Migration Responses to Abortion Policy Changes[*]

Teresa Molina[†] Nicole Siegal[‡]

May 22, 2025

Preliminary Draft (Click here for most recent version)

Abstract

In this paper, we examine whether and how migration decisions respond to state-level changes in abortion policy in the United States. Using data from Guttmacher Institute and the American Community Survey for information on gestational age limits and interstate migration from 2006-2019, we estimate a gravity model of migration. We predict bilateral migration flows using gestational age restrictions in the origin and destination states, a variety of economic, demographic, and political control variables for both states, as well as state-pair and year fixed effects. While out-migration does not respond to gestational age restrictions, in-migration does: individuals are significantly less likely to move to states that implement a 20-week gestational age limit (the most restrictive policy in our study period). Heterogeneity analysis reveals similar effects for men and women, suggesting these effects are driven by ideological preferences, not just the potential future need for an abortion. Results are robust to the use of Nagengast and Yotov (2025) methods that account for heterogeneous treatment effects with staggered treatment adoption in non-linear models.

[*]We thank seminar participants at UH Manoa and SEA Annual Meeting for helpful feedback.

[†]University of Hawaii at Manoa, IZA, UHERO; tmolina@hawaii.edu

[‡]Indiana University

1 Introduction

There is a large theoretical and empirical literature on the phenomenon of 'voting with one's feet,' or the idea that people will choose to live in communities where public policies best satisfy their preferences (Tiebout, 1956). Existing work shows that individuals migrate in response to welfare benefits (Agersnap et al., 2020; Fiva, 2009; Gelbach, 2004), crime (Cullen and Levitt, 1999), environmental factors (Banzhaf and Walsh, 2008; Noy, 2017), and political ideology (Bove et al., 2023; Bracco et al., 2018; Brox and Krieger, 2021; Efthyvoulou et al., 2023). One type of policy that has received little attention in this literature, however, is abortion policy.

Abortion restrictions have been at the forefront of U.S. political discourse in recent years (Belluck, 2024; Dura, 2024; Krieg, 2003). There are strong opinions on both sides of the debate (Leonhardt, 2023; Pew Research Center, 2022). There is also substantial evidence, reviewed in Clarke (2024), that these restrictions have meaningful effects on women's lives. In addition to affecting decisions and outcomes related to contraception and fertility (Fischer et al., 2017; Haas-Wilson, 1996; Levine et al., 1996; Lindo and Pineda-Torres, 2021; Lindo et al., 2019), access to abortion affects women's long-run health and economic outcomes (Londoño-Vélez and Saravia, 2025; Miller et al., 2023). Given this – the divisive nature of the abortion issue combined with the large effects of restricting abortion on women's lives – abortion restrictions could serve as an important impetus for people to vote with their feet. Indeed, Dench et al. (2025) find increases in net migration from states with abortion bans following the 2022 Dobbs Supreme Court decision in the United States.

In this paper, we examine less extreme changes in abortion policy in the United States prior to the Dobbs decision. We ask whether and how migration decisions respond to state-level changes in gestational age limits, usually expressed as a number of weeks of gestation after which an abortion cannot legally be performed. Though less extreme than the abortion bans studied in Dench et al. (2025), gestational age limits are still consequential laws that have been found to be associated with higher infant and maternal mortality (Hawkins et al., 2020; Karletsos et al., 2021).

Broadly, there are two main reasons why abortion restrictions might affect individuals' location choices. First, women of child-bearing age who think they may need an abortion at some point in the future may choose to leave a state (or else choose not to move to a state) with strict restrictions on abortion. Other work has shown that women do indeed change their behavior based on expectations about future exposure to abortion policy: Pennington and Venator (2024) document that the take-up of more effective contraceptive methods increase in response to expected and actual increases in abortion policy restrictiveness. Another possibility is that people derive utility from living in a state whose official policies align with their religious or ideological views. In our setting, this would lead those with anti-abortion views to avoid states with strict abortion restrictions (by either moving away from one or choosing not to move to one), and those with pro-abortion views to do the opposite.

To investigate this question empirically, we use data from Guttmacher Institute and the American Community Survey to measure restrictiveness of abortion policies and interstate migration, respectively. Our main dataset includes all destination-origin state pairs from 2006-2019. We use the PPML estimator to estimate a gravity model of interstate migration, where the number of movers between states is expressed as a function of gestational age limits in the origin and destination states, a variety of economic, demographic, and political control variables for both states, as well as state-pair and year fixed effects. The structure of our data and our empirical approach provide two main advantages over existing work: we are able to separately identify effects on in-migration and out-migration, as well as heterogeneity based on characteristics like age and gender, which helps shed light on mechanisms.

Our results show that when a state implements a 20 week gestational age limit, the strictest policy in our study period, migration to that state is significantly reduced. 24-week limits have no significant effects, and neither of these policies have significant effects on out-migration. Results are similar whether we use a standard two-way fixed effects (TWFE) approach and the Nagengast and Yotov (2025) extended two-way fixed effects (ETWFE) methods, though estimates are slightly larger using the latter method, which is robust to heterogeneous treatment effects. Event study analysis reveals lower migration in the several years after a state implements a 20-week limit, without significant changes in migration in the years leading up to the policy change, implying reverse causality is unlikely to be a concern.

Interestingly, we find that migration responses are similar for men and women. While this could simply be an indication that men and women often move together in couples or families, this also points to ideological preferences (as opposed to the actual need for an abortion) being an important mechanism behind our results. Consistent with this, we find significant effects for women over 50 years old. Although other researchers have documented migration responses to politically contentious issues like immigration and same-sex marriage, effects in these studies are primarily driven by people directly affected by the policies in question: immigrants are less likely to move to Italian municipalities with mayors associated with anti-immigration parties (Bracco et al., 2018) and gay men are more likely to move to U.S. states that legalized same-sex marriage (Marcén and Morales, 2022). Unlike these papers, we find evidence of migration responses from people not directly affected by the abortion policies in question. This is consistent with the hypothesis that the tendency to migrate towards politically aligned areas can be motivated by a sense of belonging and fitting in (Efthyvoulou et al., 2023). These findings suggest pro-choice preferences among the movers driving our results.

Given the rich set of controls we include, along with the clear pattern of the event study coefficients, we argue that the changes in migration we document are being driven by abortion policy specifically and not other (potentially correlated) drivers of migration, like economic conditions or shifts in the demographic composition of a state. However, we cannot rule out the possibility that these migration responses are, to some extent, being driven by other unobserved changes that happen to coincide with changes in abortion policy – for example, the overall political climate of a state (to the extent that this is not adequately captured by our vote share controls). That said, it is still notable that changes in a state's political or cultural environment that either drive or take place alongside more restrictive abortion policy significantly reduce the number of people willing to migrate to that state.

2 Data

2.1 Abortion Policy Data

The abortion policy data were provided by Guttmacher Institute. Upon request, they provided state-level abortion policies from 2006-2022 in their series of data tables titled "Overview of Abortion Laws." The tables included each state's gestational age limit, the latest week of gestation at which an abortion can legally be performed under normal circumstances, as of January 1st of each year. Because policies that are in effect on the first day of the year were typically signed into law several months prior, we record each policy's implementation year as the year before it first shows up in the Guttmacher tables. Gestational age limits were recorded either as a specific week of 20 or 24 weeks, the third trimester, or "at viability." We group together laws which do not include a specified week or limit (viability, third trimester, and no stated policy) as 28 weeks, given that these are in practice viewed as less restrictive than a policy specifying a 20 or 24 week limit.[1]

2.2 Migration Data

Migration data were compiled from the American Community Survey (ACS), collected by the United States Census Bureau and provided by IPUMS USA. Individuals who moved in the past year were identified using the Census question, "Did this person live in this house or apartment 1 year ago?", with those who responded "No, different house in the United States or Puerto Rico" identified as movers. The follow up question for movers asked for the address one year ago, and the Census provided a variable noting the state code of residence the year prior. Interstate movers were identified as those who moved and whose current state and

[1] Viability has been a contentious term given the variability in fetal development between pregnancies. The American College of Obstetricians and Gynecologists and the Society for Maternal–Fetal Medicine (2017)'s guidelines to interpret "viability" detail how a fetus is typically considered viable between 20-25 weeks, but some as late as 28 weeks.

prior-year state codes were different. This individual level data was weighted using the person-weight value given, and then collapsed to the origin-destination state pair level. Finally, this was merged with the policy information and control variables for both the destination and origin states.

2.3 Summary Statistics

Table 1: Summary Statistics

	(1) Pair-Year	(2) Pair-Year (Main Sample)	(3) State-Year
Movers between states	2950.23	2955.06	
	(5841.60)	(5845.16)	
Movers between states / Origin state pop.	5.86	5.87	
	(10.73)	(10.74)	
Gestational age limit = 20 weeks			0.18
			(0.38)
Gestational age limit = 24 weeks			0.14
			(0.35)
Female proportion			0.51
			(0.01)
White proportion			0.80
			(0.12)
Black proportion			0.11
			(0.10)
AI and Alaskan native proportion			0.02
			(0.03)
Asian proportion			0.04
			(0.06)
NHPI proportion			0.00
			(0.01)
House of Reps. Democratic two-party vote share			0.47
			(0.12)
House of Reps. Republican two-party vote share			0.49
			(0.12)
Labor force participation rate			0.49
			(0.07)
Unemployment rate			0.06
			(0.02)
Observations	34258	34202	700

Notes: Column 1 and 2 use state-pair observations from 2006 to 2019. Column 2 drops all state pairs with zero movers throughout this time period. Column 3 uses state-year observations.

Summary statistics are presented in Table 1, with the first two columns reporting variables that vary at the pair-year level and the third column reporting variables that vary at the state-year level. Column 1 uses the full population of pair-year observations from 2006 to 2019, while column 2 drops state pairs with zero movers throughout the entire study period (as these are dropped from the PPML regressions described in the next section). In this analysis sample, an average of 2,955 people moved from one state to another each year, which corresponds to an average rate of 5.87 movers per 10,000 residents in the origin state. 18% of state-year observations prohibited abortions at 20 weeks, while an additional 14% prohibited at 24 weeks, with the remaining observations not specifying an exact numerical limit. The remainder of Table 1 reports summary statistics for state-level characteristics, which are taken from the U.S. Census Bureau's State Characteristics Population Estimates, Bureau of Labor Statistics' Local Area Unemployment Statistics, and MIT Election Data and Science Lab.

Following the 1973 Roe vs. Wade Supreme Court decision to protect abortion rights in all 50 states, many states saw an expansion of laws that began to erode this right to access by adding additional requirements. These requirements included waiting periods, restrictions on which physicians can perform the procedure or prescribe oral abortion medication, as well as gestational age limits, the focus of this paper.

Gestational age limits tightened over our study period. As shown in Figure 1, the change in state policies varied across the nation, with the most common change being a shift from having no specified limit to the strictest 20 weeks limit. In 2006, North Carolina was the only state to have a gestational age limit of 20 weeks. By 2019, 19 states restricted at 20 weeks. During the period of 2006-2019, there were very few states that increased the gestational age limit. Interestingly, North Carolina was the only state that removed its 20-week limit over the study period, relaxing it in 2019 (and reinstating it in 2022). Year-by-year changes are depicted in Figure 2. The share of states with a 20-week limit began to increase in 2011 and continued to do so until the end of the study period. The share of states with no specified limit decreased substantially, while the share with a 24-week limit decreased slightly.

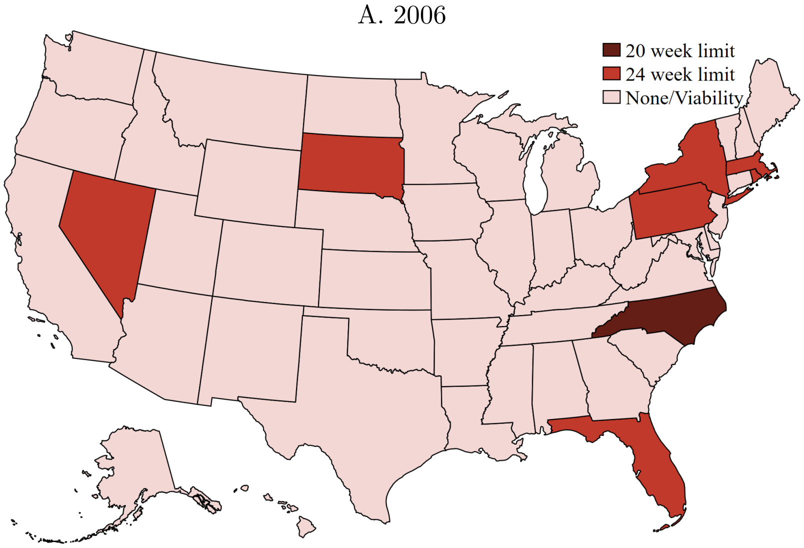
3 Empirical Strategy

To estimate the relationship between gestational age limits and interstate migration, we begin with a standard gravity model of migration, which can be derived from a random utility model where individuals choose whether to migrate based on a comparison of location-specific utilities (Beine et al., 2015). Following recommendations from the existing literature on gravity models (Larch and Shikher, 2025), we use the PPML estimator to estimate

$$M_{jkt} = \exp\{\beta_1 \text{OriginRestrictions}_{jt} + \beta_2 \text{DestinationRestrictions}_{kt} + \alpha X_{jt} + \delta Y_{kt} + \gamma_{jk} + \lambda_t\} \times \epsilon_{jkt}, \quad (1)$$

Figure 1: Geographic distribution of gestational age limits in 2006 and 2019

A. 2006



B. 2019

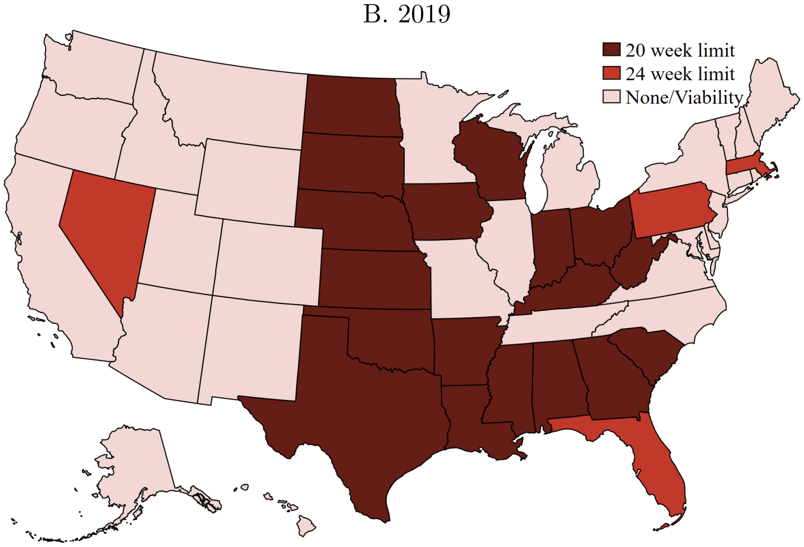
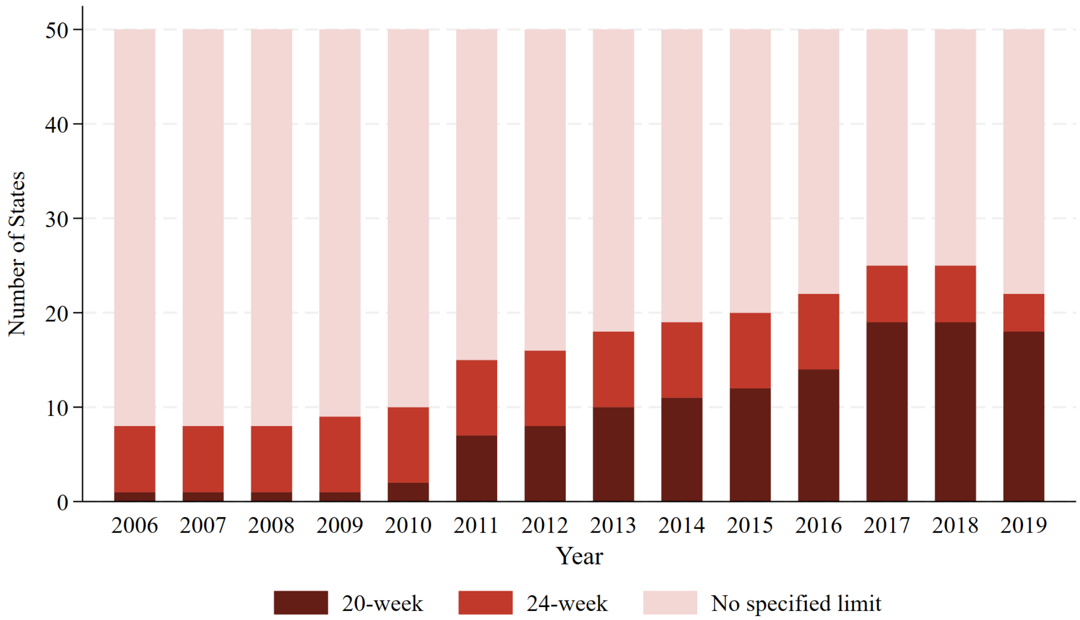


Figure 2: Distribution of gestational age limits over time



where M_{jkt} represents the number of movers from state j to state k, as a fraction of the origin state j's population, in year t. $\text{OriginRestrictions}_{jt}$ and $\text{DestinationRestrictions}_{kt}$ capture origin and destination state abortion policies. Because states are coded into one of three gestational age limit categories (24 weeks, 20 weeks, or no specified limit), we use dummy variables for a 24-week and 20-week limit (in the origin and destination state), leaving states with no specified limit as the omitted category. State pair fixed effects (γ_{jk}) account for any time-invariant determinants of migration between two states (like geographic distance). λ_t controls for common time trends across state-pairs. Standard errors are clustered at the state-pair level.

Because our independent variables of interest vary either at the origin-year or destination-year level, we are unable to include origin-time or destination-time fixed effects (commonly used in trade and migration gravity models) in the regression above. Therefore, it is important to control for time-varying characteristics of the origin and destination states that could be driving migration: X_{jt} and Y_{kt} represent shares of each race, Democratic and Republic House of Representative vote shares, unemployment rates, and labor force participation rates in the origin and destination states.

To explore heterogeneity by age and gender, we repeat the analysis described above, using separate regressions for migration rates calculated for specific groups: males and females under 18, males and females between 18-30, males and females between 31-50, and males and females over 50.

Equation (1) takes the form of a two-way fixed effects (TWFE) regression with a staggered treatment that begins in different areas in different time periods. A rapidly growing literature has highlighted that the coefficients from such regressions can be difficult to interpret when there exist heterogeneous treatment effects over time and across areas that were treated at different times, and several papers have proposed solutions for linear models (Borusyak et al., 2022; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). More recent work has proposed solutions for non-linear models, including Nagengast and Yotov (2025), who apply methods from Wooldridge (2023) to a gravity model of international trade. Therefore, to account for potential heterogeneous treatment effects in our setting, we also use the extended two-way fixed effects (ETWFE) methods outlined by Nagengast and Yotov (2025).

To apply the ETWFE methods, we focus on the destination 20-week gestational age limit, the only policy variable for which we document significant effects on migration. To estimate the effects of a destination state's 20-week gestational age limit while accounting for the possibility of heterogeneous treatment effects, we begin by estimating a PPML regression that replaces this policy variable with a separate set of dummy variables for each cohort g, defined as a set of states treated in the same year s. Defining D_{gt} as a dummy equal to 1 for pair-year observations where the destination state belongs to cohort g and the year t is on or after the state's first treatment year s, we estimate

$$M_{jkt} = \exp\{\sum_{g=q}^{T}\sum_{s=g}^{T}\beta_{gs}D_{gs} + \alpha X_{jt} + \delta Y_{kt} + \gamma_{jk} + \lambda_t\} \times \epsilon_{jkt}. \tag{2}$$

Following Nagengast and Yotov (2025), this analysis drops always-treated states (North Carolina) and uses both never-treated and not-yet-treated states as controls.[2] The estimated β_{gs} coefficients can then be aggregated to a single β_{ETWFE} using the following formula:

$$\beta_{ETWFE} = \sum_{g=q}^{T}\sum_{s=g}^{T}\frac{N_{gs}}{N_D}\beta_{gs}, \tag{3}$$

where N_D represents the total number of treated pair-year observations. We can also estimate separate coefficients for each relative year (i.e., the year of, the year after, and two years after the policy change) and generate event study plots to demonstrate how the treatment effects change over time.

This method relies on the parallel trends assumption, where trends are defined as the ratio of outcomes in different periods (as opposed to the difference, as in the more common linear setting) . Therefore, a causal interpretation requires that, conditional on the fixed effects and controls, the growth in outcomes in the treated group would have been the same as the growth in outcomes in the control group (not-yet-treated and never-treated states), had the policy not been implemented. Another important assumption is the "no anticipation" assumption: the policy cannot have an effect before its implementation. Because some

[2]North Carolina was not technically treated for the entire study period, but it was treated at the beginning of the study period and remained treated until the very last year.

gestational age limit laws may have received media attention even before they were signed into law, we also estimate specifications that shift the treatment implementation back by one year.

4 Results

4.1 Baseline Results

Table 2: Migration responses to gestational age limits

	(1)	(2)	(3)	(4)
Origin 24-Week Limit	0.0056	0.023		
	(0.038)	(0.038)		
Destination 24-Week Limit	0.059	0.046		
	(0.039)	(0.037)		
Origin 20-Week Limit	0.019	-0.018	-0.019	
	(0.018)	(0.017)	(0.018)	
Destination 20-Week Limit	-0.074***	-0.067***	-0.067***	-0.067***
	(0.022)	(0.019)	(0.019)	(0.017)
Observations	34,202	34,202	34,202	34,202
Dep. var mean	5.87	5.87	5.87	5.87
Controls	None	Yes	Yes	Yes
Additional Fixed Effects	None	None	None	Origin-Year

Notes: Standard errors (clustered at the state pair level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use bilateral migrants divided by the total origin population as the dependent variable and control for state-pair fixed effects and year fixed effects. "Controls" include racial composition shares, Republican and Democrat vote shares, unemployment rates, and labor force participation rates in the origin and destination states.

In Table 2, we report the results of equation (1), which includes dummies for origin and destination states having 24-week and 20-week gestational age limits. Column 1 reports results without any additional controls. Migration does not appear to be affected by 24-week limits in either the origin or destination state. 20-week limits, on the other hand, significantly reduce in-migration, as evidenced by the negative and statistically significant coefficient on the destination 20-week limit dummy. When we add demographic, economic, and political controls in column 2, which are important given that we are unable to control for origin-year or destination-year fixed effects in this specification, our conclusions largely remain unchanged. Given that 24-week limits do not appear to be affecting in-migration or out-migration, we restrict our focus to 20-week limits for the remainder of the paper. In column 3, we drop the 24-week dummies from the regression and confirm that the coefficient estimates for the 20-week dummies remain unchanged.

Finally, we explore the inclusion of origin-year dummies. As mentioned above, origin-year and destination-year fixed effects are commonly used in gravity models of trade and migration, but we exclude them from our main specification because our key policy variables vary at either the origin-year or destination-year level. However, because we do not find any significant effects of origin state abortion policy, we estimate regressions that drop origin state policy and instead control for origin-year fixed effects. In column 4, the coefficient on the destination policy dummy is identical to those estimated in columns 3 and 4. Across all columns, a destination state 20-week limit is estimated to reduce in-migration by approximately 7%.

Table 3: Migration responses to gestational age limits, by gender and age

	(1)	(2)	(3)
	Male	Female	Difference
A. Ages 0-17			
Destination 20-Week Limit	-0.083**	-0.10**	-0.018
	(0.039)	(0.040)	(0.043)
Observations	31,654	31,556	
Dep. var mean	5.27	5.25	
B. Ages 18-30			
Destination 20-Week Limit	-0.063***	-0.050**	0.012
	(0.023)	(0.025)	(0.030)
Observations	33,558	33,348	
Dep. var mean	12.92	12.95	
C. Ages 31-50			
Destination 20-Week Limit	-0.020	-0.020	0.00052
	(0.030)	(0.028)	(0.029)
Observations	32,802	32,690	
Dep. var mean	6.19	5.36	
D. Ages 51+			
Destination 20-Week Limit	-0.073	-0.082**	-0.0087
	(0.053)	(0.039)	(0.037)
Observations	31,738	32,326	
Dep. var mean	3.30	3.27	
Additional Fixed Effects	Origin-Year	Origin-Year	Origin-Year

Notes: Standard errors (clustered at the state pair level) are in parentheses * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. All regressions use bilateral migrants divided by the total origin population as the dependent variable and control for state-pair fixed effects, origin-year fixed effects, as well as racial composition shares, Republican and Democrat vote shares, unemployment rates, and labor force participation rates in the destination state.

Table 3 explores heterogeneity by age group and gender. We repeat our regression in column 4 of Table 2 using migration rates calculated for different age and gender categories. In each panel, we report results for males in column 1, females in column 2, and the difference between the two coefficients in column 3. There are no statistically significant differences between males and females in any age group. This is not what we would expect if these migration responses were purely driven by women anticipating the potential need for an abortion in the future, though still possible if men and women typically move together. Our results appear to be driven by those aged 30 or younger (panels A and B), which could be a result of young families (adults under 30 and their children) making migration decisions as a unit. Interestingly, however, we also find large and statistically significant effects for women over the age of 50, and the estimate for men in this age group is of a similar magnitude and not significantly different from the women's estimate. This suggests that ideological preferences, not just the future need for an abortion, could be an important driver of these migration responses.

4.2 ETWFE Results

As described above, the baseline results in the previous tables may not have a straightforward interpretation if there are heterogeneous treatment effects over time or across states that implemented a 20-week gestational age limit in different years. We therefore also use the Nagengast and Yotov (2025) ETWFE methods to generate heterogeneity-robust estimates of the policy effects.

In Table 4, we first repeat our regression from column 4 of Table 2, dropping the origin policy dummy. We then show that dropping the single always-treated state (North Carolina), which will be dropped from the ETWFE regressions, yields almost identical results. In column 3, we report the ETWFE estimate β_{ETWFE}, which is slightly larger than the coefficient estimate from the standard TWFE regression. In the final column, as a sensitivity test, because this method relies on the no anticipation assumption, we shift back the treatment date by one year to allow for one year of anticipation. The coefficient estimate is similar though slightly smaller than the one in column 3. Having reported the aggregated treatment effects, we now move on the event study plot in Figure 3, using the specification in column 3 of Table 4. Effects are immediate and fairly consistent from year 0 to 5 since treatment onset.

Finally, we investigate the extent to which treated states may have experienced different migration trends prior to the implementation of a 20-week age limit. We do this by estimating equation (3) using only untreated observations (i.e., pre-treatment observations for states that eventually implement a 20-week gestational age limit, as well as all observations for never-treated states). We then aggregate the cohort-year specific estimates for each relative year prior to the implementation of the ban, which provides us with a set of pre-treatment coefficient estimates. These capture the difference between not-yet-treated and never-treated

Table 4: Migration responses to gestational age limits (ETWFE)

	(1)	(2)	(3)	(4)
Destination 20-Week Limit	-0.067***	-0.074***	-0.097***	-0.081***
	(0.017)	(0.018)	(0.021)	(0.019)
Observations	34202	33516	33516	33516
Dep. var mean	5.87	5.78	5.78	5.78
Additional Fixed Effects	Origin-Year	Origin-Year	Origin-Year	Origin-Year
Specification	Baseline TWFE	Drop Always-Treated	ETWFE	ETWFE (allowing for anticipation)

Notes: Standard errors (clustered at the state pair level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions use bilateral migrants divided by the total origin population as the dependent variable and control for state-pair fixed effects, origin-year fixed effects, as well as racial composition shares, Republican and Democrat vote shares, unemployment rates, and labor force participation rates in the destination state.

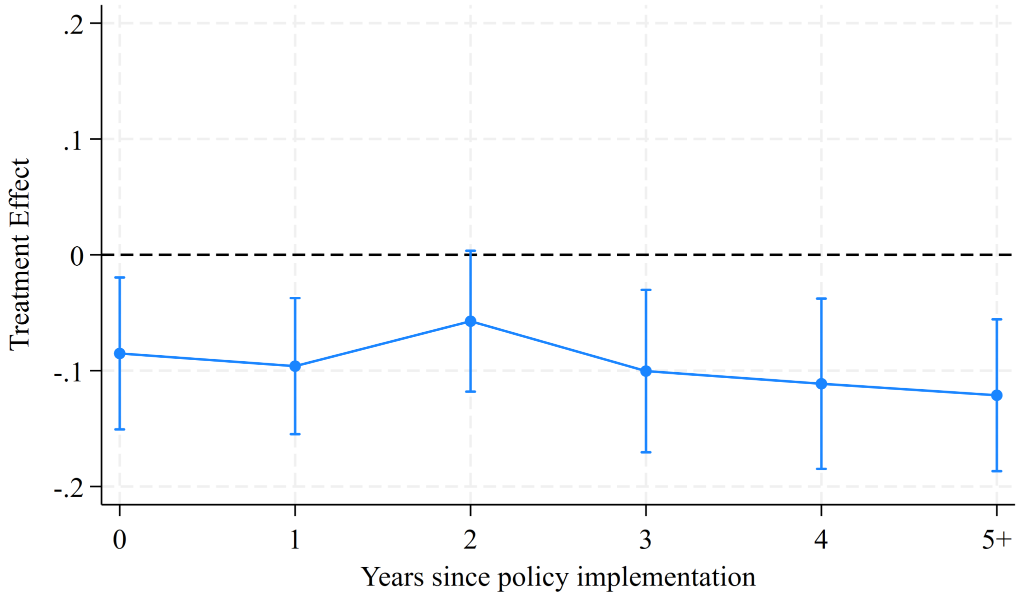
states in the years before the implementation of a 20-week gestational age limit. If our identifying assumption holds – that is, if the change in migration in the treated and untreated states would have been the same in the absence of the policy – then we would expect to see no significant coefficients in the pre-period, which is indeed what Figure 4 shows.

5 Conclusion

In this paper, we examined whether and how interstate migration decisions respond to state-level changes in abortion policy in the United States. Using various specifications that examine the relationship between the number of movers between states (as a fraction of origin state population) onto both states' gestational age limit policies and a variety of control variables and fixed effects, we consistently find that individuals are less likely to move to states with more restrictive policies.

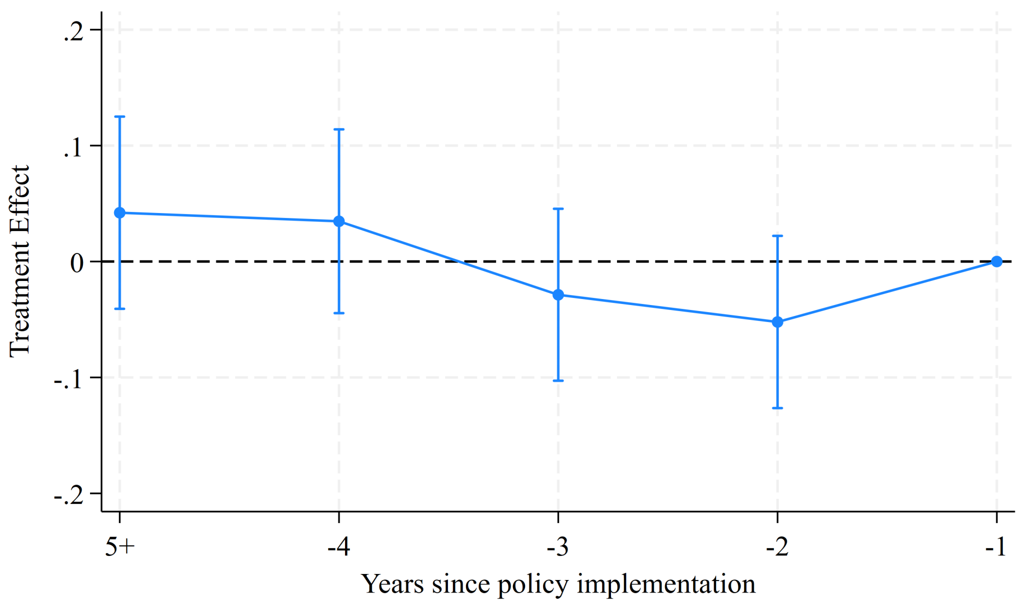
Our results reveal lower migration to states with a 20-week gestational age limit, the most restrictive policy in our study period. Estimates of pre-trends show that migration was not trending differently in states that were about to implement a 20-week limit, which is consistent with the idea that these policies are what caused migration patterns to change. Because abortion policies are not randomly assigned, however, it is difficult to attribute all of our estimated effects to the causal effect of gestational age limits specifically. Nevertheless, it is clear that (unobservable) changes in a state's political or cultural environment that drive or take place alongside more restrictive abortion policy significantly reduce migration to that state. Because our results are not only driven by women who might value having less restrictions on abortion access in the

Figure 3: Event-Time Specific Treatment Effects



Notes: Figure plots event-time specific treatment effect estimates and 95% confidence intervals (using standard errors clustered at the state pair level). All regressions use bilateral migrants divided by the total origin population as the dependent variable and control for state-pair fixed effects, origin-year fixed effects, as well as racial composition shares, Republican and Democrat vote shares, unemployment rates, and labor force participation rates in the destination state.

Figure 4: Pre-Trend Estimates



Notes: Figure plots event-time specific pre-trend effect estimates and 95% confidence intervals (using standard errors clustered at the state pair level). All regressions use bilateral migrants divided by the total origin population as the dependent variable and control for state-pair fixed effects, origin-year fixed effects, as well as racial composition shares, Republican and Democrat vote shares, unemployment rates, and labor force participation rates in the destination state.

future, this suggest pro-choice preferences are at least partially driving the estimated effects.

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