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Migration Responses to State Abortion Policy

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Abstract

This paper examines whether and how migration decisions respond to state-level changes in abortion policy in the United States. Using information on gestational age limit abortion restrictions 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, and large effects for women past reproductive age, suggesting these effects are driven at least in part by ideological preferences, not just the potential future need for an abortion. Results are robust to the use of an extended two-way fixed effects (ETWFE) estimator that accounts for heterogeneous treatment effects with staggered treatment adoption in non-linear models.

JEL classification

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Keywords

abortion restrictions, gestational age limits, interstate migration

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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 the political environment (Bove et al., 2023; Bracco et al., 2018; Brox and Krieger, 2021; Downey and Liu, 2024; 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 decades (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 et al., 2019; Lindo and Pineda-Torres, 2021), 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 and morbidity (Hawkins et al., 2020; Karletsos et al., 2021; Siegal and Soni, 2026).

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 both actual and expected 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 Poisson Pseudo Maximum Likelihood (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. 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 a potentially important mechanism behind our results. Our finding of large and statistically significant effects for women aged 65 and older is consistent with this explanation. Given the sign of our estimated coefficients (reduced migration to more restrictive states), our results appear to be driven by movers (or would-be movers) who support abortion access.

Results are similar whether we use a standard two-way fixed effects (TWFE) approach or the Nagengast and Yotov (2025) extended two-way fixed effects (ETWFE) approach, which is robust to heterogeneous treatment effects. Estimates from the latter method are slightly larger and correspond to an approximately 9% reduction in migration. Event study analysis reveals lower migration in the five years after a state implements a 20-week limit, without significant changes in migration in the years leading up to the policy change. This implies reverse causality is unlikely to be a concern and our identifying assumption – that migration trends would have remained similar in states that did and did not implement a 20-week limit in the absence of this policy change – is plausible.

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. We also show that our results are not driven by other policies or state characteristics that might drive migration, including Medicaid expansions, other types of abortion restrictions, consumer spending, house prices, and gun control.

Our results contribute to the literature in several ways. First, they highlight the far-reaching consequences of abortion policy, expanding on a large literature documenting how abortion policy affects women's lives (Clarke, 2024). Given that residential locations are important for health, human capital, and economic outcomes (Acton et al., 2025; Deryugina and Molitor, 2021; Redding and Rossi-Hansberg, 2017), gestational age limits may have large effects on individual well-being and its geographic distribution through their effect on migration decisions. Importantly, we do not only find migration responses among adults. Children also demonstrate migration responses likely because they are moving with their families, which may have lasting effects given the importance of childhood neighborhoods in shaping adult outcomes (Chetty and Hendren, 2018).

Second, while there are a number of papers that document migration responses to other types of policy, we are among the first to examine migration responses to abortion policy specifically. Using data on state-level migration outflows and inflows, Dench et al. (2025) document increased net out-migration in response to post-Dobbs abortion bans, consistent with our finding that individuals are less likely to move to states with more restrictive policies. One advantage of our data is the availability of origin-destination pair-level migration flows, which allows us to separately examine in-migration and out-migration responses: we detect a reduction in in-migration to more restrictive states but no effects on out-migration. The identification of this precise channel speaks to the exit-voice hypothesis of Hirschman (1970): that in unsatisfactory situations, individuals can choose either to exit from their position (the country, state, employment relationship, etc.) or stay and voice their concerns to try to remedy the situation. In our context, we find no evidence of increased exit in response to greater abortion policy restrictiveness. This raises the question of whether individuals who do not move, but who are dissatisfied with these policies, are instead voicing their concerns through protests or voting.[1] Importantly, the absence of an out-migration response is consistent with the finding that in-migration (as opposed to out-migration) explains most of the variation in internal migration in the United States (Monras, 2018).

Another advantage is our ability to examine separate effects for different gender and age groups, which sheds light on a novel mechanism. As described above, the fact that our results are not only driven by women of reproductive age suggests that ideological preferences may be an important explanation for the findings. 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

[1] In the context of worker-firm relationships, the importance of voice in reducing worker exit appears to be context-specific (Adhvaryu et al., 2026, 2022; Harju et al., 2025).

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).

Finally, these findings contribute to the literature on the relationship between migration and political polarization in the United States. Studies have documented a high degree of residential segregation across partisan lines that has increased over time across the entire country (Brown et al., 2023; Brown and Enos, 2021). Internal migration has contributed to this increase in partisan sorting (Brown et al., 2023; Liu et al., 2019). Our results suggest that state-level policy changes can serve as an impetus for internal migration that can lead to greater partisan segregation as individuals sort into states where government policy aligns with their views. This in turn can lead to greater political polarization as officials cater to electorates with more uniform political preferences.

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.[2]

[2]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.

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 prior-year state codes were different. Table A1 compares the characteristics of interstate movers to non-movers (dropping international migrants). From 2006 to 2019, there were about 7 million movers per year, who are more likely to be in their twenties to forties relative to the non-mover population. Although movers are more likely to have a college degree than non-movers, they are also are more likely to be below the poverty line, unemployed, and uninsured.

These individual level data were 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 for both the destination and origin states. We restrict to years 2006-2019 in order to avoid the COVID-19 pandemic, which drastically changed the nature of migration across the country, and the 2022 Dobbs Supreme Court decision, which resulted in abortion bans taking effect in many states.

2.3 Summary Statistics

Summary statistics are presented in Table A2, 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 2,954 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 A2 reports summary statistics for state-level characteristics that are used as controls: state racial composition variables, Democratic and Republican vote shares, labor force participation, and unemployment. These 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 push back on this 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, which illustrates the distribution of state policies in each year. 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.

As shown in Figure A1, the change in state policies between 2006 and 2019 varied across the nation. 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. The most common change over this time period was a shift from having no specified limit to the strictest 20-week limit. During the period of 2006-2019, there were very few states that relaxed 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).

2.4 Additional Policy Controls

Because our goal is to estimate the causal effects of gestational age limits on inter-state migration, it is important to understand whether changes in gestational age limits tend to be correlated with other policy changes. We therefore draw on a number of other sources of data to obtain information about various state-level policies and characteristics that might also influence migration.

We obtain data on health insurance policy, gun policy, and general measures of economic activity and cost of living. Data on state Medicaid expansion decisions under the Affordable Care Act (ACA) are from the Kaiser Family Foundation (Kaiser Family Foundation, 2025); we include a binary indicator equal to one in years after a state implemented the expansion. State firearm law data are from the RAND State Firearm Law Database (Hoch et al., 2026); we include a count of firearm law categories in effect in each state-year.[3] Per capita personal consumption expenditures are from the Bureau of Economic Analysis (U.S. Bureau of Economic Analysis, 2024), and state-level house prices are measured using the log of the all-transactions house price index from the Federal Housing Finance Agency (Federal Housing Finance Agency, 2026).

We also consider other policies related to reproductive health. Data on targeted regulation of abortion providers (TRAP laws) are from Jones and Pineda-Torres (2024) and Adkins et al. (2024). We create an indicator for state-years that have any one of the following TRAP laws: building standards, distance requirements, transfer agreements, and admitting privileges. We also include a binary indicator for whether a state had a pre-*Dobbs* trigger ban, defined as a law that would automatically ban most abortions upon the

[3] There are a total of 20 firearm law categories, including waiting periods, background checks, and open carry.

overturning of *Roe v. Wade*, using data from the Guttmacher Institute (Nash and Guarnieri, 2022).

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

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

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 (20 weeks, 24 weeks, or no specified limit), we use dummy variables for a 20-week and 24-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 all state-pairs. In this baseline specification, 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 the population shares of each race, Democratic and Republican 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 (children who, if they move, are likely to be moving with their families), males and females between 18-24 (tertiary-school-aged young adults who are very mobile, as seen in Table A1), males and females between 25-44, males and females between 45-64, and males and females aged 65 and older.

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.[4] 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, where a cohort is defined as a set of states treated in the same year g. The earliest year of treatment in the sample is denoted by q. 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 g, 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.[5] Excluding North Carolina, the earliest treatment year in the sample (q) is 2011. 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 a version of 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

[4]In these and any other regressions where we focus only on destination state policy, we cluster standard errors at the destination state level.

[5]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.

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 a 20-week gestational limit not been implemented. Another important assumption is the "no anticipation" assumption: the policy cannot have an effect before its implementation. Because some 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

In Table 1, we report the results of equation (1), which includes dummies for origin and destination states having 20-week and 24-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. On the other hand, 20-week limits significantly reduce in-migration, as evidenced by the negative and statistically significant coefficient on the destination 20-week limit dummy. The coefficient on the origin 20-week limit is not statistically significant. 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 first 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. Because this regression only includes a policy variable specific to the destination state, we cluster our standard errors at the destination state level. 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 6.5-7% (corresponding to PPML coefficient estimates of -0.067 to -0.074).

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 2, we first repeat our regression from column 4 of Table 1, which includes origin-year fixed effects and drops 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 an ETWFE estimate β_{ETWFE} of -0.097, slightly larger than the coefficient estimate from the standard TWFE regression. This corresponds to a 9.2% reduction in in-migration as a result of a destination's 20-week gestational age limit. 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 -0.08, similar though slightly smaller than the one in column 3 (corresponding to a 7.6% reduction in migration). Results are very similar when we drop the origin-year fixed effects (Table A3).

Having reported the aggregate treatment effects, we now move on the event study plot in Figure 2A, using our preferred specification, column 3 of Table 2. Effects are immediate and fairly consistent in years 0 to 5 from treatment onset. We also 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. This includes pre-treatment observations for states that eventually implement a 20-week gestational age limit, as well as all observations for never-treated states, which serve as the control group. We then aggregate the cohort-specific estimates for each relative year prior to the implementation of the ban, which provides us with a set of pre-treatment coefficient estimates, which we plot in a separate figure (as Nagengast and Yotov (2025) do), Figure 2B. These capture the difference between not-yet-treated and never-treated states in the years before the implementation of a 20-week gestational age limit. If our identifying assumption that the change in migration in the treated and untreated states would have been the same in the absence of the policy holds, then we would expect to also see no change in migration in (eventually) treated and untreated states prior to the policy. This is indeed what we find in Figure 2B, where no pre-period coefficients are statistically significant and the trend across all coefficients is relatively flat.

4.3 Heterogeneity

Table 3 explores heterogeneity by age group and gender. We repeat our TWFE regression from column 4 of Table 1 using migration rates calculated for different age and gender categories (column 1 for males and column 3 for females). We also report our preferred ETWFE specification – column 3 of Table 2 – in column 2 for males and column 4 for females. Each panel reports a different age group. Observation counts vary slightly across sub-samples because the PPML estimator drops all pairs with zero migration throughout the entire sample period. The TWFE and ETWFE observation counts vary because the ETWFE drops always-treated observations.

Based on our ETWFE specification, which is robust to heterogeneous treatment effects, we find statistically significant effects for both genders in all age groups under the age of 45, males aged 45-64, and women 65 and older. Overall, effects do not appear to be driven primarily by women, which 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. While similarly sized effects for both genders could simply be an indication that men and women typically move together, the similarities across genders, combined with the significant effects on women over reproductive age (65 and older), suggests it is not just the future need for an abortion that matters. The desire to live in a state where official policy aligns with one's ideological preferences could be an important driver of these migration responses. In this setting, because we find lower migration to states with more restrictive abortion policy, this suggest that these responses are being driven by those supportive of abortion access.

4.4 Robustness Checks

Our empirical specification controls for state-pair and year fixed effects, which means that time-invariant, pair-level unobservables and common time trends do not pose threats to identification. However, time-varying state characteristics or state-level policy changes that roughly coincide with the implementation of a 20-week gestational age limit do pose a problem, as they could result in migration responses that we incorrectly attribute to gestational age limit changes. In Table A4, we explore whether changes in our policy variable of interest – an indicator for states with a 20-week gestational age limit in place – are correlated with other state-level changes.

Policies related to health insurance and gun control may influence migration decisions, due to individuals' needs and preferences. Because these issues and gestational age limits are often politically polarized, policy shifts across these areas may occur concurrently, which could result in incorrectly attributing our effects to gestational age limits instead of these other policies. For similar reasons, we also consider other policies related to reproductive health. We examine restrictions on abortion providers using an indicator for the existence of any TRAP law, the fastest-growing type of abortion restriction during our study period (Jones and Pineda-Torres, 2024). We also consider trigger bans, laws that would result immediately or quickly in an abortion ban following the overturning of Roe v. Wade. Although trigger bans only came into effect after the 2022 Dobbs decision, individuals in the 13 states that passed these laws (between 2005 to 2022) could have responded in anticipation of their future enactment. Migration decisions are also driven by economic conditions, which could potentially correlate with state-level policy changes. We therefore consider consumer per capita expenditures, as an indicator of economic activity, and housing prices.

Using state-year-level data, we regress our 20-week limit indicator on the state-level policies and characteristics described above, controlling for state and year fixed effects. We first estimate separate regressions and then include all additional controls in the same regression in column 7. In column 1, the negative coefficient, statistically significant at the 10% level, shows that states are less likely to have a 20-week gestational age limit in place in years they have elected to expand Medicaid coverage under the ACA, though this coefficient is no longer significant in column 7. Other than this, there are no significant relationships between 20-week limits and TRAP laws, gun control policies, or trigger bans. Consumer expenditure per capita and house prices, characteristics that are important factors in location decisions, are also uncorrelated with 20-week gestational age limits, conditional on state and year fixed effects.

While these results suggest that the migration responses we document are unlikely to be driven by these other policies and characteristics, given that some coefficients are large in magnitude (though statistically insignificant) we also test the sensitivity of our results to the inclusion of these additional state-level controls. In Table A5, we repeat the age- and gender-specific analysis from Table 3, controlling for Medicaid expansion, indicator for any TRAP law, consumer expenditures, gun laws, housing prices, and trigger bans in the destination state (as the origin-year fixed effects account for origin state characteristics). Our results are very similar to our baseline results, suggesting the migration responses we document are indeed being driven by destination state gestational age limits.

5 Conclusion

In this paper, we examined whether and how interstate migration decisions respond to state-level changes in gestational age limits in the United States. Using a gravity model of migration and the Nagengast and Yotov (2025) ETWFE approach, we find that the implementation of a 20-week gestational age limit, the most restrictive in our study period, significantly reduces in-migration to that state. These results are not driven by pre-existing trends, nor are they explained by correlated economic or policy changes.

Our findings offer several contributions to our understanding of how individuals may vote with their feet. First, we pinpoint reduced in-migration, as opposed to increased out-migration, as the main result of restrictive gestational age limits. This suggests limited “exit” – in the context of the Hirschman (1970) exit-voice hypothesis – in our setting. Second, we highlight a novel mechanism behind these patterns. The fact that migration responses are consistent across genders and remain economically and statistically significant for women over the age of 65 suggests that the need for abortion services is not the sole driver. Rather, individuals appear to be making location choices based on a desire to live in a political environment that reflects their values. These results therefore have implications for the ongoing partisan polarization of the United States.

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Tables and Figures

Figure 1: Distribution of gestational age limits over time

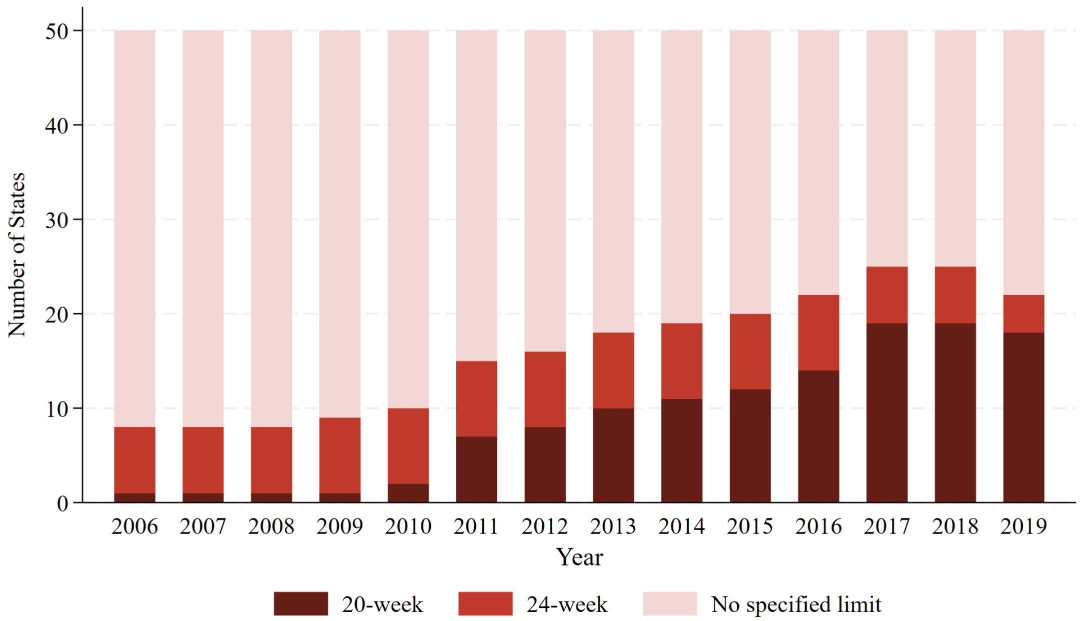


Table 1: Migration responses to gestational age limits

	(1)	(2)	(3)	(4)
Origin 24-Week Limit	0.0055	0.022		
	(0.038)	(0.038)		
Destination 24-Week Limit	0.059	0.045		
	(0.039)	(0.037)		
Origin 20-Week Limit	0.019	-0.017	-0.018	
	(0.018)	(0.017)	(0.018)	
Destination 20-Week Limit	-0.074***	-0.067***	-0.067***	-0.067***
	(0.022)	(0.019)	(0.019)	(0.020)
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 in columns 1 to 3 and at the destination state level in column 4) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions are estimated using PPML, 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.

Table 2: Migration responses to gestational age limits (ETWFE)

	(1)	(2)	(3)	(4)
Destination 20-Week Limit	-0.067***	-0.074***	-0.097***	-0.081***
	(0.020)	(0.021)	(0.022)	(0.015)
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)

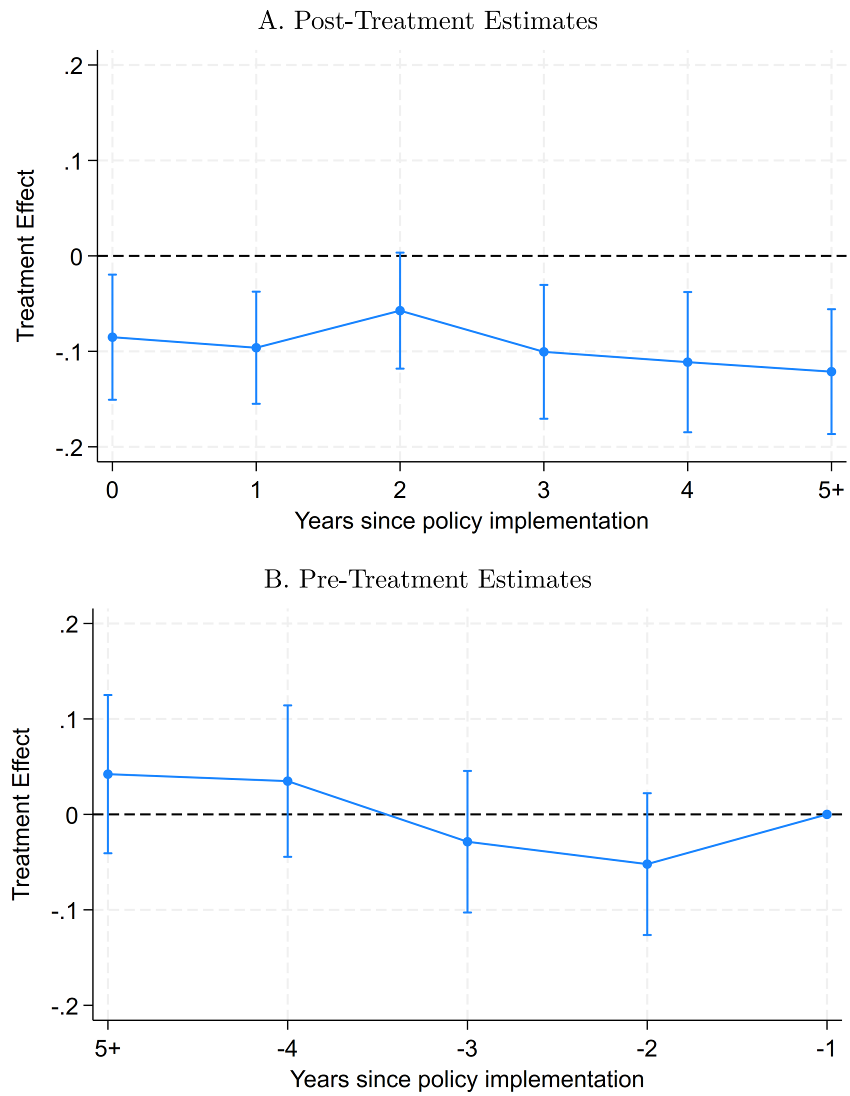
Notes: Standard errors (clustered at the destination state level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions are estimated using PPML, use bilateral migrants divided by the total origin population as the dependent variable, and control for state-pair fixed effects, origin-year 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: Migration responses to gestational age limits, by gender and age

	Males		Females	
	(1)	(2)	(3)	(4)
A. Ages 0-17				
Destination 20-Week Limit	-0.083**	-0.13***	-0.10***	-0.12***
	(0.041)	(0.048)	(0.033)	(0.033)
Observations	31654	30968	31556	30870
Dep. var mean	5.27	5.17	5.25	5.16
B. Ages 18-24				
Destination 20-Week Limit	-0.075***	-0.098***	-0.031	-0.070**
	(0.025)	(0.029)	(0.028)	(0.029)
Observations	32872	32186	32312	31626
Dep. var mean	13.9	13.6	14.6	14.5
C. Ages 25-44				
Destination 20-Week Limit	-0.022	-0.062*	-0.059*	-0.13***
	(0.036)	(0.034)	(0.031)	(0.032)
Observations	33390	32704	33222	32536
Dep. var mean	8.58	8.47	7.73	7.62
D. Ages 45-64				
Destination 20-Week Limit	-0.054	-0.100***	-0.032	-0.044
	(0.034)	(0.038)	(0.028)	(0.042)
Observations	31738	31052	32116	31430
Dep. var mean	3.82	3.76	3.59	3.53
E. Ages 65+				
Destination 20-Week Limit	-0.13*	-0.10*	-0.12***	-0.14***
	(0.075)	(0.058)	(0.033)	(0.048)
Observations	27118	26460	28756	28084
Dep. var mean	3.49	3.47	3.41	3.38
Additional Fixed Effects	Origin-Year	Origin-Year	Origin-Year	Origin-Year
Specification	Baseline TWFE	ETWFE	Baseline TWFE	ETWFE

Notes: Standard errors (clustered at the destination state level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions are estimated using PPML, 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 2: Event-Time Specific Treatment Effects (ETWFE)

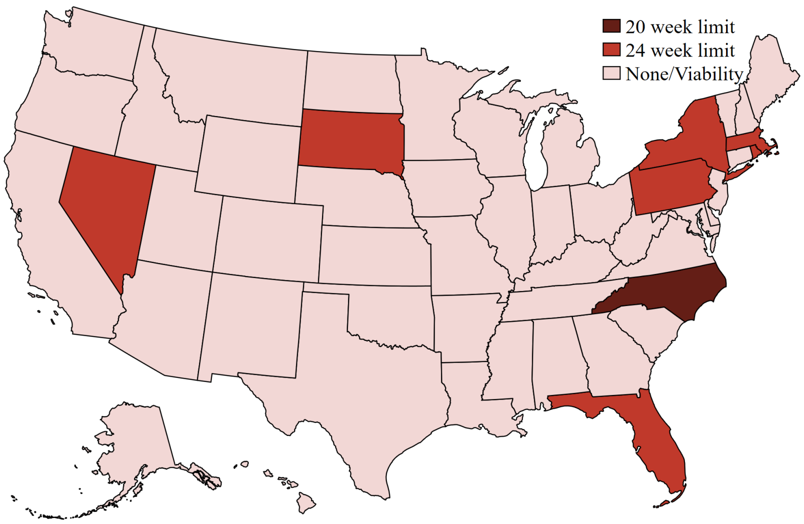


Notes: Figure plots ETWFE event-time specific treatment effect estimates and 95% confidence intervals (using standard errors clustered at the destination state level). All coefficients are calculated using estimates from PPML regressions that use bilateral migrants divided by the total origin population 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.

A Appendix

Figure A1: Geographic distribution of gestational age limits in 2006 and 2019

A. 2006



B. 2019

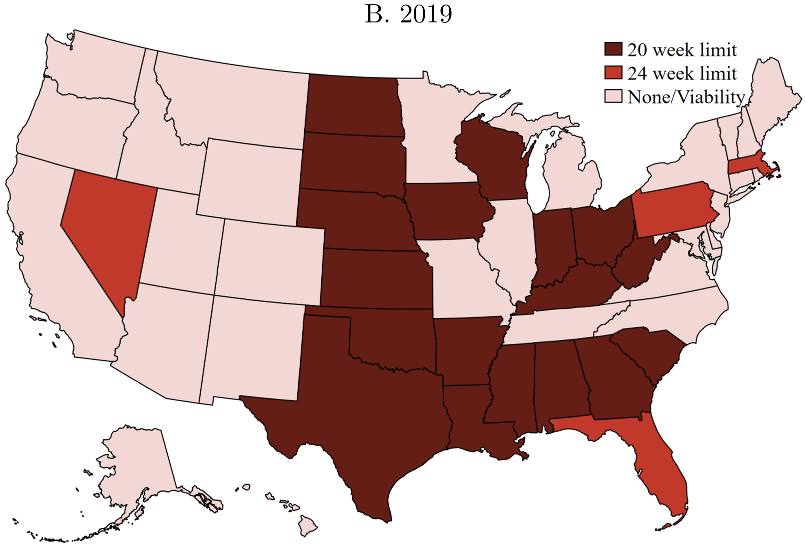


Table A1: Characteristics of Movers vs. Non-Movers

	Non-Movers	Movers
Overall		
Avg. per year (millions)	306.91	7.28
By Gender (%)		
Female	50.8	48.8
Male	49.2	51.2
By Age Group (%)		
Age 0–17	23.5	18.9
Age 18–24	9.5	22.4
Age 25–44	26.5	35.9
Age 45–64	26.2	15.8
Age 65+	14.3	7.0
By Race/Ethnicity (%)		
Non-Hispanic White	62.8	66.1
Non-Hispanic Black	12.2	12.0
Hispanic	17.0	12.2
Non-Hispanic Asian/PI	5.1	5.8
Non-Hispanic Other	3.0	3.9
By Education, Age 25+ (%)		
Less than HS	10.6	6.8
HS or GED	36.8	28.8
Some college	22.8	22.7
BA or higher	29.9	41.7
By Poverty Status (%)		
Below poverty line	14.2	19.2
100–200% poverty	18.1	18.3
200%+ poverty	67.6	62.6
By Employment Status, Age 25–64 (%)		
Employed	73.4	67.0
Unemployed	4.5	8.2
Not in labor force	22.1	24.8
By Health Insurance (%)		
Insured	87.7	83.0
Uninsured	12.3	17.0
By Nativity (%)		
US-born	86.0	87.9
Foreign-born	14.0	12.1
Fertility (Women 15–50, %)		
Had birth in past year	5.3	6.0

Notes: Statistics average across years 2006-2019.

Table A2: Summary Statistics

	(1) Pair-Year	(2) Pair-Year (Main Sample)	(3) State-Year
Movers between states	2,946.61	2,953.85	
	(5,838.93)	(5,844.27)	
Movers per 10,000 origin state residents	5.85	5.87	
	(10.72)	(10.73)	
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 vote share			0.47
			(0.12)
House of Reps. Republican vote share			0.49
			(0.12)
Labor force participation rate			0.49
			(0.07)
Unemployment rate			0.06
			(0.02)
Observations	34300	34216	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.

Table A3: Migration responses to gestational age limits (ETWFE), without origin-year fixed effects

	(1)	(2)	(3)	(4)
Destination 20-Week Limit	-0.068***	-0.076***	-0.098***	-0.080***
	(0.018)	(0.019)	(0.021)	(0.016)
Observations	34202	33516	33516	33516
Dep. var mean	5.87	5.78	5.78	5.78
Additional Fixed Effects	None	None	None	None
Specification	Baseline TWFE	Drop Always-Treated	ETWFE	ETWFE (allowing for anticipation)

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

Table A4: 20-week limit and other state policies

	20-Week Limit						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
ACA	-0.1668*						-0.1161
	(0.0933)						(0.0913)
TRAP Law		0.1415					0.1282
		(0.1522)					(0.1673)
Consumer Expenditure per Capita			-0.0472				-0.0480
			(0.0398)				(0.0398)
Gun Laws Count				-0.0352			-0.0376
				(0.0388)			(0.0386)
House Price Index					8.28×10^{-5}		0.0004
					(0.0003)		(0.0003)
Trigger Ban						0.1239	0.0530
						(0.1560)	(0.1694)
Observations	700	700	700	700	700	700	700

Notes: Standard errors (clustered at the state level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions control for state and year fixed effects.

Table A5: Migration responses to gestational age limits, by gender and age, controlling for additional state characteristics

	Males		Females	
	(1)	(2)	(3)	(4)
A. Ages 0-17				
Destination 20-Week Limit	-0.083*	-0.10*	-0.13***	-0.13***
	(0.045)	(0.054)	(0.032)	(0.035)
Observations	31654	30968	31556	30870
Dep. var mean	5.27	5.17	5.25	5.16
B. Ages 18-24				
Destination 20-Week Limit	-0.058**	-0.067**	-0.038	-0.069**
	(0.027)	(0.032)	(0.028)	(0.028)
Observations	32872	32186	32312	31626
Dep. var mean	13.9	13.6	14.6	14.5
C. Ages 25-44				
Destination 20-Week Limit	-0.034	-0.051	-0.068**	-0.11***
	(0.032)	(0.038)	(0.030)	(0.038)
Observations	33390	32704	33222	32536
Dep. var mean	8.58	8.47	7.73	7.62
D. Ages 45-64				
Destination 20-Week Limit	-0.056*	-0.093***	-0.039	-0.052
	(0.033)	(0.036)	(0.025)	(0.040)
Observations	31738	31052	32116	31430
Dep. var mean	3.82	3.76	3.59	3.53
E. Ages 65+				
Destination 20-Week Limit	-0.12*	-0.11	-0.15***	-0.21***
	(0.070)	(0.070)	(0.036)	(0.061)
Observations	27118	26460	28756	28084
Dep. var mean	3.49	3.47	3.41	3.38
Additional Fixed Effects	Origin-Year	Origin-Year	Origin-Year	Origin-Year
Specification	Baseline TWFE	ETWFE	Baseline TWFE	ETWFE

Notes: Standard errors (clustered at the destination state level) are in parentheses * p< 0.1, ** p< 0.05, *** p< 0.01. All regressions are estimated using PPML, 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, labor force participation rates, ACA Medicaid expansion status, existence of any TRAP law, consumer expenditure per capita, number of firearm law categories in place, housing price index, and trigger ban implementation in the destination state.