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ABSTRACT

In this study, we investigate whether reproductive rights affect migration. We do so using a synthetic difference-in-differences design that leverages variation from the 2022 Dobbs decision, which allowed states to ban abortion, and population flows based on change-of-address data from the United States Postal Service. The results indicate that abortion bans cause significant increases in net migration outflows, with effect sizes growing throughout the year after the decision. The most recent data point indicates that total abortion bans come at the cost of more than 36,000 residents per quarter. The effects are more prominent for single-person households than for family households, which may reflect larger effects on younger adults. We also find suggestive evidence of impacts for states that were hostile towards abortion in ways other than having total bans.

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1 Introduction

Though the Dobbs decision was preceded by a myriad of restrictions on abortion, the ruling fundamentally altered abortion access across the United States. For many individuals, it sparked intensely negative reactions due to concerns about bodily autonomy, reproductive autonomy, health and safety, equity, and a host of other personal and societal issues. News reports and social media have provided anecdotal evidence that these concerns may have caused people to move away from states restricting abortion access or dissuaded people from moving to such states. Survey data are consistent with this evidence (CNBC, 2024). In a random sample of more than 1,000 people aged 18-34 nationwide in 2024, 62 percent reported that they would "definitely not" or "probably not" live in a state that banned abortion, and 45 percent reported that they would "definitely" or "probably" reject an offer from a potential employer if that employer was in a state that banned abortion. Conversely, 35 percent reported that they would "probably accept," and only 20 percent reported that they would "definitely not reject" an offer from an employer in a state that banned abortion.

The economic ramifications could be profound if people act on these stated intentions, as migration decisions have important implications for individual well-being, labor markets, and regional economies (Moretti, 2012). Business leaders have argued that abortion restrictions make it difficult to recruit and retain workers. For example, an amicus brief filed in *Zurawski v. State of Texas* signed by 40 businesses argued that Texas's policy was driving women of reproductive age and their partners from Texas.[1] Related, hundreds of employers announced policies covering out-of-state travel for abortion in the immediate aftermath of the Dobbs decision (Goldberg, 2022).

In this study, we examine whether abortion policies in the post-Dobbs era have affected migration on a large scale. We do so using a synthetic difference-in-differences design and migration measures constructed using the United States Postal Service (USPS) Change of Address (COA) records from July 2018 to June 2023. We find that total abortion bans increase net population outflows (outflows minus inflows). Specifically, our point estimates indicate that a total abortion ban reduces a state's population by 4.3 people per 10,000 residents each quarter in the year following its implementation. The most recent data, corresponding to the second quarter of 2023, indicate that the 13 states with total abortion bans immediately following the Dobbs decision are collectively losing 36,000 residents per quarter due to these bans. Additionally, we find evidence that the effects are more prominent for single-person households than for family households, which may reflect larger effects on younger adults. We also find suggestive evidence of effects for states that were hostile towards abortion in ways other than implementing total bans.

[1] Brief for Amici Curiae Bumble Inc. and Other Businesses and Businesspeople in Support of Appellees, State of Tex. v. Zurawski, No. 23-0629 (Tex. Nov. 20, 2023)

Our study contributes to the literature on the degree to which the effects of reproductive rights policies extend beyond their direct impacts on fertility and health outcomes.[2] Studies using methods from causal inference to explore the effects of such policies on other aspects of women's lives have documented effects on their educational attainment (Goldin and Katz, 2002; Jones and Pineda-Torres, 2024) and financial well-being (Bailey, 2006; Foster, Biggs, Ralph, Gerdts, Roberts, and Glymour, 2022; Miller, Wherry, and Foster, 2023), in addition to effects on living circumstances of children (Ananat, Gruber, Levine, and Staiger, 2009; Foster, Biggs, Raifman, Gipson, Kimport, and Rocca, 2018; Bailey, Malkova, and McLaren, 2019; Ananat and Hungerman, 2012). Our study contributes to this literature by documenting migration responses to state-level abortion bans following the Dobbs decision. In doing so, we provide novel evidence on the broader implications of abortion restrictions for individual location choices, family mobility, and the spatial distribution of people.

Furthermore, our study contributes to the literature on residential choice, which has important implications for the distribution of human capital and economic growth. Economists' longstanding interest in residential choice also stems from the idea that it provides a revealed preference measure of how people value place-based attributes. In the classic Rosen-Roback model, local amenities cause net in-migration (and reduce wages), while disamenities cause net out-migration (and increase wages).[3] Along these lines, research on residential choice has shed light on how individuals value a wide range of state and local factors, including cultural similarity and diversity (Card, 2001), same-sex marriage laws (Marcén and Morales, 2022), natural beauty and climate (Chen and Rosenthal, 2008; Albouy, Graf, Kellogg, and Wolff, 2016), transportation infrastructure (Barwick, Li, Waxman, Wu, and Xia, 2024), pollution (Banzhaf and Walsh, 2008), crime rates (Cullen and Levitt, 1999), school quality (Bayer, Ferreira, and McMillan, 2007), and access to public services (Gelbach, 2004; Goodman, 2017; Agersnap, Jensen, and Kleven, 2020). Our findings contribute to this literature by demonstrating that state abortion policies alter the relative attractiveness of locations and the geographic distribution of human capital.

In the following sections, we first review the landscape for abortion access in the immediate aftermath of the Dobbs decision, which has implications for the coding used in our empirical analyses. We then discuss the Change of Address dataset and how we use it to measure cross-state migration. In the subsequent sections, we discuss how we implement the synthetic difference-in-differences research design, the results of our analyses, and then conclude.

[2] Bailey and Lindo (2018) review this literature. For some more recent studies, see Lu and Slusky (2016), Fischer, Royer, and White (2018), Lu and Slusky (2019), Lindo and Packham (2017), Lindo, Myers, Schlosser, and Cunningham (2020), Clarke and Mühlrad (2021), and Flynn (2024).

[3] For an extensive review of theoretical models of migration, including Rosen (1979) and Roback (1982, 1988), see Jia, Molloy, Smith, and Wozniak (2023).

2 Background and Policy Coding

Two landmark Supreme Court decisions—Roe v. Wade and Casey v. Planned Parenthood—established the right to an abortion before fetal viability. The Dobbs v. Jackson Women's Health decision, released on June 24, 2022, allowed states to enforce pre-viability abortion bans. Such bans took effect immediately or shortly after the ruling in 13 states: Alabama, Arkansas, Idaho, Kentucky, Louisiana, Mississippi, Missouri, Oklahoma, South Dakota, Tennessee, Texas, West Virginia, and Wisconsin. Wisconsin, which had never repealed its pre-Roe ban, saw that ban into effect in June 2022 until a court ruling allowed abortion services to resume in September 2023. West Virginia had legal uncertainty around its pre-Roe abortion laws and, as was widely expected, enacted a ban in September 2022. We treat these 13, shown in Column 1 of Table 1 which summarizes our coding of states, as "ban states" in our analyses. However, to account for potential heterogeneity due to state-specific factors—such as the factors mentioned above and Texas effectively banning abortions past six weeks in September 2021 through civil penalties—we assess the sensitivity of our main results to the exclusion of any given state.

For comparison, we use a set of 25 states that maintained or protected abortion access in the aftermath of Dobbs. Specifically, this set of states is comprised of 25 states that have specific laws or constitutional protections in place protecting abortion or allowing it up to a point of pre-Dobbs state-defined viability and no actively hostile legislative efforts to ban abortion during our study period.[4] Henceforth, we refer to this set of states as "abortion-protecting states."

While our primary focus is on the effect of a total abortion ban, as opposed to maintaining or protecting access, we also consider the effects in 13 states where total bans did not go into effect immediately but where abortion access was impaired or threatened. This set of states includes three—Utah, Wyoming, and North Dakota—that had trigger bans at the time of the Dobbs ruling that were not enforced due to legal reasons. North Dakota subsequently enacted a new law banning abortion, which went into effect in April 2023.[5] Indiana, which banned abortion in August 2023, is also included in the set of abortion-hostile states. The set also encompasses states that have enacted "gestational age bans," which restrict abortion based on gestational age. Notably, Georgia and Ohio implemented a 6-week ban immediately following the Dobbs decision, while three additional states—Florida, Iowa, and South Carolina—enforced 6-week bans in the subsequent months.[6] Moreover, Arizona, Nebraska, North Carolina, and Utah all implemented gestational age bans ranging from 12-18 weeks. We also follow Center for Reproductive Rights (2023) in classifying

[4] See Dench, Pineda-Torres, and Myers (2024) Appendix A for why states received protected state classifications, which included a review of state laws and comparison to Center for Reproductive Rights (2023) codings.

[5] This law is currently not in effect after a legal challenge that blocked it in October 2024 but the only abortion provider in the state has already moved to Minnesota.

[6] Ohio's 6-week ban was blocked after three months of enforcement. The state passed a constitutional amendment protecting abortion in November 2023.

Table 1
State Coding

Total Ban	Protecting	Hostile
Alabama	Alaska	Arizona††
Arkansas	California$^{\circ}$	Florida†
Idaho	Colorado$^{\circ}$	Georgia†
Kentucky	Connecticut	Indiana†
Louisiana	Delaware	Iowa†
Mississippi	DC	Nebraska††
Missouri	Hawaii	North Carolina††
Oklahoma	Illinois$^{\circ}$	North Dakota$^{\ddagger\ddagger\ \S}$
South Dakota	Kansas	Ohio†
Tennessee	Maine	Pennsylvania¶
Texas	Maryland	South Carolina†
West Virginia*	Massachusetts	Utah$^{\dagger\dagger\ \S}$
Wisconsin**	Michigan$^{\circ}$	Wyoming§
	Minnesota	
	Montana	
	Nevada	
	New Hampshire	
	New Jersey$^{\circ}$	
	New Mexico$^{\circ}$	
	New York$^{\circ}$	
	Oregon$^{\circ}$	
	Rhode Island	
	Vermont	
	Virginia	
	Washington$^{\circ}$	

* Had legal uncertainty around its pre-Roe abortion laws immediately following Dobbs and enacted a ban in September 2022.
** Pre-Roe total ban was never repealed and went into effect in June 2022 before being overturned in September 2023.
$^{\circ}$ Have taken steps to expand abortion rights since Dobbs.
† Had 6-week gestational age bans go into effect with the passage of Dobbs (Georgia, Ohio) or enacted them soon thereafter (Florida, Iowa, and South Carolina).
†† Enacted 12-18 week gestational age bans shortly following Dobbs.
§ Had trigger bans at the time of the Dobbs ruling that were not enforced due to legal reasons.
‡‡ Banned abortion but significantly later.
¶ Classified as hostile by the Center for Reproductive Rights.

Pennsylvania as an abortion-hostile state. Henceforth, we refer to this set of states as "abortion-hostile states."

3 Data and Variable Construction

Our primary data source is the Change of Address (COA) dataset from the United States Postal Service (USPS).[7] This dataset captures all mail forwarding requests submitted to the USPS. The USPS COA service processes address changes through multiple channels (online, mail, or in-person) and compiles them monthly at the ZIP Code level. Each entry is categorized by the number of moves into and out of an area, the type of move (family, individual, or business), and changes of permanent and temporary addresses.[8] For privacy protection, the USPS only discloses COA volumes exceeding 10.[9]

These data have two key advantages over alternatives, such as the Internal Revenue Service (IRS) migration data and the Census Bureau's migration data. First, they have been released more quickly, allowing for analyses of recent policy changes.[10] Second, they measure migration monthly rather than annually, making it possible to analyze whether there are immediate effects and how effects evolve over time.

However, the COA migration data are not without shortcomings. First and foremost, the data do not capture moves in which individuals do not file a change-of-address request with the USPS. As such, we expect these data to undercount moves and, as a result, produce conservative estimates of effects. Second, although these data capture migration out of the United States (when individuals submit change-of-address requests), they do not capture migration into the United States. Third, while the data provide counts of moves into and out of each area, they do not provide origin-to-destination counts.

Our approach to calculating population flows based on these change-of-address data involves multiple steps. We begin by calculating "net change-of-address outflows" for each state (and quarter) as the sum of changes of address out of the state minus changes of address into the state. To convert this measure into "net population outflows," we follow Ramani and Bloom (2021) by multiplying household change-of-address requests by 1.7 to account for the average size of moving families and assuming individual change-of-address involves just one person. To improve comparability in this measure across states of varying sizes and to facilitate interpretation, we divide "net change-of-address outflows" by each state's 2018 population, as measured by the U.S. Census Bureau, multiplied by 10,000. The resulting measure expresses states' quarterly

[7] We obtained data spanning from July 2018 to July 2022 from Freedom of Information Act requests and more recent data from the USPS FOIA Library. USPS provides the total COA requests to and from each ZIP Code in each quarter. The data includes 1,409,438 change-of-address requests from June 2018 to June 2023 across 59 U.S. states and territories, encompassing 31,946 ZIP Codes. We drop ZIP Codes designated as military bases, assuming that most people moving to military bases do not have substantial autonomy over their place of residence. We also drop U.S. territories.

[8] A family move is defined as a change of address for a household where multiple family members (typically those sharing a last name) are relocating together. An individual move is defined as a change of address for a single person moving alone, typically someone living by themselves or relocating separately from their family. This classification may underestimate family moves, as single-parent families with underage children might submit only one change of address request. The USPS classifies a change-of-address request as temporary or permanent based on the respondent's intent to return to their original address. Specifically, it is based on responses on change of address request forms to the question which reads: "Are you planning on returning to your old address in six months or less? Selecting 'Yes' will classify your Change-of-Address as Temporary. Selecting 'No' will classify your Change-of-Address as Permanent."

[9] We impute 5 in these instances.

[10] Census data and IRS data are not presently available to be able to capture migration in the Post-Dobbs era.

net migration flows per 10,000 pre-Dobbs residents. Finally, we deseasonalize the data to reduce variance and address the possibility that seasonal migration patterns might correlate with treatment status.[11]

While our primary focus is on "net population outflows per 10,000 residents," we also analyze net family change-of-address outflows per 10,000 family households and net individual change-of-address outflows per 10,000 non-family households.[12] Furthermore, we analyze net permanent change-of-address outflows per 10,000 addresses and net temporary change-of-address outflows per 10,000 addresses.[13,14]

4 Empirical Strategy

Our analyses use a synthetic difference-in-differences (SDID) research design to compare changes in net outflows for "total ban" states to a weighted counterfactual drawn from "protected" states. This research design was previously used to evaluate the effect of post-Dobbs abortion bans on birth rates (Dench et al., 2024). In that context, a pre-specified analysis plan showed that SDID was superior to two-way fixed effects in terms of power and robustness to panel length.

We treat all 13 states with total bans in 2022 as "treated" as of the third quarter of 2022. We use the 25 states maintaining or protecting abortion rights, as discussed in Section 2, for potential comparison.

The SDID method combines features of Synthetic Control methods (SC) and Difference-in-Differences (DID). Like DID, it accounts for pre-Dobbs differences in outcomes between ban states and comparison states. Estimated effects are based on how outcomes change over time (post-Dobbs versus pre-Dobbs) for each state. Thus, estimated effects on ban states capture changes over and above what is expected based on their histories.

The SDID method refines the comparison. Like SC, it reweights and matches on pre-exposure trends to weaken the reliance on parallel trends while simultaneously being invariant to additive unit-level shifts and allowing for valid large-panel inference like DID (Arkhangelsky, Athey, Hirshberg, Imbens, and Wager, 2021). Unlike SC methods, it does not select a weighted set of control units that minimize average differences in levels in the pre-period, but rather, it selects a weighted set of control units that minimize differences in

[11] Specifically, we deseasonalize the data by estimating a separate regression model for each state, with quarterly indicators and a linear trend using pre-Dobbs data, and then using the coefficient estimates on the quarterly indicator variables to remove expected seasonality from all quarters of data (both pre and post-Dobbs).

[12] The number of family and non-family households in each state is based on the ACS Households and Families 5 Year Estimates for 2018. A family household is a housing unit containing a householder and at least one other person in the household who is related to the householder by birth, marriage, or adoption. Multigenerational, married-couple, and single-parent homes are included in the count of family households. A non-family household contains a householder living alone or with nonrelatives. Unmarried-partner households are considered non-family unless there is another person in the housing unit who is related to the householder by birth or adoption.

[13] The number of addresses in each state in 2018 is based on total occupied business and residential addresses reported by the U.S. Department of Housing and Urban Development (HUD) USPS ZIP Code Crosswalk Files.

[14] All of these alternative measures of migration are seasonally adjusted in the same manner as the net population flows measure.

trends in the pre-period. This addresses concerns raised and similarly addressed in Ferman and Pinto (2021) about the biasedness of SC when pre-treatment fit is imperfect and treatment is correlated with unobserved confounders. In addition, SDID selects time weights that minimize the level difference in the post-period and the pre-period among all control units. Both procedures use only the outcomes in state and time for selection of weighting, leaving little for the researcher to select. Together, these features minimize variation between treatment and control units and time periods, improving statistical power while best satisfying the fundamental assumption of DID—parallel trends—without introducing researcher degrees of freedom through selective deletion of treatment or control groups or choices of control variables.

Specifically, we estimate the average causal effect of *Dobbs* on net population outflow rate by obtaining:

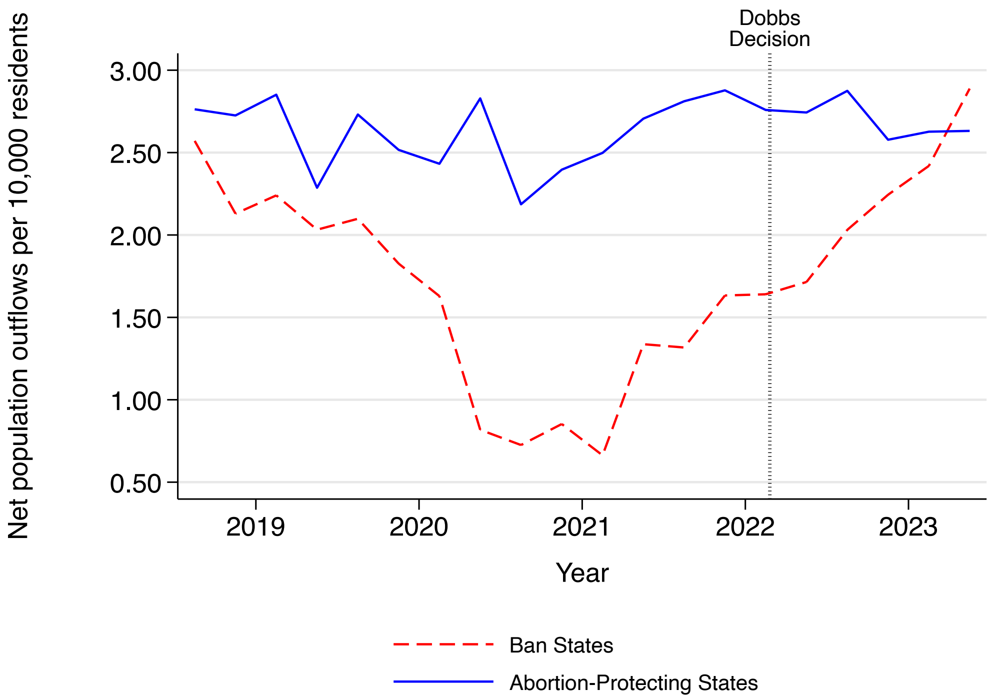
$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \underset{\tau,\mu,\alpha,\beta}{argmin}\{\sum_{i=1}^{N}\sum_{t=1}^{T}(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid}\} \tag{1}$$

where ω_i^{sdid} is chosen to minimize the average squared difference in trends between the treatment and control groups subject to a regularization parameter to increase dispersion and ensure the uniqueness of weights. In other words, regularization prevents overfitting to decrease estimator variance without a substantial increase in bias.

λ_t^{sdid} is chosen to minimize the sum of squared differences between the time-weighted pre-period outcomes of the control states and the simple average of the post-period outcomes in the control states. This down-weights values in the pre-treatment period that are unusual for the control states relative to the post-period. For example, if an unexpected shock like a hurricane or a pandemic affects the outcome in the pre-period for a short period of time so that they do not resemble the post-period, but other pre periods do, SDID will down-weight the unusual pre-periods. For statistical inference, we rely on block bootstrap methods.[15] To estimate SDID event studies with confidence intervals, we follow Clarke, Pailañir, Athey, and Imbens (2023) and use the difference between the treatment and control group in each period relative to the difference observed in the time-weighted pre-period, and use bootstrap inference for the calculation of 95 percent confidence intervals.

[15] Arkhangelsky et al. (2021) derives three methods for inference under different assumptions: block-placebo inference, block-bootstrap inference, and jackknife inference. Their placebo inference procedure relies on assignment of equal number of pseudotreated units to the set of control units, so can be used in all cases where control units outnumber treatment units. However, placebo inference assumes that the error distribution for the treatment groups has equal variance to the control groups, which is not testable in realized data. Jackknife standard errors are robust to this concern but rely on the assumption that the time weights of the treatment unit absent treatment are similar to the control unit's selected time weights. Jackknife inference may also be overly conservative and, thus, underpowered. In contrast, block-bootstrap methods rests on the assumption that the number of treated units and control units is consistent when the number of treatment units and control units is sufficiently large and does not assume equal variance in treatment and control groups or equal time weights between treatment units and control groups.

Figure 1
Net Population Outflow Rates for Abortion-Ban States vs Abortion-Protecting States



Notes: This figure plots seasonally adjusted trends in net population outflows per 10,000 residents. The 13 "ban states" and 25 "abortion-protecting states" are listed in Table 1 and discussed in Section 2. Quarterly net population outflow rates for each set of states are the sum of quarterly net population outflows divided by the sum of states' 2018 population and multiplied by 10,000. Quarterly net population outflows for each state are estimated by multiplying family change-of-address requests to the US Postal Service by 1.7 (average U.S. household size for moving families) and adding individual COA requests. The resulting two data series are seasonally adjusted based on pre-Dobbs trends.

5 Results

5.1 Graphical evidence of changes over time

Before presenting our estimates of the effects of abortion bans, we first present graphical evidence of trends over time for context. In Figure 1, we show net population outflow rates over time for ban states versus states protecting or maintaining abortion access. We note that net outflow rates are consistently positive for both sets of states, which reflects the fact that the change-of-address data captures emigration from the United States (from requests to the USPS to forward mail internationally) but not immigration to the United States.[16]

Regarding differences between ban states and abortion-protecting states, Figure 1 shows several important patterns. First, before the Dobbs decision, net population outflow rates were consistently lower for ban

[16]Net outflow rates are also consistently positive for individual states for the same reason.

states than for abortion-protecting states. Second, this difference grew rapidly at the onset of the COVID-19 pandemic. This aligns with recent work on migration patterns during the pandemic.[17] Starting in the first quarter of 2021, the gap between the two groups began to narrow as outflows from ban states increased.

Following the Dobbs decision, net outflows from states with abortion bans continued to increase, appearing to accelerate relative to trends from the preceding three quarters, while net outflows from states with abortion protections remained relatively stable. By the most recent quarter for which data are available (the second quarter of 2023), net outflows from ban states exceeded those from states maintaining or protecting abortion access.

We interpret these patterns as suggestive evidence that abortion bans increase net population outflows. However, it is important to recognize that the pre-Dobbs trends for the two sets of states are not the same, which highlights the importance of our SDID research design that identifies an appropriate comparison on the basis of common pre-Dobbs trends.

5.2 Main results

We present our main results in Figure 2. This figure shows event-study estimates based on the model specified in Equation 1 using seasonally adjusted net migration outflow rates for each state from 2018 to 2023.

The estimates in the pre-Dobbs period do not systematically deviate from zero, indicating stable differences in population outflow rates between ban states and the weighted set of control states in the years leading up to the Dobbs decision. Thus, these estimates provide evidence supporting the validity of our SDID research design.

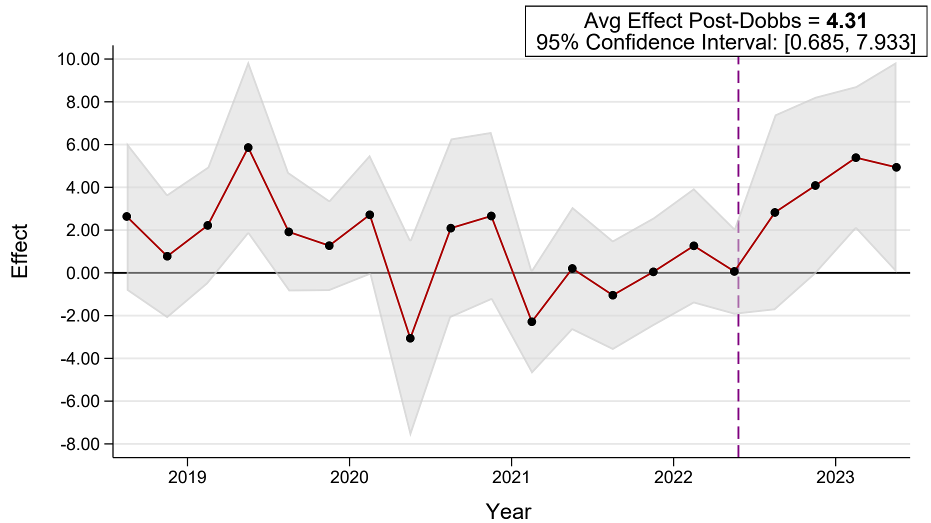
Moreover, the event-study estimates in Figure 2 show a trend break immediately following the Dobbs decision, indicating that bans increase net outflow rates. They also suggest that the immediate effects are smaller than the effects in subsequent quarters. The estimate for the most recent quarter of data (the second quarter of 2023 and four quarters after the Dobbs decision) indicates that having a total abortion ban reduces a state's population by 4.9 people per 10,000 residents quarterly. This corresponds to 36,880 people across the 13 states with total abortion bans in the immediate aftermath of the Dobbs decision.[18]

The average effect across all four quarters following the decision indicates that bans reduced states' populations by 4.3 people per 10,000 residents. This implies that abortion bans resulted in a net population

[17] Foster, Fiorio, and Ellis (2024) show that the number of address changes increased by 2% between 2019 and 2020, and then returned to pre-2019 levels in 2021. They find that these COVID-era changes favored migration away from the Pacific and mid-Atlantic to the South, primarily driven by young adults, high-income earners, and individuals.

[18] This is calculated by multiplying the quarterly estimated effect of 4.94 people per 10,000 residents by the pre-treatment (2018) population across the 13 states with total abortion bans (74,651,967).

Figure 2
Effect of Abortion Bans on Net Population Migration Outflows (per 10,000 residents)



Notes: This figure presents quarterly synthetic difference-in-differences estimates and 95 percent confidence intervals and 95 percent confidence intervals obtained using block bootstrap inference as outlined in Arkhangelsky et al. (2021). The dependent variable is the quarterly net population outflow rate, which is calculated for each state as (outflows - inflows)/(2018 state population) × 10,000 and seasonally adjusted based on pre-Dobbs trends. Population flows are estimated by multiplying family change-of-address requests to the US Postal Service by 1.7 (average U.S. household size for moving families) and adding individual COA requests.

loss of 128,700 residents across the 13 states with such laws in the year following the Dobbs decision.[19]

To assess the robustness of our results, we conducted a leave-one-out sensitivity analysis, sequentially omitting each state from the sample and re-estimating the post-Dobbs effect. The 38 resulting estimates, shown in Appendix Figure A1, are consistent with our main finding, ranging from 3.5 to 4.9. Furthermore, all estimates are statistically significant or nearly significant at the 5% level.

5.3 Heterogeneity Analyses

In this section, we consider whether the effects of abortion bans differ for family versus individual migration, and whether they impact permanent moves differently from temporary ones. We then consider the effects for abortion-hostile states.

Individual and family households may respond differently to abortion bans for many reasons. First, individuals in single-person households tend to be younger, and younger individuals are more likely to be directly affected by restricted access to abortion and to oppose total abortion bans.[20] Additionally, younger

[19]This is calculated by multiplying the quarterly estimated effect of 4.31 people per 10,000 residents by 4 quarters and the pre-treatment (2018) population across the 13 states with total abortion bans (74,651,967).

[20]According to Pew Research Center (2024), 23 percent of those aged 18–29 say abortion should be illegal in all or most

adults tend to be more mobile (Molloy, Smith, and Wozniak, 2011; National Institute on Aging, 2024). This may be due to the fact that relocation tends to be more costly for families, who must coordinate multiple jobs, schooling, and childcare arrangements. Families are also more likely to encounter the logistical and financial challenges of selling and/or purchasing a home. These factors suggest that the effects of abortion bans may be more pronounced for individuals in single-person households than for families.

This hypothesis is supported by the results in Figure 3, which shows estimated effects on net *family* change-of-address outflows per 10,000 in Panel A and estimated effects on net *individual* change-of-address outflows per 10,000 in Panel B. Both sets of estimates are consistent with our main results in exhibiting a trend-break after the Dobbs decision and indicating statistically significant effects of bans on net outflows. However, the magnitude of the effect and the extent to which it appears to be growing over time is much greater for individual movers.

The estimated effects on net family outflows per 10,000 range from 4.8 to 8.0 across the four quarters following the Dobbs decision. The pattern of estimates offers some suggestive evidence that the effects rose from the first to second quarter following the decision and then declined; however, the estimates and their confidence intervals are also consistent with persistent and/or growing effects. On average, our estimates indicate that bans increase quarterly net family outflows by 6.3 families per 10,000. This corresponds to 11,600 families across the 13 ban states in each quarter following the Dobbs decision.[21]

In contrast, the estimated effect on net individual outflows is approximately 7 per 10,000 nonfamily households in the first two quarters after the Dobbs decision, grows to 21 per 10,000 in the subsequent quarter, and grows further to 27 per 10,000 in the subsequent quarter. Together, these estimates suggest that abortion bans cost the states that implemented them a total of 57,000 individuals living in single-person households in the year after the Dobbs decision.[22] The most recent estimate suggests an effect of 24,800 individuals quarterly.[23]

We next consider whether bans affect permanent or temporary moves, which is crucial for understanding whether the effects we have documented thus far are likely to result in persistent changes in residential patterns. The results in panels C and D of Figure 3 shed light on this issue. Specifically, Panel C shows estimated effects on net permanent change-of-address outflows per 10,000 addresses and Panel D net temporary change-of-address outflows per 10,000 addresses. These results indicate that our estimated effects on migration are driven entirely by permanent moves.

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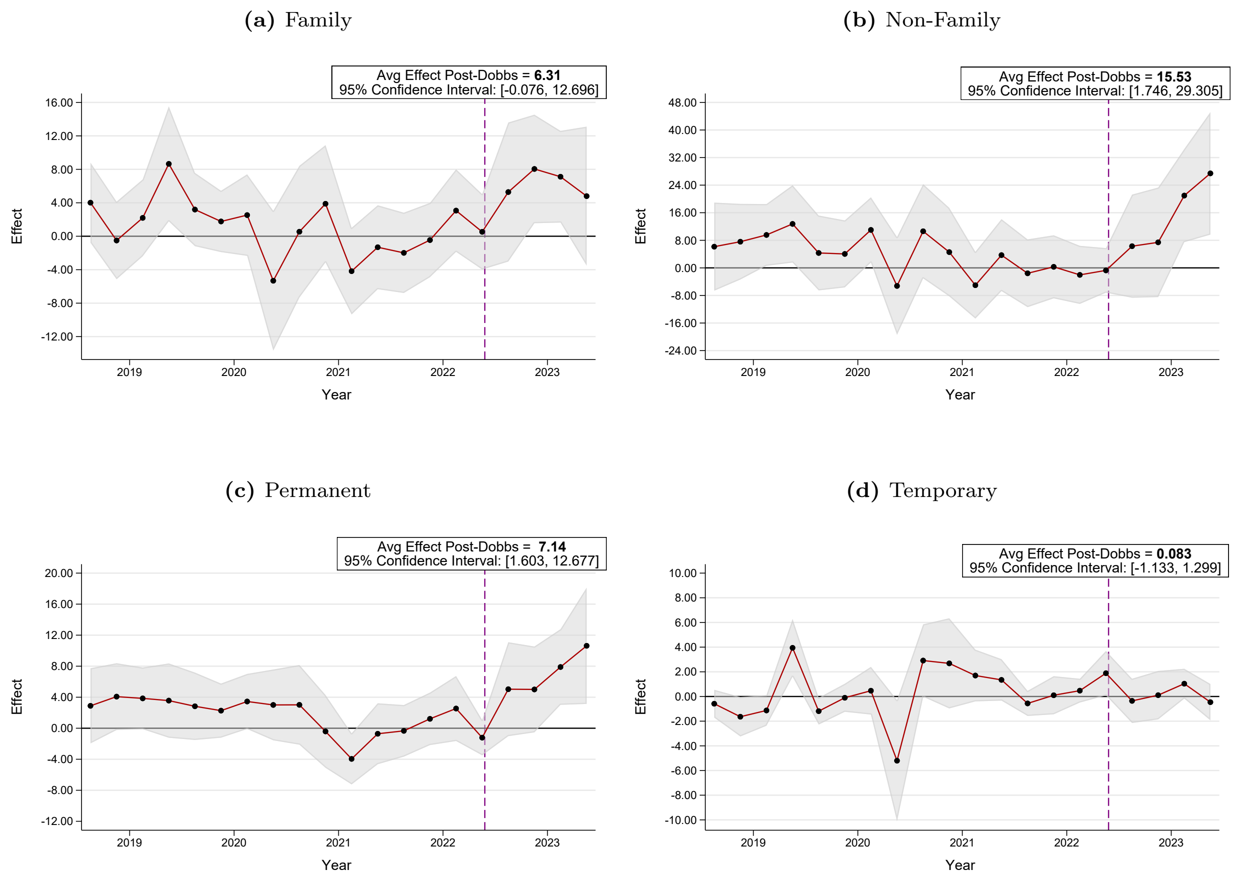
cases, versus 37 percent or more among older age groups.

[21]This is calculated by multiplying the quarterly estimated effect of 6.3 families per 10,000 family households by the pre-treatment (2018) number of family households across the 13 states with total abortion bans (18,421,819).

[22]This is calculated by summing the estimated effects of nonfamily households across the four quarters (62 per 10,000), multiplied by the pre-treatment (2018) number of nonfamily households across the 13 states with total abortion bans (9,189,596).

[23]This is calculated by multiplying the quarterly estimated effect of 27 per 10,000 nonfamily households by the pre-treatment (2018) average number of nonfamily households across the 13 states with total abortion bans (9,189,596).

Figure 3
Effects of Abortion Bans on Different Mover Types



Notes: This figure presents quarterly synthetic difference-in-differences estimates and 95 percent confidence intervals obtained using block bootstrap inference as outlined in Arkhangelsky et al. (2021). For panels (a) and (b), the outcome variables are quarterly net family outflows per 10,000 and quarterly net non-family outflows per 10,000, respectively. These are calculated based on separate counts of family and non-family change-of-address requests submitted to the US Postal service, and using 2018 American Community Survey estimates of the number of family and non-family households in each state. For panels (c) and (d), the outcome variables are net permanent change-of-address outflows per 10,000 addresses and net temporary change-of-address outflows per 10,000, respectively. These are calculated based on separate counts of permanent and temporary change-of-address requests submitted to the US Postal Service, and 2018 address counts from the US Department of Housing and Urban Development. Each measure is seasonally adjusted based on pre-Dobbs trends.

We next consider whether the Dobbs decision affected "abortion-hostile states." As detailed in Section 2, abortion access was either directly impaired or perceived to be under threat in these states following the Dobbs decision. In Table 2, we present the estimated effects on these states in Panel B after reproducing the estimated effects of total bans in Panel A.

The estimated effects of abortion hostility are all positive and similar in magnitude to the estimated effects of total bans. The standard errors, however, are larger for each outcome we consider, implying that there is increased uncertainty with these estimated effects compared to our estimated effects of total bans. For example, the estimated effect of abortion hostility on the net population outflow rate is 3.9 per

Table 2
Effects of Total Bans and Abortion Hostility on Net Outflow Rates

	(1) Population	(2) Family	(3) Individual	(4) Permanent	(5) Temporary
Panel A: Total Bans as Treatment					
Estimated Effect (Quarterly)	4.31**	6.31**	15.53**	7.14***	0.83
	(1.85)	(3.18)	(6.92)	(2.78)	(0.61)
Observations	760	760	760	760	760
Panel B: Abortion Hostility as Treatment					
Estimated Effect (Quarterly)	3.92	7.96**	11.73	6.05*	0.29
	(2.52)	(3.73)	(8.88)	(3.25)	(1.05)
Observations	760	760	760	760	760

Notes: The reported coefficients are synthetic difference-in-difference estimates of effects of having a total abortion ban (Panel A) or being hostile towards abortion in other ways (Panel B) as opposed to protecting or maintaining abortion access. Standard errors in parentheses are obtained using block bootstrap methods as outlined in Arkhangelsky et al. (2021). The 13 ban states, 13 abortion-hostile states, and 25 abortion-protecting states are listed in Table 1 and discussed in Section 2. Column (1) reports effects on net population outflow rates per 10,000 people. Columns (2) and (3) report effects on net family and net individual outflows per 10,000, respectively. Columns (6) and (7) report effects on permanent and temporary net migration outflows per 10,000, respectively.
* p<0.1, ** p<0.05, *** p<0.01.

10,000 (compared to 4.3 per 10,000 for bans), though the estimate is not statistically significant at the five percent level. That said, the estimated effects on family-household outflow rates and permanent moves are statistically significant at the five- and ten-percent levels, respectively.

6 Discussion and Conclusion

This study shows that state-level abortion bans following the Dobbs decision increased net migration outflows, highlighting that reproductive healthcare access has a measurable effect on residential decisions. The effects are particularly large and growing over time for single-person households, suggesting an outsized influence of reproductive rights on younger, more mobile populations.

If our most recent estimated effect is sustained over a five-year period, it would imply a 0.98% population loss for states banning abortion as opposed to protecting or maintaining abortion access.[24] This "disamenity effect" on population size is comparable to the impact of a 10% increase in local crime rates (Cullen and Levitt, 1999) or one-tenth the effect of community exposure to a toxic release inventory chemical (Banzhaf and Walsh, 2008).

More broadly, our results show that reproductive rights policies can significantly affect where people choose to live. It will be important for future research to evaluate impacts on state economies and labor markets. States with abortion bans may face challenges in attracting and retaining workers, especially

[24]Based on the effect in the second quarter of 2023 (4.9 per 10,000) multiplied by 20 quarters.

younger workers who represent future economic potential. These population flows and demographic shifts could affect a wide range of economic factors from tax bases to housing markets to the availability of workers in key industries.

It will be important for future research to quantify such broad-based economic effects, along with addressing several additional questions. What are the economic consequences for those who relocate versus those who do not? Similarly, who is being deterred from living in states with restricted abortion access? The fact that highly educated individuals tend to be more mobile (Molloy et al., 2011) and more supportive of abortion access (Pew Research Center, 2024) suggests potentially significant heterogeneity across education levels, with important implications for state economies. Finally, future research should explore how businesses respond to these changes, as their actions could either alleviate or exacerbate economic effects.

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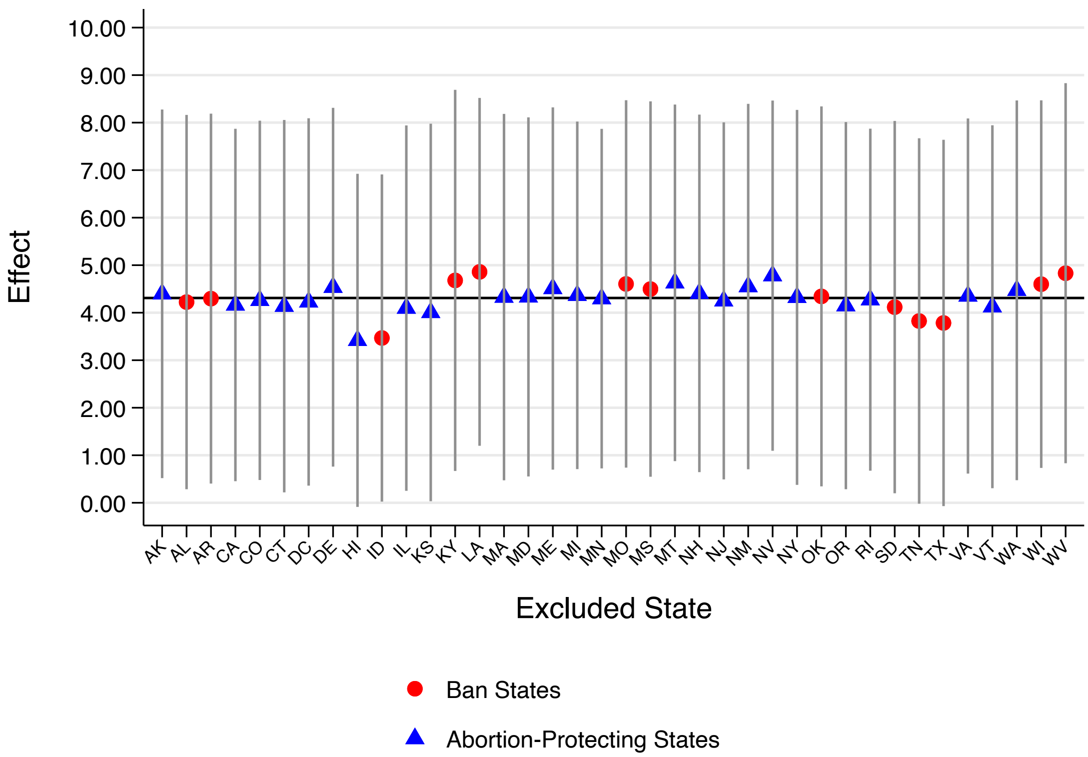
Appendix

Figure A1
Leave-One-Out Sensitivity Analysis of Estimated Effects on Net Population Outflow Rates



Notes: This figure presents the results of our leave-one-out sensitivity analysis, in which we sequentially omit each state from the sample and re-estimate the post-Dobbs effect on net population outflows per 10,000. Spikes extending from each point estimate represent 95 percent confidence intervals. The bold horizontal line depicts the estimated effect using the full sample of states.