Econ 191 Final Paper

The Effect of Abortion Bans on Home Value

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June 1, 2023

1 Introduction

Empirical investigation reveals a corpus of literature underscoring the multifaceted, detrimental socio–economic, financial, and healthcare consequences instigated by denial of abortion services (Ralph et al., 2020; Miller et al., 2020). Following the pivotal verdict of *Dobbs v. Jackson* by the U.S. Supreme Court, which effectively abrogated the previously entrenched ruling in *Roe v. Wade*, a plethora of state–enforced abortion prohibitions have been promulgated, leading to a surfeit of alarming instances of abortion denial extensively reported in media outlets. These episodes include a 10–year–old sexual assault victim denied an abortion in Oklahoma (ABC News, 2022), and a woman carrying a fetus diagnosed with a lethal genetic anomaly known as Acrania, denied an abortion due to her location in Louisiana, a state which has instituted an outright ban on abortions (NBC News, 2022).

Evidently, the advocates of these austere state legislations, which inhibit, outlaw, and, as evidenced in the case of Texas, criminalize medical abortion procedures, appear undeterred by the profound implications of the policy constructs they endorse. It can be hypothesized that their unyielding support for these legislative constraints may originate from a lack of direct personal impact experienced by the policy endorsers and supporters, yet there could potentially be unidentified indirect repercussions adversely affecting these same individuals. Consequently, the impetus for this research is to elucidate potential unintentional ramifications associated with the prohibition and criminalization of medical abortions that have bearing not solely on those directly seeking such services, but also on the wider demographic residing within jurisdictions that opt to enforce such prohibitions.

This research therefore endeavors to quantify the implications of state–enforced abortion bans on real estate valuations, specifically by examining the inclination of residents to *vote with their feet*—migrating to states where governance and policy frameworks are more closely aligned with their personal preferences. The research methodology entails a search for evidential instances of inter–state migration, using a difference–in–differences approach to measure alterations in property prices prior and subsequent to the public dissemination of the U.S. Supreme Court case brief, which correctly led many to anticipate the imminent abrogation of the legal protections for medical abortions as stipulated under *Roe v. Wade*.

The structure of this paper is organized as follows: Section 2 comprises a critical review of pertinent literature. Section 3 introduces the dataset underpinning this study and provides a comprehensive analysis thereof, including summary statistics and a balance test. Section 4 explicates the empirical methodology for assessing policy impact, delineating relevant variables, the estimation strategy via a difference–in–differences model, the hypothesis and inference formulation, and the identification strategy. Section 5 presents and scrutinizes the study's findings. In Section 6, the robustness of the results is ascertained through several methods, including a placebo test, an analysis of estimator bias, and an additional model adjusting for the number of active home listings. The paper concludes with Section 7.

The fundamental aim of this research project is to investigate the nexus between civil liberties, specifically access to abortion services, and inter–state migration. This is operationalized by evaluating shifts in average property prices between two states with differing policies, subsequent to the enforcement of a state abortion ban in one of the states. This novel research augments the existing literature by offering the inaugural exploration of the economic ramifications of inter–state migration following the enforcement of state abortion bans, and by investigating a putative causal relationship between civil liberties, migration, and property prices within the United States.

2 Literature Review

The paradigm of unrestricted labor mobility across state borders, coupled with a substantial prevalence of internal migration within the United States, engenders opportunities for its populace to express their preferences regarding state abortion policy via intra–national migration (Molly et al., 2011). This reflects a theoretical framework whereby consumers enact a form of 'foot voting'.

In the U.S. context, elevated degrees of economic and personal freedom within a given state correlate with an increase in net in–migration to that state (Cebula, 2014). Given the constraints imposed by state abortion bans on the economic and personal autonomy of women, the implementation of such policies may trigger a migration flux, with individuals transitioning from states with restrictive policies to those with more liberal stances, thereby optimizing their economic and personal liberties. Hofmann's [2019] work substantiates this theory, indicating that conservative regions with a predilection for stringent federal abortion regulations essentially export the demand for abortion services to areas adopting more liberal abortion regulations, thus leading to migration guided by preference within Switzerland. This suggests that the implementation of post–Roe state abortion bans in the United States could potentially exert an influence on internal migration patterns between states, a trend that ought to manifest in corresponding deviations in housing prices.

In the context of migration decision models, individuals seek to optimize their location based on utility maximization relative to alternative locales. A key mechanism facilitating this optimization is self–selection: migrants sort themselves according to regional disparities in returns to skills (Borjas et al., 1992; Ellis et al., 1993; Hunt, 2004). Dahl [2002] ingeniously integrates this motivation for migration rooted in human capital investment decision–making with an equilibrium model, wherein individuals distribute themselves across states as a function of demand for their human capital and state–embedded amenities.

In the aftermath of the Covid–19 pandemic's advent, the transformative effect of work–from–home technologies altered the fundamental structure of preferences dictating individuals' optimal location choices. This technology enables individuals to decouple their residential and work locations, thus creating opportunities for spatial arbitrage and, consequently, exerting a downward pressure on housing prices and rents in locations characterized by high productivity (Brueckner, 2021). The implications of work–from–home technology on location choices carry considerable weight, particularly considering that a decline in longer–distance migration over the past decade has been hypothesized to be linked to reduced labor mobility (Jia et al., 2022).

2.1 Contributions to the Literature

The objective of this research is to augment the current body of literature concerning the interplay between access to civil liberties—with a specific emphasis on abortion services—and the subsequent patterns of inter–state migration. The mechanism employed to measure this effect involves an examination of variations in median housing prices located within a restricted proximity along a pair of bordering states, predicated on the assumption that these prices accurately mirror housing demand in each respective state as a result of their restricted proximity to one another across the states' shared border, following the information shock predictive of state abortion prohibitions. This methodological approach addresses a notable lacuna in the existing literature, as economic studies scrutinizing the impact of state abortion bans on inter–state migration are conspicuously absent. Furthermore, this research enhances the existing body of knowledge by postulating and testing for a causal relationship between access to civil liberties, migratory patterns, and housing prices within the context of the United States.

3 Data and Descriptive Statistics

3.1 Data

This empirical research utilizes housing inventory data sourced from the Federal Reserve Economic Database (FRED), originally released by Realtor.com. A balanced panel data approach is employed, focusing on the monthly average prices of active home listings per county, which is then transformed into a log–linearized form. Monthly county–level aggregate average home prices are examined for counties within New Mexico and Texas. The decision to use average prices over median prices was motivated by the rationale that average prices would capture any skewness in the distribution of home prices, thus better reflecting the economic mobility of households, which are hypothesized to be more pervasive amongst 'foot voters' in the initial stages following the implementation of policy changes.

The choice of Texas and New Mexico as the subjects of this study is motivated by their respective abortion policies (or lack thereof), and their geographic proximity, which helps to mitigate potentially confounding macroeconomic effects. It should be noted that New Mexico does not maintain a ban on abortions, whereas Texas has implemented not only a ban but also a legislative act that criminalizes seeking a medical abortion both within and outside of the state for Texas residents. I postulate that this stringent legislation is likely to engender a stronger incentive for residents to migrate out of state compared to states with policies only banning medical abortions, thus optimizing for a higher probability of measuring any treatment effect during such an early time–frame following the treatment shock.

The dataset used in this study spans from July 2020 to November 2022. Despite the availability of data dating back to 2017, I have chosen to disregard data prior to July 2020 to mitigate potentially irrelevant violations of the parallel trends assumption, whilst ensuring that the sample size remains sufficiently large and balanced, thus eliminating concerns regarding the validity of the results due to power issues. Moreover, I opted to utilize county–

level average prices to overcome the residual power problem, thereby expanding the sample size that would have otherwise produced insignificant evidence for the results due to being restricted to merely seven observations existing following the treatment shock.

Certain limitations associated with the data must be acknowledged. These include a relatively small number of observations available for analyzing the post–treatment response, as well as the restriction of the data to county–level average prices on a monthly basis. Ideally, these constraints could be addressed by augmenting the frequency of observations of county–level average home prices. The optimal scenario would involve the use of a random sampling of individual–level home prices within New Mexico and Texas, thus circumventing these limitations.

3.2 Data Analysis

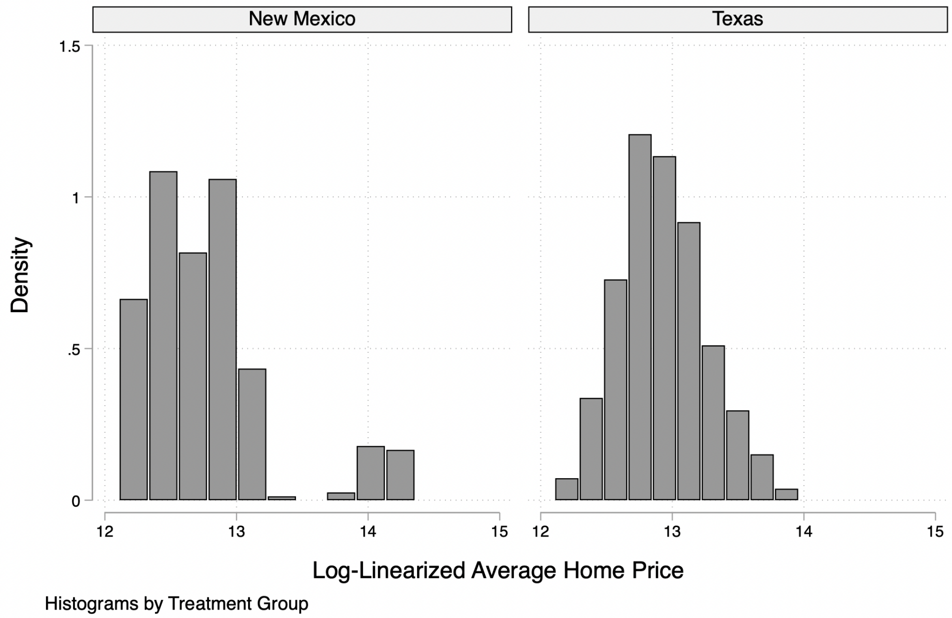
As summarized in Table 1 below, the averages across the entire time series and in the pre–treatment phase for the complete sample, the control group (New Mexico), and the treatment group (Texas) are relatively proximate. However, there is a slight increase in variability in the post–treatment averages, with the Texas average significantly raising the full sample average compared to the New Mexico average. The standard deviations for the distribution of log–linearized average home prices in New Mexico are markedly higher than those in Texas. Nevertheless, the minimum, median, and maximum values are reasonably similar across all distributions of log–linearized average home prices, across treatment groups, and treatment periods.

Table 1: Summary Statistics of Log–Linearized Average Home Prices

	Full Sample			New Mexico			Texas		
	Overall	Pre	Post	Overall	Pre	Post	Overall	Pre	Post
Minimum	12.106	12.106	12.138	12.106	12.106	12.138	12.106	12.106	12.321
Median	12.871	12.842	12.951	12.64282	12.623	12.851	12.900	12.873	12.984
Mean	12.901	12.869	13.001	12.76303	12.731	12.864	12.929	12.897	13.029
Maximum	14.358	14.358	14.234	14.35793	14.358	14.234	13.961	13.961	13.925
Std. dev.	0.36604	0.36372	0.35539	0.4896334	0.49071	0.47494	0.32860	0.32517	0.31944
Variance	0.13399	0.13229	0.12630	0.2397409	0.24079	0.22557	0.10798	0.10573	0.10204
Skewness	0.65440	0.70571	0.60777	1.567638	1.6157	1.5842	0.36217	0.40806	0.28953
Kurtosis	3.8244	3.9780	3.6560	5.360227	5.4890	5.3775	2.8516	2.9572	2.6464
N	2,059	1,562	497	348	264	84	1,711	1,298	413

Additionally, as presented in Table 1, all distributions exhibit a degree of positive skewness, suggesting each distribution has an extended right tail and is shorter on the left side. Furthermore, all distributions demonstrate a positive kurtosis, indicating each distribution has a heightened peak and heavier tails in comparison to a normal distribution. This is validated by the histograms in Figure 1 below, which display the log–linearized average home prices over the entire time span for New Mexico and Texas. These histograms reveal that while average home prices in Texas approximately adhere to a normal distribution, average home prices in New Mexico are significantly more positively skewed, both relative to the normal distribution and to the distribution of average home prices in Texas.

Figure 1: Histograms of Log–Linearized Average Home Prices for New Mexico and Texas



Considering the disparities in variances reported in the summary statistics, the t–tests for the treatment and control averages must be adjusted to account for this variability, in order to yield precise measures, which are presented in Table 2 below. All of the t–tests result in values with absolute magnitudes somewhat larger than two, indicating the difference in averages for treatment and control groups for the entire time span, pre–treatment time period, and post–treatment period are statistically significant. However, given that the magnitude of the t–statistics does not deviate substantially from the optimal range, the data adequately satisfies the requirements for this research.

Table 2: Balance Test of Means and T-Statistics for Unequal Variances

	Control Mean	Treatment Mean	Difference in Means	T-Statistic
Overall	12.763	12.929	-0.16547	-6.0342
	(0.02625)	(0.00794)	(0.02742)	
Pre-Treatment	12.731	12.897	-0.16588	-5.2625
	(0.03020)	(0.00903)	(0.03152)	
Post-Treatment	12.864	13.029	-0.16420	-3.0323
	(0.05182)	(0.01572)	(0.05415)	
N	348	1,711		

4 Empirical Methodology

To evaluate the impact of state abortion bans on migratory behavior, or ‘foot voting’, I employ a difference–in–differences regression analysis on home values in two adjacent states: Texas, which implemented state abortion bans, and New Mexico, which did not enact any state abortion bans. The aim of the difference–in–differences regression analysis is to compare the effect of the abortion bans on average home values in our treatment group, represented by houses listed for sale in Texas counties, with the average home values in our control group, which includes houses listed for sale in New Mexico counties.

4.1 Variables

I leverage dummy variables and an interaction term to isolate and capture the effect of state abortion policy on monthly average home prices between the control group (New Mexico) and the treatment group (Texas) both pre–treatment and post–treatment, with the treatment being the leak of the U.S. Supreme Court case brief on May 2, 2022. The definitions of these variables are described in Table 3 below.

Table 3: Data Description for Variables

Notation	Variable	Description
Y_{it}^G	Outcome (USD)	Average home listing price for county i with treatment status $G = (T, C)$ during time period t
D_{it}^G	Treatment Dummy	Captures home prices affected by Texas state policy ban and criminalization of abortion
D_{it}	Post Dummy	Captures home prices after the leak of a U.S. Supreme Court brief on *Roe v. Wade*
$(D_{it}^G \times D_{it})$	Interaction Term	Post–leak home prices affected by Texas state policy ban and criminalization of abortion
i	County	Indexes average home prices by county
G	Treatment Group	Indexes average home prices by treatment group (by state), where $G = (T = 1, C = 0)$
t	Treatment Period	Indexes average home prices by treatment period, where $t = (\text{Pre} = 0, \text{Post} = 1)$

4.2 Estimation Strategy

I employ a two-period difference-in-difference model, derived from the canonical model in Card et al. [1993], with the adaptation of including controls for both state fixed effects and time fixed effects to estimate the Average Treatment Effect (ATE) of a state–enacted abortion ban on the county aggregate log–linearized average home prices in the state of

Texas. The model is defined

$$Y_{it}^{G} = \alpha + \beta D_i^G + \gamma D_{it} + \delta(D_i^G \times D_{it}) + StateFE + TimeFE + \varepsilon_{it}$$

where α denotes the constant, β denotes treatment group specific effects, γ denotes the common time trend for treatment and control groups, and δ denotes the Average Treatment Effect (ATE) of a state–implemented abortion ban on county aggregates for log–linearized average home price that is estimated using ordinary least squares regression.

4.3 Hypotheses

The two hypotheses I aim to test are defined as follows:

1. The first hypothesis is defined as a null hypothesis of $H_0 : \delta = 0$, where the alternate hypothesis is $H_A : \delta \neq 0$.

2. The second hypothesis is defined as a null hypothesis of $H_0 : \delta < 0$, where the alternate hypothesis is $H_A : \delta \geq 0$.

4.4 Inference

To account for heterogeneity across counties, the standard errors are made robust through clustering county–level data by state and including a control for state fixed effects for counties. Additionally, a control for time fixed effects is used to make the standard errors robust to heteroskedasticity across time periods. Given the method of clustering by state results in only two clusters, the utilization of boottest for fast and wild bootstrapping of standard errors as outlined in Roodman [2019] is required to bootstrap effectively to obtain correct robust standard errors.

4.5 Identification Strategy

The interaction term, denoted as δ, is assumed to be an unbiased estimator. This assumption entails that (1) the expected value of $\hat{\delta}$ is equal to δ; (2) the error term is on average equal to zero; and, (3) the error term is uncorrelated with other variables in the model such that the covariance equals zero for the two dummy variables and the interaction term. Additionally, regarding the exogeneity assumption necessary for the choice of an identification strategy in the research design for establishing causality, the conventional practice of the U.S. Supreme Court is to announce case decisions in June. Hence, the premature leak of the briefing to the public on May 2, 2022, represents an exogenous shock. This unexpected event contributes to the validity of the research design, as it is unlikely that housing prices or migration patterns could have anticipated or influenced the early release of the Supreme Court decision. Thus, the plausibility of the identifying assumption—that the treatment and control groups would have followed parallel trends in the absence of the treatment—is strengthened.

4.5.1 Parallel Trends Assumption

While pre–trends in average home prices appear to follow a relatively analogous pattern in New Mexico and Texas, some deviations in pre–trends suggest non–parallel slopes in certain observations. Thus, time fixed–effects are incorporated into the difference–in–differences model as a preventive measure for a closer comparison to the likely counterfactual. Figures 2 and 3 below portray time–series graphs of the pre–trends and the differences in log–linearized average home prices in New Mexico and Texas, respectively.

Figure 2: Log–Linearized Average Home Prices for New Mexico and Texas

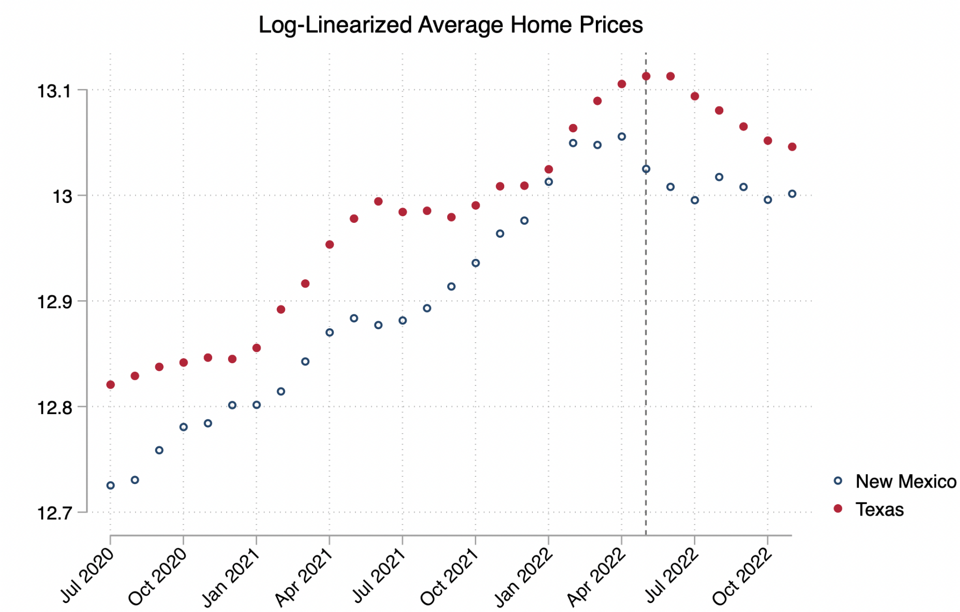
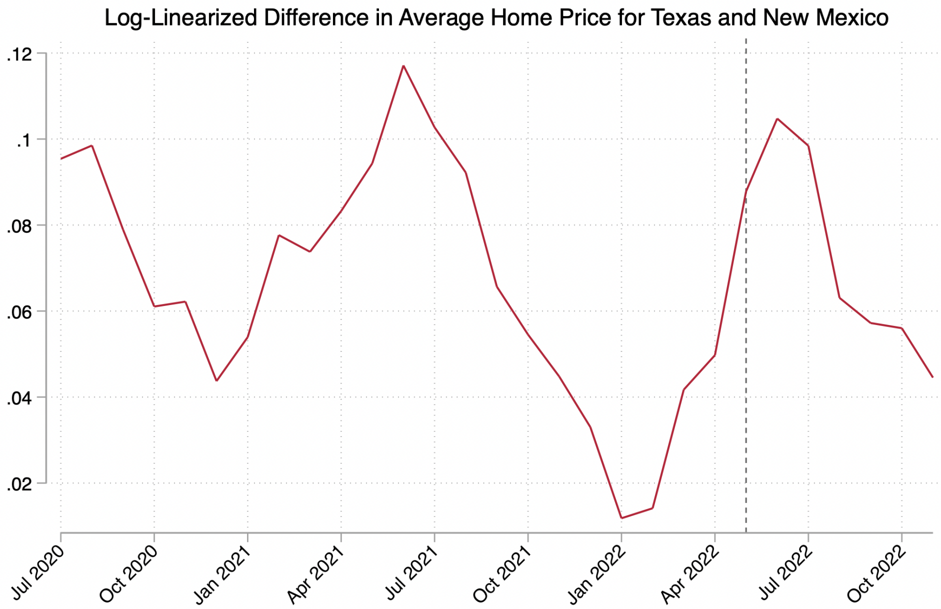


Figure 3: Difference in Log–Linearized Average Home Prices for New Mexico and Texas



5 Results

The results give the coefficients for the difference–in–differences model to be

$$Y_{i1} = 12.653 - 0.123D_i^1 + 0.231D_{i1} - 0.002(D_i^1 \times D_{i1}) + StateFE + TimeFE + \varepsilon_{i1}.$$

In USD, the resulting difference–in–differences model can be expressed as

$$Y_{i1} = \$300988.93 - \$43006.861D_i^1 + \$89427.37D_{i1} - \$7577.42(D_i^1 \times D_{i1}) + StateFE + TimeFE + \varepsilon_{i1}.$$

The findings indicate that the decrease in Texas home prices by 12.3% compared to those in New Mexico shows a distinct impact of the state abortion ban on the housing market. The overall rise in average home prices during the post–treatment period by 23.1% compared to the pre–treatment period suggests other influences that could be driving up all home prices after the abortion ban. However, the remarkable finding here is that the average home prices in counties affected by the Texas state abortion ban are instead valued 0.2% less, translating into $7,577.42 decrease in monetary value, than those in counties unaffected by the ban. This decreasing effect of treatment on the outcome has a p–value of 0.000, suggesting this result is statistically significant and indicating there is a small negative effect of state abortion bans on home value. The full results of the difference–in–difference regression are outlined in Table 4 below.

Table 4: Results of Difference–in-Differences Regression

	Coefficients	Robust Standard Error	T–Statistic	P–Value	95% Confidence Interval
Treatment Group	-0.123***	(0.000)	-1.9e+12	0.000	[-0.1230356, -0.1230356]
Post-Treatment Period	0.231*	(0.012)	19.08	0.033	[0.0771053, 0.3844224]
Interaction Term	-0.002***	(0.000)	-2.2e+10	0.000	[-0.0016758, -0.0016758]
Constant	12.653***	(0.001)	1.2e+04	0.000	[12.64014, 12.66592]
N	2059				
R^2	0.958				

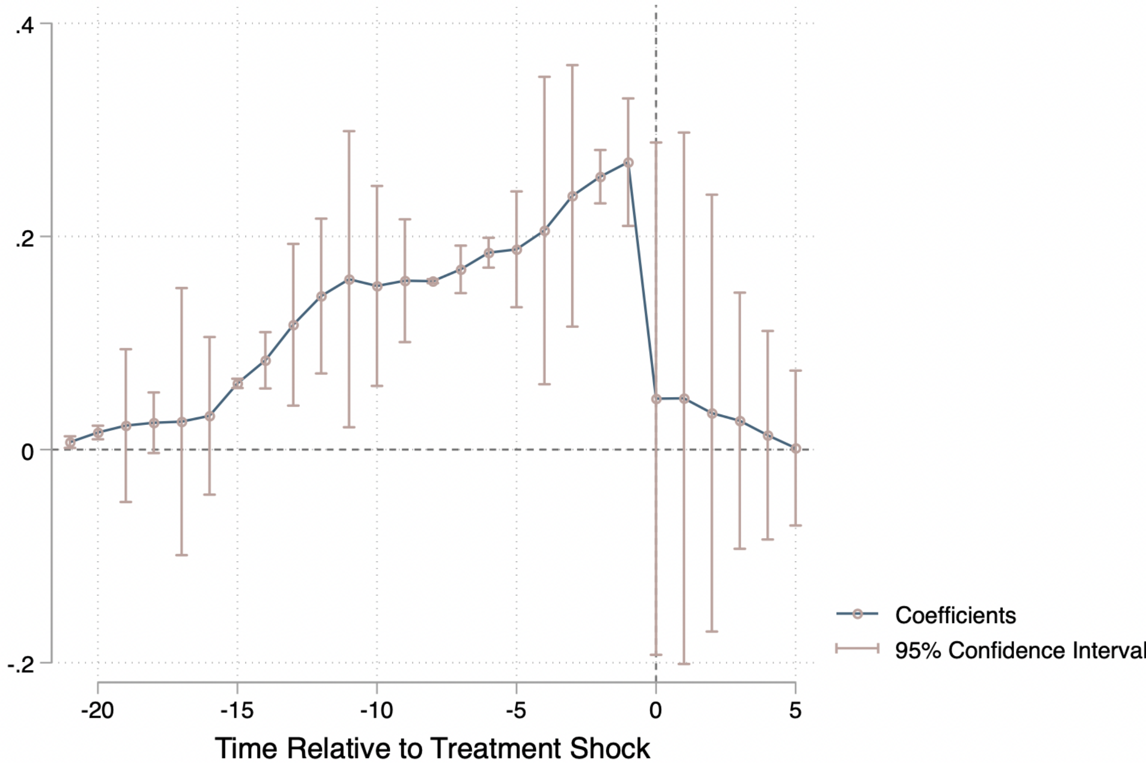
Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

It is important to note that the standard errors clustered by state make use of few clusters (two). Therefore, I employ the boottest method to bootstrap the clustered standard errors, yielding $t = -2.661e{+}14$ with $p = 0.0000$ and confidence interval $[-0.00168, -0.00168]$ when the null hypothesis $H_0 : \delta = 0$ is imposed. In comparison to when the null hypothesis is not imposed, the boottest method yields $t = -3.968e{+}14$ with $p = 0.0000$ and confidence interval $[-0.00168, -0.00168]$. This indicates the coefficient on the interaction term is statistically significant. Thus, the difference–in–difference estimator from the regression holds. Therefore, the results support rejecting the null hypothesis $H_0 : \delta = 0$ for the alternative hypothesis $H_A : \delta \neq 0$ and failing to reject the null hypothesis $H_0 : \delta < 0$. Additionally, a diagram of the event study in Figure 4 below depicts the confidence intervals over observations, demonstrating a consistent downward trend in Texas home prices post–treatment shock as a result of the treatment.

In other words, the implications of this causal effect, as demonstrated in the event study diagram in Figure 4 below, shows this conclusion adds to the understanding of the economic impact of state policies on individual civil liberties, in this case, abortion services. The results provide an initial quantitative measure of the potential economic impact of restrictive abortion policies on a state's housing market. This research helps elucidate some of the previously less understood effects of policy changes in the area of civil liberties on economic metrics, contributing to a more comprehensive evaluation of such policy implementations.

Figure 4: Event Study Diagram of Coefficients Relative to Time of Treatment Shock



6 Robustness

6.1 Placebo Test with Fake Treatment Period

I use a placebo test with a fake treatment period defined from the data's pre–treatment observations. The result of the placebo difference–in–difference regression yields ATE $\delta = 0.004$ with $p = 0.000$. That is, the results suggest a small positive ATE on average home prices for the placebo treatment period. However, bootstrapping for few clusters with boottest yields $t = 1684.5$ with $p = 0.5000$ and confidence interval $[-0.00447, 0.01882]$ when the null hypothesis $H_0 : \delta \neq 0$ is imposed. Whereas, it yields $t = 1684.451$ with $p = 0.0000$ and confidence interval $[0.00381, 0.00382]$ when the null hypothesis is not imposed. This indicates the coefficient on the interaction term is not significantly different from zero. Thus, the placebo test holds for the fake treatment period.

Table 5: Results for Placebo Test with Fake Treatment Period

	Coefficient	Robust Standard Error	T–Statistic	P–Value	95% Confidence Interval
Treatment Group	-0.121***	(0.000)	-9.8e+04	0.000	[-0.1205828, -0.1205514]
Post-Treatment Period	0.268*	(0.005)	48.90	0.013	[0.1980818, 0.337145]
Interaction Term	0.004***	(0.000)	1684.45	0.000	[0.0037834, 0.0038409]
Constant	12.663***	(0.001)	9944.51	0.000	[12.64652, 12.67888]
N	1562				
R^2	0.965				

Robust standard errors in parentheses

* p <0.05, ** p <0.01, *** p <0.001

6.2 Unbiased Estimator

While I confirm that the estimator for the interaction term, δ, is an unbiased estimator since $\mathbb{E}[\hat{\delta}] = \delta$, the sum of errors $\mathbb{E}[\varepsilon_i] = 0.08678$ indicates there is omitted variable bias present in the model.

Table 6: Test for Bias Estimator with Inclusion of Control for Active Listings

	Pre-Treatment	Post-Treatment	Difference in Treatment Period
Texas	12.89665	13.02865	0.132
New Mexico	12.73077	12.86444	0.13367
Difference in Treatment Groups	0.16588	0.16421	-0.00167
$\alpha = 12.60461$	$\beta = 0.132$	$\gamma = 0.16421$	$\delta = -0.00167$
$\varepsilon_0^T = 0.16004$	$\varepsilon_1^T = -0.1295$	$\varepsilon_0^C = -0.03805$	$\varepsilon_1^C = 0.09562$
$\mathbb{E}[\varepsilon_i] = 0.0867775$			

6.3 Reducing Omitted Variable Bias

By including a control for active monthly listings, the results of the difference–in–difference regression vary from the primary model in this study in that it shows to be more precise. The inclusion of a control for monthly active listings eliminates most of the omitted variable bias that was present in the prior model. The results for the updated model with the inclusion of this control is displayed below in Table 7.

Table 7: Difference–in-Differences Results with Control for Monthly Active Listings

	Coefficients	Robust Standard Error	T–Statistic	P–Value	95% Confidence Interval
Treatment Group	-0.12272**	(0.00021)	-579.42	0.001	[-0.12541, -0.120030]
Post-Treatment Period	0.23163	(0.01127)	20.55	0.031	[0.08843, 0.37483]
Interaction Term	-0.00329	(0.00108)	-3.03	0.203	[-0.01705, 0.01049]
Monthly Listing Count	0.00001	(0.00000)	1.48	0.378	[-0.00004, 0.00005]
Constant	12.651***	(0.00109)	0.00012	0.000	[12.637, 12.664]
N	2059				
R^2	0.9584				

Robust standard errors in parentheses

* p <0.05, ** p <0.01, *** p <0.001

The correct p–value for the coefficient of the interaction term δ is correctly determined after bootstrapping clustered standard errors with boottest. The correct significance results for the regression are $t = -3.0301$ with $p = 0.0000$ and 95% confidence interval $[-0.01342, -0.002321]$ when the null is imposed; and, $t = -3.0301$ with $p = 0.0000$ and 95% confidence interval $[-0.00486, -0.001707]$ when the null is not imposed. Therefore, contrary to the initial regression results, the coefficient on the interaction term is statistically significant.

Verifying the inclusion of a control variable for monthly active listings reduces omitted variable bias, the expected value for the error term is much closer to zero than with the prior model without the control variable, as outlined in Table 8.

Table 8: Improvements of Estimator Bias with Inclusion of Control for Active Listings

	Pre-Treatment	Post-Treatment	Difference in Treatment Period
Texas	12.89665	13.02865	0.132
New Mexico	12.73077	12.86444	0.13367
Difference in Treatment Groups	0.16588	0.16421	-0.00167
$\alpha = 12.65063$	$\beta = 0.2316303$	$\gamma = 0.2316303$	$\delta = -0.0032835$
$\varepsilon_0^T = 0.0143897$	$\varepsilon_1^T = -0.0819571$	$\varepsilon_0^C = -0.1514903$	$\varepsilon_1^C = -0.0178203$
$\mathbb{E}[\varepsilon_i] = -0.0592195$			

7 Conclusion

In conclusion, the purpose of this research is to explore the impact of statewide prohibitions on abortion on residential property values, by analyzing the migratory patterns of individuals between states. The study employs data pertaining to the volume of active home listings and the average property prices at the county level in New Mexico and Texas. These states were chosen due to the contrast in their abortion laws and their geographic proximity. The data, which covers the period from July 2020 to November 2022, is analyzed using a difference–in–differences econometric approach, integrating controls for both state and time–fixed effects.

The findings suggest that on average, property prices in Texas are appraised at a value 0.2% lower than those in New Mexico, which can be attributed to the implementation of an abortion ban in Texas, and this difference is statistically significant. Furthermore, the study detects a correlation between the implementation of the abortion ban and an increase in the number of active property listings in Texas. To ensure the robustness of these results, several tests were conducted including a placebo test, analysis of estimator bias, and an additional model incorporating controls for the number of active home listings. This additional analysis suggests that properties in Texas are actually valued 0.3% less than those in New Mexico, due to the introduction of the abortion ban in Texas.

Potential improvements to this research could encompass the replication of this experiment using propensity score matching for county–level average property price data. This approach would serve to improve the sample by incorporating a control group that more accurately represents a counterfactual to the treatment group. Additionally, a replication of this study should make use of individual–level data on property values within close proximity of the shared border between the states, randomly sampled from New Mexico and Texas, to enhance the exogeneity of treatment groups by providing a more random sample compared to using aggregate county-level data for average property prices, as was done in this research. Moreover, a larger quantity of post–treatment data should be included in future iterations of this research. Alternatively, the methodological framework of this research could be improved through the adoption of a different empirical strategy. Specifically, synthetic difference–in–differences could be employed to construct a "synthetic Texas" to serve as a counterfactual, leveraging individual–level data from randomly selected properties listed for sale across all of Texas. Finally, the implementation of additional control tests in the difference–in–differences framework could serve to further reduce additional estimator bias.

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