



Original research article

Quantifying the impact of targeted regulation of abortion provider laws on US abortion rates: a multi-state assessment [☆]

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ABSTRACT

Objectives: In this paper, we estimate the average effect of two common TRAP (targeted regulation of abortion providers) laws on abortion rates using a novel longitudinal database of state-level policy shifts. **Study design:** We merged several sources of policy, abortion, and sociodemographic data from 1991–2014. We used a difference-in-differences design to control for time-fixed state-level characteristics and common factors affecting abortion trends across all states, as well as measured time-varying state-level factors that may impact TRAP enforcement and abortion rates. We used generalized linear models with cluster-robust standard errors to obtain our estimates.

Results: Enforcement of ambulatory surgical center (ASC) laws reduced the abortion rate by 1.25 abortions per 1000 women aged 15–44 (95% CI: –3.39, .89), and admitting privilege laws increased the abortion rate by .57 abortions per 1000 women aged 15–44 (95% CI: –.68, 1.83), but neither effect was statistically distinguishable from zero. Our findings were robust to the inclusion of covariates and various sensitivity analyses.

Conclusion: Our results suggest that ASC and admitting privilege laws did not, on average, lead to a meaningful change in abortion rates.

Implications: US abortion rates are currently at record lows, but our findings suggest that TRAP laws are not a meaningful driver of this trend. However, this does not mean that these laws are without consequence in a particular state (or a given year). Researchers should assess the average long-run impact of TRAP laws on other outcomes in the future.

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1. Introduction

In a landmark 2016 case [1], the United States Supreme Court determined that certain provisions of House Bill 2 (or HB2), which was enacted in Texas in 2013 and only partially enforced prior to the ruling, were unconstitutional. HB2 consisted of a group of abortion restrictions, including a requirement that all abortion clinics in the state meet the same standards as ambulatory surgical centers (ASCs), and that all providers hold admitting privileges at a hospital within a 30 mile radius. HB2 had a striking effect on service availability, with a 46% decline in Texas abortion clinics within the year following enforcement of the admitting privilege provision [2]. The Supreme Court ruled that these regulations, which offered no discernable benefit to women's health and safety, constituted an undue burden to women seeking abortion.

Restrictive abortion policies are not new, nor are they unique to Texas. In the US, states have considerable control over abortion regulation, and over half of all states currently have four or more restrictive policies in place [3]. States enacted nearly a third of all existing restrictions from 2010 to present, reflecting a notable upsurge in restrictive legislation in recent years [4,5] that coincides with, but is not necessarily responsible for, a steady decline in the US abortion rate (**Appendix 1**). We can conceptually partition abortion restrictions into demand-side (e.g. mandatory counseling laws, parental notification laws) and supply-side policies [6]. Provider-focused laws like the ones enacted in Texas fall under the supply-side category and are often collectively referred to as TRAP (targeted regulation of abortion providers) laws. These regulations can be very costly to clinics and may become serious barriers to continued service provision, particularly in rural areas and locations where hospitals are unlikely to extend admitting privileges to abortion providers.

There is some evidence that TRAP enactment is associated with a decrease in provider availability: certain supply-side regulations,

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like those enacted in Texas, are so severe that existing providers are unable to comply, forcing them to close [2,4]. However, compared to Texas, there is less information about the effect of TRAP laws on provider availability in other states. It is also relevant to consider the impact of TRAP laws on state-level abortion rates, which may be an important downstream consequence of reductions in provider availability [2,7]. Little is known about this association, at least beyond the context of short-term, single state settings that may be susceptible to bias. A recent systematic review of quantitative evidence on the health effects of supply-side policies [8] found little rigorous quantitative evidence on the relationship between TRAP laws and abortion rates. Certain TRAP laws – specifically admitting privilege and ASC requirements – appeared to be associated with a decrease in abortion rates [2,9], but the available evidence may not be generalizable to the greater US. Furthermore, half of the studies in the review assessed policy effects from 2006 or earlier, which does not capture recent trends in TRAP enforcement.

The Supreme Court decision means that many of the more stringent existing policies are likely to be challenged and overturned, but this will be a gradual process [4]. It is therefore important to understand the impact of these laws on US abortion rates. To date, several factors have precluded this type of analysis, most notably a lack of high-quality, longitudinal data on state policy changes. Furthermore, as it is infeasible to randomize states to TRAP exposure, it is essential to employ other methods of control for both time-invariant state characteristics and secular trends common to all states, as both could bias estimates (researchers have also noted this concern in literature reviews on demand-side policies [10–12]).

The goal of this paper is to address the challenges highlighted above and estimate the effect of ASC and admitting privilege law enforcement, respectively, on US abortion rates using data from 1991 to 2014. We expand upon existing work by utilizing a novel longitudinal database of state-level supply-side policy activity. These data allow us to employ a difference-in-differences approach, which accounts for confounding by secular trends and time-invariant state-level characteristics.

2. Methods

2.1. Data

We obtained state-level data on abortion by state of occurrence from 1991 to 2014 (the most recent year for which data were available) from the Guttmacher Institute. These data were available as rates and counts: we used the count data for the purposes of this analysis. The policy data collection process is described in detail elsewhere [13]. Briefly, two coders (NA, JM) systematically searched state-level policy documents and a range of secondary sources to track state-level TRAP enactment and enforcement over time. The resulting policy dataset captures information on state-level policy presence, administrative codes associated with the law(s), year of enactment, year of enforcement, and (if applicable) the year a given law was blocked/enjoined. Given the complexity of these data and the data collection process, in sensitivity analyses we included an indicator of our coders' uncertainty. The data are available for public use [13].

We defined state-level exposure to ASC laws as the presence of an enforced policy that explicitly required clinics to adhere to a state's ASC regulations. We defined exposure to admitting privilege laws as the state-level presence of enforced laws requiring individual providers to obtain these privileges at a nearby hospital. As enforcement of ASC and admitting privilege laws may occur independently, simultaneously, or consecutively, we considered a state

“exposed” to the first policy it enforced from 1991 to 2014 in order to isolate the effect of individual policies. We excluded states simultaneously enforcing both laws from all analyses ($n=2$), as this prevented us from disentangling policy effects and there was insufficient information in this subgroup to assess the effect of joint enforcement. However, we retained states that simultaneously enforced an ASC or admitting privilege law alongside a transfer agreement component, as patient transfer regulations were very common and often embedded in existing ASC guidelines.

Evidence on the determinants of TRAP laws suggests that policy enforcement is plausibly exogenous [14]: a state's governing political party has a meaningful impact on policy enforcement, but other factors (religious composition, state abortion rate) may not. Additionally, many factors, such as the enforcement of subsequent TRAP laws, may mediate the relationship between an earlier law and abortion rates. Our models therefore control for time-varying factors hypothesized to feasibly impact TRAP enforcement and abortion rates: specifically, governor's political party, the percentage of state residents living below the poverty line, the presence of demand-side abortion policies (parental notification, mandatory waiting period, and/or mandatory counseling laws [15]), and a dummy variable reflecting whether a state permits the use of public funds for abortion. We also collected data on other state-level characteristics for descriptive purposes (population size, median household income, educational attainment, etc.). We provide a full listing of all the variables in our dataset, and their respective sources, in **Appendix 2**.

2.2. Analytical approach

We used a difference-in-differences design to control for time-fixed state-level characteristics and common factors affecting abortion trends across all states, as well as measured time-varying state-level factors that may impact TRAP enforcement and abortion rates. This study design relies on longitudinal data to construct an appropriate control group based on pre-policy trends [16]. States were eligible to serve as controls if they did not experience a shift in ASC or admitting privilege policy over the observation period. This design assumes that states that did not change their abortion policy provide a valid estimate of what would have happened to abortion rate trends in TRAP states, had these states *not* changed their policy. The central assumptions of this approach are parallel pre-exposure trends and “common shocks” – in other words, any factors that occur throughout the observation period and affect abortion rates in the exposed and control groups equally will not generate bias [17].

We obtained model estimates via generalized linear models with state and year fixed effects and a time-varying policy term reflecting the presence/absence of a policy in a given state/year. We fit negative binomial models with cluster-robust standard errors, clustered at the state level [18], and an offset term (the log of the population of women in their childbearing years). We hypothesized that any impact of ASC or admitting privilege enforcement on abortion rates would be immediate (generally observable within the year of enforcement) given the reported association between TRAP legislation and service availability [2]. To relax this assumption, we conducted sensitivity analyses using 1–2-year lags to explore the possibility of a delayed policy impact on abortion rates. We also examined 1–2-year leads to assess the possibility of pre-policy shifts in abortion rates, which could indicate reverse causality. As additional robustness and sensitivity checks, we: (1) distinguished between ASC laws applying to all abortions and those applying to only later abortions; (2) excluded states with preexisting TRAP laws (enforced prior to 1991) from the comparison group; (3) restricted to states where the policy data was reasonably clear with respect to timing (we refer to these

Table 1
Demographic characteristics by group: Exposed^a vs. control^b states

	1991		2014		Percent change	
	Exposed (n=7)	Control (n=35)	Exposed (n=7)	Control (n=35)	Exposed	Control
Population	8,097,515	4,546,173	10,256,132	5,788,856	27	27
High school diploma (%)	77.2	80.8	88.8	89.8	15	11
Bachelor's degree+ (%)	19.8	22.2	31.5	31.7	59	43
Median household income ^c	50,904	51,639	54,158	55,047	6	7
Percent living in poverty	14.6	13.3	14.2	13.6	-3	2
Unemployment rate ^d	7.2	6.6	5.6	5.3	-22	-20
Birthrate ^e	16.1	15.6	12.7	12.2	-21	-22
Fertility rate ^f	67.9	67.4	63.7	63.2	-6	-6
Teen birthrate ^g	65.0	56.3	26.5	23.2	-59	-59
Republican governor (%)	42.9	42.9	57.1	51.4	33	20
Parental notification (%)	43.0	25.7	100	71.4	133	178
Waiting or counseling (%)	71.4	40.0	71.4	42.9	0	7
Permits use of public funds for abortion (%)	0	34.3	14.3	45.7	0	33

^a Enforced an **ASC law** between 1991 & 2014 (before enforcing any other TRAP law, if applicable);

^b Did not enforce any type of TRAP law between 1991–2014;

^c Income in 2014 dollars;

^d Seasonally adjusted percentage of workforce unemployed as of November of a given year;

^e live births/1000 people;

^f live births/1000 females,

^g live births/1000 females aged 15–19.

as “high confidence” states); and (4) restricted the comparison group to states that enacted, but did not enforce, a TRAP law from 1991 to present, as these states may better represent counterfactual trends in the exposed. We conducted all analyses using Stata 14 (StataCorp. 2015. *Stata Statistical Software: Release 14*. College Station, TX, USA: StataCorp). Study data and code are available at (*redacted for purposes of review*) in the interest of reproducibility.

3. Results

We list states by policy category in **Appendix 3**. Following exclusion of states that simultaneously enforced both laws (two states) and states with competing policy shifts over the observation period for each exposure of interest, seven states met our ASC exposure definition and five met our admitting privilege exposure definition; 35 states were retained in the control group. We provide a summary of descriptive statistics in **Table 1**. This table compares characteristics of states enforcing ASC laws to states with no TRAP policy shift over the observation period; we provide a similar comparison (for admitting privilege enforcers and control states) in **Appendix 4**. Our data reflect a substantial decline over time in various factors linked to abortion rates in both groups, and the percent change over time in many potentially relevant covariates was generally comparable in exposed and unexposed groups. Notable exceptions include trends in higher education, the percentage of states with a Republican governor, and the presence of various demand-side policies, particularly for the admitting privilege comparison (though it is possible that TRAP enforcement may have impacted some of these factors by the end of our observation period). These changes over time may not have been linear; nonetheless, this approach offers some evidence that shifts in various factors were comparable over time between our exposure groups, which suggests that the “common shocks” assumption may be reasonable for the analysis of ASC laws, and perhaps less so for the admitting privilege analysis.

Evaluating parallel pre-policy trends is straightforward when an intervention occurs at a fixed point in time for all exposed units. However, as there is no defined pre-policy period in this analysis (states enforced TRAP laws at different points), we capitalized on the fact that the majority of exposed states in our sample first enforced policies several years into the observation period. Our data suggested that most ASC policies (89%) were enforced after

1998 and most admitting privilege policies (78%) were enforced after 2010. Using these years as our cut-off values, we defined a “pseudo” pre-exposure period for each exposure for the parallel trends assessment. Abortion trends appeared reasonably parallel for each exposure contrast: **Fig. 1** illustrates these trends on the relative scale, **Appendix 5** plots the same trends on the absolute scale, and **Appendix 6** briefly describes the additional statistical testing we conducted to verify this assumption.

Model estimates are presented in **Table 2**, expressed as incidence rate ratios (IRRs) for the effect of TRAP exposure on abortion rates per 1000 women aged 15–44, conditional on state/year fixed effects and model covariates. Naïve models (estimated without state and year fixed effects) overestimate policy impact and illustrate the importance of accounting for both fixed state-level characteristics and secular trends common across states: in models without year fixed effects (but controlling for state fixed effects), ASC enforcement decreased abortion rates by a factor of .68 (95% CI: .60, .77) compared to non-enforcers. Failure to control for time-fixed state-level characteristics also led to biased point estimates. The fully-adjusted difference-in-differences estimates were comparatively modest and not statistically distinguishable from zero: ASC enforcement decreased abortion rates by a factor of .93 (95% CI: .81, 1.06), and admitting privilege laws were associated with a 4% increase in the abortion rate, relative to states without these laws (IRR=1.04, 95% CI: .96, 1.11). All findings were robust to the inclusion of covariates, and there was no evidence that abortion trends were shifting differentially between groups prior to policy enforcement.

Table 3 expresses these findings on the absolute scale. Using the fully adjusted negative binomial model to generate marginal predictions, we found that states enforcing ASC laws could expect to have (on average) 1986 fewer abortions than states not enforcing these laws (95% CI: -5408, 1435), and a rate reduction of 1.25 abortions per 1000 women aged 15–44 (95% CI: -3.39, .89). Again, neither estimate was statistically distinguishable from zero. Findings for admitting privilege enforcement were similar to ASC laws, but point estimates were closer to the null. We also present predicted marginal differences based on a crude model (without state or year fixed effects) to further illustrate the importance of controlling for these factors in this context: failing to account for state and year fixed effects leads to a substantial overestimation of policy-driven differences in abortion counts and abortion rates.

Parallel trends verification: Pre-policy abortion trends by exposure status (relative scale)

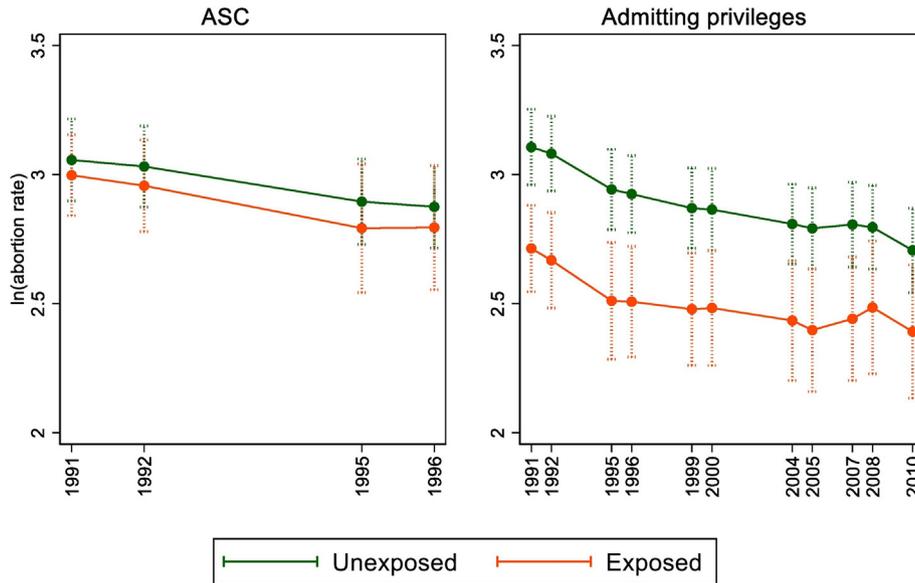


Fig. 1. Parallel trends verification: Pre-policy abortion trends by exposure status.

Table 2
Effect of TRAP policy enforcement on abortion rates^a

	ASC enforcement ^b (n=42)		Admitting enforcement ^c (n=40)	
	IRR	95% CI	IRR	95% CI
Naïve^d				
Crude	0.74	0.53, 1.05	0.44	0.33, 0.58
Year fixed effects only	0.85	0.60, 1.20	0.52	0.36, 0.75
State fixed effects only	0.68	0.60, 0.77	0.74	0.70, 0.79
Difference in differences				
No covariates ^e	0.92	0.81, 1.05	1.01	0.94, 1.09
Fully adjusted ^f	0.93	0.81, 1.06	1.04	0.96, 1.11
Leads				
t-1	0.95	0.82, 1.09	1.04	0.97, 1.12
t-2	0.96	0.84, 1.11	1.02	0.93, 1.11
Lags				
t+1	0.94	0.85, 1.04	1.01	0.96, 1.07
t+2	0.92	0.86, 0.98	1.01	0.96, 1.06

^a Abortions per 1000 women aged 15–44;
^b Defined as enforcement of an ASC law from 1991–2014, excluding states that first enforced an admitting provision within that period;
^c Exposure defined as enforcement of an admitting privilege law from 1991–2014, excluding states that first enforced an ASC law within that period;
^d Estimation using neither type, or only one of the two types, of fixed effects in the final model;
^e Accounts for state and year fixed effects, but no covariates;
^f Adjusted for annual state-level poverty, parental notification laws, informed consent laws, whether a state permits the use of public funds for abortion, and governor's political affiliation.

We conducted several sensitivity analyses to test the robustness of our findings and the assumptions required for unbiased difference-in-differences analyses (Fig. 2; Appendix 7). Point estimates shifted slightly but qualitative findings were quite robust, with one exception: laws targeting later abortions had a stronger impact on abortion rates (IRR=.79, 95% CI: .67, .94), which was unexpected as later abortions account for the minority of all procedures. However, this estimate was driven by two states (MO, TX) and should therefore be interpreted with caution. We also com-

pared our results to models estimated using generalized estimating equations (GEE) to examine if our approach of clustering standard errors at the state level (rather than explicitly modeling a correlation structure) offered sufficient control for state-level correlation over time in this context. Estimates and confidence intervals were similar, which suggested that our approach was acceptable.

4. Discussion

This work adds to a growing body of quantitative evidence on the impact of restrictive abortion laws that target the supply-side. We found little evidence that ASC or admitting privilege laws have a meaningful impact on state-level abortion rates. Our findings do not necessarily mean that abortion rates in a particular state (or a given year) will be unaffected by the enforcement of these policies; rather, this analysis suggests that the average effect of these policies on abortion rates is consistent with no effect. Our findings were robust to the inclusion of covariates and to most of our sensitivity analyses, with the exception of potentially heterogeneous effects of ASC laws depending on the trimester the law takes effect.

We acknowledge a number of important limitations, many of which are directly linked to our data sources. Abortion data are only available at the state level, which limited us to state-level inference in this analysis. On the policy side, it can be difficult to establish whether a state has actually enforced an enacted law, and this is compounded by variability in the quality and specificity of policy information by state, which raises the possibility of exposure misclassification. We examined this by conducting a sensitivity analysis restricting to states with especially clear policy data, and findings did not shift meaningfully. We also focused on policy enforcement, rather than enactment, in this study, but evidence from Texas suggests that enacted laws may impact service provision even before they are enforced [2,19].

We did not control for the severity of individual TRAP laws in our main analysis. For example, Arkansas requires abortion provi-

Table 3
Absolute effects of TRAP enforcement based on marginal predictions: Differences in counts and rates^a

	ASC enforcement ^b (n=42)		Admitting enforcement ^c (n=40)	
	Difference: Counts	Difference: Rates	Difference: Counts	Difference: Rates
Crude ^d	–5612 (–11,451, 227)	–4.45 (–9.08, 0.18)	–10,098 (–13,617, –6779)	–9.35 (–12.42, –6.28)
Adjusted ^e	–1986 (–5408, 1435)	–1.25 (–3.39, 0.89)	829 (–981, 2639)	0.57 (–0.68, 1.83)

^a Derived from fully-adjusted negative binomial model with state/year fixed effects unless otherwise specified;

^b Defined as enforcement of an ASC law from 1991–2014, excluding states that first enforced an admitting provision within that period;

^c Exposure defined as enforcement of an admitting privilege law from 1991–2014, excluding states that first enforced an ASC law within that period;

^d Naive estimation without fixed effects;

^e Adjusted for annual state-level poverty, parental notification laws, informed consent laws, whether a state permits the use of public funds for abortion, and governor's political affiliation.

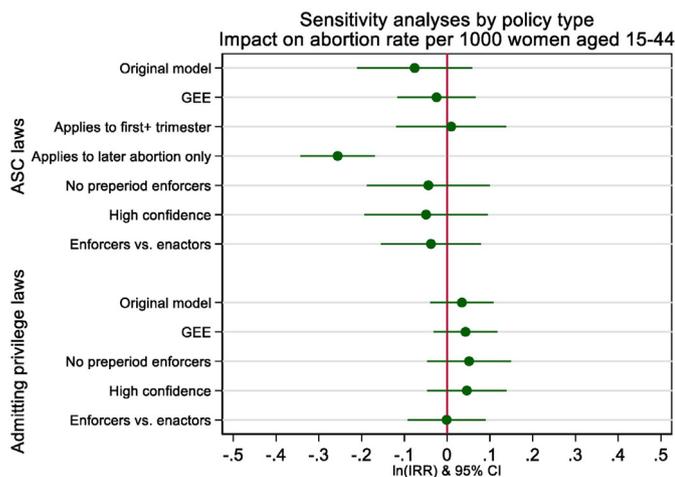


Fig. 2. Sensitivity analyses by policy type: Impact on abortion rate per 1000 women aged 15–44.

ders to be within a 30 mile radius of a hospital, while Utah requires providers to be within 15 minutes of a hospital. As more stringent laws may have a different effect on abortion rates and associated outcomes, a more explicit assessment of the often nuanced differences between state TRAP laws could add an interesting layer of complexity. A recently-released cross-sectional data source [20] provides comprehensive information on the specific components of these laws, and may be useful to researchers seeking to advance this work.

A final challenge in this analysis is identifying causal effects of a policy in the presence of strong temporal trends in abortion rates. The cause of the decline remains unknown, but it is likely a combination of decreased need for abortions and an increase in abortion restrictions [21]. If these factors are driving down rates at a rapid pace in nearly all states, it may be more difficult to detect an impact of ASC or admitting privilege policy enforcement. Our initial exploratory analyses supported this hypothesis, as a basic linear model for state rates that included only state and year fixed effects explained the majority of the variation ($R^2=.92$).

States often pass supply-side laws under the pretense of protecting women, but abortion is already an exceedingly safe procedure in the absence of these laws [22]. Nevertheless, a recent cross-sectional analysis found that states have targeted abortion provision and providers to a greater extent than other office-based interventions [23]. Our findings suggest that ASC and admitting privilege laws do not, on average, lead to a meaningful change in abortion rates, but this does not mean that these laws are without consequence. Given their apparent link to reductions in provider

availability [2], these laws have the capacity to increase travel burdens, costs, and general hardship associated with obtaining an abortion [2,23], and the introduction of structural barriers may also increase second trimester abortions [2]. While we can expect states to repeal many supply-side laws in the coming years, further work is required to better assess the causal impact of particularly restrictive policies and the potentially differential effects of these laws on disadvantaged women.

Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.contraception.2019.06.003>.

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