from the "total ban" states, and the comparison group is composed of state-level outcomes from the "protected" states.

Figure 1. Treatment Definition



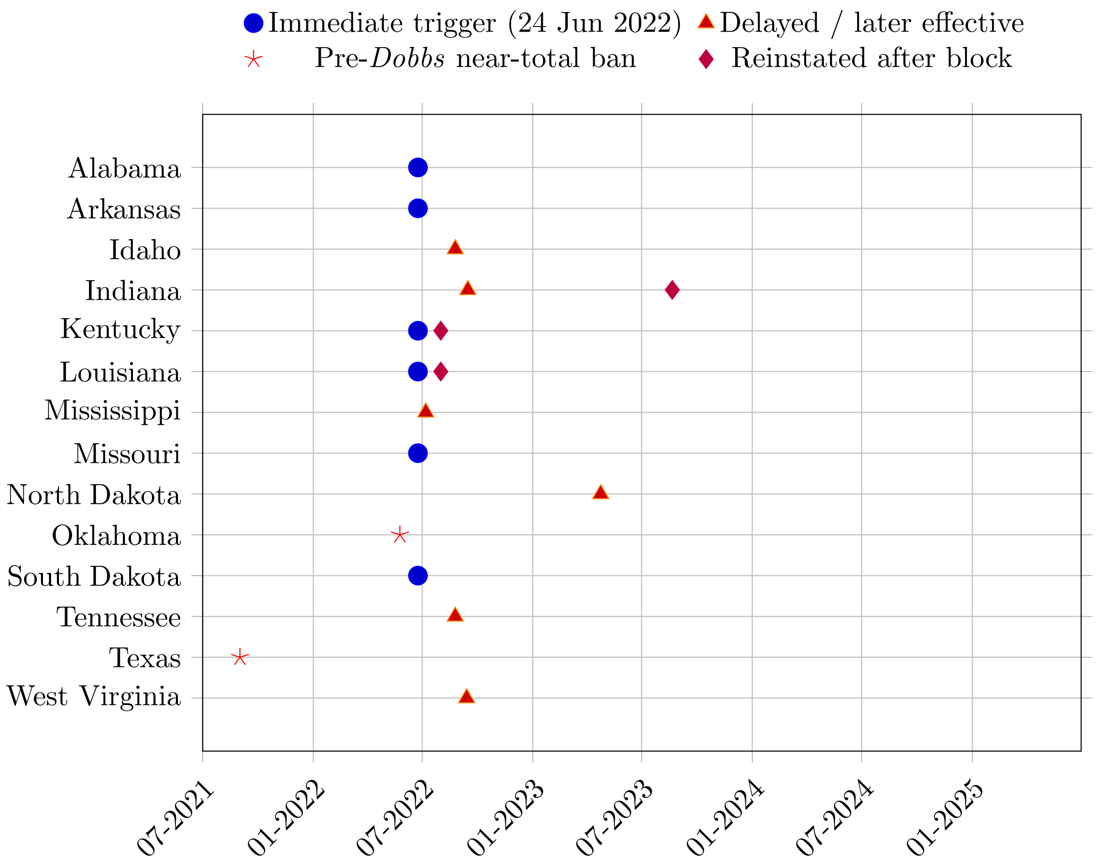
Treatment status is an August 2024 snapshot of the NYT ongoing classification (McCann and Walker, 2022). Total-ban states had a total ban on abortion as of August 2024. Threatened states may have had a gestational ban in place or were deemed likely to enforce bans in the future. Finally, protected states either expanded abortion rights in the aftermath of *Dobbs* or kept protections in place.

[15] Presented in table form in Appendix Table A3.

[16] Wisconsin is classified as threatened in the main analysis. This differs from Dench et al. (2024), Dench et al. (2025), and Myers et al. (2025), who classify Wisconsin as a total ban state because providers stopped offering abortions after *Dobbs*. I do not classify Wisconsin as a total ban state because this de facto ban period was temporary, ending when services resumed on 9/18/23. However, results are robust to classifying Wisconsin as a July 2022 total ban state, as I show in the robustness section. For Indiana and North Dakota, I use more recent legal information and classify each based on the later adoption of its total ban, while the prior work had classified them as "threatened."

In Figure 2, I provide a timeline of when the “total ban” states implemented their policies. This analysis employs a staggered adoption design to evaluate the impact of total abortion bans implemented at various times following the *Dobbs* decision on June 24, 2022. The implementation of these bans was often subject to legal challenges, with some trigger bans being immediately enacted, others delayed, and a few blocked by courts indefinitely. To create a tractable definition of policy exposure, treatment is defined based on the first quarterly observation in which a state's total abortion ban was continuously in effect, and is an absorbing state.

Figure 2. Timeline of total or near-total abortion bans taking legal effect (September 2021 – June 2025).



Circles mark bans that became enforceable on the day of the *Dobbs* decision (24 June 2022). Triangles show bans that took effect after a delay (e.g. trigger-law waiting periods or post-injunction start dates). Stars indicate states that implemented near-total bans before *Dobbs*. Diamonds denote bans reinstated after an initial court block. The exact dates come from state trigger statutes, enacted legislation, and court orders documented in the Center for Reproductive Rights and Guttmacher Institute trackers (Center for Reproductive Rights, 2025; Guttmacher Institute, 2025). See also Dench et al. (2024), Appendix A for a full list of state statutes.

States are grouped according to the quarter their ban was enacted, which defines the onset of treatment. Quarters correspond to NPPES snapshot months (January, April, July, and October), and treatment is coded as ‘on’ beginning with the first snapshot after a ban takes effect. In practice, the snapshots are typically taken around the 10th of the month.

The first group of early adopters consists of six states that implemented total bans shortly before or immediately after the *Dobbs* decision. Oklahoma enacted a 6-week gestational ban on May 3, 2022, a total civil ban on May 25, 2022, and a trigger ban on June 24, 2022. Alabama, Arkansas, Missouri, and South Dakota had immediate trigger bans that took effect on June 24, 2022. Mississippi followed on July 7, 2022. Because each of these bans was effective prior to the July NPPES snapshot, their treatment status is coded as 'on' beginning with the July 2022 quarterly observation.

A second group of five states adopted bans later in the year. Bans in Idaho, Kentucky,[17] Louisiana,[18] Tennessee, and West Virginia[19] all became effective between August and September 2022. Consequently, their treatment is coded as 'on' beginning with the October 2022 quarterly observation.

Finally, three states with unique implementation timelines are treated as distinct cases. Texas introduced a 6-week gestational ban (SB8) effective September 1, 2021, well before the *Dobbs* decision, and its treatment is therefore coded as 'on' from the October 2021 observation. In contrast, North Dakota implemented a new total ban in late April 2023, with its treatment beginning in the July 2023 observation. Similarly, Indiana saw its total ban take effect in August 2023 after a legal challenge was resolved, so its treatment begins with the October 2023 observation.[20]

This classification produces a framework that incorporates the staggered timing of state abortion bans and serves as the basis for the empirical analyses that follow.

3.5 Panel Construction

The individual-level panel is defined as follows. For each provider in the panel data, I set the binary indicator "leaver" equal to 1 if they report a different practice state in the quarter $t+1$. Thus, for each state-quarter (s,t), I am able to identify the number of providers who are no longer in that state as of $(s,t+1)$.

I now define the state-level panel. For each state-quarter (s,t), I aggregate NPPES records by a provider group g; for example, obstetrician-gynecologists (OBGYNs) or primary-care physicians. Within each g, I further disaggregate providers by partisan affiliation using the DIME campaign-contribution database, producing counts N_{gpst} indexed by (potential) party $p \in \{\text{Dem}, \text{Rep}, \text{All}\}$. I merge the state-quarter panel with the treatment definition shown in Figure 1.

Population denominators are taken from the 2017–2021 American Community Survey (ACS)

[17] Kentucky's trigger ban took effect on 6/24/22, before enforcement was blocked by a temporary restraining order on 6/30/22. Enforcement resumed in August 2022. Because I classify states based on the first period in which a ban was continuously in effect, I classify Kentucky based on the August 2022 reinstatement.

[18] Louisiana's trigger ban took effect on 6/24/22, before enforcement was temporarily blocked on 6/27/22. Continuous enforcement resumed on 8/1/22. Because I classify states based on the first period in which a ban was continuously in effect, I classify Louisiana based on the August 2022 reinstatement.

[19] West Virginia banned abortion from 6/24/22 to 7/20/22, when enforcement was blocked by a preliminary injunction. The state then passed a new total ban on 9/13/22, which took effect immediately. Because I classify states based on the first period in which a ban was continuously in effect, I classify West Virginia based on the 9/13/22 law.

[20] Two states, North Dakota and Missouri, rolled back their abortion bans late in the sample, on 9/12/24 and 12/23/24, respectively. I classify both as total ban states through the end of the sample in April 2025. In Appendix Figure A10, the leave-one-out analyses show that results are robust to independently removing these states from the analysis.

five-year estimates. I use the reproductive-age population (females ages 15–49), following prior literature (Strasser et al., 2024; Zhu et al., 2025a).

3.6 Outcome Definitions

Stock Outcome (Provider Density): The outcome is *provider density*, defined as the number of active physicians per 100,000 females of reproductive age.

For provider subpopulation g (defined by specialty and demographic traits) and party $p \in \{\text{Dem}, \text{Rep}, \text{All}\}$, the density in state s at quarter t is:

$$Y_{gpst}^{\text{Stock}} = 100{,}000 \times \frac{N_{gpst}}{\text{RepPop}_s}, \tag{1}$$

where N_{gpst} is the count of physicians in group g with party affiliation p, and RepPop_s is the reproductive-age female population defined previously, held fixed for all periods at the 2017-2021 level.

Flow Outcome (Extensive Margin): The outcome is the *share of leavers*. For provider group g and party p:

$$Y_{gpsy}^{\text{Ext}} = \frac{\sum_{t \in y} N_{gpst}^{\text{Leavers}}}{\overline{N}_{gpsy}}, \tag{2}$$

Given that quarterly migration is sparse, I collapse this outcome to the yearly level. First, I sum the total number of leavers for a given (g, p, s) in a given year y to obtain the numerator.[21] Second, I calculate the denominator $\overline{N}_{gpsy}$ by taking the average stock of providers of type g, p (e.g., the mean count of Democratic, Republican, or all female OBGYNs in Texas) across all quarters in that year. Missing values due to sparsity are imputed using the average share of leavers among observations in the same calendar year, provider group g, party p (if applicable), and policy environment, though this is quite rare.[22]

Flow Outcome (Destination Choice Margin): As a descriptive supplement to the incumbent out-migration outcome, I tabulate where incumbent movers locate before and after *Dobbs*. For each origin policy environment, I calculate the share of movers whose destination is a protected, banned, or threatened state. These shares are conditional on moving.

3.7 Descriptive Statistics

Table 1 summarizes the physician samples used throughout the analysis. Panel A compares OBGYNs, as defined in Appendix Table A1, with primary care physicians and general surgeons. These

[21]For 2018, where I only observe 3 quarters, I multiply this sum by 4/3 to get the approximate yearly number of leavers. For 2025, I only observe one quarter of leavers, so I exclude that year from the flow analysis.

[22]There are no rurally designated ZIP codes in DC, so the rural flow outcome for DC is imputed in this manner. There are no early-career OBGYNs in DE, ID, ME, ND, or SD in calendar year 2018, so the early-career flow outcome for those states is also imputed in that year. Results are robust to dropping these states entirely in the flow analysis.

Table 1. Provider Summary Statistics

Panel A: OBGYNs vs. Comparison Physicians

	(1)		(2)		(3)		(4)	(5)
	OBGYN		Primary Care		General Surgeon		(2-1)	(3-1)
	Mean	SD	Mean	SD	Mean	SD	Diff	Diff
Out-Migrated	0.02	0.14	0.02	0.14	0.03	0.17	-0.00	0.01
Female	0.59	0.49	0.42	0.49	0.24	0.43	-0.17	-0.35
Enumeration Year	2008.1	3.7	2008.9	4.4	2008.4	3.9	0.82	0.35
Matched to DIME	0.40	0.49	0.30	0.46	0.38	0.49	-0.10	-0.02
Matched to NDF	0.77	0.42	0.76	0.43	0.76	0.43	-0.01	-0.01
Protected State	0.56	0.50	0.57	0.50	0.55	0.50	0.00	-0.02
Threatened State	0.23	0.42	0.23	0.42	0.24	0.43	0.00	0.01
Total Ban State	0.21	0.40	0.20	0.40	0.21	0.41	-0.01	0.00
Observations	51436		294458		43568		345894	95004

Panel B: DIME-Matched vs. Unmatched OBGYNs

	(1)		(2)		(3)
	Matched		Unmatched		(2-1)
	Mean	SD	Mean	SD	Diff
Out-Migrated	0.02	0.13	0.02	0.15	0.01
Female	0.52	0.50	0.63	0.48	0.12
Enumeration Year	2007.1	2.8	2008.7	4.0	1.67
Protected State	0.54	0.50	0.58	0.49	0.04
Threatened State	0.25	0.43	0.22	0.42	-0.02
Total Ban State	0.22	0.41	0.20	0.40	-0.02
Observations	20430		31006		51436

Panel C: Democratic vs. Republican OBGYNs

	(1)		(2)		(3)
	Democratic		Republican		(2-1)
	Mean	SD	Mean	SD	Diff
Out-Migrated	0.02	0.14	0.01	0.10	-0.01
Female	0.65	0.48	0.26	0.44	-0.38
Enumeration Year	2007.5	3.1	2006.2	1.7	-1.30
Protected State	0.62	0.48	0.39	0.49	-0.23
Threatened State	0.21	0.41	0.31	0.46	0.11
Total Ban State	0.17	0.38	0.30	0.46	0.13
Observations	10188		5430		15618

This table reports means and standard deviations from an April 2022 pre-*Dobbs* snapshot. Panel A compares OBGYNs, primary care physicians, and general surgeons; columns (4) and (5) report primary care minus OBGYN and general surgery minus OBGYN differences, respectively. Panel B compares OBGYNs matched and unmatched to DIME; differences are unmatched minus matched. Panel C compares Democratic and Republican OBGYNs matched to DIME, excluding providers with only non-partisan contributions; differences are Republican minus Democratic. All panels use the full relevant sample and the treatment definition in Figure 1. Out-Migrated indicates that a provider practices in a different state 12 months later (through April 2023, overlapping with post-Dobbs ban implementation). Enumeration Year records when a provider was first established in the NPPES. Observations report the relevant group or comparison sample sizes. Appendix Figure A1 shows enumeration years by DIME match status.

provider types have similar geographic distributions, average enumeration years, and annual out-migration rates of between two and three percent. They differ most sharply by gender: OBGYNs are majority female, while primary care physicians and general surgeons are majority male. Around 40 percent of OBGYNs and general surgeons are matched to campaign contributions, compared with 30 percent of primary care physicians.

Panel B compares OBGYNs matched to the campaign contribution dataset with those who are not. Consistent with Bonica et al. (2014) and Kim (2025), matched OBGYNs are more likely to be male and belong to earlier professional cohorts. Out-migration rates and geographic distributions, however, are similar across matched and unmatched physicians. Panel C compares Democratic and Republican contributors. At baseline, Democratic providers are more likely to be female, belong to more recent cohorts, and practice in states that protected access to abortion care.

4 Empirical Strategy

4.1 State-Level Stocks

At the state level, the analysis utilizes a synthetic difference-in-differences (SDID, Arkhangelsky et al. (2021)) approach to compare relative changes in providers per 100K reproductive-age females in states that implemented a near-total abortion ban to a weighted counterfactual of those that protected abortion access, before and after bans allowed by the *Dobbs* decision. In doing so, I estimate the causal effect of post-*Dobbs* "total ban" abortion laws on the population-relevant provider density. I use collapsed state-quarter panel data from 2018 to 2025.

Because the raw, unweighted data do not follow parallel pre-ban trajectories, I utilize SDID to construct a more appropriate counterfactual. In the staggered adoption setting, SDID constructs cohort-specific unit weights $\hat{\omega}_{s,a}$ to align pre-*Dobbs* provider density trajectories between treated (total ban) and control (protected) states within each adoption cohort a, and cohort-specific time weights $\hat{\lambda}_{t,a}$ that place more weight on the pre-treatment periods that best predict post-treatment outcomes in the donor states. Together, these weights make the comparison local by emphasizing donor states that align with the treated states' pre-treatment trajectory and pre-treatment periods in which donor-state outcomes resemble their post-treatment outcomes, so the identifying parallel trends requirement applies to the weighted comparison rather than the raw data.[23]

Appendix Figure A4 shows that the weighted comparison path is similar when I estimate donor weights after excluding the five quarters immediately before treatment, providing evidence of ro-

[23] Appendix Figures A2 and A3 plot provider density in total-ban states alongside both the unweighted mean of abortion-protecting states and the SDID-weighted comparison, overall and by subgroup. The unweighted protected-state series generally do not follow parallel pre-*Dobbs* trajectories with total-ban states, illustrating the difficulty of applying a standard difference-in-differences design in this setting. The SDID-weighted series track the total-ban-state trajectories more closely before treatment. After bans, the overall series shows little divergence, but the subgroup paths mirror the compositional results reported below. The negative subgroup gaps generally emerge through slower growth in total-ban states relative to continued growth in the weighted protected-state comparison, rather than through sharp declines in total-ban states. These figures use weights from the July 2022 adoption cohort and are illustrative, as the formal estimates aggregate across adoption cohorts.

bustness to the training window.[24]

These weights are then used in a weighted least squares regression that combines features of both synthetic control and traditional difference-in-differences. This procedure is implemented separately for each adoption cohort a, yielding cohort-specific SDID estimates that are later aggregated.

Formally, for each adoption cohort a, I estimate:

$$(\hat{\tau}_a, \hat{\mu}_a, \hat{\alpha}_a, \hat{\beta}_a) \;=\; \underset{\tau,\mu,\alpha,\beta}{\arg\min} \sum_{s \in \mathcal{S}_a} \sum_{t=2018_{Q2}}^{2025_{Q2}} \Big[\text{Density}_{st} - \mu - \alpha_s - \beta_t - \tau \, \text{TotalBan}_{st}\Big]^2 \; \hat{\omega}_{s,a} \, \hat{\lambda}_{t,a}. \quad (3)$$

where $\mathcal{S}_a$ represents the cohort-specific estimation sample consisting of the treated states in cohort a and all never-treated protected donor states, Density_{st} is the number of OBGYNs per 100,000 females of reproductive age in state s and quarter t (as defined in (1)), TotalBan_{st} is an absorbing indicator equal to 1 during and after the first quarter t in which state s continuously enforces a total or near-total abortion ban, and α_s and β_t are state and quarter fixed effects.

I aggregate these cohort-specific estimates to obtain the average treatment effect on the treated (ATT). To ensure the estimate reflects the average effect on a treated state-quarter, I weight the aggregation by the total number of treated observations in each cohort, following Clarke et al. (2024):

$$\hat{\tau}^{\text{SDID}} = \sum_{a \in A} W_a \hat{\tau}_a^{\text{SDID}}, \quad \text{where} \quad W_a = \frac{N_a T_{\text{post}}^a}{\sum_{a' \in A} N_{a'} T_{\text{post}}^{a'}}. \quad (4)$$

Here, N_a is the number of treated states in cohort a, and T_{post}^a is the number of post-treatment periods observed for that cohort.

Standard errors are clustered at the state level and obtained via 1,000 bootstrap replications. I also estimate SDID event studies with confidence intervals following Ciccia (2024), who extends the approach established by Clarke et al. (2024). Because SDID calculates cohort-specific unit and time weights, a standard event-study plot is not naturally defined. The Ciccia (2024) extension solves this by calculating the dynamic difference between treated units and their specific synthetic counterfactuals within each adoption cohort a, relative to the $\hat{\lambda}_{t,a}$-weighted average pre-treatment difference, and then aggregating these differences weighted by the size of the treated cohort ℓ periods after treatment, a small modification of (4):

$$\hat{\tau}_\ell^{\text{SDID}} = \sum_{a \in A_\ell} \frac{N_{tr}^a}{N_{tr}^\ell} \hat{\tau}_{a,\ell}^{\text{SDID}}. \quad (5)$$

Valid identification relies on the assumption that the weighted comparison approximates the counterfactual trajectory of the treated states. As in a standard event study, the pre-treatment coefficients provide evidence supporting this assumption and should exhibit no systematic departures

[24] Specifically, I compare the protected-state paths implied by the main SDID weights with paths constructed from weights estimated only on earlier pre-ban quarters, excluding event quarters -5 through -1. Both paths are normalized to the early pre-period.

from zero. Fit is especially important in the specific windows where the algorithm concentrates the time weights ($\hat{\lambda}_{t,a}$), as these periods determine the pre-treatment baseline.

4.2 State-Level Migratory Responses

To evaluate relative changes in extensive-margin migration in states that implemented total abortion bans relative to those that protected abortion access, I again employ an SDID approach. The primary difference in this section is that my outcomes are at the state-year level.

I collapse to the yearly level because cross-state migration is low-frequency (about 2 percent in my sample, year to year), and finer periods (e.g., exploiting the quarterly data) would have insufficient cross-period location changes to identify changes in the propensity for migration.

The estimation procedure follows the same staggered adoption logic described in the previous section, including the construction of unit weights, time weights, and the aggregation of cohort-specific effects. I suppress the cohort (a) and event-study (ℓ) indexing in the notation below, but the mechanics remain identical, beyond the change to state-year data. Formally, I estimate:

$$(\hat{\tau}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \sum_{s=1}^{N} \sum_{y=2018}^{2024} \Big[\text{MigrationOutcome}_{sy} - \mu - \alpha_s - \beta_y - \tau\, \text{TotalBan}_{sy}\Big]^2 \hat{\omega}_s \, \hat{\lambda}_y. \quad (6)$$

where $\text{MigrationOutcome}_{sy}$ is the share of leavers in state s and year y (as defined in (2)).

Collapsing to the yearly level limits some of my ability to assign staggered treatment adoption. In this section, I assign treatment based on the year of adoption, rather than the quarter of adoption, so some states are differentially exposed to treatment depending on when their policies went into place during a year.

$\text{TotalBan}_{sy} = 1$ if state s enforces a near-total abortion ban at any point in year y, and α_s and β_y are state and year fixed effects. As before, I cluster errors at the state level and use 1,000 bootstrap iterations.

For the destination choice outcome, I do not follow an SDID framework because small mover counts make origin by destination by subgroup cells too sparse for reliable estimation. Instead, I report descriptive destination shares among incumbent movers.

5 Results

5.1 Stocks

In stock analyses, I ask, relative to a pre-ban baseline, how the supply of providers per 100K reproductive-age females evolved in states that enacted abortion bans relative to those that protected abortion access. I present coefficients in Table 2, and the corresponding event-study plots in text.

Throughout the stock analysis, I report two summary estimates. The first is the average post-ban effect, labeled "Effect of Ban." The second is the average effect in the later post-ban period, defined as quarters 7 to 10 after a ban is implemented and labeled "Effect (Q7–Q10)." I use the Q7–Q10 estimate as my preferred estimate because physician stocks may take time to adjust, as migration, new entry, and retirements occur gradually. These quarters represent the latest observed post-ban period in the data, rather than a long-run steady state. Consistent with gradual adjustment, the Q7–Q10 estimates are generally larger in absolute value than the average post-ban estimates.

Aggregate Supply. Figure 3 presents the SDID event-study estimates for the overall stock of OBGYNs. As discussed in Section 4, identification relies on the weighted comparison approximating the counterfactual trajectory of the treated states, particularly over the pre-treatment periods receiving substantial time weight (see Appendix Figure A5). For ease of interpretation, I plot a symmetric window, with 11 pre-periods and 11 post-periods.[25,26]

Post-ban, there is no evidence of differential OBGYN density in states that banned access to abortion care, relative to those that protected access. I estimate statistically insignificant average and late-period declines of 0.239 and 0.394 OBGYNs per 100,000 reproductive-age females (SEs: 0.368 and 0.525), relative to a baseline mean of 58.6. The 95% confidence intervals allow me to rule out average and late-period declines larger than 1.64% (0.960 providers per 100,000) and 2.43% (1.42 providers per 100,000). These precise nulls suggest that the overall headcount of providers has not responded to the implementation of abortion bans in the short run.

Compositional Changes. While the overall density of providers did not change as a result of bans on abortion care, the next section documents results on the composition of the OBGYN workforce. Specifically, the SDID event studies in Figure 4 present results for the density of Democratic, Republican, female, male, rural, early-career, top-50, and unranked OBGYNs. As before, the time weights are centered close to the introduction of a ban.[27] Identification rests on the weighted comparison tracking the total-ban states, especially in the high-weight periods. The pre-period coefficients generally match this interpretation, though the rural event study is noisier and shows imperfect pre-period fit.

Panels (a) and (b) outline the event-study coefficients for Democratic and Republican OBGYNs matched to DIME, respectively. Given that campaign contributors are a selected subset of the sample, these estimates primarily reflect location choice among the most politically engaged physicians, but are less generalizable to physicians who identify as Democratic or Republican, but do not make political donations.

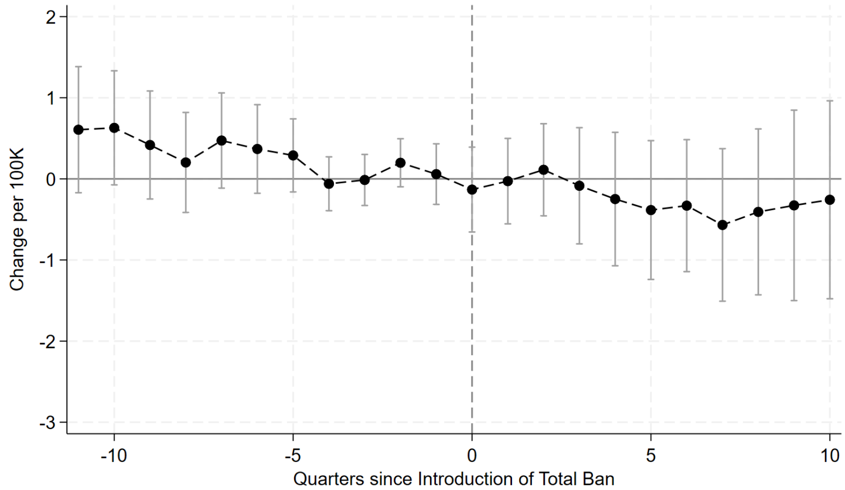
States with total abortion bans experience an average decline of 0.482 Democratic OBGYNs

[25] Because the introduction of bans differs across states, not all treated states contribute to every event-time coefficient. Indiana (event times +7 to +10) and North Dakota (event times +8 to +10) are excluded from the furthest post-periods due to the lack of post-ban data.

[26] Appendix Figure A6 extends the event-study paths from event times −17 through +10 and compares SDID's automatic unit-weight regularization with a specification that sets the regularization parameter to half of the default. Reducing regularization improves fit in the remote pre-treatment periods the estimator weights less heavily, but the post-treatment paths are close to indistinguishable. The extended pre-period spans the COVID-19 pandemic, and the combination of unit and time weights makes SDID more robust to potential structural breaks in pre-treatment outcomes.

[27] See Appendix Figure A7 for the time weights and weighted trends in subgroup outcomes.

Figure 3. Changes in State-Level Provider Density



This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined previously in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. The analysis utilizes the full sample of providers. Coefficients are for all OBGYNs.

per 100K females of reproductive age in the post-ban period. Relative to a pre-*Dobbs* mean of 9.45 Democratic OBGYNs per 100K in banned states, this corresponds to an average 5.10 percent reduction in Democratic provider density.

This shortfall grows over time. In the later post-ban period, the pooled estimate implies a gap of 0.808 Democratic OBGYNs per 100K females, corresponding to an 8.55 percent reduction relative to the pre-*Dobbs* baseline. Given the total population of females of reproductive age across total ban states, this implies an aggregate shortfall of approximately 145 Democratic OBGYNs in a given post-ban quarter.[28]

In contrast, panel (b) shows that the stock of Republican OBGYNs is fairly stable, and the effects are precisely estimated. I estimate a statistically insignificant decline of 0.0701 Republican OBGYNs per 100,000 reproductive-age females (SE: 0.0745) relative to a baseline mean of 9.47.

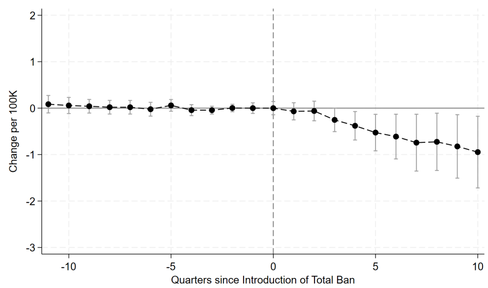
The partisan sorting documented above is mirrored by significant shifts in the demographic composition of the workforce. Panels (c) and (d) plot the event-study coefficients by provider gender. I estimate that banned states lost 0.971 female OBGYNs per 100,000 females (a 3.16% decline) while simultaneously gaining 0.378 male OBGYNs per 100,000 (a 1.35% increase), relative to the counterfactual. Using my preferred estimates in quarters 7 to 10 post-ban, the gender gap widens further, with female density falling by 4.31% and male density rising by 1.89% relative to the counterfactual.[29] These late-period effects correspond to an approximate shortfall of 237 female

[28]This calculation applies the estimated gap of 0.808 Democratic OBGYNs per 100K females to the total reproductive-age female population of 17,934,858 across total ban states: $0.808 \times (17{,}934{,}858/100{,}000) \approx 145$.

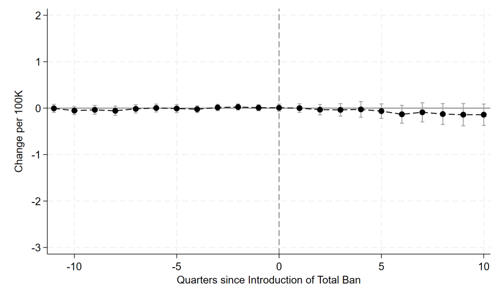
[29]The male Q7–Q10 point estimate falls just short of conventional statistical significance ($p = 0.102$), but the

Figure 4. Heterogeneity in OBGYN Supply Response to Abortion Bans

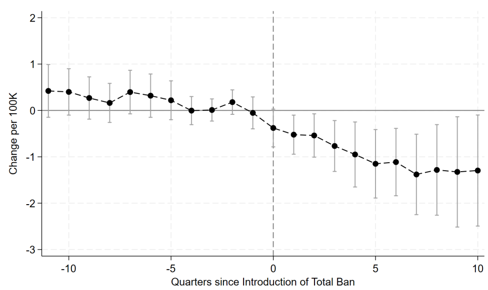
(a) Democratic OBGYNs



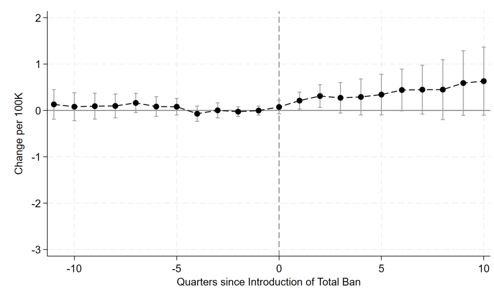
(b) Republican OBGYNs



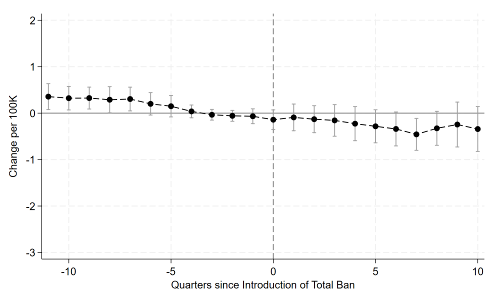
(c) Female OBGYNs



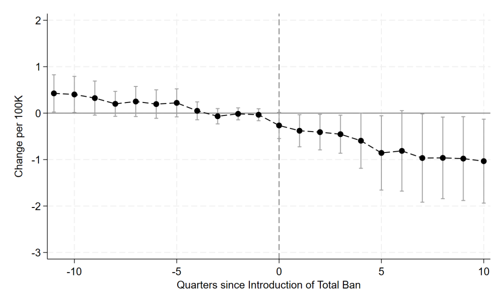
(d) Male OBGYNs



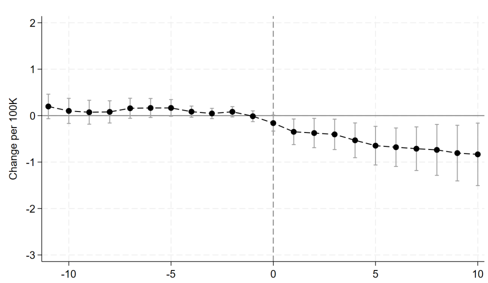
(e) Rural OBGYNs



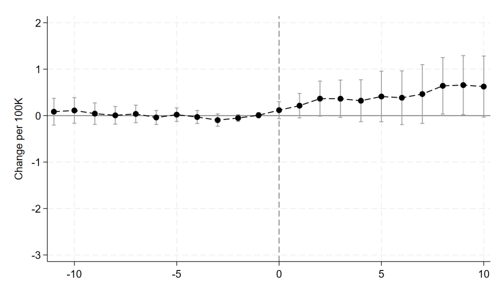
(f) Early-Career OBGYNs



(g) Top-50 OBGYNs



(h) Unranked OBGYNs



This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined previously in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Panels (a) and (b) utilize the sample of providers matched to DIME, panels (g) and (h) utilize the sample of providers matched to NDF, while panels (c), (d), (e), and (f) utilize the full sample of providers.

OBGYNs and an increase of roughly 95 male OBGYNs across all total-ban states.[30]

Next, panels (e) and (f) examine indicators of future supply and access in underserved areas. Although less precisely estimated, and with non-negligible pre-trends, there is suggestive evidence that the number of rural OBGYNs per capita declined in banned states relative to protected states (-0.345, SE: 0.206), corresponding to roughly a 3.17 percent decline in the later post-ban period from an already low baseline supply of rural providers. Taken literally, this magnitude implies roughly 62 fewer rural OBGYNs across all ban states 7 to 10 quarters after a ban is introduced.

When restricting to early-career OBGYNs—those registering in NPPES after 2014—I estimate a post-ban decline of 0.714 providers per 100,000 reproductive-age females in total ban states relative to protected states. Averaging over the final four post-ban quarters, the combined effect is -0.985 providers per 100,000, or roughly a 21.9 percent decline relative to the pre-*Dobbs* mean of 4.49 per 100K reproductive-age females in banned states. In levels, this corresponds to approximately 177 fewer early-career OBGYNs in ban states during the later post-ban period.

Finally, I examine whether abortion bans change the observable training credentials of the OBGYN workforce. To do so, I classify NDF-linked OBGYNs by the composite medical-school rankings used in Schnell and Currie (2018), as described in Section 3. This sample is selected,[31] but serves as useful evidence for Medicare-serving physicians.[32]

The compositional shifts documented above also extend to physician training background. Panels (g) and (h) plot the event-study coefficients for OBGYNs trained at top-50 medical schools and named but unranked medical schools, respectively. I estimate that total-ban states lost 0.771 top-50-trained OBGYNs per 100,000 reproductive-age females, a 9.03% decline relative to the pre-*Dobbs* baseline of 8.545 providers per 100,000. In contrast, the density of OBGYNs from named but unranked medical schools rose by 0.595 providers per 100,000, a 3.55% increase relative to the pre-*Dobbs* baseline.[33] These late-period effects correspond to an approximate shortfall of 138 top-50-trained OBGYNs and an increase of roughly 107 OBGYNs from named but unranked medical schools across all total-ban states.[34] These estimates provide further evidence that stable aggregate headcounts mask underlying changes in the composition of the provider workforce.

event-study trajectory is supportive of a growing compositional change.

[30]This calculation applies the estimated Q7–Q10 gaps in provider density to the total reproductive-age female population of 17,934,858 across total-ban states. For female OBGYNs: $-1.32 \times (17{,}934{,}858/100{,}000) \approx -237$. For male OBGYNs: $0.528 \times (17{,}934{,}858/100{,}000) \approx 95$.

[31]The NDF-linked sample comprises 39,602 providers, representing 77.0 percent of the April 2022 OBGYN stock. Compared to unlinked providers, NDF-linked OBGYNs are more likely to be female (63.7 percent vs. 42.8 percent) and exhibit higher baseline mobility (12-month out-migration of 2.54 percent vs. 0.586 percent). Geographic distributions are similar across both groups, as are rates of matching to DIME (39.4 percent vs. 40.7 percent).

[32]Appendix Table A4 presents the full suite of NDF-linked sample cuts. Within the NDF sample, aggregate OBGYN density does not fall in ban states relative to states that protect access to abortion care (+0.664 in quarters 7 to 10, SE: 0.630). Ranked declines persist at different threshold values (top 20, all ranked).

[33]The training-background result overlaps with demographic characteristics, but is not reducible to them. Ranked/top-ranked declines are concentrated among female OBGYNs and early-career providers, while named-unranked density rises more broadly across subgroups.

[34]This calculation applies the estimated late-period gaps in provider density to the total reproductive-age female population of 17,934,858 across total-ban states. For top-50-trained OBGYNs: $-0.771 \times (17{,}934{,}858/100{,}000) \approx -138$. For named but unranked OBGYNs: $0.595 \times (17{,}934{,}858/100{,}000) \approx 107$.

These heterogeneity estimates involve two selection issues. First, the partisan estimates apply only to DIME-matched physicians, capturing responses among contributors rather than all physicians who may identify with a party. To contextualize this, Appendix Table A5 partitions OBGYNs by DIME match status. Within the DIME-matched sample, overall density declines by 0.755 providers per 100K (SE = 0.483) in the late period. This contributor decline is highly concentrated among Democrats, who experience a late-period gap of 0.808 providers per 100K. While this represents an 8.55 percent drop relative to the Democratic baseline, it corresponds to 1.38 percent of the aggregate pre-*Dobbs* baseline density (58.60). Among unmatched OBGYNs, the aggregate estimate is negative but imprecise (−0.440; SE = 0.430), providing no evidence that non-contributors offset this decline.[35]

Second, given that gender and political affiliation are correlated among OBGYNs, the female estimates may partly reflect partisan sorting. Appendix Table A6 estimates effects separately by gender and party within the DIME-matched sample. The Democratic decline is heavily concentrated among female physicians, while Democratic male and all Republican densities remain stable. This pattern suggests that, among contributors, the post-*Dobbs* response is strongest at the intersection of gender and political affiliation, rather than along either dimension alone.

Because the SDID estimator derives optimal control group weights for each outcome variable to maximize pre-period fit, the synthetic counterfactual varies across specifications, and the subgroup estimates need not sum to the aggregate total. As I show in Section 6, however, these compositional findings are not driven by differences in the control groups. When employing a "unified counterfactual" that applies a single, fixed set of donor weights uniformly across all subgroups, the subgroup estimates sum to the total effect, and the overall patterns remain consistent.

[35] This partition also suggests the demographic shifts extend beyond contributors. In the unmatched sample, female density declines by 1.103 providers per 100K, early-career density declines by 0.788 providers per 100K, and male density increases by 0.420 providers per 100K in the later post-ban period. This is consistent with younger, early-career cohorts having less time and fewer resources to establish contribution histories. Further, the medical school background results persist in the unmatched sample.

Table 2. Changes in OBGYN Density, Overall and by Subgroup

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	-0.239	-0.482**	-0.0701	-0.971**	0.378*	-0.244	-0.714*	-0.583**	0.386^{+}
	(0.368)	(0.186)	(0.0745)	(0.328)	(0.189)	(0.160)	(0.306)	(0.203)	(0.222)
Effect (Q7–Q10)	-0.394	-0.808*	-0.124	-1.322**	0.528	-0.345^{+}	-0.985*	-0.771**	0.595^{+}
	(0.525)	(0.337)	(0.111)	(0.513)	(0.323)	(0.206)	(0.442)	(0.287)	(0.315)
Pre-*Dobbs* Treated Mean	58.60	9.447	9.474	30.68	27.92	10.88	4.493	8.545	16.76
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN									
Effect of Ban	-0.194	-0.478*	-0.0908	-0.990**	0.365	-0.239	-0.648*	-0.618**	0.437^{+}
	(0.416)	(0.192)	(0.0885)	(0.360)	(0.231)	(0.199)	(0.328)	(0.225)	(0.264)
Effect (Q7–Q10)	-0.438	-0.832*	-0.146	-1.371*	0.504	-0.351	-0.995*	-0.790**	0.668^{+}
	(0.568)	(0.350)	(0.121)	(0.543)	(0.366)	(0.237)	(0.470)	(0.296)	(0.354)
Pre-*Dobbs* Treated Mean	58.50	9.411	9.967	29.82	28.67	11.85	4.356	7.560	16.32
Observations	1073	1073	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. The main specification does not include additional covariates, as the SDID approach weights units and periods to mirror pre-treatment trends. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect. Appendix Table A7 shows that the results are robust to the inclusion of state-level economic controls.

5.2 Protected-State Spillovers

The stock estimates compare provider density in total-ban states to provider density in protected states. One concern is the potential for spillovers between these states. In particular, the estimates would overstate the magnitude of a stock decline in total-ban states if physicians who otherwise would have practiced in total-ban states instead locate in protected states. Symmetrically, the estimates would overstate a stock increase in total-ban states if those who would have practiced in protected states instead locate in total-ban states. In either case, the relative contrast reflects both a shift in total-ban states and a change in the protected comparison group. Other margins, such as moves to threatened states, exits from the observed OBGYN stock, or moves outside the protected comparison group, may contribute to stock changes in total-ban states but do not bias the estimates.

I can benchmark these spillovers. Suppose the total-ban-state stock change is L providers, and a share d of this change has an opposite-signed change in protected states. The density change in total-ban states is L/Pop_{ban}, while the density shift in protected states is $-dL/Pop_{protected}$. Thus, the observed ban-versus-protected density gap is inflated by

$$M(d) = 1 + d\frac{Pop_{ban}}{Pop_{protected}}.$$

Total-ban states are smaller in population than protected states, so the ratio $Pop_{ban}/Pop_{protected}$ is 0.461. This means spillovers must be smaller than the hypothetical upper bound of $M(d) = 2$ that would apply without per-capita rescaling, where all coefficients would simply be divided by two. No protected-state spillover implies $M(0) = 1$, so the estimates are unchanged. One-to-one reallocation implies $M(1) = 1.46$. This full-reallocation case is extreme, since it assumes every provider lost by one environment is gained by the other. Empirically, benchmarks based on incumbent transitions and entrant redistribution imply smaller multipliers, between 1.17 and 1.32. I provide further explanation in Appendix B, where Appendix Table B1 presents the adjusted stock estimates. Under these benchmarks, protected-state spillovers attenuate the magnitudes but do not explain away the stock findings.

5.3 Extensive and Destination Choice Margins of Migration

In this section, I decompose migration into (i) who moves (extensive margin) and (ii) where movers land (destination choice). Movers are defined quarterly. I classify states each quarter as Protected or Banned and track transitions between these policy regimes.

5.3.1 Extensive Margin

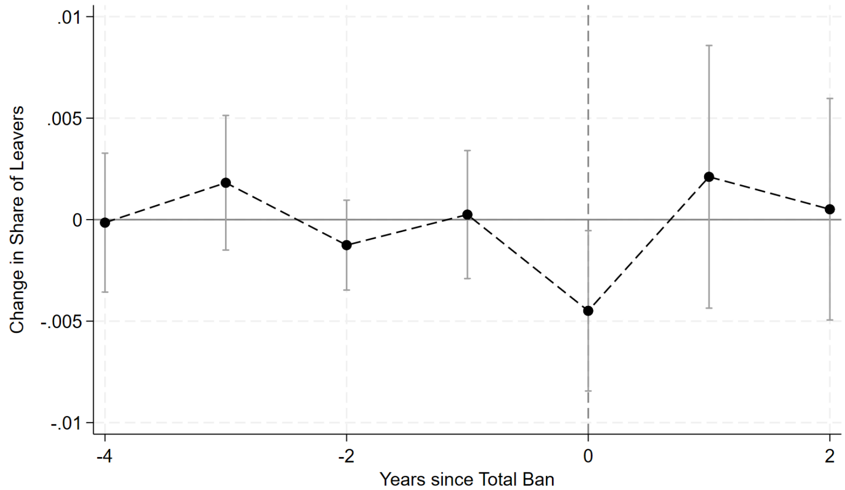
With the SDID approach defined in Equation 6, I ask, relative to a pre-ban baseline, how the share of providers who move states evolved in states that enacted abortion bans, relative to those that protected abortion access.

Aggregate Stability. Figure 5 plots the SDID event-study estimates for the overall out-migration probability of OBGYNs. Similar to the stock analysis, the pre-treatment coefficients are indistinguishable from zero, providing support for the design.

Post-ban, I find no evidence of outflows at the aggregate level. As reported in Column 1 of Table 3, the estimated ATT is a precise null (−0.0599 percentage points, SE: 0.177). The 95% confidence interval rules out migration increases larger than 0.29 percentage points, suggesting that the average incumbent OBGYN did not emigrate in response to the policy shock.

Compositional Sorting. While aggregate migration rates remained stable, Figure 6 reveals that this masks heterogeneity across partisan lines, while highlighting stability across the other dimensions.

Figure 5. OBGYN Migration Response to Abortion Bans



This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 6 for OBGYNs. The coefficients represent the change in the share of providers who move as defined previously in (2) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. This uses the full sample of providers.

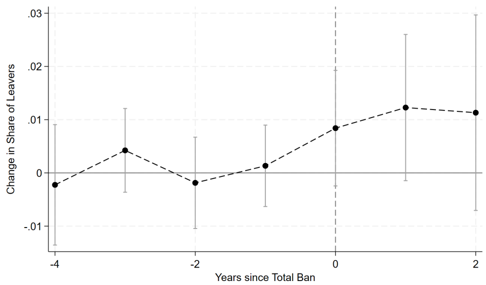
Panels (a) and (b) display the migration response for incumbent Democratic and Republican providers, respectively. As shown in Column 2 of Table 3, Democratic OBGYNs exhibit a statistically significant increase in out-migration of +1.06 percentage points (SE: 0.530 pp). Relative to a baseline migration rate of 2.69%, this represents a nearly 40% increase in the probability of out-migration. For Republican OBGYNs, however, I find no statistically significant change in out-migration. As shown in Column 3, the point estimate is negative (-1.27 percentage points, SE: 0.807 pp) but imprecise. Given the low baseline leaver share of 0.930% and the negative point estimate, I interpret this as evidence that migration remained relatively rare.

Panels (c) through (h) examine migration responses by gender, geography, career stage, and training background. In contrast to the partisan results, I find no statistically significant differential migration response among incumbents for female OBGYNs (0.0835 pp, SE: 0.269 pp), male OBGYNs (−0.242 pp, SE: 0.340 pp), rural providers (0.0313 pp, SE: 0.462 pp), early-career providers (−0.206 pp, SE: 1.31 pp), top-50 OBGYNs (0.836 pp, SE: 0.718 pp), or unranked OBGYNs (−0.535 pp, SE: 0.657 pp).

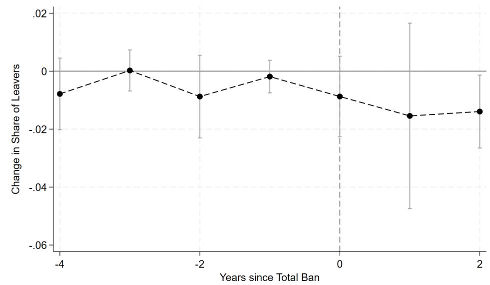
Taken together, these estimates provide little evidence that the stock changes documented in Section 5 reflect changes in out-migration among incumbent providers, although Democratic OBGYNs are a clear exception.

Figure 6. Heterogeneity in OBGYN Migration Response to Abortion Bans

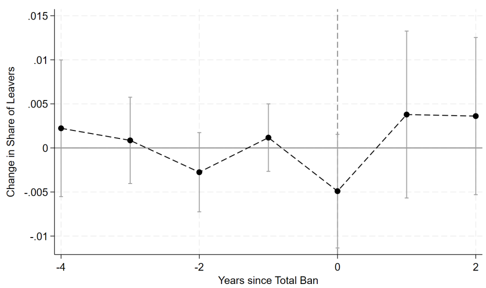
(a) Democratic OBGYNs



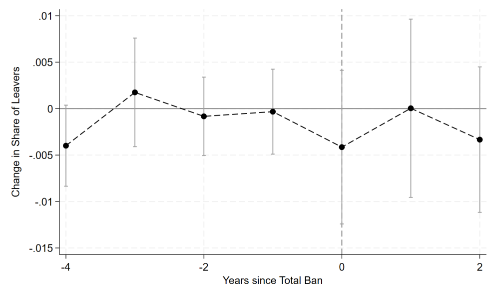
(b) Republican OBGYNs



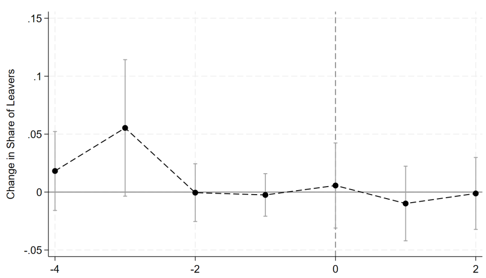
(c) Female OBGYNs



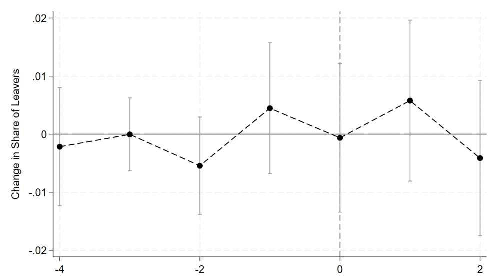
(d) Male OBGYNs



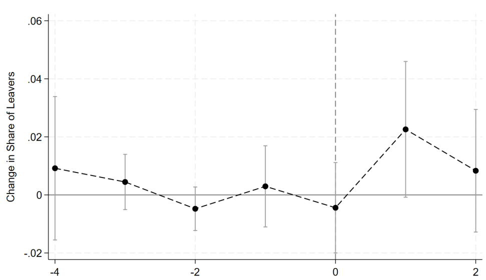
(e) Early-Career OBGYNs



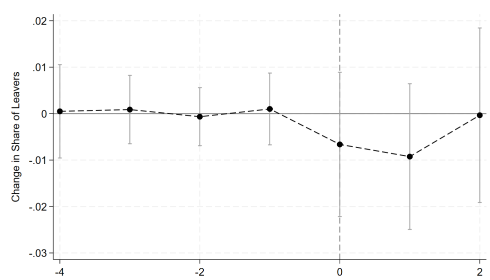
(f) Rural OBGYNs



(g) Top-50 OBGYNs



(h) Unranked OBGYNs



This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 6 for OBGYNs. The coefficients represent the change in the share of providers who leave their state as defined previously in (2) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Panels (a) and (b) utilize the sample of providers matched to DIME, panels (g) and (h) utilize the sample of providers matched to NDF, while panels (c), (d), (e), and (f) utilize the full sample of providers.

Table 3. Changes in the Share of OBGYN Leavers, Overall and by Subgroup

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	-0.000599	0.0106^{*}	-0.0127	0.000835	-0.00242	0.000313	-0.00206	0.00836	-0.00535
	(0.00177)	(0.00530)	(0.00807)	(0.00269)	(0.00340)	(0.00462)	(0.0131)	(0.00718)	(0.00657)
Pre-*Dobbs* Treated Mean	0.0228	0.0269	0.00930	0.0278	0.0189	0.0245	0.0286	0.0298	0.0262
Observations	280	280	280	280	280	280	280	280	280
Panel B: Excl. TX, ND, IN									
Effect of Ban	-0.0000498	0.0119^{+}	-0.0131	0.00132	-0.000894	-0.000134	-0.00125	0.0122	-0.00576
	(0.00185)	(0.00635)	(0.00859)	(0.00339)	(0.00331)	(0.00522)	(0.0154)	(0.00778)	(0.00785)
Pre-*Dobbs* Treated Mean	0.0241	0.0230	0.00999	0.0319	0.0173	0.0282	0.0292	0.0352	0.0306
Observations	259	259	259	259	259	259	259	259	259

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 6 for OBGYNs. The coefficients represent the change in the share of providers who leave their state as defined previously in (2) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from 2021, the final year before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

5.3.2 Destination Choice

The extensive-margin analysis estimates whether the share of physicians who leave their origin state changes before and after a ban, relative to the weighted counterfactual. In this section, I examine whether movers' destination choices across different policy environments differ after *Dobbs*.

My data are underpowered to study the changes in destination choice through an SDID framework because many state-year subgroup cells, especially in smaller states, lack any movers. I proceed with descriptive evidence, and calculate the share of movers from each origin whose destination is a protected, banned, or threatened state.

The destination choice shares condition on incumbent physicians who move. As shown in earlier sections, abortion bans alter the composition of providers across states and within observable groups. As a result, changes in conditional destination shares may reflect shifts in who moves, in addition to changes in destination choice among marginal movers.[36] This is further reason to interpret the destination choice results as descriptive.

Conditional on moving, per-origin destination probabilities are essentially unchanged for the

[36]For example, if the marginal Democratic mover is more political, then this may bias the estimated destination shares toward protected states.

full sample (Appendix Figure A8). Among providers leaving banned (B) states, the share moving to protected (P) states increased by only 1 percentage point (from 42% to 43%) after *Dobbs*, as did the share moving to other banned states (from 26% to 27%).

Appendix Figure A9 displays conditional destination shares by subgroup to identify potential heterogeneity in provider decisions. For Democratic providers, conditional destination shares do not shift drastically after *Dobbs*, with a 5 percentage point increase in moves to protected states (from 45% to 50%) and a 2 percentage point increase in moves to banned states (from 21% to 23%), which by construction account for a 7 percentage point decline in moves to threatened states. Republican destinations show greater fluctuation, but are limited by imprecision due to a small absolute number of movers.

Moving to gender, there is also stability, with both female and male destination shares remaining stable in the post-ban period. The share of male and female OBGYNs moving to protected states changes by at most 3 percentage points, across all policy environments.

Trends for rural providers exhibit a shift towards protected states, but small N leaves open the possibility that this is due to statistical noise. Early-career providers exhibit stable destination shares, conditional on moving.

Finally, the training background destination shares show some variation, but no consistent pattern. Among top-50 OBGYNs leaving ban states, the protected-state destination share rises from 45% to 53% after *Dobbs*. However, top-50 movers also become more likely to move to banned states across origin groups. For OBGYNs from named but unranked medical schools, the pattern looks more like persistence: protected-origin movers become slightly more likely to land in protected states, while ban-origin movers become more likely to land in banned or threatened states.

While I am unable to make a definitive case for the changes in margins, this evidence suggests that post-*Dobbs* changes in OBGYN supply are unlikely to be driven by incumbents changing their destination choices conditional on moving.

5.4 Reconciling Stocks and Flows

In the preceding sections, I documented changes in density for several key subgroups: Democratic, female, early-career, and top-50 relative density declined, while male and named-unranked relative density increased. However, the incumbent flow results indicate limited movement for most groups, with relative out-migration only increasing significantly for Democratic providers. Further, conditional on moving, incumbents' destination choices appear relatively stable. Mechanically, if incumbent outflows and destination choices are relatively stable, these compositional shifts must be driven by the "pipeline" margin—specifically, changes in new entrants and exits from the observed OBGYN stock.

To decompose changes in the stock outcome, Y_{gpst}^{Stock}, I define corresponding flow components on the same density scale. Y_{gpst}^{Entry} and Y_{gpst}^{Exit} represent gross inflows into and gross outflows from the observed OBGYN stock, including transitions into and out of the OBGYN taxonomy classification. Y_{gpst}^{InMig} and Y_{gpst}^{OutMig} capture cross-state migration among continuing physicians. Finally, $Y_{gpst}^{\text{NetSwitch}}$

represents net transitions into or out of subgroup g among same-state incumbents. Here, this accounts for an incumbent who relocates between rural and nonrural practices or whose gender classification changes across snapshots, as the other subgroup classifications are time-invariant.

The quarter-to-quarter density identity is:

$$Y_{gpst}^{\text{Stock}} - Y_{gps,t-1}^{\text{Stock}} = \left(Y_{gpst}^{\text{Entry}} - Y_{gpst}^{\text{Exit}}\right) + \left(Y_{gpst}^{\text{InMig}} - Y_{gpst}^{\text{OutMig}}\right) + Y_{gpst}^{\text{NetSwitch}} \tag{7}$$

Equivalently, the cumulative change relative to a baseline quarter 0 is:

$$Y_{gpst}^{\text{Stock}} - Y_{gps0}^{\text{Stock}} = \underbrace{\sum_{k=1}^{t}\left(Y_{gpsk}^{\text{Entry}} - Y_{gpsk}^{\text{Exit}}\right)}_{\text{Cumulative Net Entry-Exit}} + \underbrace{\sum_{k=1}^{t}\left(Y_{gpsk}^{\text{InMig}} - Y_{gpsk}^{\text{OutMig}}\right)}_{\text{Cumulative Net Migration}} + \underbrace{\sum_{k=1}^{t} Y_{gpsk}^{\text{NetSwitch}}}_{\text{Cumulative Net Subgroup Transitions}} \tag{8}$$

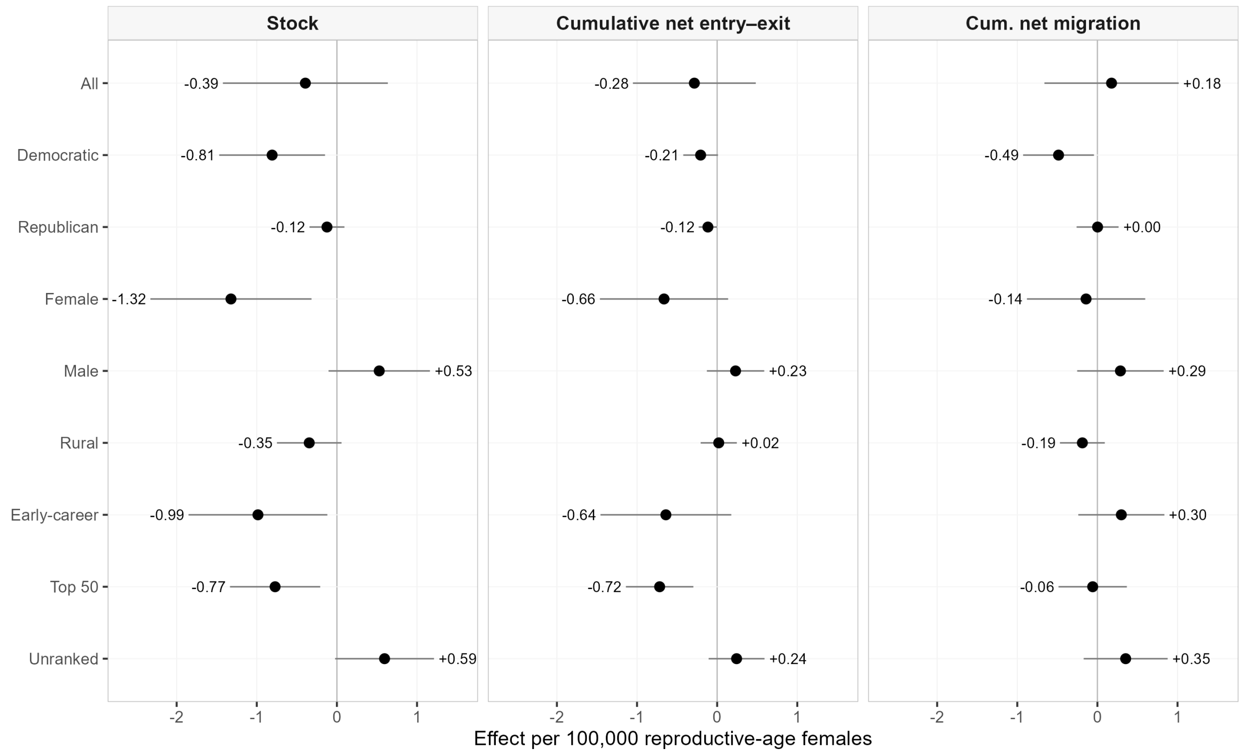
I use this accounting framework to connect the stock and flow results. Figure 7 reports the average effects for the late post-treatment period (Q7–Q10), estimating the stock, cumulative net entry-exit, and cumulative net migration components separately for each subgroup. Because each component is estimated with its own SDID weights, the sum of the flows will not mechanically equal the total stock effect, but their relative magnitudes help uncover the underlying mechanisms.

For all OBGYNs, both cumulative entry-exit and cumulative migration are small, matching the aggregate stock result. The Democratic decomposition corroborates the earlier incumbent-flow evidence, as the cumulative migration effect is relatively large and negative. Republican stability holds for both the entry-exit and migration components.

For the remaining subgroups, the accounting generally points more toward entry-exit than migration. Female, early-career, and top-50 density declines have larger cumulative entry-exit components than migration components, consistent with weaker accumulation into the observed OBGYN stock rather than broad incumbent relocation. The male and named-unranked increases are split more evenly across entry-exit and migration, though the individual components are imprecisely estimated. Finally, rural density moves little on either margin.

Together, this decomposition demonstrates that the stability of the aggregate workforce masks compositional churn through a mix of incumbent relocation and a shifting pipeline. In Section 6.6, I corroborate this evidence with provider-cohort restrictions and find that the effects for subgroups with large entry-exit components are concentrated among new entrants, rather than differential exits from the observed workforce.

Figure 7. Stock-flow accounting by subgroup



Bars report Q7–Q10 average effects from component-specific SDID event studies, in density units per 100,000 reproductive-age females. Entry and exit refer to transitions into and out of the observed OBGYN stock. Migration is net cross-state migration among continuing physicians. The net-switching component is omitted because it is functionally zero for every subgroup except rural providers. For rural providers, the switcher estimate is -0.12 (SE $= 0.06$). Horizontal bars represent the 95% confidence intervals, constructed with 1000 bootstraps. Components are estimated separately and are not constrained to add mechanically to the stock effect, by the nature of subgroup-specific SDID weights.

6 Robustness and Mechanisms

6.1 Leave-One-Out and Policy Sensitivity

One concern is that there might be particular states with large inflows or outflows driving the findings. However, Appendix Figure A10 shows that the results are not driven by any single state. Across subgroups, the point estimates are stable under the sequential exclusion of states from the estimation sample. For outcomes that are statistically significant in the full sample, estimates for Democratic, female, early-career, and top-50 physicians remain significant at the 95 percent level following the omission of each state. Most estimates for male physicians remain significant at conventional thresholds, and point estimates are uniformly positive. Named-unranked estimates are noisier, but the point estimates closely mirror the full sample findings.

Another concern is sensitivity to the coding of state policy environments. Wisconsin is one example, as some prior work (e.g., Dench et al. (2024), Dench et al. (2025), and Myers et al. (2025)) classifies it as a total ban state. I classify Wisconsin as threatened because its interruption in abortion care was temporary and legally contested. Providers in Wisconsin suspended services shortly before *Dobbs* due to uncertainty around an 1849 statute, but began reopening on September 18, 2023, after a lower-court ruling. Appendix Table A17 reruns the main SDID stock specification, but treats Wisconsin as a July 2022 total-ban state. The results are stable after this inclusion, suggesting that coding Wisconsin differently has no impact on the interpretation of this analysis.

6.2 Other Specialties

To assess whether the OBGYN results reflect a broader shift in the physician labor market or are concentrated among physicians most directly exposed to abortion restrictions, I broaden my analysis sample to include primary care providers and general surgeons, specialties with different exposure to abortion restrictions. Primary care providers serve as an intermediate comparison, given that some provide reproductive health services but face less direct exposure to abortion restrictions than OBGYNs. General surgeons practice in broader healthcare environments similar to those of OBGYNs but have minimal clinical exposure to abortion restrictions.

Among primary care providers, Appendix Table A18 shows limited evidence of an aggregate decline in density in ban states relative to protected states. In the later Q7–Q10 period, the estimate is -4.38 providers per 100,000 reproductive-age females (SE: 3.05). I also find no comparable partisan sorting: the Democratic (0.169, SE: 0.561) and Republican (-0.309, SE: 0.357) estimates are both statistically indistinguishable from zero. Female primary-care density declines by 4.32 providers per 100,000 reproductive-age females (SE: 1.47), or 3.23 percent from baseline. The early-career (-4.46, SE: 2.67, or 7.21 percent) and top-50 (-1.15, SE: 0.653) estimates are negative and marginally significant. The remaining subgroup estimates are statistically indistinguishable from zero.

Turning to general surgeons, a plausibly less-exposed specialty, Appendix Table A19 shows no evidence of an aggregate decline in general-surgeon density. The Q7–Q10 aggregate estimate is positive but statistically indistinguishable from zero (1.09, SE: 0.721), a 1.89 percent increase from baseline. I likewise find no partisan sorting: the Democratic (-0.0184, SE: 0.156) and Republican (-0.0739, SE: 0.135) estimates are both statistically indistinguishable from zero. General surgeons are 76 percent male in my sample, and male density increases in total-ban states relative to protected states (1.18, SE: 0.550), a 2.53 percent increase from baseline. Rural general-surgeon density declines by 0.357 providers per 100,000 reproductive-age females (SE: 0.180), a 2.87 percent decline from baseline. The remaining subgroup estimates are statistically indistinguishable from zero.

Together, these findings suggest that responses to abortion bans vary with both specialty and physician characteristics. Negative estimates for female, early-career, and top-50-trained providers also appear in primary care, suggesting that abortion bans have broader labor-market salience even where professional exposure is lower. OBGYN responses extend across more dimensions, including partisan identity, consistent with direct clinical exposure amplifying these broader responses.

6.3 Within-State Identification Strategy

To distinguish changes in OBGYN density from shocks shared across physician specialties within a state, I compare OBGYNs with general surgeons within each state and quarter.

I estimate a model analogous to a triple-difference design, comparing how the gap between OBGYN and general-surgeon density changes from the pre-ban to post-ban period across policy regimes. Although OBGYNs and general surgeons differ meaningfully in gender composition, surgeons provide a useful professional benchmark because they practice in similar healthcare environments as OBGYNs but have much less exposure to abortion restrictions. Differencing OBGYN and general-surgeon density within each state and quarter absorbs state-specific, time-varying shocks to physician supply that affect both specialties similarly.

Appendix Table A20 reports the results. In the later Q7–Q10 period, the OBGYN–general-surgeon density gap in total-ban states declines by 1.67 providers per 100,000 reproductive-age females (SE: 0.714), consistent with a separately estimated negative, imprecise OBGYN change and a positive, imprecise general-surgeon change. The Q7–Q10 relative decline is statistically significant for Democratic (−0.653, SE: 0.290) and top-50-trained (−0.763, SE: 0.298) physicians and marginally significant for female (−1.16, SE: 0.630) and early-career (−1.11, SE: 0.579) physicians. The Republican (−0.00539, SE: 0.201), male (−0.483, SE: 0.520), rural (−0.183, SE: 0.235), and named-unranked (0.498, SE: 0.382) estimates are statistically indistinguishable from zero, though the average post-ban named-unranked estimate remains positive and marginally significant.

Together with the null aggregate estimates for both specialties in levels, these findings provide little evidence of a broad physician exodus. Instead, OBGYN density declines relative to general-surgeon density within the same states, mirroring the partisan, gender, career-stage, and training-background differences in the main results.

6.4 Time-Varying Denominator

The primary specification fixes each state's reproductive-age female population using the 2021 ACS five-year estimate. This keeps the population scale fixed, so post-ban changes in provider density reflect changes in provider stocks rather than contemporaneous population changes. This choice reflects the object of interest: the analysis aims to capture the impact of abortion bans on physician location choices, while using the reproductive-age female population to scale provider stocks by the relevant population served. That said, if reproductive-age female population shifted differentially across ban and protected states after *Dobbs*, then fixed-denominator densities could differ from current population ratios. Appendix Table A21 presents the main stock estimates using ACS female 15–49 population estimates interpolated to the quarterly panel, rather than a fixed denominator. Aggregate OBGYN density remains close to zero, while declines for Democratic, female, rural, early-career, and top-50 OBGYNs and increases for male and named-unranked OBGYNs remain significant at conventional or marginal levels. Republican density is also suggestively negative, though the estimate remains close to its main-specification value.

6.5 Excluding Post-2020 Party Contributors

A potential concern with the primary analysis is that the *Dobbs* decision itself may have mobilized political participation, rendering the partisan classification endogenous to the treatment. To address this concern, I estimate a robustness specification that calculates partisanship based on contributions only up to the 2020 election cycle. This restriction conditions the analysis on physicians whose partisan affiliation is revealed before *Dobbs*. In the baseline specification, each physician is assigned a time-invariant partisan affiliation using all observed contributions through the 2024 election cycle. Although this affiliation is fixed for the entire sample period, it incorporates post-*Dobbs* donation behavior, motivating the robustness check.

Appendix Table A22 presents these results. Under this restriction, the Democratic estimate remains statistically significant, while the Republican estimate is null on average and suggestively negative in Q7–Q10, with the late-period point estimate 0.056 providers per 100,000 more negative than in the main specification. In the later Q7–Q10 period, Democratic provider density declines by 0.469 per 100,000 reproductive-age females (SE: 0.189), corresponding to a 5.56 percent decline relative to the pre-ban baseline of 8.44. Although this magnitude is smaller than the 8.55 percent decline in the main specification, the attenuation is consistent with the composition of the restricted sample, which disproportionately selects older, less mobile physicians.

6.6 Provider-Cohort Restrictions

One way to further test the entry-exit margin and separate the two components is to use different sample restrictions. In this section, I re-estimate the late-period stock effects using four samples: the full sample, providers observed at the start of the panel (excluding later entrants), providers observed at the end of the panel (excluding those who exited before the end of the panel), and a balanced panel of providers observed at both endpoints (excluding both margins).

Appendix Table A23 reveals an asymmetric pattern across the different samples. When I exclude later entrants in Panel B, the female, early-career, and training background point estimates attenuate by 64-70% relative to full sample. In contrast, when I include later entrants but exclude exits in Panel C, the estimates for these subgroups closely track the main results. The balanced sample (Panel D) estimates resemble those in Panel B. The Democratic decline persists across samples, the male effect attenuates to a lesser extent, and the aggregate and Republican estimates remain indistinguishable from zero.[37]

Together, these estimates provide evidence for the underlying mechanism. Given that female, early-career, and training-background effects shrink primarily when entrants are removed from the sample, the evidence points to changes concentrated among new entrants rather than differential attrition. The persistent Democratic decline across samples aligns with an important role for incumbent migration, while the more muted male attenuation is consistent with mixed margins. Combined with the evidence in Section 5.4, these results point to an important role for the pipeline

[37]The rural estimate has a weaker pre-period fit, so I do not interpret it, but report coefficients for completeness.

of new entrants in shaping future provider composition.

6.7 Sensitivity to Control Group Composition

One concern with the primary analysis is that the synthetic counterfactual varies across subgroups, complicating direct comparisons (e.g., the control group for male providers differs from that for female providers). To address this, I implement a "unified counterfactual" robustness check following Sun et al. (2025). I derive a single set of donor weights based on the aggregate OBGYN analysis and apply these fixed weights to all subgroup analyses using the Callaway and Sant'Anna (2021) estimator. More details are provided in Appendix C.

I find that the main results persist with a unified counterfactual. Appendix Table C2 shows that the aggregate estimate remains close to zero in the later Q7–Q10 period (−0.499, SE: 0.556). Democratic OBGYN density declines by 0.510 providers per 100,000 reproductive-age females (SE: 0.219), or 5.40 percent relative to baseline. Female density declines by 1.30 providers (SE: 0.511), while male density increases by 0.805 providers (SE: 0.316), corresponding to changes of 4.25 percent and 2.88 percent, respectively. Top-50 density declines by 0.588 providers per 100,000 reproductive-age females (SE: 0.269), while named-unranked density increases by 0.779 providers (SE: 0.331), corresponding to changes of 6.88 percent and 4.65 percent, respectively. Rural density declines by 0.439 providers (SE: 0.216), or 4.04 percent, and early-career density declines by 0.758 providers (SE: 0.325), or 16.9 percent. Both subgroups have imperfect pre-period fit under the unified counterfactual, so I refrain from interpreting these effect sizes literally. The Republican estimate remains essentially zero (−0.000302, SE: 0.127). This consistency provides evidence that the observed heterogeneity is driven by differential responses to the bans, rather than by compositional changes in the counterfactual control group.

7 Conclusion

This paper evaluates how post-*Dobbs* abortion bans have reshaped the physician labor market. While existing studies find limited aggregate changes in OBGYN supply, I show that this top-line stability masks a sharp and multidimensional reallocation of the workforce. Beneath the aggregate null, Democratic OBGYN density declines by 8.55 percent of baseline in the later post-ban period, while Republican stocks remain stable. Gender margins also diverge: female density declines, while male density rises. As migration, entry, and exit occur gradually, these compositional gaps emerge over time, with late-period estimates generally larger in absolute value than the average post-ban estimates.

The compositional changes extend to career stage and the observable human capital of the workforce. In the later post-ban period, early-career OBGYN density falls by 21.9 percent relative to baseline. The density of top-50-trained OBGYNs also declines, while named-unranked density rises. Taken together, the evidence suggests that aggregate stability in the short run can mask changes in who provides reproductive care, where new providers choose to practice, and the training backgrounds represented in the active workforce. A stock-flow decomposition points to entry and exit, rather than incumbent relocation, as the primary margin behind most of these compositional shifts. Evidence from provider-cohort restrictions indicates that the entry-exit margin is concentrated among new entrants rather than differential attrition, while migration plays an important role in the Democratic decline.

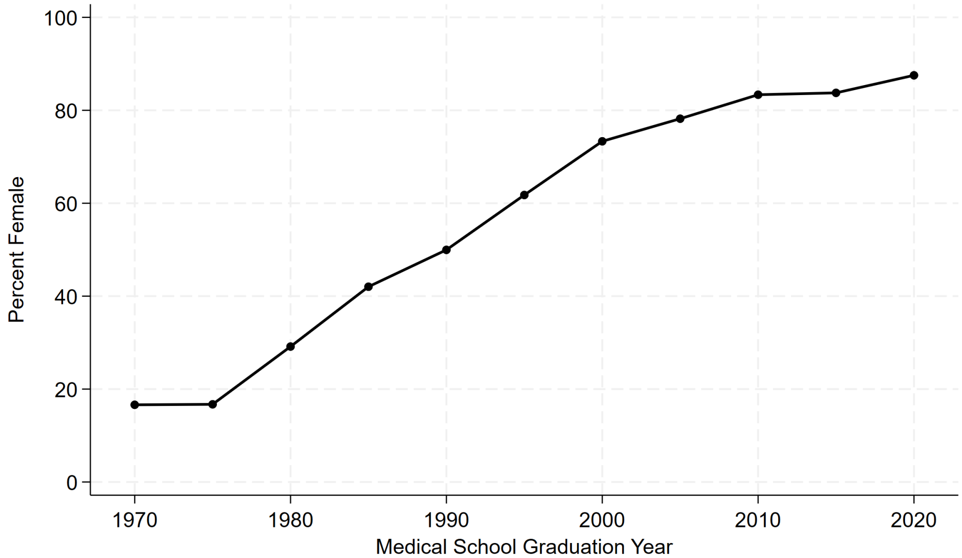
Although aggregate OBGYN density has remained stable in the short run, Figure 8 illustrates why this stability may not persist. Recent cohorts have shifted toward the groups most responsive to abortion bans: in my sample, almost 90 percent of 2020 graduates are female, compared with less than 20 percent of 1970 graduates, while among partisan contributors, over 90 percent are Democratic, compared with less than 50 percent of 1970 graduates. As the incumbent workforce—which is disproportionately male, Republican, and late-career—retires, current compositional gaps could eventually translate into aggregate supply shortfalls in states with total bans on abortion care.

The sorting identified here also has important short-run implications for reproductive care markets, even absent aggregate changes. Changes in which physicians practice in total-ban states may alter the clinical perspectives represented in local markets and the types of care available to patients. These shifts may affect patient–provider matching, especially in total-ban states, where average OBGYN density was already lower than in protected states before *Dobbs*, potentially leaving patients with fewer alternative providers.

This paper demonstrates the value of linking administrative practice location data to political contribution records to study heterogeneous labor market responses to polarized policy shocks. Doing so makes political alignment observable, capturing an otherwise hidden dimension of preferences that may help explain why workers respond differently to the same policy environment. Combined with a synthetic difference-in-differences design, this linkage reveals compositional changes obscured by stable aggregate supply. Future work should assess whether similar compositional changes arise in response to other polarized healthcare policies, such as restrictions on gender-affirming care and vaccine mandates, and how these changes affect the quality and quantity of care provided. More broadly, applying this framework to other high-skill occupations would allow researchers to assess how polarized state policies shape workforce composition.

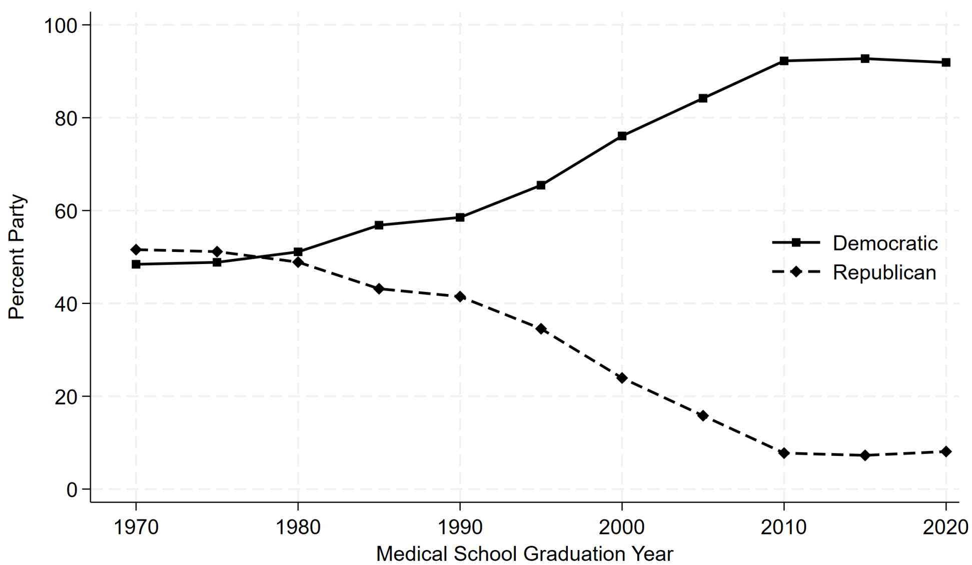
Figure 8. Demographic Shifts Over Time

(a) Gender Composition of OBGYN Graduates, 1970–2020



This figure displays the share of new OBGYNs who are female, by medical school graduation year. Graduation years are grouped into five-year cohorts. This uses the set of providers matched to the NDF. Appendix Figure A11 shows this sample follows similar patterns to the full sample, for whom enumeration dates reflect new entrants from 2008 onward.

(b) Party Composition of OBGYN Graduates, 1970–2020



This figure displays the share of new OBGYNs who are classified as Democrats or Republicans (omitting undecided contributors), by medical school graduation year. Graduation years are grouped into five-year cohorts. This uses the set of providers matched to the NDF and DIME. Appendix Figure A11 shows that this sample follows similar patterns to the broader DIME-matched sample, for whom enumeration dates reflect new entrants from 2008 onward.

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Online Appendix

The Impact of Abortion Bans on the Physician Labor Market: Compositional and Pipeline Effects

Ethan Bergmann (2026)

A Appendix Tables and Figures

Appendix Table A1. Categorizing OBGYNs

Primary taxonomy	Name
Panel A: Obstetrics-Gynecology Physicians	
207V00000X	Obstetrics & Gynecology Physician
207VF0040X	Female Pelvic Medicine and Reconstructive Surgery (Obstetrics & Gynecology) Physician
207VX0201X	Gynecologic Oncology Physician
207VG0400X	Gynecology Physician
207VM0101X	Maternal & Fetal Medicine Physician
207VX0000X	Obstetrics Physician

This table displays the list of taxonomy codes selected to build the analysis sample. I gather any providers in the NPPES data with one of these specialties listed as their primary taxonomy. The list follows Zhu et al. (2025a), though they include a broader classification of OBGYNs, while I utilize only those in Panel A, under the guidance of medical practitioners.

Appendix Table A2. Categorizing Primary Care and General Surgeons

Primary taxonomy	Name
Panel A: General Surgery Physicians	
208600000X	Surgery Physician
2086H0002X	Hospice and Palliative Medicine (Surgery) Physician
2086S0120X	Pediatric Surgery Physician
2086P0122X	Physician Nutrition Specialist (Surgery)
2086S0122X	Plastic and Reconstructive Surgery Physician
2086S0105X	Surgery of the Hand (Surgery) Physician
2086S0102X	Surgical Critical Care Physician
2086X0206X	Surgical Oncology Physician
2086S0127X	Trauma Surgery Physician
2086S0129X	Vascular Surgery Physician
Panel B: Primary Care Physicians	
207Q00000X	Family Medicine Physician
207QA0505X	Adult Medicine Physician
207QG0300X	Geriatric Medicine (Family Medicine) Physician
207R00000X	Internal Medicine Physician
207RG0300X	Geriatric Medicine (Internal Medicine) Physician
2083P0901X	Public Health & General Preventive Medicine Physician
208D00000X	General Practice Physician

This table displays the list of taxonomy codes selected to complement the main analysis sample. I gather any providers in the NPPES data with one of these specialties listed as their primary taxonomy to supplement the main specialties in Appendix Table A1.

Appendix Table A3. Treatment Definition

State Classification	States
Total Ban States	Alabama, Arkansas, Idaho, Indiana, Kentucky, Louisiana, Mississippi, Missouri, North Dakota, Oklahoma, South Dakota, Tennessee, Texas, West Virginia
Threatened States	Arizona, Florida, Georgia, Iowa, North Carolina, Nebraska, Ohio, South Carolina, Utah, Wisconsin, Wyoming
Protected States	Alaska, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Kansas, Massachusetts, Maryland, Maine, Michigan, Minnesota, Montana, New Hampshire, New Jersey, New Mexico, Nevada, New York, Oregon, Pennsylvania, Rhode Island, Virginia, Vermont, Washington, District of Columbia

Treatment status is gathered from the NYT ongoing classification (snapshot, August 2024) (McCann and Walker, 2022). Total-ban states had a total ban on abortion as of August 2024. Threatened states may have had a gestational ban in place or were deemed likely to enforce bans in the future. Finally, protected states either expanded abortion rights in the aftermath of *Dobbs* or kept protections in place.

Appendix Table A4. Changes in OBGYN Density by Medical School Ranking

	(1) NDF linked	(2) Ranked	(3) Top 10	(4) Top 20	(5) Top 50	(6) Unranked	(7) Other
Panel A: All States							
Effect of Ban	0.451	-0.706	-0.0753	-0.233**	-0.583**	0.386+	-0.220
	(0.424)	(0.455)	(0.0561)	(0.0779)	(0.203)	(0.222)	(0.211)
Effect (Q7–Q10)	0.664	-1.121	-0.125	-0.352**	-0.771**	0.595+	-0.423
	(0.630)	(0.690)	(0.0883)	(0.111)	(0.287)	(0.315)	(0.284)
Pre-*Dobbs* Treated Mean	47.61	21.57	0.916	2.090	8.545	16.76	9.288
Observations	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN							
Effect of Ban	0.611	-0.720	-0.0755	-0.252**	-0.618**	0.437+	-0.317
	(0.512)	(0.483)	(0.0577)	(0.0833)	(0.225)	(0.264)	(0.231)
Effect (Q7–Q10)	0.727	-1.155	-0.124	-0.367**	-0.790**	0.668+	-0.529+
	(0.697)	(0.710)	(0.0944)	(0.115)	(0.296)	(0.354)	(0.295)
Pre-*Dobbs* Treated Mean	47.43	21.69	1.062	2.121	7.560	16.32	9.422
Observations	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. All columns utilize the sample of providers linked to the Medicare National Downloadable File (NDF). Coefficients are for subsets of this sample based on composite medical school rankings: (1) all NDF-linked OBGYNs, (2) OBGYNs from ranked medical schools, (3) OBGYNs from top-10 medical schools, (4) OBGYNs from top-20 medical schools, (5) OBGYNs from top-50 medical schools, (6) OBGYNs from named but unranked medical schools, and (7) OBGYNs from other medical schools. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. The main specification does not include additional covariates, as the SDID approach weights units and periods to mirror pre-treatment trends. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A5. Changes in OBGYN Density by DIME Match Status and Subgroup

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: Full OBGYN Sample									
Effect of Ban	-0.239	-0.482**	-0.070	-0.971**	0.378*	-0.244	-0.714*	-0.583**	0.386+
	(0.368)	(0.186)	(0.074)	(0.328)	(0.189)	(0.160)	(0.306)	(0.203)	(0.222)
Effect (Q7–Q10)	-0.394	-0.808*	-0.124	-1.322**	0.528	-0.345+	-0.985*	-0.771**	0.595+
	(0.525)	(0.337)	(0.111)	(0.513)	(0.323)	(0.206)	(0.442)	(0.287)	(0.315)
Pre-*Dobbs* Treated Mean	58.60	9.447	9.474	30.68	27.92	10.88	4.493	8.545	16.76
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: DIME-Matched OBGYNs									
Effect of Ban	-0.388	-0.482**	-0.070	-0.333*	0.021	-0.127	-0.184	-0.240**	0.023
	(0.269)	(0.186)	(0.074)	(0.151)	(0.107)	(0.084)	(0.125)	(0.081)	(0.111)
Effect (Q7–Q10)	-0.755	-0.808*	-0.124	-0.637*	0.079	-0.290*	-0.208	-0.303**	0.048
	(0.483)	(0.337)	(0.111)	(0.266)	(0.166)	(0.116)	(0.164)	(0.115)	(0.162)
Pre-*Dobbs* Treated Mean	23.13	9.447	9.474	10.10	13.03	4.392	0.833	3.847	6.033
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel C: DIME-Unmatched OBGYNs									
Effect of Ban	-0.409	–	–	-0.890**	0.326*	-0.222*	-0.589*	-0.336*	0.268
	(0.343)	–	–	(0.274)	(0.150)	(0.108)	(0.258)	(0.148)	(0.172)
Effect (Q7–Q10)	-0.440	–	–	-1.103**	0.420+	-0.261+	-0.788*	-0.466*	0.470+
	(0.430)	–	–	(0.348)	(0.250)	(0.138)	(0.355)	(0.221)	(0.263)
Pre-*Dobbs* Treated Mean	35.47	–	–	20.58	14.89	6.489	3.660	4.698	10.73
Observations	1160	–	–	1160	1160	1160	1160	1160	1160

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. The panels denote the sample of providers utilized for estimation: Panel A utilizes the full sample of OBGYNs, Panel B restricts the sample to providers matched to DIME campaign contribution records, and Panel C restricts the sample to strictly unmatched providers. The columns denote the specific subgroup analyzed within each panel's sample: (1) all OBGYNs, (2) OBGYNs deemed Democrats, (3) OBGYNs deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. Columns (8) and (9) utilize the sample of providers matched to NDF within each panel. Because partisan identities are unobserved in the unmatched sample, Columns (2) and (3) are omitted from Panel C. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column-panel cell is estimated via a separate synthetic difference-in-differences specification on a subgroup-specific sample, drawing on subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A6. Changes in OBGYN Density by Gender among Democrats/Republicans

	(1) Dem Fem	(2) Dem Male	(3) Rep Fem	(4) Rep Male
Panel A: All States				
Effect of Ban	-0.530**	-0.0207	-0.0303	-0.0691
	(0.150)	(0.0617)	(0.0369)	(0.0595)
Effect (Q7–Q10)	-0.856**	-0.00543	-0.0372	-0.114
	(0.269)	(0.0818)	(0.0561)	(0.0947)
Pre-*Dobbs* Treated Mean	5.34	4.11	2.45	7.03
Observations	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN				
Effect of Ban	-0.549**	-0.0117	-0.0419	-0.0822
	(0.153)	(0.0735)	(0.0439)	(0.0723)
Effect (Q7–Q10)	-0.902**	-0.00216	-0.0461	-0.130
	(0.274)	(0.0926)	(0.0611)	(0.105)
Pre-*Dobbs* Treated Mean	5.15	4.26	2.47	7.50
Observations	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. This utilizes the sample of providers matched to DIME, with the majority of their donations to one of the two major parties. Coefficients are for subsets of said sample: (1) Democratic female OBGYNs, (2) Democratic male OBGYNs, (3) Republican female OBGYNs, and (4) Republican male OBGYNs. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A7. Changes in OBGYN Density, Overall and by Subgroup (Economic Controls)

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: Population Trend and Unemployment									
Effect of Ban	-0.319	-0.521**	-0.0703	-0.993**	0.370*	-0.256	-0.710*	-0.672**	0.345
	(0.361)	(0.186)	(0.0782)	(0.373)	(0.183)	(0.165)	(0.341)	(0.213)	(0.236)
Effect (Q7–Q10)	-0.503	-0.857*	-0.123	-1.391*	0.514^{+}	-0.358^{+}	-0.996*	-0.910**	0.538^{+}
	(0.511)	(0.334)	(0.115)	(0.590)	(0.309)	(0.212)	(0.496)	(0.300)	(0.327)
Pre-*Dobbs* Treated Mean	58.60	9.447	9.474	30.68	27.92	10.88	4.493	8.545	16.76
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Plus House Price Index									
Effect of Ban	-0.452	-0.522**	-0.0724	-1.165**	0.377^{+}	-0.267	-0.760^{+}	-0.685**	0.312
	(0.404)	(0.198)	(0.0804)	(0.444)	(0.219)	(0.163)	(0.415)	(0.224)	(0.239)
Effect (Q7–Q10)	-0.794	-0.854*	-0.124	-1.763*	0.549	-0.330	-1.187*	-0.939**	0.473
	(0.613)	(0.358)	(0.118)	(0.713)	(0.358)	(0.210)	(0.603)	(0.327)	(0.337)
Pre-*Dobbs* Treated Mean	58.60	9.447	9.474	30.68	27.92	10.88	4.493	8.545	16.76
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect. Panel A adjusts for the state's seasonally adjusted unemployment rate from FRED and a linear population trend constructed by interacting the state's 2015–2019 working-age population growth rate from the American Community Survey with a consecutive quarterly time index. Panel B additionally adjusts for the non-seasonally adjusted state house-price index from the Federal Housing Finance Agency. All specifications use the projected covariate method in the sdid-event package. Standard errors are based on 1,000 state-clustered bootstrap replications.

Appendix Table A8. SDID Unit Weights: All OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.038	0.033	0.014	0.000	0.010
CA	0.053	0.056	0.059	0.061	0.049
CO	0.038	0.005	0.011	0.000	0.060
CT	0.021	0.000	0.004	0.026	0.003
DC	0.008	0.000	0.000	0.020	0.006
DE	0.078	0.087	0.112	0.090	0.079
HI	0.001	0.000	0.000	0.000	0.093
IL	0.043	0.054	0.051	0.017	0.043
KS	0.062	0.085	0.082	0.056	0.066
MA	0.055	0.107	0.093	0.090	0.028
MD	0.052	0.039	0.051	0.068	0.047
ME	0.046	0.017	0.033	0.086	0.047
MI	0.065	0.149	0.122	0.022	0.052
MN	0.003	0.000	0.000	0.000	0.000
MT	0.032	0.026	0.030	0.037	0.017
NH	0.030	0.000	0.000	0.000	0.065
NJ	0.042	0.032	0.033	0.054	0.032
NM	0.048	0.070	0.068	0.114	0.020
NV	0.031	0.000	0.000	0.042	0.010
NY	0.046	0.028	0.032	0.041	0.046
OR	0.042	0.000	0.009	0.020	0.056
PA	0.051	0.087	0.080	0.034	0.056
RI	0.000	0.000	0.000	0.013	0.000
VA	0.046	0.057	0.050	0.000	0.068
VT	0.024	0.069	0.052	0.070	0.000
WA	0.043	0.000	0.014	0.037	0.047

The table shows the SDID unit weights from Equation 3 for the main specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A9. SDID Unit Weights: Democratic OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.041	0.023	0.024	0.048	0.004
CA	0.040	0.036	0.036	0.000	0.041
CO	0.034	0.018	0.022	0.000	0.017
CT	0.024	0.022	0.023	0.060	0.030
DC	0.057	0.070	0.074	0.117	0.011
DE	0.054	0.098	0.084	0.000	0.004
HI	0.016	0.001	0.002	0.129	0.022
IL	0.040	0.053	0.051	0.000	0.063
KS	0.054	0.076	0.067	0.000	0.043
MA	0.035	0.034	0.040	0.077	0.046
MD	0.039	0.044	0.048	0.013	0.047
ME	0.021	0.019	0.020	0.088	0.010
MI	0.046	0.067	0.061	0.000	0.044
MN	0.026	0.000	0.000	0.086	0.025
MT	0.064	0.067	0.059	0.000	0.044
NH	0.033	0.000	0.000	0.000	0.061
NJ	0.035	0.043	0.040	0.027	0.065
NM	0.038	0.051	0.056	0.270	0.052
NV	0.051	0.044	0.041	0.000	0.057
NY	0.040	0.031	0.029	0.000	0.050
OR	0.045	0.028	0.038	0.000	0.026
PA	0.038	0.038	0.035	0.000	0.065
RI	0.032	0.019	0.025	0.000	0.000
VA	0.031	0.028	0.029	0.000	0.023
VT	0.030	0.065	0.068	0.031	0.077
WA	0.037	0.025	0.028	0.054	0.070

The table shows the SDID unit weights from Equation 3 for the Democratic OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A10. SDID Unit Weights: Republican OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.027	0.000	0.021	0.000	0.079
CA	0.040	0.031	0.038	0.000	0.042
CO	0.043	0.032	0.046	0.000	0.062
CT	0.039	0.052	0.027	0.029	0.000
DC	0.054	0.118	0.062	0.071	0.007
DE	0.041	0.009	0.067	0.029	0.032
HI	0.032	0.043	0.051	0.161	0.018
IL	0.043	0.061	0.041	0.061	0.000
KS	0.044	0.055	0.047	0.075	0.025
MA	0.034	0.058	0.046	0.170	0.005
MD	0.040	0.024	0.052	0.000	0.058
ME	0.029	0.017	0.054	0.154	0.043
MI	0.041	0.040	0.043	0.042	0.023
MN	0.042	0.039	0.041	0.004	0.060
MT	0.021	0.000	0.002	0.000	0.064
NH	0.041	0.036	0.043	0.018	0.043
NJ	0.041	0.045	0.040	0.042	0.021
NM	0.036	0.038	0.002	0.000	0.090
NV	0.045	0.000	0.043	0.000	0.110
NY	0.040	0.037	0.030	0.000	0.045
OR	0.036	0.025	0.023	0.000	0.063
PA	0.042	0.059	0.035	0.044	0.010
RI	0.047	0.093	0.031	0.043	0.008
VA	0.037	0.033	0.037	0.000	0.060
VT	0.023	0.000	0.025	0.004	0.027
WA	0.043	0.051	0.050	0.052	0.003

The table shows the SDID unit weights from Equation 3 for the Republican OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A11. SDID Unit Weights: Female OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.047	0.054	0.041	0.000	0.016
CA	0.061	0.071	0.078	0.105	0.067
CO	0.023	0.000	0.000	0.000	0.005
CT	0.005	0.000	0.000	0.000	0.000
DC	0.001	0.000	0.000	0.000	0.011
DE	0.078	0.099	0.133	0.163	0.123
HI	0.000	0.000	0.000	0.000	0.025
IL	0.036	0.034	0.027	0.000	0.026
KS	0.084	0.117	0.130	0.190	0.121
MA	0.057	0.079	0.078	0.115	0.043
MD	0.043	0.033	0.033	0.032	0.037
ME	0.030	0.000	0.000	0.000	0.011
MI	0.074	0.121	0.125	0.138	0.085
MN	0.000	0.000	0.000	0.000	0.000
MT	0.058	0.058	0.063	0.004	0.087
NH	0.033	0.010	0.000	0.000	0.028
NJ	0.033	0.018	0.006	0.000	0.012
NM	0.043	0.042	0.038	0.080	0.019
NV	0.037	0.019	0.018	0.000	0.028
NY	0.041	0.019	0.011	0.000	0.049
OR	0.039	0.005	0.000	0.000	0.039
PA	0.051	0.071	0.073	0.064	0.050
RI	0.000	0.000	0.000	0.000	0.000
VA	0.055	0.059	0.054	0.000	0.057
VT	0.035	0.078	0.085	0.109	0.019
WA	0.036	0.010	0.008	0.000	0.041

The table shows the SDID unit weights from Equation 3 for the female OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A12. SDID Unit Weights: Male OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.047	0.047	0.061	0.112	0.074
CA	0.053	0.037	0.046	0.000	0.064
CO	0.039	0.035	0.027	0.000	0.000
CT	0.013	0.047	0.019	0.000	0.000
DC	0.003	0.078	0.023	0.000	0.000
DE	0.050	0.017	0.035	0.000	0.000
HI	0.064	0.000	0.055	0.146	0.169
IL	0.028	0.042	0.027	0.000	0.000
KS	0.063	0.029	0.051	0.155	0.096
MA	0.023	0.046	0.025	0.000	0.000
MD	0.038	0.020	0.030	0.000	0.000
ME	0.035	0.042	0.023	0.000	0.006
MI	0.019	0.058	0.018	0.000	0.000
MN	0.028	0.050	0.039	0.000	0.018
MT	0.019	0.036	0.062	0.153	0.097
NH	0.048	0.035	0.043	0.000	0.069
NJ	0.034	0.035	0.036	0.000	0.000
NM	0.021	0.043	0.033	0.000	0.000
NV	0.057	0.041	0.063	0.274	0.144
NY	0.035	0.049	0.038	0.000	0.000
OR	0.050	0.026	0.044	0.000	0.052
PA	0.037	0.042	0.024	0.000	0.000
RI	0.058	0.040	0.050	0.030	0.072
VA	0.040	0.046	0.041	0.000	0.064
VT	0.037	0.031	0.043	0.130	0.019
WA	0.061	0.028	0.045	0.000	0.054

The table shows the SDID unit weights from Equation 3 for the male OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A13. SDID Unit Weights: Rural OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.035	0.029	0.032	0.000	0.009
CA	0.054	0.065	0.064	0.031	0.028
CO	0.037	0.017	0.024	0.065	0.009
CT	0.037	0.056	0.041	0.066	0.094
DC	0.055	0.063	0.064	0.036	0.028
DE	0.063	0.038	0.076	0.013	0.000
HI	0.002	0.000	0.000	0.000	0.000
IL	0.060	0.083	0.073	0.048	0.035
KS	0.048	0.003	0.037	0.000	0.000
MA	0.057	0.070	0.073	0.045	0.025
MD	0.044	0.026	0.036	0.051	0.049
ME	0.026	0.000	0.000	0.030	0.000
MI	0.048	0.055	0.051	0.000	0.021
MN	0.029	0.033	0.025	0.066	0.085
MT	0.019	0.005	0.002	0.075	0.000
NH	0.005	0.000	0.000	0.059	0.015
NJ	0.050	0.041	0.050	0.000	0.000
NM	0.031	0.000	0.000	0.153	0.153
NV	0.045	0.045	0.045	0.033	0.067
NY	0.051	0.064	0.060	0.055	0.051
OR	0.000	0.000	0.000	0.000	0.025
PA	0.041	0.040	0.049	0.090	0.082
RI	0.055	0.063	0.064	0.036	0.028
VA	0.060	0.082	0.081	0.047	0.032
VT	0.009	0.092	0.019	0.000	0.124
WA	0.039	0.031	0.033	0.000	0.042

The table shows the SDID unit weights from Equation 3 for the rural OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A14. SDID Unit Weights: Early-Career OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.000	0.005	0.000	0.081	0.000
CA	0.084	0.078	0.123	0.150	0.093
CO	0.052	0.053	0.051	0.022	0.032
CT	0.000	0.000	0.000	0.000	0.004
DC	0.000	0.000	0.000	0.000	0.001
DE	0.070	0.070	0.108	0.020	0.041
HI	0.000	0.000	0.000	0.000	0.000
IL	0.000	0.002	0.000	0.000	0.000
KS	0.072	0.055	0.059	0.054	0.062
MA	0.049	0.048	0.029	0.037	0.021
MD	0.063	0.051	0.044	0.000	0.075
ME	0.043	0.034	0.006	0.027	0.089
MI	0.000	0.014	0.000	0.000	0.000
MN	0.000	0.019	0.000	0.000	0.004
MT	0.060	0.061	0.076	0.117	0.066
NH	0.069	0.064	0.078	0.000	0.019
NJ	0.076	0.071	0.098	0.202	0.082
NM	0.039	0.043	0.023	0.026	0.078
NV	0.063	0.061	0.071	0.118	0.077
NY	0.022	0.034	0.000	0.000	0.049
OR	0.059	0.054	0.053	0.000	0.057
PA	0.000	0.007	0.000	0.000	0.000
RI	0.000	0.000	0.000	0.049	0.000
VA	0.067	0.065	0.081	0.047	0.056
VT	0.033	0.042	0.000	0.000	0.000
WA	0.077	0.069	0.098	0.050	0.094

The table shows the SDID unit weights from Equation 3 for the early-career OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A15. SDID Unit Weights: Top-50 OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.002	0.000	0.001	0.000	0.000
CA	0.043	0.036	0.032	0.000	0.036
CO	0.006	0.000	0.000	0.000	0.000
CT	0.012	0.000	0.000	0.001	0.088
DC	0.013	0.009	0.008	0.000	0.012
DE	0.074	0.079	0.078	0.122	0.064
HI	0.017	0.000	0.000	0.006	0.092
IL	0.037	0.040	0.044	0.049	0.038
KS	0.079	0.110	0.116	0.167	0.036
MA	0.043	0.044	0.036	0.003	0.017
MD	0.037	0.026	0.025	0.000	0.043
ME	0.019	0.000	0.000	0.000	0.049
MI	0.071	0.098	0.106	0.148	0.019
MN	0.000	0.000	0.000	0.000	0.021
MT	0.071	0.067	0.061	0.066	0.075
NH	0.036	0.000	0.000	0.000	0.056
NJ	0.058	0.072	0.072	0.046	0.026
NM	0.017	0.023	0.015	0.000	0.000
NV	0.044	0.055	0.051	0.002	0.018
NY	0.046	0.043	0.041	0.016	0.047
OR	0.028	0.011	0.002	0.000	0.032
PA	0.063	0.077	0.086	0.124	0.050
RI	0.000	0.000	0.000	0.000	0.020
VA	0.053	0.051	0.050	0.037	0.062
VT	0.080	0.126	0.150	0.166	0.007
WA	0.050	0.031	0.029	0.047	0.090

The table shows the SDID unit weights from Equation 3 for the top-50 OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A16. SDID Unit Weights: Unranked OBGYNs

State	Oct 21	July 22	Oct 22	July 23	Oct 23
AK	0.054	0.047	0.043	0.000	0.129
CA	0.043	0.048	0.044	0.000	0.021
CO	0.040	0.033	0.038	0.000	0.081
CT	0.040	0.038	0.040	0.000	0.065
DC	0.007	0.001	0.005	0.552	0.104
DE	0.034	0.029	0.041	0.003	0.000
HI	0.045	0.034	0.040	0.000	0.084
IL	0.046	0.048	0.043	0.000	0.031
KS	0.052	0.054	0.043	0.000	0.019
MA	0.046	0.048	0.042	0.000	0.043
MD	0.048	0.042	0.040	0.000	0.059
ME	0.027	0.019	0.033	0.000	0.010
MI	0.038	0.033	0.034	0.000	0.017
MN	0.036	0.042	0.040	0.000	0.024
MT	0.035	0.052	0.045	0.019	0.000
NH	0.035	0.031	0.029	0.027	0.012
NJ	0.034	0.028	0.035	0.000	0.053
NM	0.040	0.052	0.044	0.000	0.008
NV	0.041	0.038	0.034	0.000	0.034
NY	0.042	0.041	0.038	0.000	0.038
OR	0.035	0.017	0.032	0.000	0.101
PA	0.052	0.060	0.046	0.000	0.000
RI	0.023	0.034	0.037	0.400	0.000
VA	0.034	0.040	0.045	0.000	0.006
VT	0.031	0.051	0.051	0.000	0.000
WA	0.042	0.039	0.037	0.000	0.061

The table shows the SDID unit weights from Equation 3 for the unranked OBGYN specification. Weights vary by adoption period. States are sorted alphabetically.

Appendix Table A17. Changes in OBGYN Density, Overall and by Subgroup (Including Wisconsin)

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	-0.237	-0.478**	-0.0865	-0.913**	0.363+	-0.166	-0.720*	-0.523*	0.357
	(0.368)	(0.176)	(0.0694)	(0.326)	(0.191)	(0.174)	(0.282)	(0.205)	(0.227)
Effect (Q7–10)	-0.355	-0.789*	-0.146	-1.214*	0.523	-0.230	-0.963*	-0.691*	0.565+
	(0.526)	(0.313)	(0.104)	(0.503)	(0.320)	(0.228)	(0.417)	(0.286)	(0.315)
Pre-*Dobbs* Treated Mean	59.09	9.950	9.340	31.30	27.79	10.89	4.570	9.065	16.20
Observations	1189	1189	1189	1189	1189	1189	1189	1189	1189
Panel B: Excl. TX, ND, IN									
Effect of Ban	-0.196	-0.473**	-0.109	-0.918**	0.348	-0.145	-0.660*	-0.543*	0.397
	(0.405)	(0.177)	(0.0817)	(0.347)	(0.219)	(0.202)	(0.302)	(0.241)	(0.244)
Effect (Q7–10)	-0.391	-0.808*	-0.169	-1.248*	0.500	-0.223	-0.969*	-0.701*	0.629+
	(0.554)	(0.319)	(0.113)	(0.530)	(0.341)	(0.248)	(0.434)	(0.316)	(0.327)
Pre-*Dobbs* Treated Mean	59.12	10.04	9.759	30.67	28.45	11.78	4.464	8.292	15.65
Observations	1102	1102	1102	1102	1102	1102	1102	1102	1102

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect. This sample includes Wisconsin in the set of total-ban states as part of the July 2022 treatment group.

Appendix Table A18. Changes in Primary Care Density, Overall and by Subgroup

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	-3.461^{+}	-0.0352	-0.0773	-3.199**	-0.457	-0.714	-3.548*	-0.786^{+}	-0.887
	(1.993)	(0.368)	(0.222)	(1.137)	(1.235)	(0.694)	(1.780)	(0.429)	(0.745)
Effect (Q7–Q10)	-4.380	0.169	-0.309	-4.319**	-0.256	-1.337	-4.455^{+}	-1.147^{+}	-1.271
	(3.049)	(0.561)	(0.357)	(1.468)	(2.012)	(1.027)	(2.668)	(0.653)	(1.166)
Pre-*Dobbs* Treated Mean	366.9	45.74	40.72	133.6	233.3	104.2	61.76	34.57	92.05
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN									
Effect of Ban	-2.536	-0.0601	-0.173	-2.461*	-0.165	-0.944	-2.395	-0.866^{+}	-0.623
	(2.171)	(0.418)	(0.253)	(1.090)	(1.417)	(0.775)	(1.824)	(0.469)	(0.746)
Effect (Q7–Q10)	-3.992	0.202	-0.389	-3.951**	-0.0912	-1.427	-3.752	-1.220^{+}	-1.167
	(3.292)	(0.602)	(0.384)	(1.504)	(2.192)	(1.137)	(2.809)	(0.686)	(1.210)
Pre-*Dobbs* Treated Mean	363.9	46.96	43.94	129.9	234.0	112.5	57.69	29.79	94.46
Observations	1073	1073	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for primary care providers. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. Primary care (PC) is defined in Appendix Table A2. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all PC, (2) PC who match to DIME and are deemed Democrats, (3) PC who match to DIME and are deemed Republicans, (4) female PC, (5) male PC, (6) PC in rural ZIP codes, (7) PC who registered in 2015 or later, (8) PC who attended a top-50 medical school, and (9) PC who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A19. Changes in General Surgeon Density, Overall and by Subgroup

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	0.554	-0.0697	-0.0172	-0.141	0.739*	-0.305*	0.132	0.108	-0.0975
	(0.429)	(0.0912)	(0.103)	(0.250)	(0.344)	(0.152)	(0.313)	(0.173)	(0.196)
Effect (Q7–Q10)	1.092	-0.0184	-0.0739	-0.0827	1.180*	-0.357*	0.359	0.204	0.0531
	(0.721)	(0.156)	(0.135)	(0.358)	(0.550)	(0.180)	(0.412)	(0.261)	(0.279)
Pre-*Dobbs* Treated Mean	57.80	6.833	11.56	11.06	46.74	12.42	5.080	9.571	13.71
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN									
Effect of Ban	0.710	-0.0509	-0.0658	-0.130	0.749^{+}	-0.286^{+}	0.310	0.169	0.0153
	(0.492)	(0.110)	(0.109)	(0.277)	(0.407)	(0.173)	(0.303)	(0.195)	(0.194)
Effect (Q7–Q10)	1.160	-0.0222	-0.101	-0.0816	1.192^{+}	-0.344^{+}	0.510	0.218	0.133
	(0.801)	(0.174)	(0.146)	(0.392)	(0.611)	(0.187)	(0.443)	(0.288)	(0.306)
Pre-*Dobbs* Treated Mean	56.46	6.741	12.26	10.19	46.27	13.27	3.662	8.329	13.10
Observations	1073	1073	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for general surgeons. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. General surgeons (GS) are defined in Appendix Table A2. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all GS, (2) GS who match to DIME and are deemed Democrats, (3) GS who match to DIME and are deemed Republicans, (4) female GS, (5) male GS, (6) GS in rural ZIP codes, (7) GS who registered in 2015 or later, (8) GS who attended a top-50 medical school, and (9) GS who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A20. Changes in OBGYN Density Relative to General Surgeon Density, Overall and by Subgroup (Full State Panel)

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	-0.989*	-0.363*	-0.0223	-0.776^{+}	-0.274	-0.0208	-0.778*	-0.594**	0.484^{+}
	(0.436)	(0.169)	(0.140)	(0.409)	(0.356)	(0.179)	(0.371)	(0.218)	(0.285)
Effect (Q7–Q10)	-1.670*	-0.653*	-0.00539	-1.162^{+}	-0.483	-0.183	-1.112^{+}	-0.763*	0.498
	(0.714)	(0.290)	(0.201)	(0.630)	(0.520)	(0.235)	(0.579)	(0.298)	(0.382)
Pre-*Dobbs* Treated Mean	0.798	2.614	-2.082	19.62	-18.82	-1.536	-0.587	-1.026	3.047
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN									
Effect of Ban	-0.989*	-0.372*	0.0165	-0.746^{+}	-0.282	-0.0262	-0.812*	-0.693**	0.395
	(0.501)	(0.186)	(0.158)	(0.452)	(0.415)	(0.232)	(0.407)	(0.223)	(0.288)
Effect (Q7–Q10)	-1.723*	-0.667*	0.00537	-1.175^{+}	-0.524	-0.210	-1.212*	-0.794**	0.473
	(0.768)	(0.308)	(0.219)	(0.660)	(0.569)	(0.263)	(0.603)	(0.306)	(0.428)
Pre-*Dobbs* Treated Mean	2.039	2.670	-2.294	19.64	-17.60	-1.423	0.694	-0.769	3.214
Observations	1073	1073	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for the gap between OBGYN density and general surgeon density, by subgroup. The coefficients represent the within state-specialty difference in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. General surgeons (GS) are defined in Appendix Table A2. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for OBGYNs and general surgeons: (1) all, (2) who match to DIME and are deemed Democrats, (3) who match to DIME and are deemed Republicans, (4) who are female, (5) who are male, (6) in rural ZIP codes, (7) who registered in 2015 or later, (8) who attended a top-50 medical school, and (9) who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A21. Changes in OBGYN Density, Overall and by Subgroup (Time-Varying Denominator)

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Effect of Ban	-0.194	-0.295*	-0.166*	-0.796*	0.386*	-0.328*	-0.697*	-0.598**	0.290
	(0.361)	(0.126)	(0.0783)	(0.315)	(0.184)	(0.137)	(0.298)	(0.192)	(0.209)
Effect (Q7–Q10)	-0.0133	-0.346^{+}	-0.242*	-0.867^{+}	0.650*	-0.401*	-0.924*	-0.673**	0.491^{+}
	(0.535)	(0.198)	(0.117)	(0.468)	(0.305)	(0.182)	(0.426)	(0.260)	(0.297)
Pre-*Dobbs* Treated Mean	57.95	9.329	9.385	30.33	27.62	10.79	4.443	8.406	16.63
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN									
Effect of Ban	-0.0313	-0.262^{+}	-0.173^{+}	-0.705*	0.410*	-0.337*	-0.608^{+}	-0.615**	0.343
	(0.398)	(0.140)	(0.0961)	(0.356)	(0.206)	(0.168)	(0.323)	(0.214)	(0.249)
Effect (Q7–Q10)	0.0874	-0.320	-0.249^{+}	-0.794	0.673*	-0.412*	-0.903*	-0.660*	0.576^{+}
	(0.573)	(0.210)	(0.130)	(0.507)	(0.326)	(0.209)	(0.456)	(0.266)	(0.332)
Pre-*Dobbs* Treated Mean	57.96	9.308	9.891	29.54	28.42	11.75	4.319	7.428	16.24
Observations	1073	1073	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in time-varying provider density (where in this specification, the density measure uses ACS female 15–49 population estimates interpolated to the quarterly panel) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Table A22. Changes in OBGYN Density by Party (Sensitivity to Contribution Cutoff)

	(1) Dem	(2) Dem≤2020	(3) Rep	(4) Rep≤2020
Panel A: All States				
Effect of Ban	-0.482**	-0.283*	-0.0701	-0.0879
	(0.186)	(0.116)	(0.0745)	(0.0788)
Effect (Q7–Q10)	-0.808*	-0.469*	-0.124	-0.180+
	(0.337)	(0.189)	(0.111)	(0.102)
Pre-*Dobbs* Treated Mean	9.45	8.44	9.47	9.04
Observations	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN				
Effect of Ban	-0.478*	-0.302*	-0.0908	-0.114
	(0.192)	(0.130)	(0.0885)	(0.0952)
Effect (Q7–Q10)	-0.832*	-0.497*	-0.146	-0.206+
	(0.350)	(0.203)	(0.121)	(0.113)
Pre-*Dobbs* Treated Mean	9.41	8.50	9.97	9.62
Observations	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. All samples use the subset of providers matched to DIME. Coefficients are for subsets of said sample: (1) OBGYNs who match to DIME and are deemed Democrats (replicating the main analysis), (2) OBGYNs who match to DIME using contributions only up to the **2020 election cycle** or earlier and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans (replicating the main analysis), and (4) OBGYNs who match to DIME using contributions only up to the **2020 election cycle** or earlier and are deemed Republicans. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*.

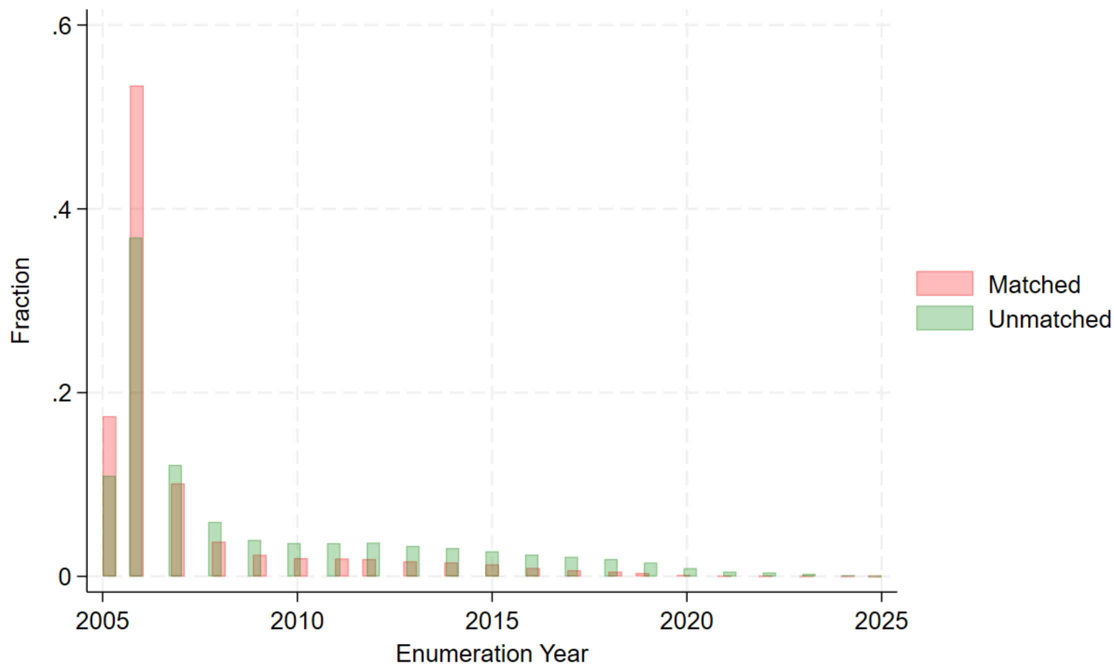
Appendix Table A23. Changes in OBGYN Density, Overall and by Subgroup (Balanced Sample)

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: Full sample									
Effect of Ban	-0.239	-0.482**	-0.0701	-0.971**	0.378*	-0.244	-0.714*	-0.583**	0.386+
	(0.368)	(0.186)	(0.0745)	(0.328)	(0.189)	(0.160)	(0.306)	(0.203)	(0.222)
Effect (Q7–Q10)	-0.394	-0.808*	-0.124	-1.322**	0.528	-0.345+	-0.985*	-0.771**	0.595+
	(0.525)	(0.337)	(0.111)	(0.513)	(0.323)	(0.206)	(0.442)	(0.287)	(0.315)
Pre-*Dobbs* Treated Mean	58.60	9.447	9.474	30.68	27.92	10.88	4.493	8.545	16.76
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Present at panel start									
Effect of Ban	0.0464	-0.358*	-0.0820	-0.312	0.261	-0.404**	-0.247	-0.215	0.165
	(0.308)	(0.146)	(0.0781)	(0.270)	(0.190)	(0.146)	(0.192)	(0.138)	(0.153)
Effect (Q7–Q10)	-0.0388	-0.620*	-0.142	-0.477	0.301	-0.646**	-0.329	-0.230	0.213
	(0.449)	(0.266)	(0.115)	(0.382)	(0.310)	(0.189)	(0.284)	(0.193)	(0.215)
Pre-*Dobbs* Treated Mean	57.72	9.394	9.457	29.95	27.77	10.84	3.609	8.432	16.68
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel C: Present at panel end									
Effect of Ban	-0.454	-0.594**	0.0460	-1.013**	0.287+	-0.104	-0.721*	-0.548**	0.398+
	(0.354)	(0.195)	(0.0753)	(0.300)	(0.161)	(0.123)	(0.305)	(0.190)	(0.218)
Effect (Q7–Q10)	-0.707	-0.957**	0.0859	-1.416**	0.441	-0.141	-0.993*	-0.718**	0.627*
	(0.524)	(0.362)	(0.114)	(0.481)	(0.268)	(0.162)	(0.440)	(0.267)	(0.310)
Pre-*Dobbs* Treated Mean	57.38	9.268	9.138	30.51	26.87	10.52	4.486	8.395	16.66
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel D: Present at both endpoints									
Effect of Ban	-0.0829	-0.436**	0.0278	-0.314	0.168	-0.274*	-0.266	-0.177	0.182
	(0.290)	(0.162)	(0.0755)	(0.255)	(0.185)	(0.117)	(0.189)	(0.123)	(0.153)
Effect (Q7–Q10)	-0.208	-0.733*	0.0601	-0.505	0.218	-0.437**	-0.350	-0.186	0.252
	(0.421)	(0.295)	(0.114)	(0.364)	(0.293)	(0.149)	(0.278)	(0.173)	(0.216)
Pre-*Dobbs* Treated Mean	56.49	9.214	9.121	29.78	26.71	10.48	3.603	8.281	16.58
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

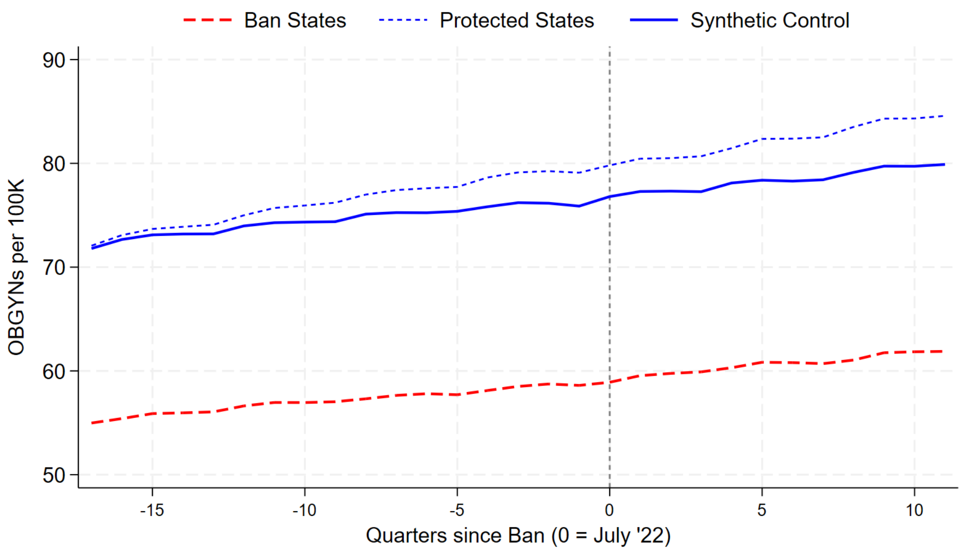
This table displays the synthetic difference-in-differences coefficients from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Panel A uses the full sample, Panel B restricts to providers observed in April 2018, Panel C restricts to providers observed in April 2025, and Panel D restricts to providers observed at both endpoints. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school. The pre-*Dobbs* treated mean is a snapshot from April 2022, the final quarter before *Dobbs*. Each column is estimated in a separate synthetic difference-in-differences specification on a subgroup-specific sample, with subgroup-specific unit and time weights. As such, subgroup estimates do not mechanically sum to the aggregate effect.

Appendix Figure A1. Histogram of Enumeration Date Mass
Matched and Unmatched to DIME



This figure plots the fraction of providers collected from all OBGYN taxonomies in Appendix Table A1 with a given enumeration year in the NPPES, split by whether they matched to DIME or did not match. This is a pre-*Dobbs* April 2022 snapshot from the sample of all OBGYNs.

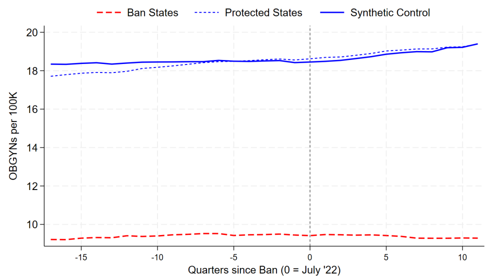
Appendix Figure A2. Trends in Providers per 100K Reproductive-Age Women (Overall)



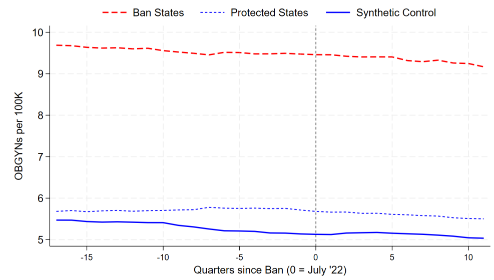
This figure displays the mean density of OBGYNs per 100K reproductive-age females, as defined in (1). These averages were calculated using the full sample and the treatment definition in Figure 1. For the synthetic abortion-protecting states, weights are shown in Appendix Table A8. SDID constructs a different set of weights for each adoption period, but this plot shows the trends for the synthetic counterfactual using the unit weights from the July 2022 treatment group (the majority of the treatment group).

Appendix Figure A3. Heterogeneity in Provider Supply Trends

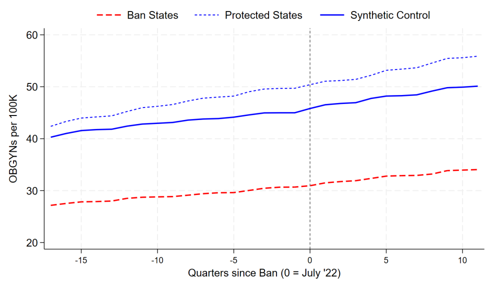
(a) Democratic OBGYNs



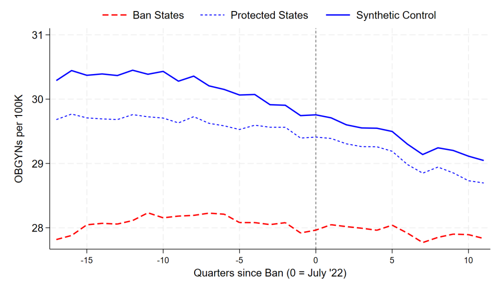
(b) Republican OBGYNs



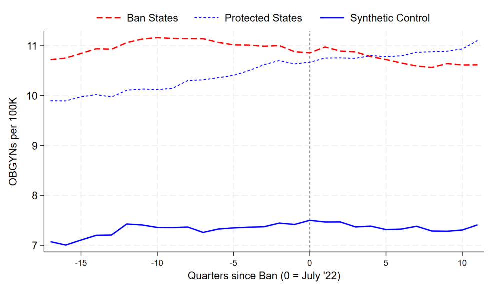
(c) Female OBGYNs



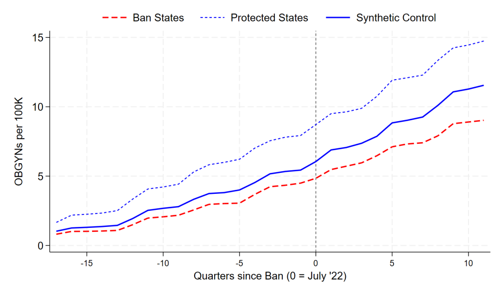
(d) Male OBGYNs



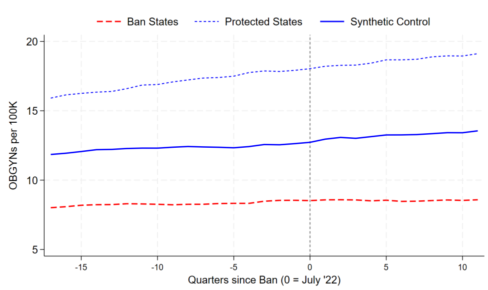
(e) Rural OBGYNs



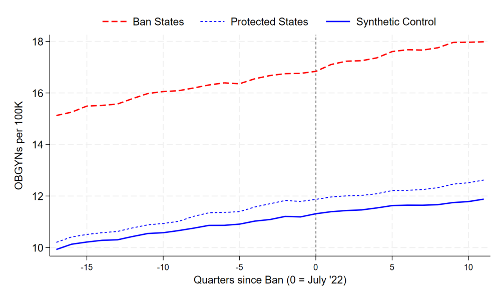
(f) Early-Career OBGYNs



(g) Top-50 OBGYNs

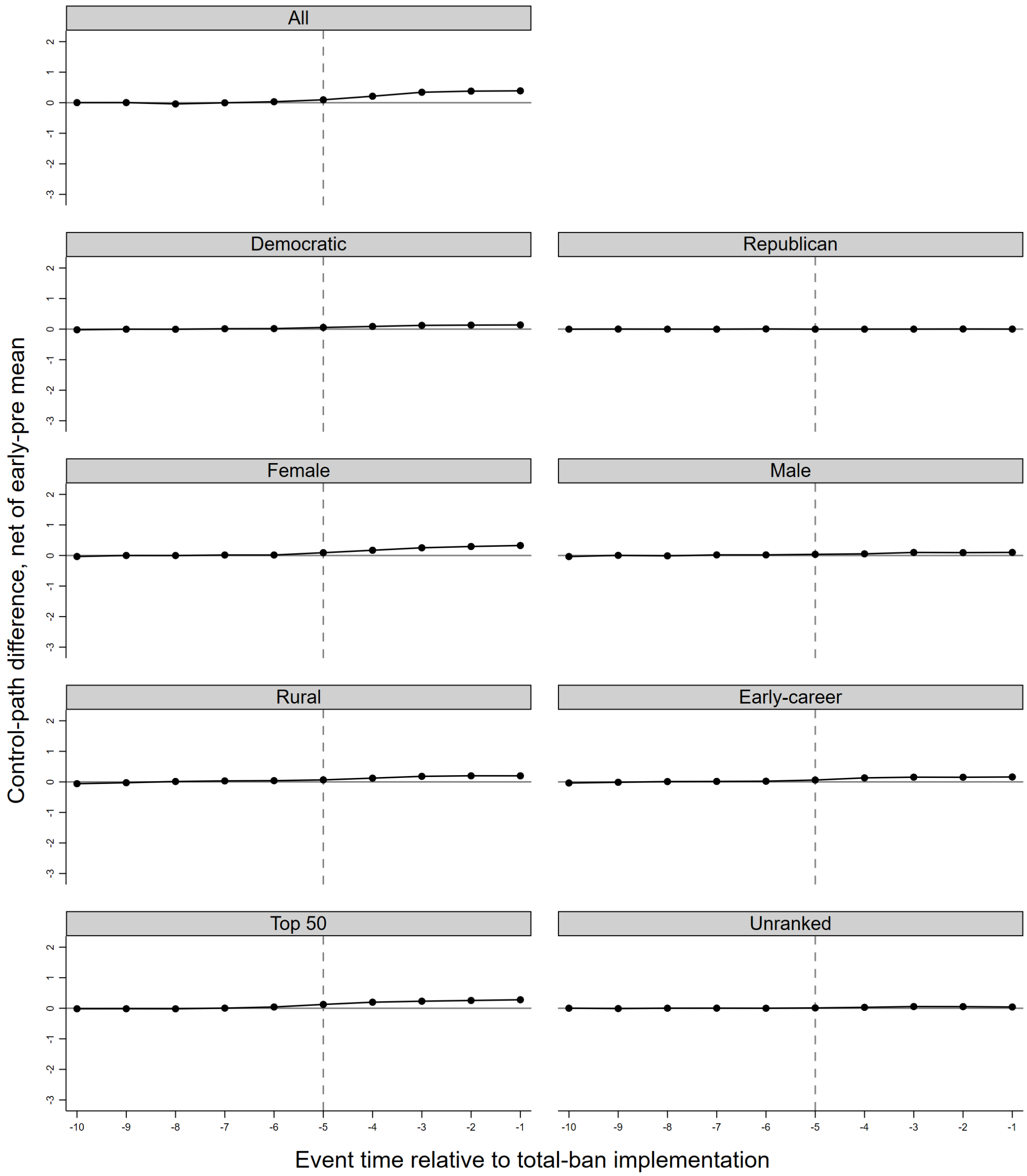


(h) Unranked OBGYNs



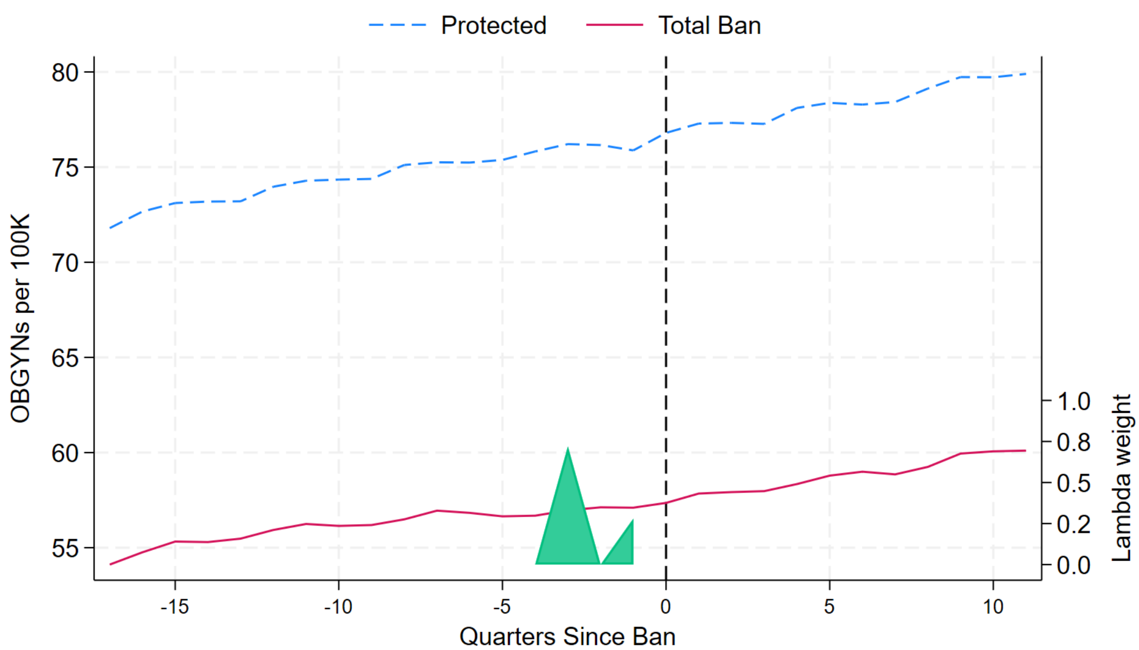
This figure displays the mean density of OBGYNs per 100K reproductive-age females, as defined in (1). Panels (a) and (b) split by party affiliation; Panels (c) and (d) by gender; Panels (e) and (f) highlight rural and early-career providers; and Panels (g) and (h) show OBGYNs who attended a top-50 medical school and a named but unranked medical school, respectively. Panels (a) and (b) use the sample matched to DIME, panels (g) and (h) use the sample matched to NDF, and panels (c) through (f) were calculated using the full sample. All panels use the treatment definition in Figure 1. For the synthetic abortion-protecting states, weights are shown in Appendix Table A9, A10, A11, A12, A13, A14, A15, and A16, respectively. SDID constructs a different set of weights for each adoption period, but this plot shows the trends for the synthetic counterfactual using the unit weights from the July 2022 treatment group (the majority of the treatment group).

Appendix Figure A4. Holdout-Weight Diagnostic for the Weighted Comparison Path



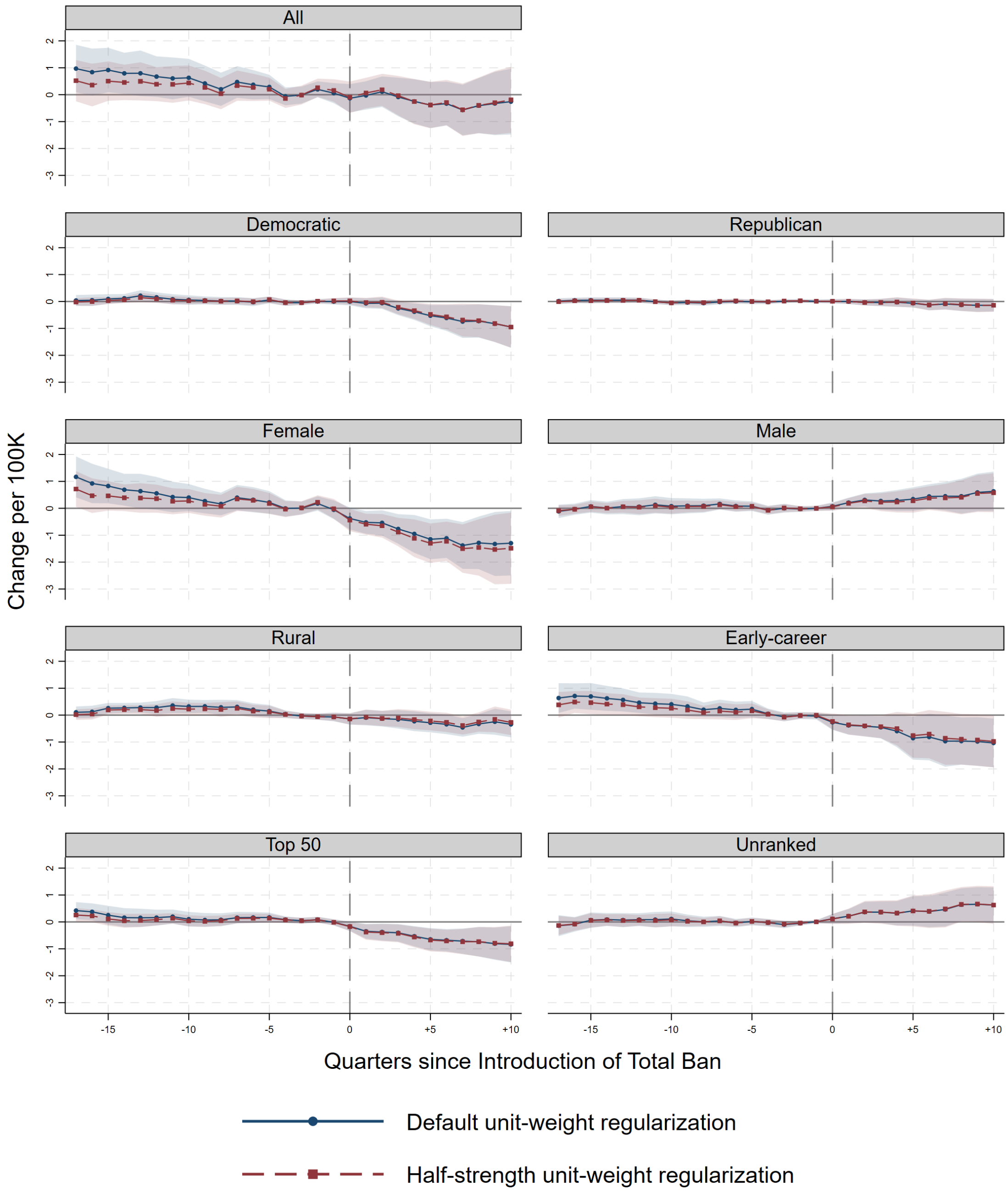
This figure compares the protected-state comparison paths implied by the main SDID weights with paths constructed from donor weights estimated only on earlier pre-ban quarters, excluding event quarters -5 through -1, using the same unit-weight regularization setup from the main specification. Both paths are normalized relative to the early pre-period, so the figure focuses on changes in the weighted comparison path rather than level differences across synthetic comparisons.

Appendix Figure A5. Overall OBGYN Trends with Time Weights



This figure displays the mean density of OBGYNs per 100K reproductive-age females, as defined in (1). These averages were calculated using the full sample and the treatment definition in Figure 1. SDID constructs a different set of weights for each adoption period, but this plot shows the trends for the synthetic counterfactual using the weights from the July 2022 treatment group (the majority of the treatment group). The time weights, λ, are plotted on the RHS y-axis.

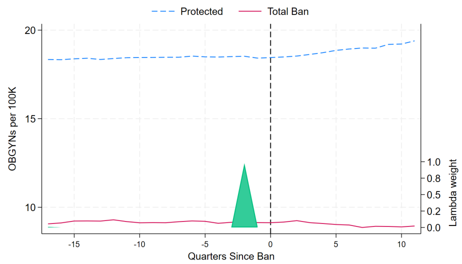
Appendix Figure A6. Extended-Window Event-Study Output



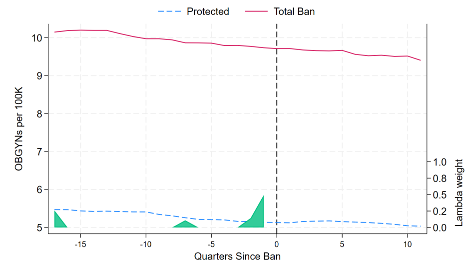
This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 3 for OBGYNs. The coefficients represent the change in provider density as defined previously in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. For this sensitivity exercise, the event window extends to 17 quarters before ban implementation. The solid blue series uses the estimator's default unit-weight regularization when estimating the cohort-specific donor-state weights, $\hat{\omega}_{s,a}$. The dashed red series halves each cohort's automatically calculated unit-weight regularization penalty; all other estimation settings are unchanged.

Appendix Figure A7. Subgroup OBGYN Trends with Time Weights

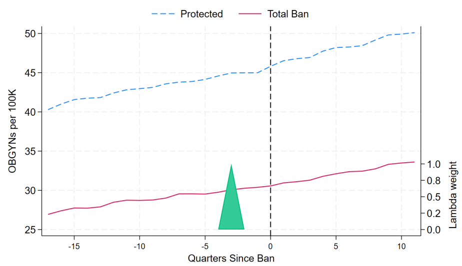
(a) Democratic OBGYNs



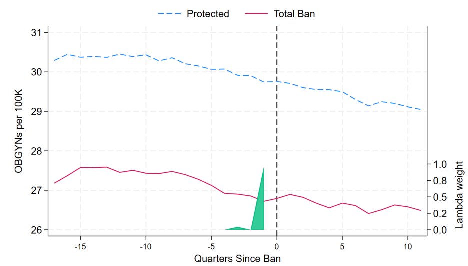
(b) Republican OBGYNs



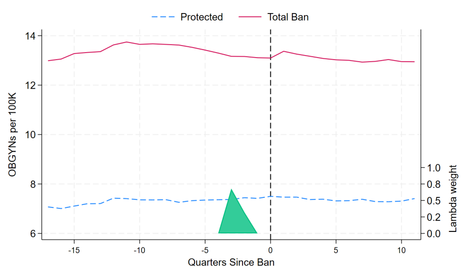
(c) Female OBGYNs



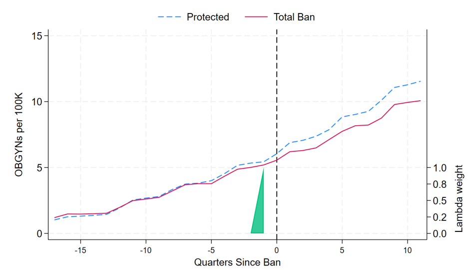
(d) Male OBGYNs



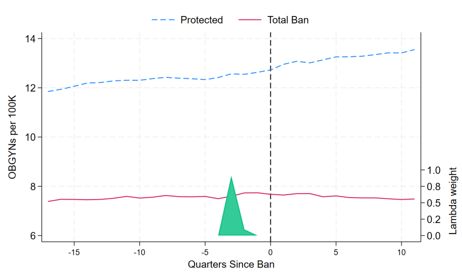
(e) Rural OBGYNs



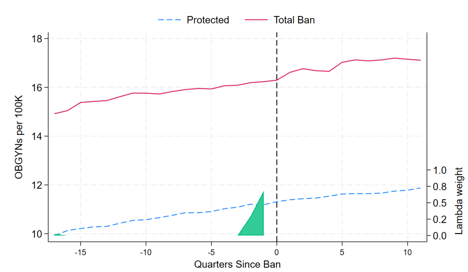
(f) Early-Career OBGYNs



(g) Top-50 OBGYNs

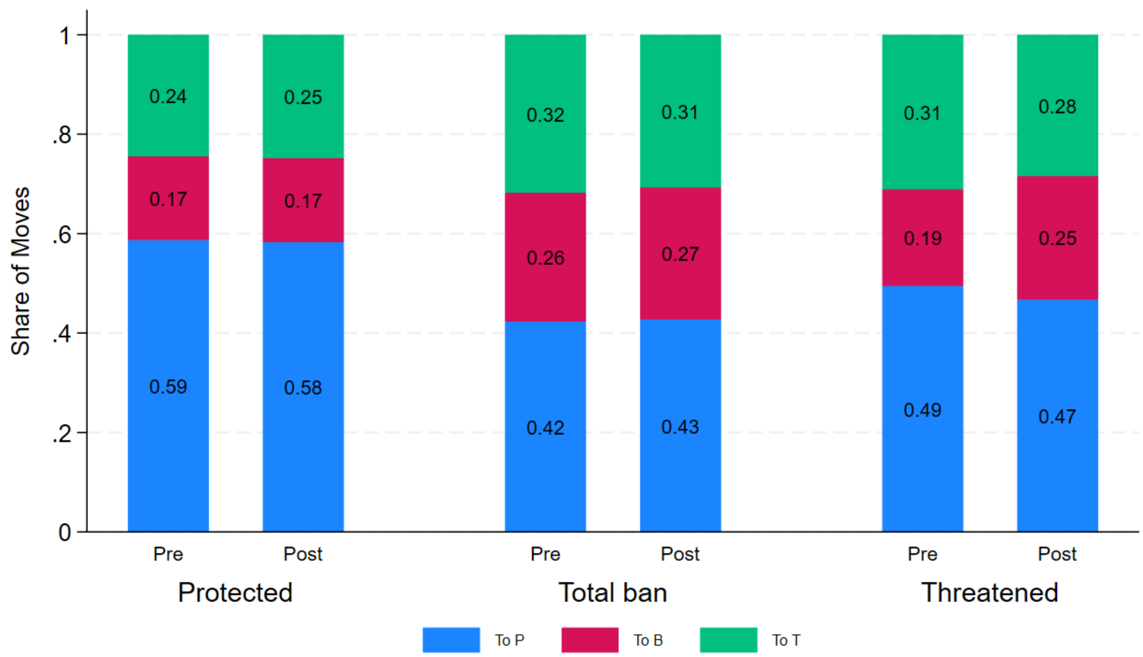


(h) Unranked OBGYNs



This figure displays the density of OBGYNs per 100K reproductive-age females by subgroup, as defined in (1). These averages were calculated using the full sample and the treatment definition in Figure 1. SDID constructs a different set of weights for each adoption period, but this plot shows the trends for the synthetic counterfactual using the weights from the July 2022 treatment group (the majority of the treatment group). The time weights, λ, are plotted on the RHS y-axis. Panels (a) and (b) utilize the sample of providers matched to DIME, panels (g) and (h) utilize the sample of providers matched to NDF, while panels (c), (d), (e), and (f) utilize the full sample of providers.

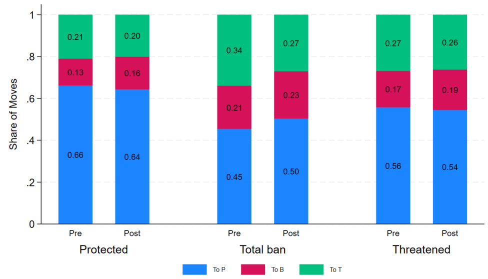
Appendix Figure A8. Conditional Destination Shares



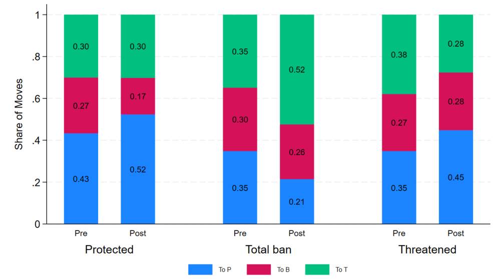
This figure displays the destination policy shares of OBGYNs who move from protected (P), banned (B), and threatened (T) states, before and after the *Dobbs* decision. Bars represent the share of providers from the origin policy environment shown below each set of bars moving to a particular policy environment in the pre-*Dobbs* and post-*Dobbs* period. This utilizes the full sample of OBGYNs. OBGYNs are defined in Appendix Table A1.

Appendix Figure A9. Conditional Destination Shares by Subgroup

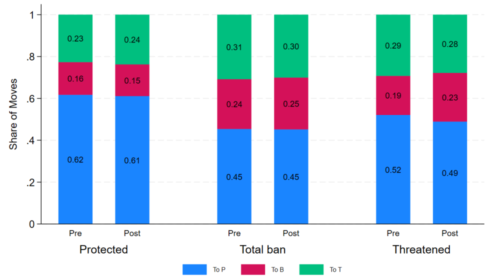
(a) Democratic OBGYNs



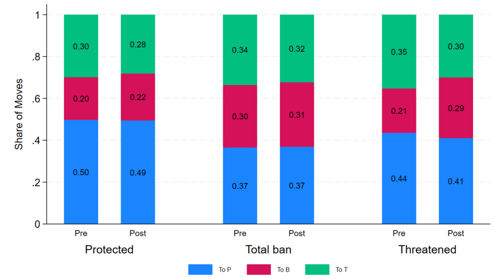
(b) Republican OBGYNs



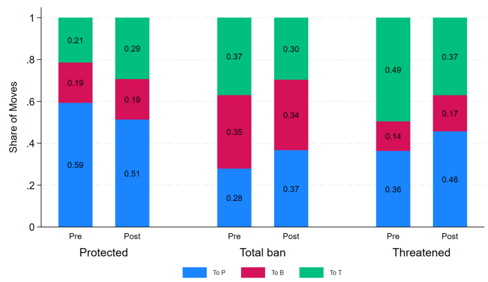
(c) Female OBGYNs



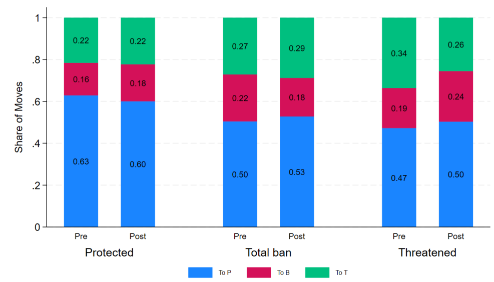
(d) Male OBGYNs



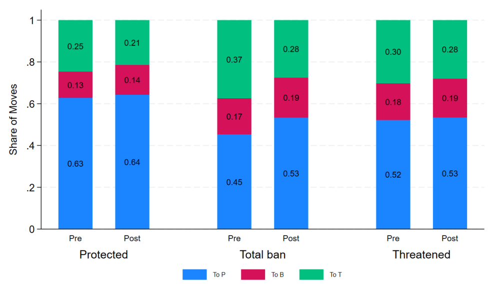
(e) Rural OBGYNs



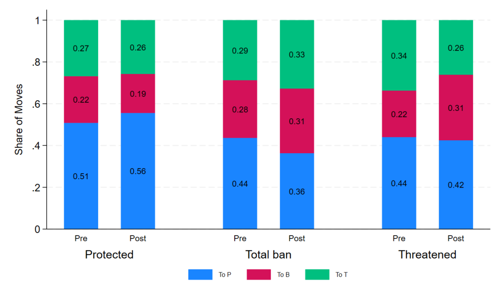
(f) Early-Career OBGYNs



(g) Top-50 OBGYNs

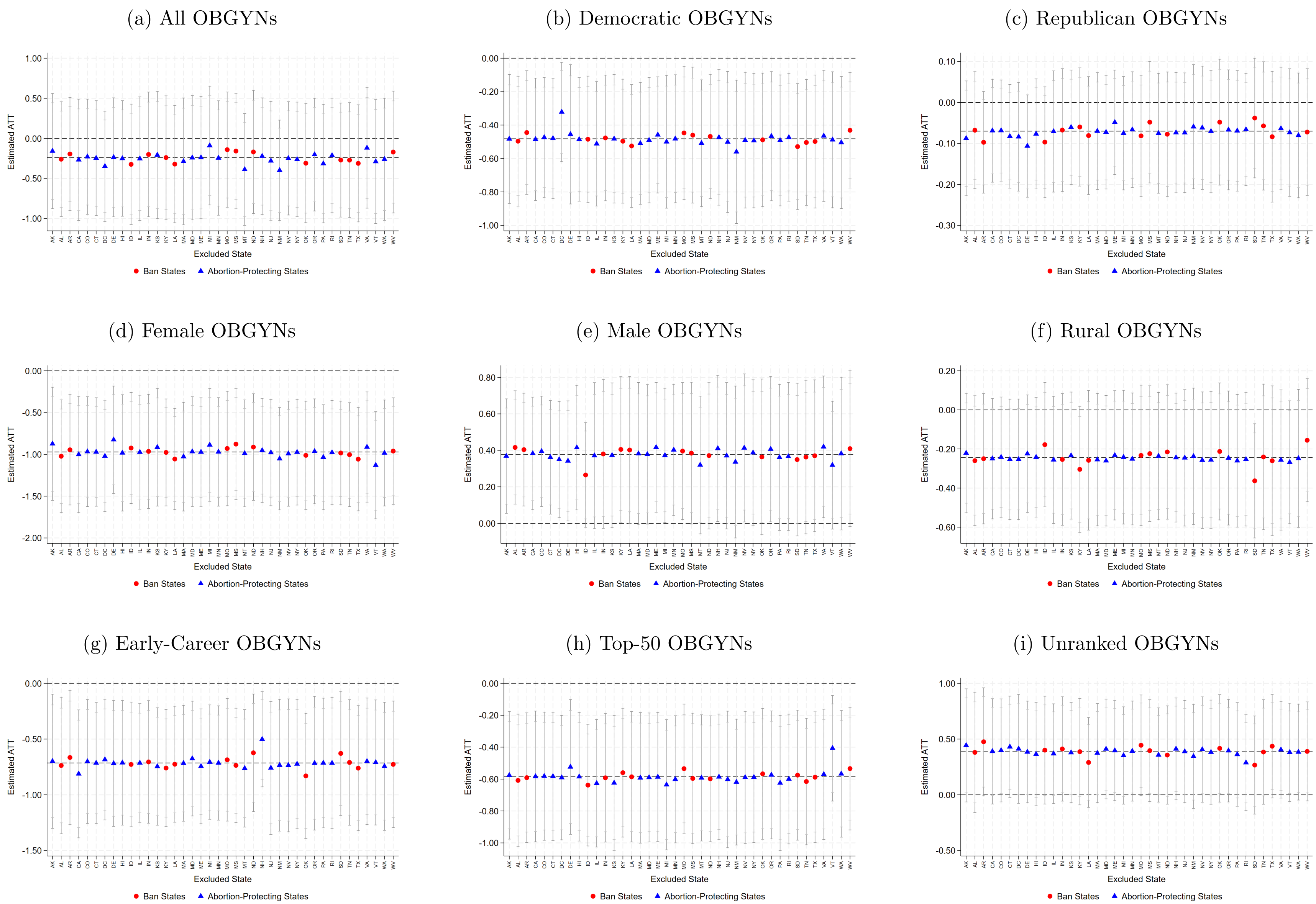


(h) Unranked OBGYNs



This figure displays the destination policy shares of OBGYNs who move from protected (P), banned (B), and threatened (T) states, before and after the *Dobbs* decision. Bars represent the share of providers from the second row policy environment moving to a particular policy environment in the pre-*Dobbs* and post-*Dobbs* periods. OBGYNs are defined in Appendix Table A1. Panels (a) and (b) utilize the sample of providers matched to DIME, panels (g) and (h) utilize the sample of providers matched to NDF, while panels (c), (d), (e), and (f) utilize the full sample of providers.

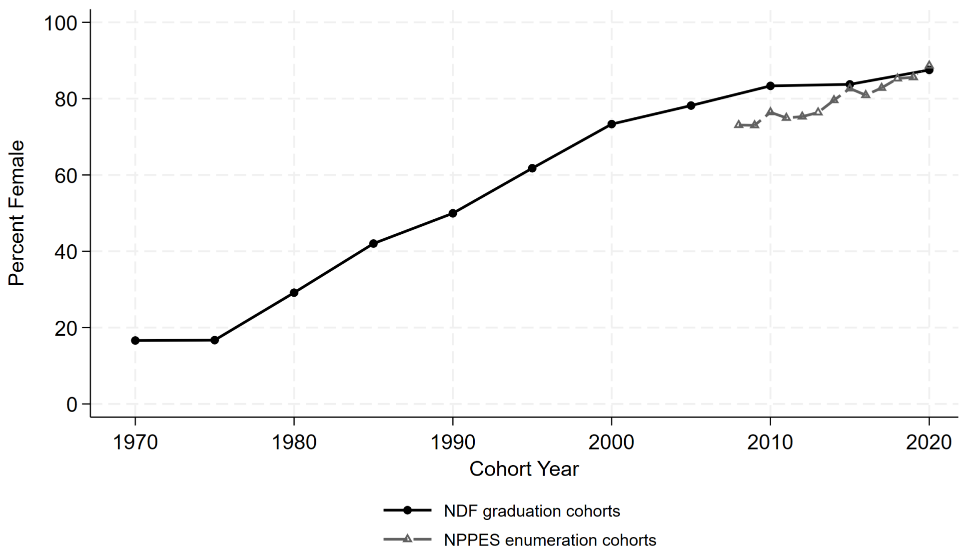
Appendix Figure A10. Leave-One-Out Analyses



This figure displays the leave-one-out sensitivity analysis coefficients, 90% confidence intervals, and 95% confidence intervals for OBGYNs. The coefficients represent the re-estimated post-ban effect on provider density as defined previously in (1), derived by sequentially omitting each state from the sample and recalculating weights. Panel (a) is the aggregate effect; Panels (b) and (c) split by party affiliation; Panels (d) and (e) by gender; Panels (f) and (g) highlight rural and early-career providers; and Panels (h) and (i) show OBGYNs who attended a top-50 medical school and a named but unranked medical school, respectively. Panels (b) and (c) use the sample matched to DIME; panels (h) and (i) use the sample matched to NDF; and panels (a) along with (d) through (g) were calculated using the full sample. A dashed horizontal line represents the estimated effect using the full sample of states, and another dashed line at zero. I use 100 bootstrap iterations for the leave-one-out analyses.

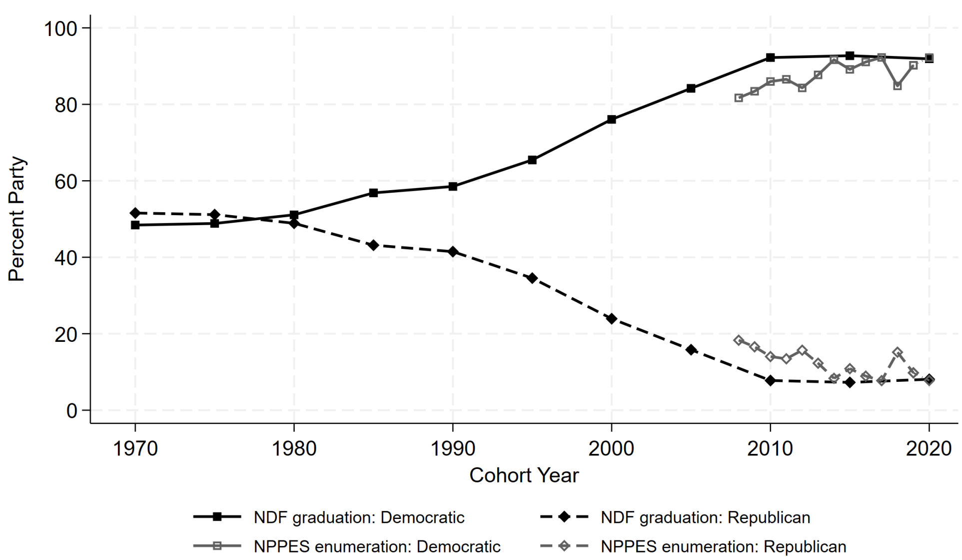
Appendix Figure A11. Demographic Shifts Over Time, Both Samples

(a) Gender Composition of OBGYN Cohorts, 1970–2020



This figure displays the share of OBGYNs who are female, by medical school graduation year for the NDF sample or by enumeration year for the NPPES sample. Graduation years are grouped into centered five-year cohorts. The NDF series uses the set of providers matched to the NDF, while the NPPES series uses the full sample. NPPES enumeration dates capture new entrants from 2008 onward. Enumeration cohorts are annual through 2019, while years 2020–2025 are pooled into a single 2020 endpoint.

(b) Party Composition of OBGYN Cohorts, 1970–2020



This figure displays the share of OBGYNs who are classified as Democrats or Republicans, omitting undecided contributors, by medical school graduation year for the NDF sample or by enumeration year for the NPPES sample. Graduation years are grouped into centered five-year cohorts. The NDF series uses the set of providers matched to the NDF and DIME, while the NPPES series uses the broader DIME-matched sample. NPPES enumeration dates capture new entrants from 2008 onward. Enumeration cohorts are annual through 2019, while years 2020–2025 are pooled into a single 2020 endpoint.

B Bounding Protected-State Spillovers

The main difference-in-differences estimates compare the change in OBGYN density in total-ban states to the change in protected states. A standard assumption in this design is the absence of spillovers (SUTVA). In this context, a SUTVA violation would occur if a physician who otherwise would have practiced in a total-ban state instead chose to practice in a protected state. Symmetrically, physicians who may have practiced in a protected state may instead practice in a total-ban state. Because this moves the treatment and control groups in opposite directions, the difference-in-differences estimator overstates the true density change in total-ban states.

I can benchmark the potential magnitude of this spillover. Suppose the total-ban-state stock change is L providers, and a share d of this change is mirrored by a change in protected states. The true change in provider density in total-ban states is L/Pop_{ban}, while the spillover affects the density in protected states by $-dL/Pop_{protected}$. The observed difference-in-differences point estimate $\hat{\tau}$ is thus inflated. Factoring out the true effect yields a multiplier:

$$M(d) = 1 + d\frac{Pop_{ban}}{Pop_{protected}}.$$

Total-ban states are smaller in population than protected states, so the ratio $Pop_{ban}/Pop_{protected}$ is 0.461 (using the April 2022 baseline). If there is no corresponding change in protected states ($d = 0$), the multiplier is 1, and there is no inflation. At the theoretical maximum ($d = 1$), each provider drawn from one environment is absorbed by the other, generating an upper-bound multiplier of $M(1) = 1.46$. Under any assumption of d, I can divide the estimated effect on total-ban states by $M(d)$ to calculate the adjusted estimates.

I present the unadjusted Q7–Q10 stock estimates alongside adjusted estimates rescaled by $M(d)$ in Appendix Table B1. First, the incumbent benchmark parameterizes d using the pre-*Dobbs* transition probabilities of providers who leave the total-ban environment entirely. It sets d to the share of these departing incumbents who relocate to protected states ($d = 0.362$), with the remainder either moving to threatened states or exiting the observed workforce. For positive estimates, d represents the share of avoided departures that would have ended in protected states. Second, the entrant benchmark assumes that providers missing from the total-ban entrant pipeline still enter the workforce but redistribute across all non-ban states. For positive estimates, additional total-ban entrants are drawn from the same counterfactual non-ban locations. Under either direction, d is set to the protected states' share of non-ban entrant destinations ($d = 0.698$). Finally, the full-diversion benchmark presents the one-to-one reallocation case ($d = 1$).

Under the empirical incumbent and entrant benchmarks, the control-group multiplier ranges from 1.17 to 1.32. Adjusting for these spillovers attenuates the magnitudes of the density shortfalls, but cannot explain away the stock declines observed in total-ban states.

Appendix Table B1. Bounding Protected-State Spillovers

Benchmark	d	$M(d)$	All	Dem.	Rep.	Female	Male	Rural	Early	Top 50	Unranked
No protected-state spillover	0.000	1.00	-0.39	-0.81	-0.12	-1.32	0.53	-0.35	-0.99	-0.77	0.59
Incumbent accounting	0.362	1.17	-0.34	-0.69	-0.11	-1.13	0.45	-0.30	-0.84	-0.66	0.51
Entrant benchmark	0.698	1.32	-0.30	-0.61	-0.09	-1.00	0.40	-0.26	-0.75	-0.58	0.45
Full diversion	1.000	1.46	-0.27	-0.55	-0.08	-0.91	0.36	-0.24	-0.67	-0.53	0.41

This table displays Q7–Q10 stock estimates under alternative assumptions about protected-state spillovers. The first row reports the unadjusted stock estimates. The remaining rows rescale the stock estimates by $\hat{\tau}/M(d)$, where $M(d) = 1 + dPop_{ban}/Pop_{protected}$. Here d is the assumed share of the total-ban-state stock change mirrored in protected states. The population ratio uses the April 2022 reproductive-age female population in total-ban and protected states. The incumbent benchmark uses the pre-*Dobbs* share of departing incumbent OBGYNs from total-ban states who moved to protected states, where for positive estimates the same share applies to avoided departures that would have ended in protected states. The entrant benchmark assumes total-ban entrants remain in the observed OBGYN stock and redistribute across non-ban states according to pre-period entrant shares. For positive estimates, providers added to total-ban states are assumed to have otherwise entered protected or threatened states in the same proportions. The full-diversion benchmark assumes every provider lost by one environment is gained by another.

C Robustness to a Unified Counterfactual

The primary specification utilizes the SDID estimator, which finds the optimal control group weights for each outcome variable (and adoption period). While this maximizes the pre-period fit, it can make comparing across specifications challenging, given that the counterfactual control group may differ.

Following the guidance of Sun et al. (2025), I provide an alternative approach to mitigate overfitting to subgroup-specific noise. Instead of estimating separate weights for each outcome, I generate a single set of state-level donor weights using the aggregate OBGYN specification and apply this vector to all subgroup outcomes.

C.1 Methodology and Weight Composition

This approach ensures that all subpopulations are compared against the exact same weighted combination of donor states, enabling a consistent comparison of effects across groups. I first estimate unit weights ($\hat{\omega}$) using the SDID algorithm on the aggregate state-level density of OBGYNs. I then construct a time-invariant "master weight" ($\bar{\omega}_s$) for each donor state by taking the weighted average of that donor's contribution across all treatment cohorts:

$$\bar{\omega}_s = \frac{\sum_{a \in A} N_a T^a_{\text{post}} \hat{\omega}_{s,a}}{\sum_{a \in A} N_a T^a_{\text{post}}}. \tag{9}$$

where $\hat{\omega}_{s,a}$ is the weight assigned to donor state s for cohort a, N_a is the number of treated states in that cohort, and T^a_{post} is the number of post-treatment quarters observed for the cohort. These are the same treated-state-quarter cohort weights used to aggregate the main SDID overall ATT in Equation (4). The resulting donor vector is fixed across outcomes, subgroups, and event times.

Table C1 displays the resulting donor weights. The distribution of weights suggests a geographically and economically diverse counterfactual. The largest contributions come from Michigan (12.1%), Delaware (9.5%), Massachusetts (9.3%), and Kansas (7.9%).

C.2 Results Comparison

I then apply these fixed weights to the subgroup analysis. Specifically, I pass the sample weights to the estimator proposed by Callaway and Sant'Anna (2021), henceforth CSDID, which is designed to handle the staggered adoption of abortion bans and allow for heterogeneous treatment effects.

Table C2 shows the results from this implementation. Across subgroups, the effect sizes are qualitatively and quantitatively similar to those in the primary specification. The Democratic, female, male, early-career, and training background results remain significant at conventional thresholds.

I then provide a graphical interpretation, where I overlay the SDID event-study plots from the main analysis with the event-study results from the weighted CSDID implementation. As expected, the averaged weights generated from the aggregate specification sacrifice the subgroup-specific pre-period fit achieved by the primary SDID design, most notably for rural and early-career OBGYNs.

Appendix Table C1. Synthetic Control Donor Weights (Unified Counterfactual)

State	Weight	State	Weight
MI	0.121	DE	0.095
MA	0.093	KS	0.079
PA	0.077	NM	0.067
CA	0.057	VT	0.056
VA	0.051	IL	0.050
MD	0.046	NJ	0.034
NY	0.033	ME	0.030
MT	0.028	AK	0.024
WA	0.013	CO	0.012
OR	0.011	NH	0.006
NV	0.006	CT	0.005
HI	0.004	DC	0.002
RI	0.001	MN	0.000

Note: Weights are derived from the aggregate SDID estimation and averaged across treatment cohorts. They are a cohort-weighted average of the weights displayed in Appendix Table A8. These weights are applied to the CSDID robustness checks.

However, the overall estimates remain broadly consistent in sign and magnitude with the primary results.

The comparison plot for the overall stock of OBGYNs is presented in Figure C1, and the subgroup analyses are presented in Figure C2. Together, these findings show that the main set of results is not the product of a particular weighting scheme, and instead reflects broad trends in states that implemented a total ban on abortion care.

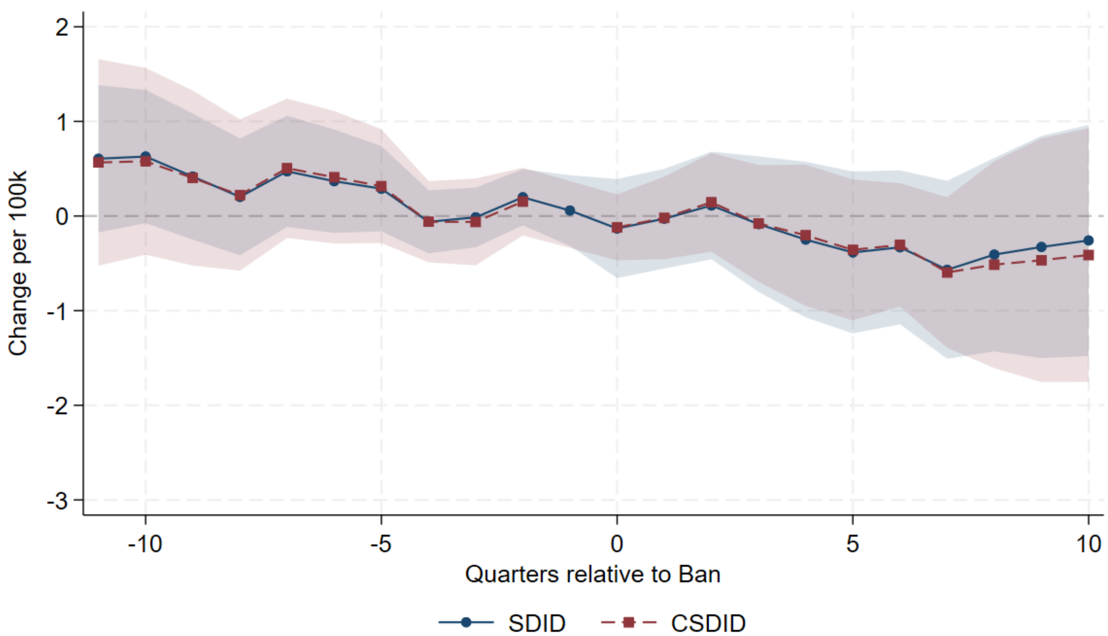
Appendix Table C2. Changes in OBGYN Density, Overall and by Subgroup (Weighted CSDID)

	(1) All	(2) Dem	(3) Rep	(4) Fem	(5) Male	(6) Rural	(7) Early	(8) Top 50	(9) Unranked
Panel A: All States									
Overall ATT	-0.258	-0.308*	0.0131	-0.836*	0.579**	-0.256^{+}	-0.602*	-0.429*	0.504*
	(0.361)	(0.140)	(0.0803)	(0.328)	(0.182)	(0.155)	(0.248)	(0.187)	(0.218)
Effect (Q7–Q10)	-0.499	-0.510*	-0.000302	-1.304*	0.805*	-0.439*	-0.758*	-0.588*	0.779*
	(0.556)	(0.219)	(0.127)	(0.511)	(0.316)	(0.216)	(0.325)	(0.269)	(0.331)
Pre-*Dobbs* Treated Mean	58.60	9.447	9.474	30.68	27.92	10.88	4.493	8.545	16.76
Observations	1160	1160	1160	1160	1160	1160	1160	1160	1160
Panel B: Excl. TX, ND, IN									
Overall ATT	-0.352	-0.345*	-0.00283	-0.924**	0.571**	-0.263	-0.531*	-0.463*	0.546*
	(0.388)	(0.155)	(0.0902)	(0.355)	(0.205)	(0.182)	(0.267)	(0.209)	(0.257)
Effect (Q7–Q10)	-0.635	-0.543*	-0.0206	-1.395**	0.760*	-0.457*	-0.735*	-0.608*	0.825*
	(0.573)	(0.232)	(0.135)	(0.531)	(0.338)	(0.231)	(0.341)	(0.284)	(0.360)
Pre-*Dobbs* Treated Mean	58.50	9.411	9.967	29.82	28.67	11.85	4.356	7.560	16.32
Observations	1073	1073	1073	1073	1073	1073	1073	1073	1073

Standard errors in parentheses, $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

This table displays the CSDID coefficients for OBGYNs. The coefficients represent the change in provider density as defined in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Columns (2) and (3) utilize the sample of providers matched to DIME; columns (8) and (9) utilize the sample of providers matched to NDF; and columns (1), (4), (5), (6), and (7) utilize the full sample of providers. Coefficients are for subsets of said sample: (1) all OBGYNs, (2) OBGYNs who match to DIME and are deemed Democrats, (3) OBGYNs who match to DIME and are deemed Republicans, (4) female OBGYNs, (5) male OBGYNs, (6) OBGYNs in rural ZIP codes, (7) OBGYNs who registered in 2015 or later, (8) OBGYNs who attended a top-50 medical school, and (9) OBGYNs who attended a named but unranked medical school.

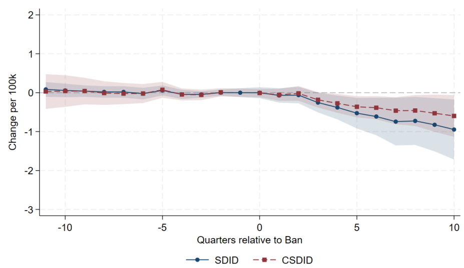
Appendix Figure C1. Changes in State-Level Provider Density (Comparison between SDID and Weighted CSDID)



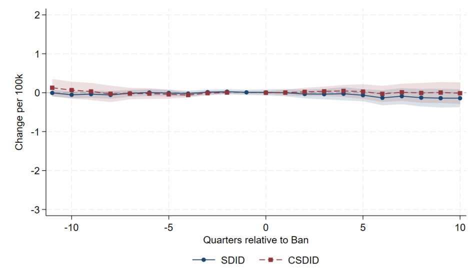
This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 3 for OBGYNs, compared to the weighted CSDID coefficients. The coefficients represent the change in provider density as defined previously in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. The analysis utilizes the full sample of providers. Coefficients are for all OBGYNs.

Appendix Figure C2. Heterogeneity in OBGYN Supply Response to Abortion Bans (Comparison between SDID and Weighted CSDID)

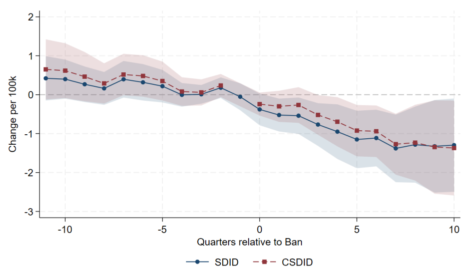
(a) Democratic OBGYNs



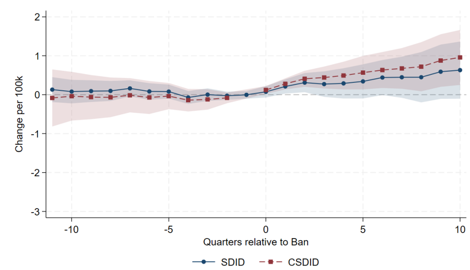
(b) Republican OBGYNs



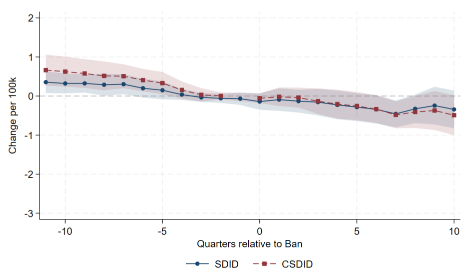
(c) Female OBGYNs



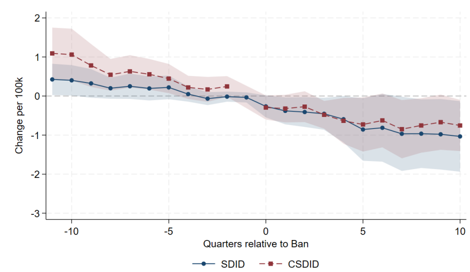
(d) Male OBGYNs



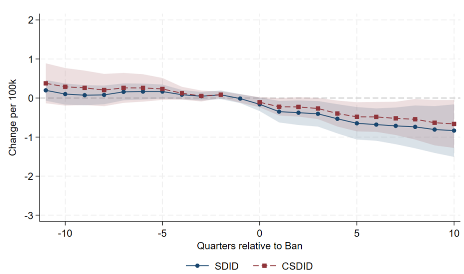
(e) Rural OBGYNs



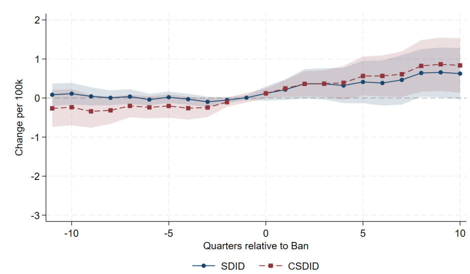
(f) Early-Career OBGYNs



(g) Top-50 OBGYNs



(h) Unranked OBGYNs



This figure displays the synthetic difference-in-differences coefficients and 95% confidence intervals from Equation 3 for OBGYNs, compared to the weighted CSDID coefficients. The coefficients represent the change in provider density as defined previously in (1) in total-ban states relative to a weighted counterfactual of protected states, before and after bans were implemented post-*Dobbs*. OBGYNs are defined in Appendix Table A1. Panels (a) and (b) utilize the sample of providers matched to DIME, panels (g) and (h) utilize the sample of providers matched to NDF, while panels (c), (d), (e), and (f) utilize the full sample of providers.