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ABSTRACT

We investigate whether reproductive rights affect migration. We use 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 bans increase net migration outflows, with effect sizes growing throughout the year after the decision. The effects are more prominent for single-person households than family households, which may reflect larger effects on younger adults. We also find suggestive evidence of impacts for states 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), which helped them to attract new workers but also reduced job satisfaction among some existing employees (Adrjan, Gudell, Nix, Shrivastava, Sockin, and Starr, 2023).

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 4.9 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 52,600 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

[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)

hostile towards abortion in ways other than implementing total bans.

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 the living circumstances of children (Ananat, Gruber, Levine, and Staiger, 2009; Ananat and Hungerman, 2012; Foster, Biggs, Raifman, Gipson, Kimport, and Rocca, 2018; Bailey, Malkova, and McLaren, 2019). 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), Lindo and Packham (2017), Fischer, Royer, and White (2018), Lu and Slusky (2019),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 *Planned Parenthood v. Casey*—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 go 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 comprises 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°	Florida†
Idaho	Colorado°	Georgia†
Kentucky	Connecticut	Indiana†
Louisiana	Delaware	Iowa†
Mississippi	DC	Nebraska††
Missouri	Hawaii	North Carolina††
Oklahoma	Illinois°	North Dakota‡‡ §
South Dakota	Kansas	Ohio†
Tennessee	Maine	Pennsylvania¶
Texas	Maryland	South Carolina†
West Virginia*	Massachusetts	Utah†† §
Wisconsin**	Michigan°	Wyoming§
	Minnesota	
	Montana	
	Nevada	
	New Hampshire	
	New Jersey°	
	New Mexico°	
	New York°	
	Oregon°	
	Rhode Island	
	Vermont	
	Virginia	
	Washington°	

* 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.
° 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."[7]

[7] While an argument can be made that Pennsylvania did not shift its abortion regulations in response to Dobbs, the abortion classification by the Center for Reproductive Rights (2023) could have created the perception of abortion restriction. When we reclassify Pennsylvania as an abortion-protecting state, the estimated effects of hostility get stronger and are significant at the 10% level, while the estimated effects of bans are largely unchanged.

3 Data and Variable Construction

Our primary data source is the Change of Address (COA) dataset from the United States Postal Service (USPS).[8] 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. We consider moves of both "families" and "single-person households."[9] We also separately analyze permanent and temporary moves.[10,11]

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.[12] Second, they measure migration monthly rather than annually, making it possible to analyze whether there are immediate effects and how effects evolve over time. Third, they allow for separate analyses of migration for families and singles.

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 multiply the total number of change-of-address requests (family and single) by 1.7, where 1.7 is the average size of moving households based on information from Data Axle described

[8] 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.

[9] 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. A single-person household (or "individual" in the COA data) move is defined as a change of address for a single person moving alone, typically someone living alone or relocating separately from their family. This classification may underestimate family moves and overestimate single moves, as single-parent families with underage children might submit only one change of address request.

[10] 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."

[11] For privacy protection, the USPS only reports COA volumes exceeding 10. When change-of-address volumes are 10 or below, the USPS suppresses these values for privacy protection. Where possible, we recover suppressed COA values using the identity that total flows equal the sum of subcategories (family + individual + business flows, and temporary + permanent flows). For the remaining suppressed values, we impute 5 following Ramani, Alcedo, and Bloom (2024). Our results are robust to instead imputing 0.

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

in Ramani et al. (2024). We discuss the robustness of our results to alternative approaches to calculating population flows from change-of-address requests in Section 5.

To improve comparability of the population flow measure across states of varying sizes and to facilitate interpretation, we divide "net population 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 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.[13]

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 single-person change-of-address outflows per 10,000 single-person households.[14] Furthermore, we analyze net permanent change-of-address outflows per 10,000 addresses and net temporary change-of-address outflows per 10,000 addresses.[15,16]

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. For comparison, we also consider two-way fixed effects estimates in our setting and, consistent with (Dench et al., 2024), find that they are substantially less precise but qualitatively similar to our SDID-based estimates.

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 systematic differences in outcomes between ban states and comparison states. Estimated effects are based on how outcomes change over time (post-Dobbs versus pre-

[13] 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).

[14] The number of family and single-person 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. Multi-generational, married-couple, and single-parent homes are included in the count of family households. A single-person (or "nonfamily" in the ACS data) household contains a householder living alone or with non-relatives. Unmarried-partner households are considered single-person households unless there is another person in the housing unit who is related to the householder by birth or adoption.

[15] 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.

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

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; instead, it selects a weighted set of control units that minimize differences in 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. 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—while limiting researcher degrees of freedom regarding selection of treatment and control units and choices regarding control variables.

Specifically, we use this approach to 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.[17] To estimate SDID event studies, we follow Clarke, Pailañir, Athey, and Imbens (2023) and use

[17]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 do not assume equal variance in

the difference between the treatment and control group in each period relative to the difference observed in the time-weighted pre-period.

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 the average net population outflow rates over time for ban states, for states protecting or maintaining abortion access weighted equally, and for states protecting or maintaining abortion access weighted using the SDID methodology described in Section 4.[18] We note that net outflow rates are consistently positive for each set 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.[19]

Critical to our SDID methodology, Figure 1 shows parallel pre-Dobbs trends for ban states and the synthetically weighted set of states protecting or maintaining abortion access. This includes the period of time during the pandemic but before the Dobbs decision (Q1 2020 to Q2 2022). Though this was a period of substantial changes in migration, the changes were quite similar for the ban states and the weighted set of comparison states. Thus, this evidence supports the notion that our SDID research design is reliable even in the face of pandemic-related migration changes.

The equally weighted set of abortion-protecting states provides useful additional context. Like the synthetic-weighted set of abortion-protecting states, they consistently exhibit higher net outflows compared to ban states. They also exhibited a substantial decline in outmigration during the pandemic. However, this decline was not as pronounced as it was in the ban states or the SDID-weighted set of comparison states. This pre-Dobbs divergence in trends observed in the equally weighted set of abortion-protecting states thus provides further support for our preferred approach using SDID.

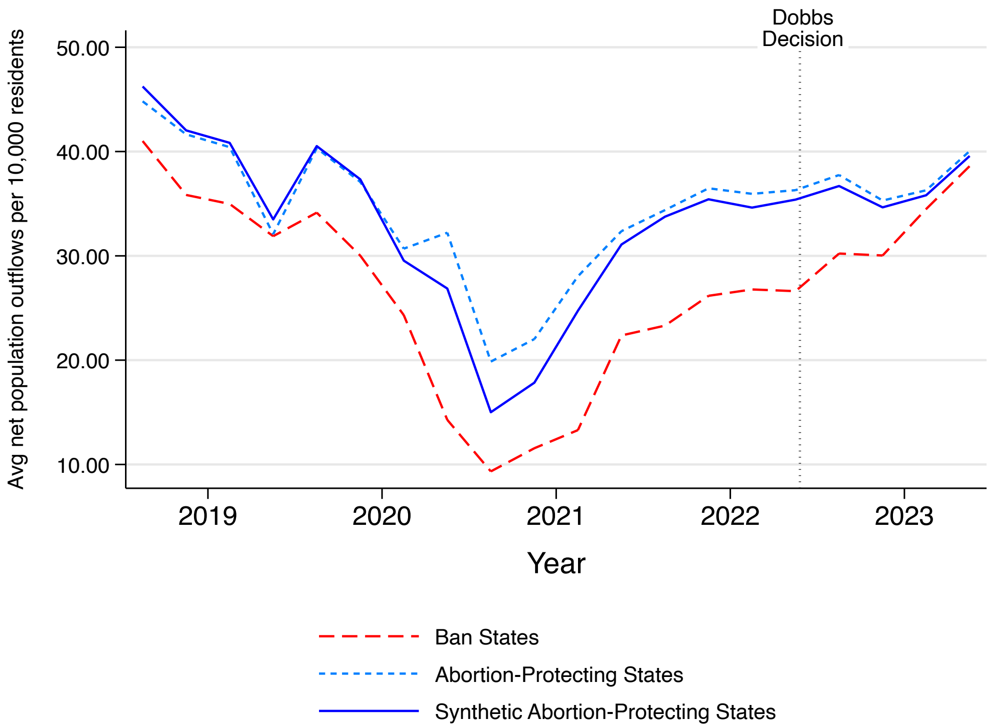
Figure 1 also provides evidence that abortion bans increase population outflows. Specifically, it shows a substantial convergence in the outflow rates in the immediate aftermath of the Dobbs decision, with ban states' outflow rates rising relative to those of abortion-protecting states. This convergence was sufficiently large that the two sets of states had nearly equal outflow rates in the most recent quarter for which data are available (the second quarter of 2023), despite the sustained difference observed over the years preceding the Dobbs decision.

treatment and control groups or equal time weights between treatment units and control groups.

[18]Weights are shown in Appendix Table A1.

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

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



Notes: This figure plots average seasonally adjusted trends in net population outflows per 10,000 residents over time. Separate series show averages for ban states weighted equally, abortion-protecting states weighted equally, and abortion-protecting states weighted using the synthetic difference-in-differences methodology described in Section 4. These weights are shown in Appendix Table A1. The 13 "ban states" and 25 "abortion-protecting states" are listed in Table 1 and discussed in Section 2. Net population outflow rates for each state-quarter are calculated as quarterly net population outflows divided by the 2018 population and multiplied by 10,000. Quarterly net population outflows for each state are estimated by multiplying the number of change-of-address requests by 1.7 to account for the average size of moving households. Each state's net population outflow rate is seasonally adjusted based on pre-Dobbs trends.

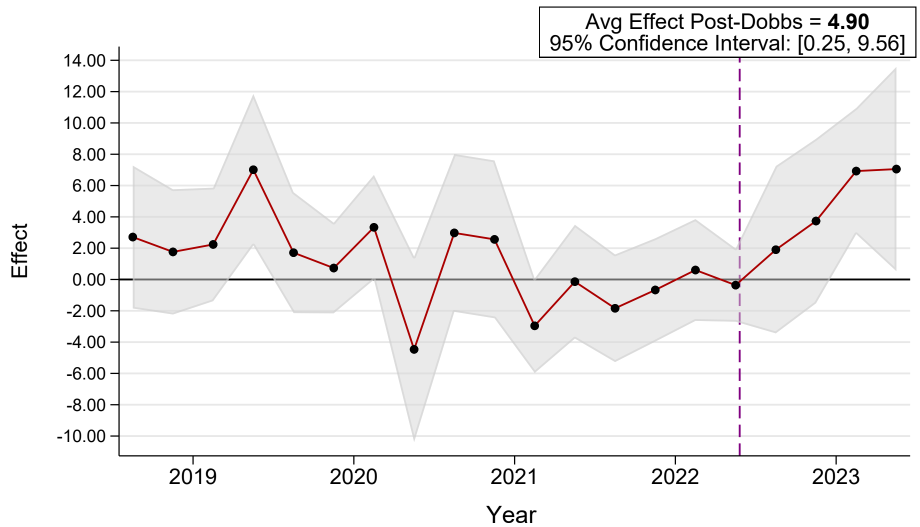
5.2 Main results

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

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 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 the number of change-of-address requests by 1.7 to account for the average size of moving households.

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 7.05 people per 10,000 residents quarterly. This corresponds to 52,600 people across the 13 states with total abortion bans in the immediate aftermath of the Dobbs decision.[20]

The average effect across all four quarters following the decision indicates that bans reduced states' populations by 4.9 people per 10,000 residents. This implies that abortion bans resulted in a net population loss of 146,300 residents across the 13 states with such laws in the year following the Dobbs decision.[21]

Demonstrating the robustness of our results, Appendix Figure A1 shows the results of a leave-one-out sensitivity analysis in which we sequentially omitted each state from the sample and re-estimated the post-Dobbs effect. The 38 resulting estimates are consistent with the estimate using all states (4.9), ranging from 3.8 to 5.7. Furthermore, most of the leave-one-out estimates are statistically significant at the 5% level and all are statistically significant at the 10% level.

We have also conducted "in-time placebo tests" to assess the validity of the SDID research design in

[20] This is calculated by multiplying the quarterly estimated effect of 7.05 people per 10,000 residents by the pre-treatment (2018) population across the 13 states with total abortion bans (74,651,967).

[21] This is calculated by multiplying the quarterly estimated effect of 4.90 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).

our specific setting. These in-time placebo tests consider placebo treatment quarters prior to the Dobbs decision to test whether our methodology finds evidence of "effects" where it should not. Specifically, we sequentially estimate "effects" using placebo treatment periods of Q2 2022, Q1 2022, Q4 2021, etc. on subsequent quarters prior to the Dobbs decision.[22] The results of these analyses are shown in Appendix Figure A2. Across all of these analyses we conduct, none produce effect patterns resembling our main results and none yield an average effect estimate that is statistically significant.

As we discussed in Section 4, prior research has demonstrated how SDID is preferable to two-way fixed effects in estimating the effects of abortion bans, both in terms of power and robustness. Along similar lines, in Figure 1 we showed that weighting the comparison states according to the SDID methodology improves the pre-Dobbs match in our setting. While these are strong reasons to prefer SDID to two-way fixed effects estimates in our setting, for comparison, we show two-way fixed effects estimates in Appendix Figure A3. These results are qualitatively similar to our main results, with point estimates that grow over time and an estimated average effect of 4.7 (versus 4.9 for our main results). However, consistent with the power analysis from Dench et al. (2024), that effect is much less precisely estimated with a standard error over 50 percent larger (at 3.86 versus 2.38 for SDID).

We have also assessed the robustness of our results to alternative approaches to calculating population flows. Our preferred approach calculates population flows as the number of change-of-address requests multiplied by the average size of moving households, where the average size of moving households is 1.7 based on information from Data Axle described in Ramani et al. (2024). We show the results from two alternative approaches in Appendix Figure A4. In one alternative approach, population flows are based on the number of family change-of-address requests multiplied by the average household size in 2018 (2.26) plus the number of single-person change-of-address requests.[23] This approach yields an average effect estimate that is statistically significant and similar in magnitude to our main result (at 4.76 vs 4.90). In another alternative approach, which is based on Ramani et al. (2024) and which we consider extremely conservative, we calculate population flows as the number of family change-of-address requests multiplied by the average household size of all movers (1.7) plus the number of single-person change-of-address requests. We deem this approach extremely conservative because one would expect families to have at least two people. However, it is possible for individuals moving by themselves to file family change-of-address requests (just as it is possible for individuals moving with their families to fill out single-person change-of-address request). Naturally, this

[22] We note that the validity of the SDID becomes more compromised as we consider earlier placebo treatment quarters because doing so reduces the number of pre-"treatment" periods of time. The consistency of SDID relies on the number of treatment units growing to infinity less slowly than the panel as a whole.

[23] The value of 2.26 represents the average population per family for individuals 18 and older in 2018, as reported in the Current Population Survey summary statistics in Census Historical Households Tables (Table HH-6. Average Population Per Household and Family: 1940 to Present), available at: https://www.census.gov/data/tables/time-series/demo/families/households.html.

conservative approach leads to smaller point estimates but the average effect (3.91) remains statistically significant.

5.3 Heterogeneity Analyses

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

Single-person 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.[24] Additionally, younger 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 have 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 family households in Panel A and estimated effects on net *single-person* change-of-address outflows per 10,000 single-person households in Panel B. Both sets of estimates are consistent with our main results in exhibiting a trend-break after the Dobbs decision. However, the magnitude of the effect and the extent to which it appears to be growing over time are much greater for individual movers.

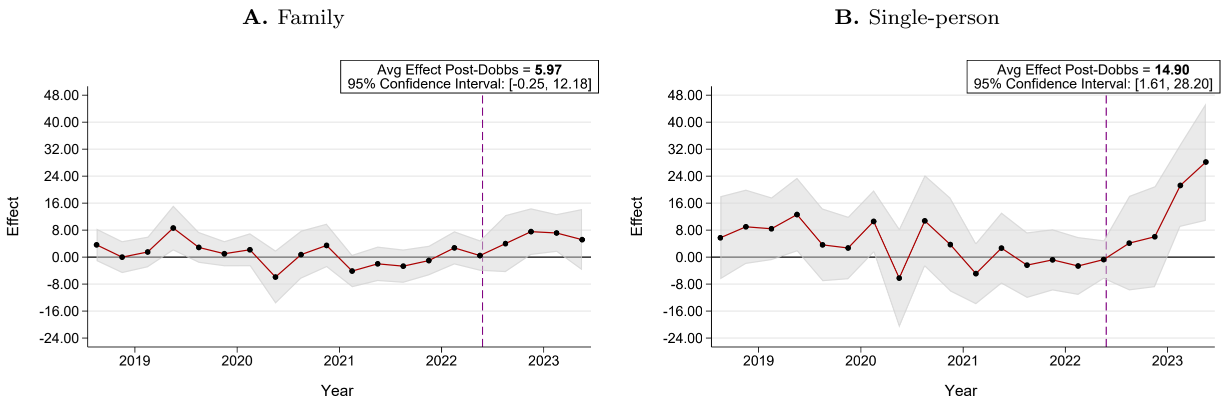
The estimated effects on net family outflows per 10,000 family households range from 4.0 to 7.6 across the four quarters following the Dobbs decision. The pattern of estimates offers some suggestive evidence that the effects rose from the first to second quarter following the decision and then declined; however, the estimates and their confidence intervals are also consistent with persistent and/or growing effects and the average effect across the four quarters is only marginally significant at the five percent level. That estimate indicates that bans increase quarterly net family outflows by 6 families per 10,000 family households. This corresponds to 11,000 families across the 13 ban states in each quarter following the Dobbs decision.[25]

In contrast, the estimated effect on net single-person outflows is approximately 5 per 10,000 single-person households in the first two quarters after the Dobbs decision, grows to 21 per 10,000 in the subsequent quarter,

[24] According to Pew Research Center (2024), 23 percent of those aged 18–29 say abortion should be illegal in all or most cases, versus 37 percent or more among older age groups.

[25] This is calculated by multiplying the quarterly estimated effect of 5.97 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).

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). The outcome variable in Panel A is quarterly net family outflows per 10,000 family households, and the outcome variable in Panel B is quarterly net single-person outflows per 10,000 single-person households, respectively. These are calculated based on separate counts of family and single-person change-of-address requests submitted to the US Postal Service, and using 2018 American Community Survey estimates of the number of family and single-person households in each state.

and grows further to 28 per 10,000 in the subsequent quarter. Together, these estimates suggest that abortion bans cost the states that implemented them a total of 54,770 individuals living in single-person households in the year after the Dobbs decision.[26] The most recent estimate suggests an effect of 25,700 individuals quarterly.[27]

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. To do so, we make use of separate counts of temporary and permanent change-of-address requests as described in Section 4. The results, shown in Appendix Figure A5, indicate that our estimated effects on migration are driven entirely by permanent moves.

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 roughly 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

[26] This is calculated by summing the estimated effects of single-person households across the four quarters (59.6 per 10,000), multiplied by the pre-treatment (2018) number of single-person households across the 13 states with total abortion bans (9,189,596).

[27] This is calculated by multiplying the quarterly estimated effect of 28 per 10,000 single-person households by the pre-treatment (2018) average number of single-person households across the 13 states with total abortion bans (9,189,596).

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

	(1) Population	(2) Family	(3) Individual
Panel A: Total Bans as Treatment			
Estimated Effect (Quarterly)	4.90**	5.97*	14.90**
	(2.38)	(3.20)	(6.81)
Observations	760	760	760
Panel B: Abortion Hostility as Treatment			
Estimated Effect (Quarterly)	4.31	7.54**	11.78
	(3.10)	(3.62)	(8.03)
Observations	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 residents. Columns (2) and (3) report effects on net family and net single-person outflows per 10,000, respectively.
* p<0.1, ** p<0.05, *** p<0.01.

compared to our estimated effects of total bans. For example, the estimated effect of abortion hostility on the net population outflow rate is 4.3 per 10,000 (compared to 4.9 per 10,000 for bans), though the estimate is not statistically significant at the five percent level. That said, the estimated effect on family-household outflow rates is statistically significant at the five-percent level. The estimated effect on single-person household outflow rates is larger in magnitude than this estimated effect on family households, but it is not close to being statistically significant.

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 1.41% population loss for states banning abortion as opposed to protecting or maintaining abortion access.[28] 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).

[28]Based on the effect in the second quarter of 2023 (7.05 per 10,000) multiplied by 20 quarters.

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 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.

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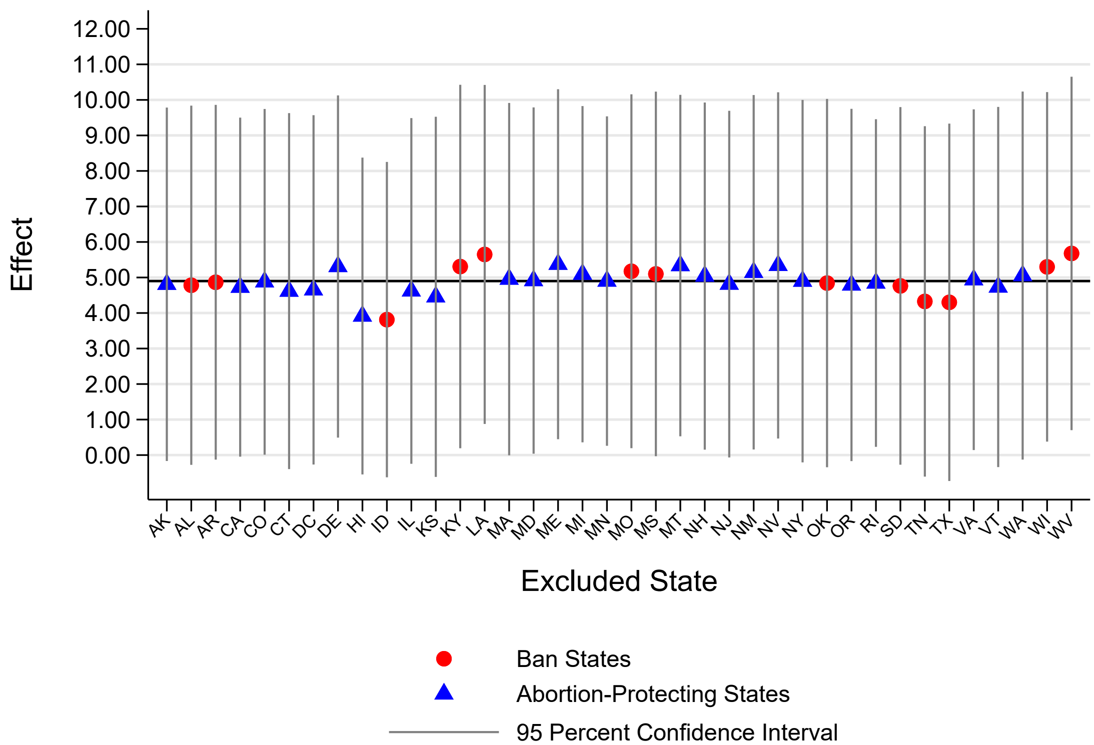
Appendix

Table A1
Unit Weights Associated with Main Results (Figure 2)

State	Weight
DC	0.023
New York	0.027
Massachusetts	0.031
Washington	0.031
Oregon	0.032
Colorado	0.033
Maine	0.036
Minnesota	0.036
New Jersey	0.037
Nevada	0.038
Alaska	0.038
Rhode Island	0.038
California	0.038
Maryland	0.040
Michigan	0.041
Illinois	0.042
Virginia	0.042
New Hampshire	0.042
Connecticut	0.043
New Mexico	0.043
Delaware	0.046
Vermont	0.049
Montana	0.056
Kansas	0.058
Hawaii	0.059

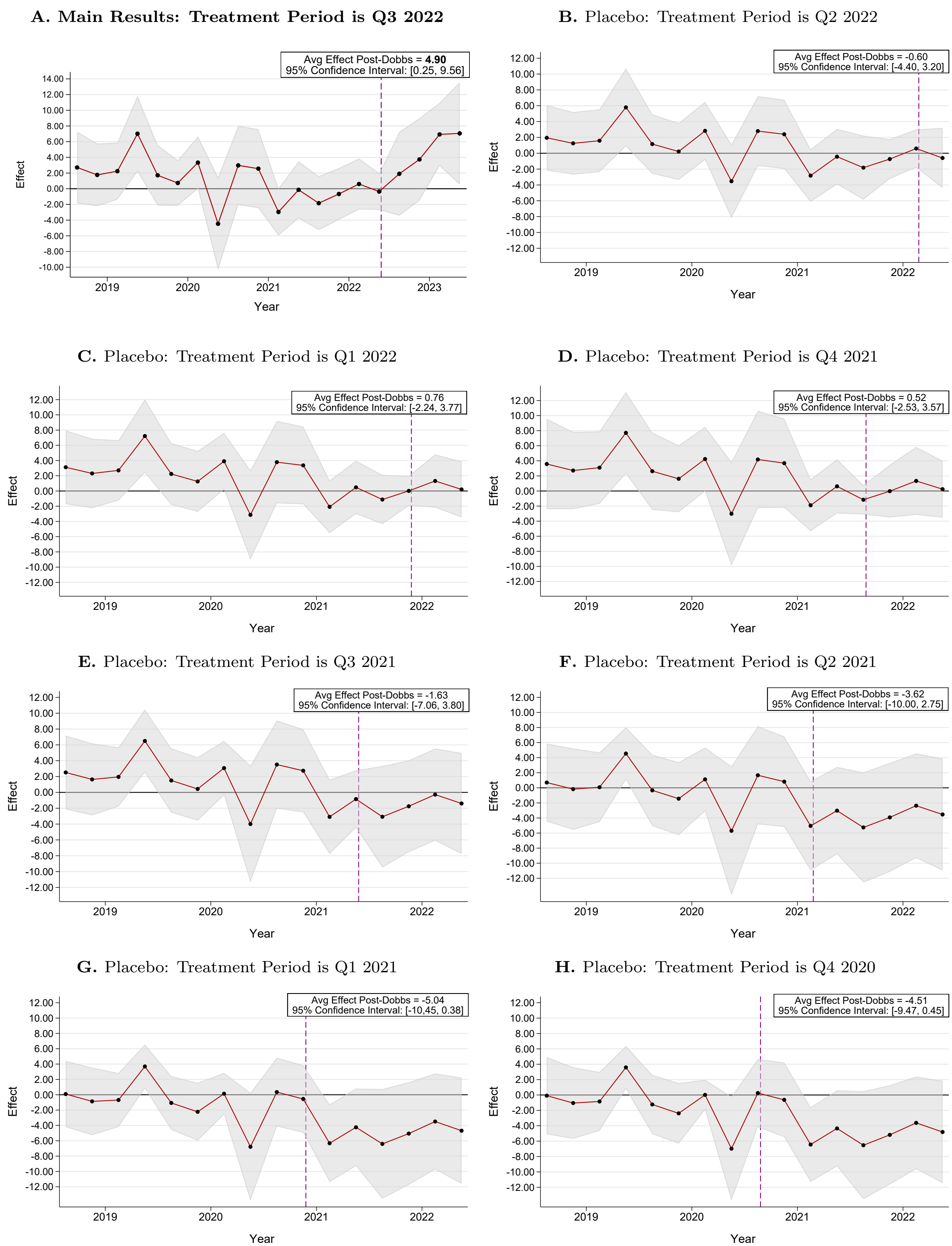
Notes: The table shows the unit weights from Synthetic difference-in-differences implemented using Clarke et al. (2023) for the event study in Figure 2 on the effect of bans on net population migration outflows. Columns are sorted by the model weights.

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



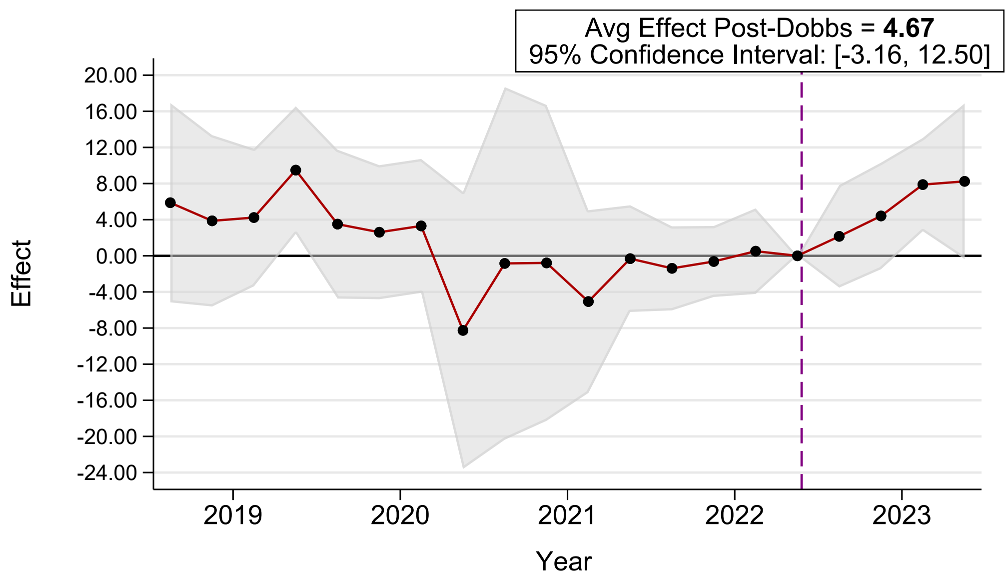
Notes: This figure presents the results of our leave-one-out sensitivity analysis, in which we sequentially omit each state from the sample and re-estimate the post-Dobbs effect on net population outflows per 10,000. Weights are recalculated after leaving out each state before estimating. Spikes extending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

Figure A2
In-time Placebo Tests for Estimated Effects on Net Population Outflow Rates



Notes: Panel A reproduces Figure 2 for comparison, while panels B through H show results of in-time placebo tests examining estimated "effects" using varying placebo treatment quarters.

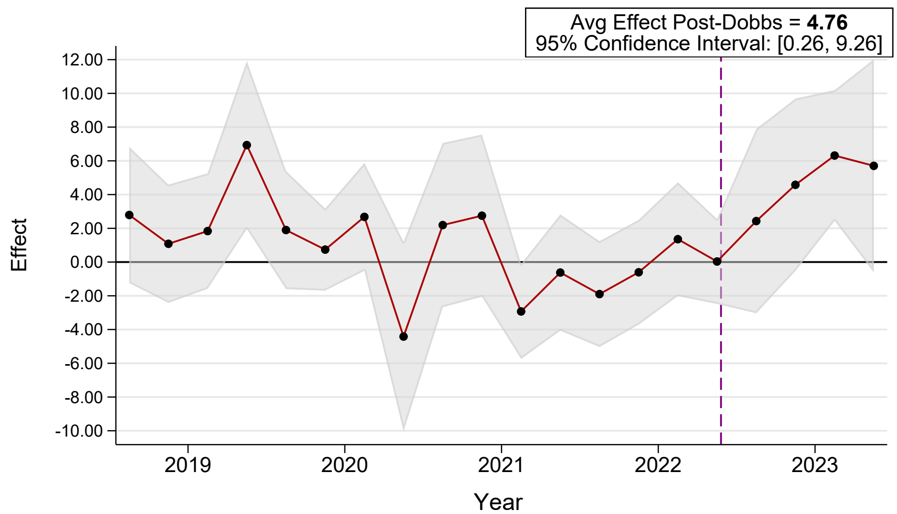
Figure A3
Two-way Fixed Effects Estimates,
Effect of Abortion Bans on Net Population Migration Outflows (per 10,000 residents)



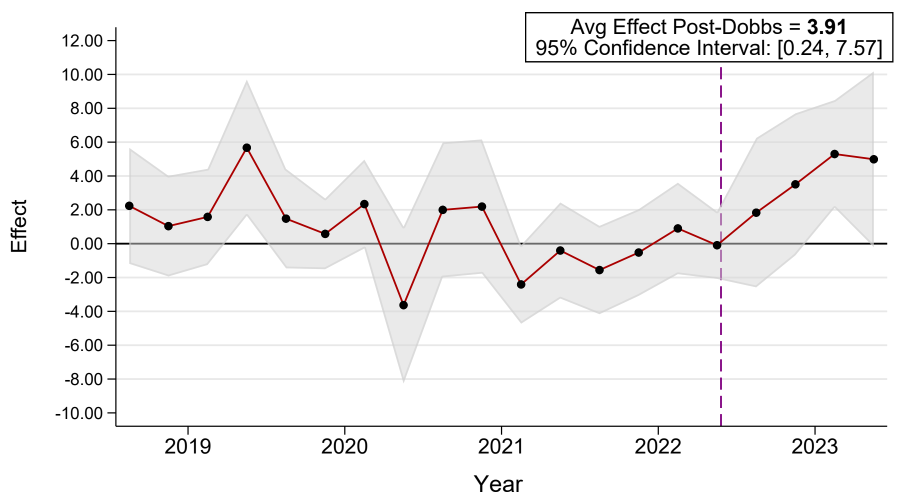
Notes: This figure presents two-way fixed effects event-study estimates of effects on net population outflows, with Q2 2022 as the reference period, ban states as the treatment group, and states maintaining or protecting abortion access as the comparison group. Standard error estimates are clustered at the state level. See notes to Figure 2 for additional details.

Figure A4
Effects of Abortion Bans Using Alternative Measures of Net Population Outflows

A. Population Moving = (Family COA Requests × 2.26) + Single-person COA Requests



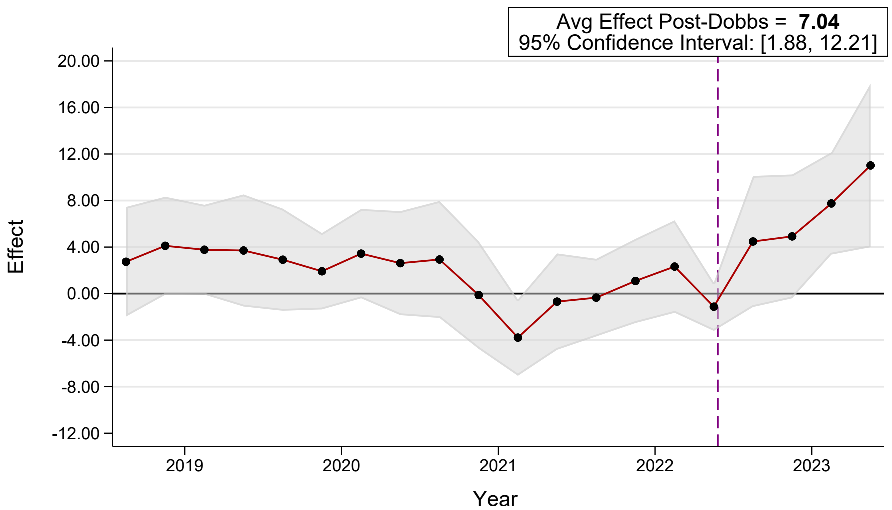
B. Population Moving = (Family COA Requests × 1.7) + Single-person COA Requests



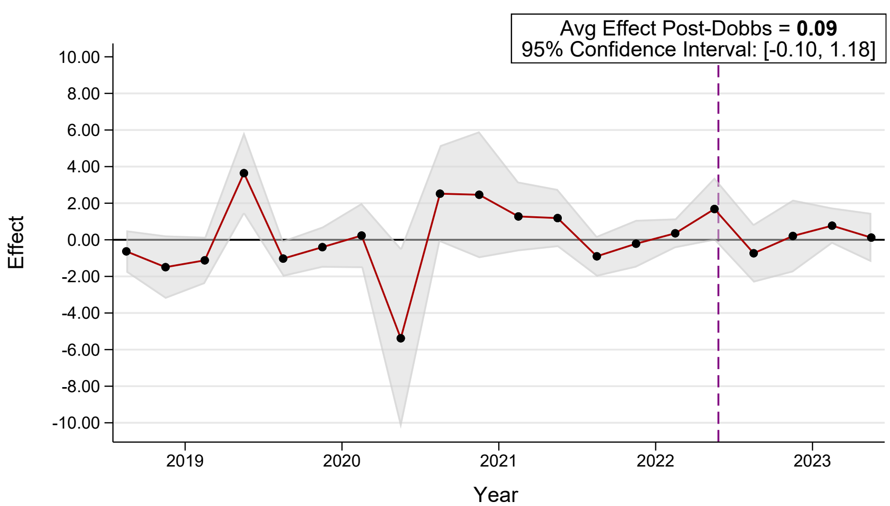
Notes: This figure presents quarterly synthetic difference-in-differences estimates and 95 percent confidence intervals obtained using block bootstrap inference as outlined in Arkhangelsky et al. (2021). In Panel A, quarterly net population outflows for each state are calculated as the number of family change-of-address requests multiplied by the average family size (2.26) according to the Census Historical Households Tables) plus the number of single-person change-of-address requests. In Panel B, quarterly net population outflows for each state are calculated as the number of family change-of-address requests multiplied by the average household size of all movers (1.7) plus the number of single-person change-of-address requests.

Figure A5
Effects of Abortion Bans on Additional Different Mover Types

A. Permanent



B. Temporary



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). The outcome variable in Panel A is net permanent change-of-address outflows per 10,000 addresses, and the outcome variable in Panel B is net temporary change-of-address outflows per 10,000. 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.