How US interstate migration responds to political shocks*

Jonas Adolph and Knut Niemann

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Abstract

We study how county-level migration in the US responds to political state-level shocks. To that end, we examine how the state-level outcomes of toss-up gubernatorial elections affect county-level out-migration. For these toss-up elections, we distinguish counties with a majority for the losing candidate (treatment) and the winning candidate (control). We then show that there are no significant differences in the number of emigrants between treated and control counties pre-election. Post-election, emigration is five percent higher in treated counties. We find that this is mainly driven by increased out-migration from Democratic counties after a Republican win. We rationalize these results in a Roy-Borjas-type selection model with individuals who differ in political views. The model predicts bilateral migration decreases in political distance, which we confirm empirically.

*Adolph: University of Tübingen, jonas-daniel.adolph@uni-tuebingen.de, Niemann: University of Tübingen, knut.niemann@uni-tuebingen.de. We thank Gernot Müller, Peter Eppinger, Wilhelm Kohler, and participants of the RSIT Brown Bag Seminar (Tübingen) and the THE Workshop (Hohenheim) for useful comments. Elias Teutschländer and Selami Doğan Akansu provided excellent research assistance.

1 Introduction

Close gubernatorial elections in the United States are a shock to the location choice problem of up to half of the state's population. These voters are exposed to changes in the political environment that do not align with their preferences. However, since migrating is costly, it is unclear if contested elections suffice to cause out-migration to other US states.

We empirically address this question by comparing out-migration from counties where a majority supported the losing candidate (hereafter, losing counties) to out-migration from counties where a majority supported the winning candidate (winning counties). First, we identify 22 gubernatorial midterm elections that provide exogenous changes in the local political environment. For these elections, we construct a novel panel on county-level out-migration to other US states derived from address changes in income tax declarations.

Using this panel, we compare the change in out-migration from losing counties to out-migration from winning counties in the same state, four years before each election until eight years after. In the years before contested elections, differences are generally not significant, providing evidence in favor of our identifying assumption. In the years after the election, out-migration into any destination increases significantly and persistently by around ten percent. This effect is robust to alternative specifications. We find the strongest increase for out-migration into strongholds of the losing candidate.

To rationalize our empirical findings, we develop a migrant selection model in the spirit of Borjas (2014), where individuals differ by political preferences rather than skills. Migration occurs between an origin state with two counties—one liberal, one conservative—and a generic destination state, with state-level policies shaping utility. Individuals derive higher utility when local policies align with their views, making location-specific policies analogous to returns to skills in the classic framework. The model predicts that (i) migration rises more from counties that backed the losing candidate, (ii) state-to-state migration declines as political distance widens, and (iii) migration deepens partisan polarization. We document this mechanism descriptively, showing that the share of counties won by presidential candidates in landslides increased substantially over time. Finally, we show evidence supporting the prediction that state-to-state migration falls with political distance. This result is robust to alternative specifications.

More in detail, we first provide a brief background on the political powers of US governors, which directly or indirectly impact nearly all state-level policies, from taxation and public spending to gun regulation and abortion rights. Afterwards, we distinguish two types of counties based on their voting behavior in gubernatorial elections. Counties with a majority for the election winner are winning counties and those with a majority for the election loser are losing counties. While people from winning counties face a governor who campaigned on policies they preferred, people in losing counties now face policies they opposed and might have an incentive to move to states where policies are more aligned with their preferences.

Using county-level voting data by Algara and Amlani (2021) on gubernatorial elections between 1994 and 2014, we identify 22 contested gubernatorial mid-term elections that are sufficiently large for meaningful comparisons between winning and losing counties. In particular, we require the current election to have a winning margin below five percentage points, while the previous and next elections in that state must have a winning margin above five percentage points. This way, we focus on episodes in states where the narrow-margin win was the only shock in a period from four years before until eight years after the election.

These elections cover a total of 51 million votes cast and involve some 1,500 counties. In total, there are 841 winning counties and 648 losing counties. Ten of these elections were Republican wins, and the remaining eleven were Democratic wins.

For migration, we use county-to-county data as provided by the IRS, which are based on address changes in individuals' income tax statements. The data cover interstate migration from 1990 to 2021 and give the number of individuals and households migrating from one county to another.

For each contested election, we compute total out-migration by year and county and stack these panels across elections for a stacked difference-in-differences (DiD) approach that compares migration in losing counties (treatment group) to winning counties (control) relative to the year before each election. The estimation uses the Poisson Pseudo Maximum Likelihood (PPML, see also Silva and Tenreyro 2006; Correia et al. 2020)

Identification is based on the simple idea that, absent the surprise election, migration trends in losing and winning counties would have remained parallel for all counties in the state. For the pre-treatment period, this is confirmed by the general lack of significance among point estimates. After the election, however, we detect a delayed but significant increase in out-migration from losing counties into any other destination state. Specifically, eight years after contested elections, out-migration has increased by around ten percent relative to the gap between winning and losing counties and compared to the baseline period.

Next, we distinguish destinations based on their political preferences as revealed in the current and previous two gubernatorial elections. States where the winner of all three elections was Democratic or Republican are strongholds for that party and swing states otherwise.

Estimating our stacked DiD approach for out-migration into these three types of destinations, we find interesting heterogeneity. After contested elections, out-migration from losing counties increases most clearly into strongholds of the election loser, but less so into swing-states and strongholds of the election winner. Put succinctly: after contested elections, people decide to move to destinations where the losing party has a strong base.

This result is robust to numerous variations of the baseline specification. Specifically, it holds when measuring migration by the number of individuals rather than households. It also holds when using presidential elections to classify destination states or a less stringent definition of contested elections.

For additional validation of this intuitive mechanism, we also consider elections that were not contested and find that these expected election outcomes do not increase out-migration from losing counties.

Our empirical analysis concludes by examining heterogeneity across winning parties and over time. We find that the migration response is strongest after Republican wins, that is, when losing counties have a Democratic majority. After Democratic wins, the increase in outmigration from Republican counties is weaker and frequently insignificant. Differences in average homeownership shares and educational attainment among Republicans and Democrats might explain this finding. We also find that the migration effects of contested elections were more pronounced during the 1990s and 2000s, and mostly insignificant during the 2010s.

To rationalize these empirical results, we introduce a migrant selection model in the spirit of Borjas (2014), where individuals differ not in their skills but in their political views. To remain close to the empirical setting, we consider migration between a generic destination state and an origin state consisting of two counties. These locations differ in the average political preferences of their residents, while policies are determined at the state level.

Individuals derive utility from residing in locations whose policies align with their own political views. Consequently, utility is higher when local policies are congruent with an individual's political orientation. In this framework, location-specific policies play a role analogous to location-specific returns to skills in the standard Borjas model.

Individuals choose to reside in the location that offers the highest returns to their political preferences, net of potential migration costs. This decision rule allows us to identify a cut-off in the distribution of political orientation: the individual who is indifferent between migrating and staying. Given the distribution of political views in the population, we can then derive the share of individuals for whom migration is utility-maximizing.

We exploit this framework in three ways. First, we assume that one county is predominantly liberal while the other is predominantly conservative. In a competitive election contested by at least one liberal and one conservative candidate, the majority in one county will have voted for the winner, while the majority in the other county will have supported the losing candidate. We model the election outcome as a change in policies at the state level. The model predicts, as we find empirically, that the share of migrants increases more in the county that voted for the losing candidate. This result stems from the fact that individuals who opposed the new policies are more likely to experience a decline in utility and thus have a higher incentive to leave.

Second, the model predicts that aggregate state-to-state migration flows decrease as the political difference between the origin and destination states increases, where political difference is defined as the absolute difference in policy orientation between states. This prediction is tested empirically in the gravity analysis.

Third, we use these insights to shed light on the growing political polarization across U.S. states. Following an election, individuals tend to relocate to states whose policies are more closely aligned with their own political views. This process reinforces existing partisan divides by shifting the average political preference in each state toward the incumbent's ideology. We provide descriptive evidence consistent with this mechanism by examining the distribution of counties won by a presidential candidate in a landslide (margin $> 20\%$) in 1992 and 2020.

In the gravity estimation section, we test the theoretical prediction that bilateral migration flows decrease as political distance between states increases. To measure political distance, we draw on data from the Manifesto Project (Lehmann et al.

2023a; Lehmann et al. 2023b), which provides a unified measure of presidential candidates' ideological positions based on their manifestos. This dataset assigns each U.S. presidential candidate from 1992 to 2020 a score on a right–left scale, where positive values indicate conservatism and negative values liberalism.

Using these data, we document that the ideological distance between Democratic and Republican presidential candidates remained relatively stable until 2008 and has since widened markedly, reaching a thirty-year high in 2020. Over the same period, we also observe an increase in the variation of average conservatism across states, further reflecting the rise in political polarization. To compute state-level political orientation, we calculate the weighted average of presidential candidates' conservatism scores, using their vote shares in each state as weights. Bilateral political distance between states is then defined as the absolute difference in these weighted averages.

Our baseline gravity model includes the logarithm of the distance between state capitals and a contiguity dummy to capture bilateral migration costs. Following Bertoli and Fernández-Huertas Moraga (2015), we also include origin-time and destination-time fixed effects to control for time-varying push and pull factors influencing migration. The dependent variable is the bilateral migrant flow in the year following each election. This approach serves two purposes: first, since elections take place in November, using the subsequent year ensures that observed migration predominantly follows the election; second, migration occurring before the election could itself affect election outcomes, so excluding it helps avoid endogeneity concerns.

Consistent with the model's prediction, we find that greater political distance between states is associated with lower bilateral migration flows. This suggests that individuals are more likely to migrate between states whose populations are politically more similar. As a robustness check, we include a set of historical dummies indicating whether the pair of states were aligned during the Civil War. The rationale is that historical alignment may shape contemporary political preferences and could also affect migration flows due to persistent social or cultural factors. Our main result remains robust to the inclusion of these controls.

The paper is structured as follows. The remainder of the introduction discusses the related literature. Section 2 provides some institutional background and introduces the data. In Section 3, we present our empirical framework and results. Section 4 rationalizes the empirical result in a migration selection model, and 5 empirically confirms predictions of the model. A final section offers a brief conclusion.

Related literature Our work is connected to three strands of literature. First, it contributes to the literature that uses difference in differences methods in the context of migration. For example, Card (1990) uses the unexpected inflow of Cuban migrants into the Miami labor market due to the Mariel boatlift as a natural experiment to measure the labor market impacts of immigration. More recently, Borjas (2017) has re-examined Card's Mariel study using a difference-in-differences approach to identify the labor market effects of the boat lift. He notes that it is key that the control cities are chosen based only on similarity to Miami *before* the treatment. Regarding the use of natural experiments in a migration context, Becker et al. (2020) argue that the separation of Germany into a capitalist West and a communist East does not serve as a proper natural experiment because, in many socio-economic characteristics, the latter border is already visible in pre-war data.

Second, our work is related to the vast body of literature that uses gravity models to study determinants of migration flows. Beine et al. (2016) offer an overview of the existing gravity-in-migration literature. They provide a theoretical motivation for the use of gravity estimation using a random-utility-maximization model with different distributions of the random component of utility. Additionally, Beine et al. (2016) also list challenges that can be encountered when using gravity to estimate the migration flows. Bertoli and Fernández-Huertas Moraga (2015) use an estimation approach similar to ours to estimate the effect of bilateral visa policies on the flows of international migrants. They use origin-time and destination-time fixed effects that control for potential push and pull factors at the location-time level. Olney and Thompson (2024) study how gaps in wages and house prices impact migration between commuting zones in the US.

Third, our model is related to the Roy-Borjas type of migrant selection model. Borjas (1987) introduced a Roy (1951)-type model to examine which part of the skill distribution in a location is incentivized to select into migration. This model has been widely applied in the literature (Borjas 1991; Borjas et al. 1992; Grogger and Hanson 2011; Ambrosini and Peri 2012). Borjas et al. (1992) apply such a Roy-type model to study the selection of internal migrants within the US. Potential migrants are heterogeneous in the skills they are endowed with, so that the individuals select themselves into migration such that they reside where the returns to their specific skills are highest.

2 Institutional background and data

This section provides some institutional background on the powers of governors and why we consider narrowly won gubernatorial elections to be policy shocks. Afterwards, we discuss the county-level data on voting and migration flows.

2.1 Institutional background

Governors are the chief executives of US states. Although most statutory changes must be enacted by state legislatures, nearly every major statewide policy—taxation, public spending, gun regulation, abortion access—either originates in the governor's executive budget or ultimately depends on the governor's signature or veto. Hence, the results of contested elections provide exogenous shifts in state-level policies.

The 2014 gubernatorial election in Kansas illustrates the stark policy contrast that can hinge on a small share of statewide votes. The Republican incumbent Sam Brownback campaigned on deepening income tax deductions and lowering the top state income-tax rate, as well as opposing abortions and Medicaid expansions, while strongly supporting gun-rights. His Democratic challenger, Paul Davis, campaigned on opposing further tax cuts, increasing education spending, protecting abortion rights and openness to introduce background checks on firearm purchases. The Republican Brownback won the election with 50 percent of the votes over the Democrat Davis with 46 percent.

While the policies that were implemented after the election apply to people in all counties of Kansas equally, people in counties with a Democratic majority revealed preferences for Democratic policies, but would now be subject to the Republican ones. Hence, by comparing out-migration from Democratic counties to that from Republican ones and distinguishing different types of destinations, we can identify the causal effect of the election outcome. While we do not investigate which components of the policy portfolio drive potential effects, we consider our results as an average over all policies that change after close elections.

Moving beyond this specific example, we label counties with a majority for the losing party as losing counties and consider them as a treatment group. Counties with a majority for the winning party are considered winning counties, and we think of them as the control group.

2.2 Voting data

The starting point for identifying suitable contested elections are the county-level voting records provided by Algara and Amlani (2021). We start with the list of all gubernatorial midterm elections that took place between 1994 and 2016, ensuring sufficient migration data is available for years both before and after elections.[1] We focus on midterm elections to ensure that there are no simultaneous shocks brought about by presidential elections. From these, we consider only those with at least five counties in both the winning and the losing groups. This ensures that comparisons of losing and winning counties within a given state are not based on a small number of counties.

For every election, we compute the Democratic margin as the difference in total votes for the Democratic candidate v_e^{dem} minus those for the Republican candidate v_e^{rep} divided by their sum:

$$m_e^{dem} = \frac{v_e^{dem} - v_e^{rep}}{v_e^{dem} + v_e^{rep}}.$$

It is positive whenever the Democratic party (or an independent candidate supported by the party) won the election, and negative otherwise. We refer to the absolute value of m_e^{dem} as the winning margin.

For identification, we focus on episodes where the current election is contested with a winning margin below five percent but the previous and next elections in the state are not close ($\text{abs}(m_e^{dem}) > 0.05$).[2] Episodes where no elections are close are taken as placebo events, since they lack the election shock. We consider them in Section 3.4.

22 elections have a state-level winning margin of less than five percentage points. They constitute our preferred list of contested elections, and we present some descriptive statistics in Table 1. In these elections, a total of 51 million votes were cast across approximately 1,500 counties. 841 counties had a majority for the winning party (control) and 648 for the losing party (treatment). Ten elections were won by Republican candidates and eleven by Democratic candidates. On average, there are 2.3 million votes cast per election, and the Democratic margin is 0.2 percent, again

[1] We omit elections in Alaska (frequent changes in county lines) and Vermont (where gubernatorial elections take place every two years, thus frequently coinciding with years of presidential elections).

[2] Our results are unchanged when not conditioning on the next election being close. See the discussion in Section 3.3.

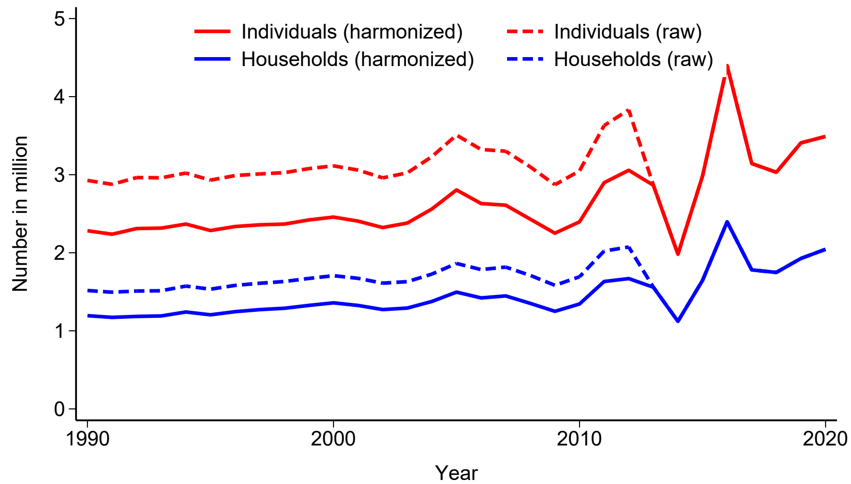
Table 1: Descriptive statistics of contested elections

Year	State	Margin	Votes	Control counties	Treated counties
1994	FL	1.5	4.2	25	42
1994	GA	2.1	1.5	84	75
1994	NY	-3.3	5.2	57	5
1994	SC	-2.5	0.9	23	23
1998	CO	-0.6	1.3	41	22
1998	IL	-3.6	3.4	59	43
2002	AL	-0.2	1.4	33	34
2002	AZ	1.0	1.2	8	7
2002	CA	4.9	7.5	18	40
2002	MI	4.0	3.2	27	56
2002	OK	0.7	1.0	51	26
2002	OR	2.9	1.3	8	28
2002	TN	3.1	1.7	54	41
2002	WI	3.7	1.8	44	28
2002	WY	2.0	0.2	8	15
2010	OH	-2.0	3.9	61	27
2010	OR	1.5	1.5	7	29
2010	SC	-4.5	1.3	21	25
2014	CO	3.3	2.0	25	39
2014	KS	-3.7	0.9	98	7
2014	MA	-1.9	2.2	7	7
2014	MI	-4.1	3.2	70	13
Sum			50.6	829	632
Average		0.2	2.3	38	29

Notes: Year is the election year, and state is the abbreviation of the state where the election took place. Margin is the number of votes for the democratic candidate minus those for the republican candidate divided by the total number of votes (labeled votes in the table), and control and treated counties give the number of treated and control counties in the election.

highlighting just how contested these elections were.[3] As the map in Figure A.2 shows, contested elections are spread relatively evenly across the US, with all major regions featuring some such elections during the sample period.

Figure 1: Total interstate migration



Notes: The figure shows the total number of individuals and households moving from one state to another every year. Solid lines use a harmonized reporting threshold of at least 20 households and dashed lines show the raw data where the reporting threshold was 10 households until 2011.

2.3 Migration data

We use county-to-county migration data as provided by the IRS (also discussed, for example, in Hauer and Byars (2019) and Olney and Thompson (2024)).[4] The data are based on the changes in the addresses reported in individuals' income tax statements and cover the years from 1990 to 2021. They report the number of returns filed (approximating the number of households that migrated) and the number of personal exemptions claimed (the number of individuals that moved) at the bilateral county level. We focus on interstate migration, that is, migration from a given county to any other county in a different state.

In 2011, data compilation underwent some changes. In particular, the reporting threshold for flows was increased from 10 households to 20. We account for this change by considering only flows (in terms of households and individuals) from at least 20 households (and setting those below 20 to zero. As Figure A.1 shows, this reduces the total number of individuals and households covered by around 10 percent each year

[3]These contested elections are fairly similar to placebo elections (with a winning margin above five percent) that are summarized in Table A.1. Both types of elections feature around two million votes per election, on average.

[4]The data and additional documentation by the IRS are available at `https://www.irs.gov/statistics/soi-tax-stats-migration-data`.

before 2011.

We discard observations for within-state migration, since we study how contested elections impact out-migration. [5] Figure 1 shows total out-migration both in terms of individuals and households. Approximately 3 million individuals (or 1.5 million households) move across state borders each year. Growth in both numbers is relatively moderate at 1.2 and 1.6 percent per year. However, there has been a noticeable increase in volatility since 2011. Our main results are based on migration flows in the years before the increase in volatility.

3 Empirical results

3.1 Estimation framework

We employ a stacked Difference-in-Differences estimator to estimate the causal effect of surprise election outcomes on migration. To set the stage, consider a single contested gubernatorial election taking place in year t_1 in a specific state. For every country c in that state, we define an ever-treated indicator D_c equal to one for losing counties (with a majority for the losing candidate) and equal to zero for winning counties (with a majority for the winning candidate). Next, we consider the period from $J = 4$ years before the election until $K = 8$ years after and track out-of-state migration from every country into different types of destinations, denoted $d \in \{\text{any, winner, swing}, \text{loser}\}$.

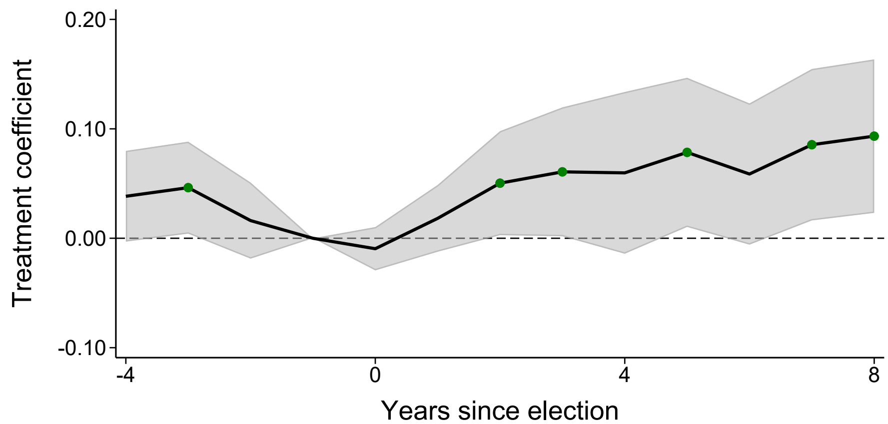
To account for all 22 contested elections indexed by e that take place in different years t_e between 1994 and 2014, we simply stack the per-election regressions and obtain our baseline specification:

$$y_{ect}^{d} = \exp\left(\alpha_{et} + \alpha_{ec} + \sum_{j \in \mathcal{J}} \beta_j^d \cdot \mathbf{1}(t - t_1 = j) \cdot D_{ec}\right) \cdot \varepsilon_{ct}^{d}, \tag{1}$$

where y_{ect}^{d} is out-of-state migration from county c in year t into destinations of type d measured by the number of households, and α_{et} and α_{ec} are state- and county-fixed effects. β_j^d are the dynamic treatment effects of interest for every period in $\mathcal{J} = \{-L, \ldots, K\} \setminus \{-1\}$, where we omit -1 as a baseline period. The β_j-coefficients measure the change in out-migration between winning and losing counties in period j relative to the baseline period.

[5] For within-state migration, the county of destination is typically not reported in detail.

Figure 2: Out-migration into any other state



Notes: Treatment effects and 95 % confidence intervals from the baseline specification. The dependent variable is the number of households migrating to any other US state. The estimation sample features 24 contested gubernatorial elections. Treatment takes place in (relative) year 0 and is a majority for the candidate who loses the election. The pre-election year (-1) is the baseline period. Dots indicate significance at the 5 % level. Full results are in Table A.2.

For the estimation, we use the Poisson Pseudo Maximum Likelihood (PPML) estimator (Silva and Tenreyro 2006; Correia et al. 2020). It allows us to keep observations where the dependent variable is zero and yields unbiased results under heteroskedasticity, unlike using OLS for the log-linearized version of the baseline specification. Throughout, standard errors are clustered by county and election.

Our identifying assumption is that absent the surprise election, migration trends in losing and winning counties would have remained parallel within each state–election pair. We validate this by considering the significance of the pre-treatment coefficients.

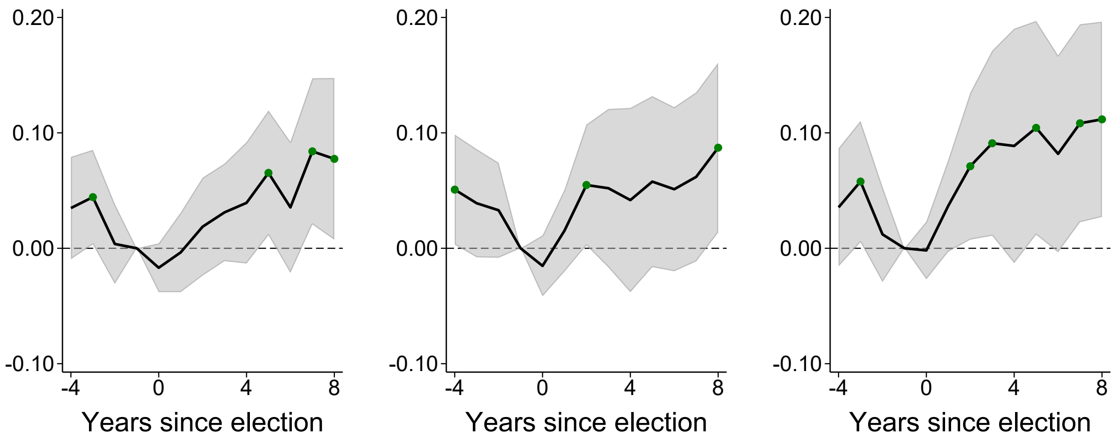
3.2 Baseline results

First, we estimate the baseline specification for outflows into any destination. Later, we zoom in on different types of destinations to investigate the causal mechanism. Results for out-migration into any destination state are in Figure 2.

In years before the election, treatment coefficients are generally insignificant, with only β_{-3} significant at the 5 % level. After the election, out-migration from losing counties increases compared to winning counties and the baseline period. These increases turn significant two years after the election and reach 10 percent eight years

Figure 3: Out-migration by destination type

(a) Strongholds of the winner (b) Swing states (c) Strongholds of the loser



Notes: Results of regressions by destination type. Treatment effects and 95 % confidence intervals from the baseline specification. Green dots additionally highlight estimates that are significant at the 5 % level. Destinations are classified based on their voting behavior in the year-0 election and the two previous ones. Full results are in Table A.2.

after the election. This indicates a persistent increase in out-migration from losing counties. The lagged response could imply that out-migration does not respond to the election itself, but to policy changes implemented afterwards. It could also be explained by the general time it takes for individuals to arrive at their migration decision and the time it takes to find and move to a suitable destination.

Next, we distinguish destination states by their political orientation as revealed in previous gubernatorial elections in those destinations.[6] When the election winner of the current and previous two elections was a Democrat (Republican), we consider that destination a stronghold of that party. Otherwise, the destination is considered a swing state.

This allows closer investigation of which destinations become more attractive to people in losing (treated) counties. For the estimation, we construct separate panels that aggregate either outflows into strongholds of the election winner, the election loser, or swing states and re-estimate our baseline specification for each panel. The results are in Figure 3.

They show that after contested elections, out-migration from losing counties into all

[6]As an alternative, we also consider voting behavior in presidential elections. See Section 3.3.

types of destinations increases compared to winning counties, with some heterogeneity across destination types. In particular, the effect is weaker for destinations that are strongholds of the winning party and swing states (Figures 3a and 3b). The strongest effects are found for destinations that are strongholds of the losing party (Figure 3c). For these destinations, treatment effects are significant in five out of eight post-treatment years. This shows that when people from losing, say Democratic, counties move after contested elections, they predominantly choose destinations where that party has a firm base.

3.3 Robustness

To establish robustness of the baseline results, we turn to alternative specifications that vary key aspects of the baseline. The results are in Figure 4.

They show that the results from the baseline specification also hold when measuring migration by the number of individuals instead of by the number of households (Panel 4a), and when using presidential elections to classify destination types (Panel 4b). Ultimately, our results also hold when using a less strict definition of contested elections. In Panel 4c, we no longer require the post-treatment election to be close. Hence, we consider all cases where the previous election was not close, but the current one is. The panel shows that our general result of increased out-migration and the ranking of effects by destination types also hold for this larger set of elections.

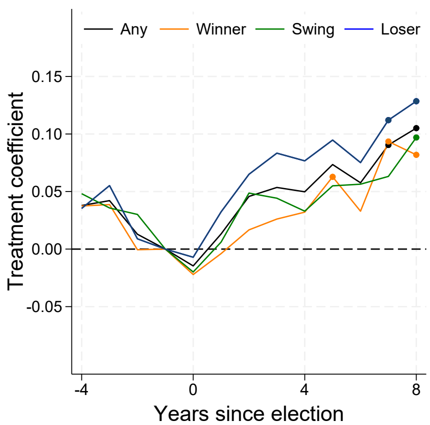
3.4 Placebo results

Our baseline results are for mid-term elections in sufficiently large states characterized by a winning margin below 5 percent. We think of the outcomes of these 54 elections as exogenous or quasi-randomly assigned, hence they constitute an exogenous shock to losing counties. In what follows, we consider elections that meet these criteria except for being won by a wide margin of more than five percent. The outcomes of these elections are not unexpected and do not constitute shocks to the migration decisions of individuals in losing counties. Hence, we expect no migration response after such uncontested elections.

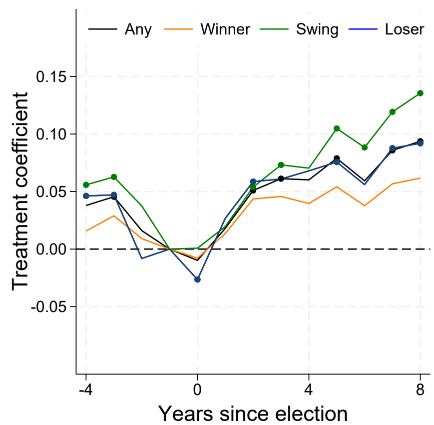
To test this, we estimate our baseline specification where instead of the 22 contested elections, we consider the 54 placebo elections and present results for out-migration into any destination and strongholds of the loser in Panels (a) and (b) of Figure 5.

Figure 4: Robustness

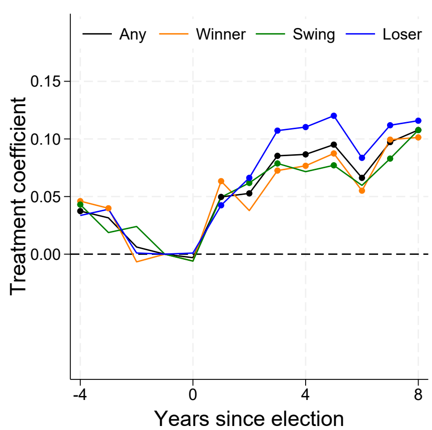
(a) Measure of migration



(b) Classifying destinations



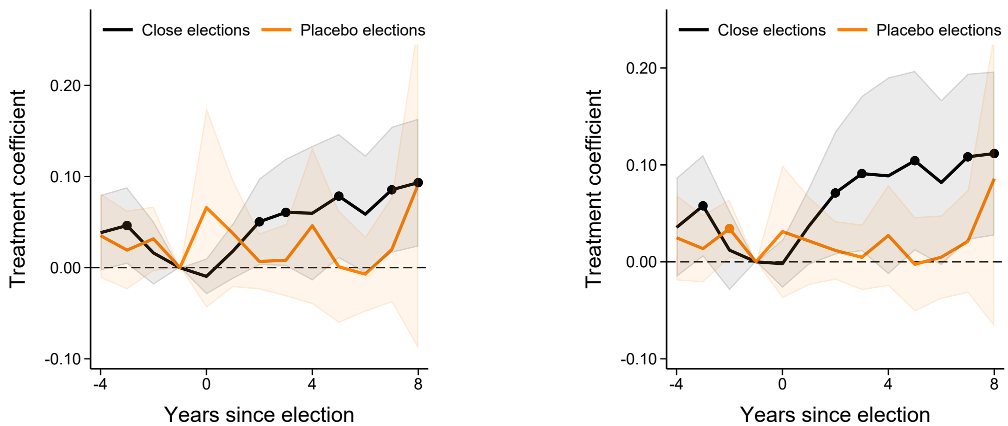
(c) Alternative set of contested elections



Notes: Regression results from variations of the baseline specifications. Panel (a) uses a different measure of migration (number of individuals instead of number of households). Panel (b) uses a different classification of destinations (based on their voting behavior in presidential elections instead of gubernatorial elections). Panel (c) uses less stringent conditions for an election to be contested. In particular, the current election is required to be close, and only the previous one was not. In the baseline, we also require the next election not to be close. Dots highlight point estimates that are significant at the 5 % level. For the full results, see Tables A.3, A.4, and A.5.

Figure 5: Placebo elections do not cause outmigration

(a) Any destination (b) Strongholds of the loser



Notes: Dynamic treatment effects for 54 placebo elections (with a margin above 5 %) estimated using the baseline specification. Results for placebo elections are in orange and, for comparison, those for close elections are in black. Point estimates are connected by lines, dots indicate significance at the five percent level, and 95 % confidence intervals are shaded areas. Full results are in Table A.6.

Regardless of the destination type, we find that placebo elections have no significant impact on outmigration in all post-treatment periods. These results provide further evidence in favor of our causal interpretation of the treatment coefficients for close elections.

3.5 Heterogeneity

Now, we investigate the heterogeneity of the election effects by splitting our sample by the winning party and by the decade in which the election took place. For each of these sub-panels of data, we re-estimate the baseline specification. The results are in Figure 6.

Panels (a) and (b) show that the effect differs for elections won by Republicans or Democrats, regardless of the type of destination (Panel (a): any, Panel (b): loser strongholds). In particular, the migration response in Democratic counties after Republican wins is weaker initially, but after eight years, it is around 15 percent larger than the difference in out-migration in the pre-election year. After Democratic wins, out-migration from Republican counties increases, as well. However, this effect turns insignificant three years after the election. Put succinctly: Democrats move after losing a contested election, while Republicans are more inclined to stay. This difference might be explained by the characteristics of the average voter of each party. Survey data (Pew Research Center 2024) shows that, for example, voters with at least a college degree are far more likely to vote Democratic than voters without any college degrees, which might affect their ability to find jobs in other states. Similarly, homeowners are more likely to lean Republican, while renters lean Democratic, thus implying lower migration costs for Democrats compared to Republicans.

Panels (c) and (d) investigate the heterogeneity of effects across decades. In the 1990s, there were six contested elections, nine in the 2000s, and nine in the 2010s. Here we also notice that regardless of the destination type (Panel (c): any, Panel (d): loser strongholds), the election effect on migration was strongest in the 1990s and 2000s, while being mostly insignificant for elections in the 2010s. Initially, the null result for elections since 2010 is somewhat surprising given the increase in political polarization. It is, however, well in line with the general decrease in US interstate migration (Jia et al. 2023; Molloy et al. 2011), which could be explained by, for example, an increase in migration costs.

4 Theory

4.1 Model set-up

To rationalize our empirical findings, we introduce a partial equilibrium selection model á la Borjas (1987), where individuals' migration decisions depend on their political views rather than their skills. There are two states, a generic destination state d and an origin state o, that consists of two counties $c = 1, 2$. Political views in the destination and in both counties of the origin follow a truncated normal distribution on the interval $[-1, 1]$. Liberal views correspond to negative values, while conservative views correspond to positive values.

Political views in the destination state have a mean of μ_d and a variance of σ_d^2. From the median voter theorem, it follows that in competitive elections the candidates, and thus the winner, offer policies close to the political preferences of the median voter. Thus, for simplicity, we assume that the type of policies in d can be expressed by the average political view μ_d. An individual's utility depends on the amenities of her state of residence and how well her political views align with the policy active in that state. Consider individual i living in the destination d. Her utility function is

$$u_{id}(\nu_{id}) = x_d + \mu_d \nu_{id}, \tag{2}$$

where x_d are the amenities of the destination and $\mu_d \nu_{id}$ is the policy premium. This premium is positive if i's views align with the type of policies active in d (if ν_{id} and μ_d have the same sign). This is quite intuitive: The utility a liberal individual ($\nu_{id} < 0$) experiences in state d is higher if the policies there are also liberal ($\mu_d < 0$).

A similar function describes utility for individuals i from county c in state o:

$$u_{io}(\nu_{ic}) = x_o + \widetilde{\mu}_o \nu_{ic},$$

where x_o are amenities of the origin and $\widetilde{\mu}_o$ the type of policy active there. For the origin state, we relax the assumption that actual policies perfectly align with average political views. Rather, we assume that active policies $\widetilde{\mu}_o$ are close to, but different from, the average political views in the origin μ_o. This allows for elections to affect policy. Both counties are inhabited by a unit mass of individuals. But they differ in the average political views: county 2 is liberal, while county 1 is not ($\mu_2 < 0 < \mu_1$).

4.2 Migration decision and elections

In the following, we assume that the policies and population of the destination state are liberal ($\mu_d < 0$), while the policies and the population of the origin are conservative ($\widetilde{\mu}_o, \mu_o > 0$).[7] Moreover, we argue that the act of migration does not affect an individual's political views. If she was born with views ν_{ic} in the origin state, then her views will still be ν_{ic} in the destination.[8] From this the migration decision of individual i from county c follows as

$$u_{io}(\nu_{ic}) = x_o + \widetilde{\mu}_o \nu_{ic} \leq x_d - k_{od} + \mu_d \nu_{ic} = u_{id}(\nu_{ic}), \tag{3}$$

where k_{od} are the costs of migrating from the origin to the destination. This equation captures the condition under which it is utility maximising for an individual from the origin to out-migrate. The political views of the individual that is indifferent between

[7] Our results also hold when this assumption is relaxed. For migration to take place it is important policies differ sufficiently across states.

[8] Borjas (2014) labeled this property "perfect transferability".

staying and migrating are then:

$$\nu_{od}^* = \frac{x_o + k_{od} - x_d}{\mu_d - \widetilde{\mu}_o}. \tag{4}$$

This level depends only on state-level variables, meaning that the marginal migrant out of both counties has the same views. This notion of the indifferent migrant allows to determine who wants to leave: If the origin is more conservative than the destination ($\mu_d < \widetilde{\mu}_o$), then it's the liberal individuals who leave (all $\nu_{ic} \leq \nu_{od}^*$). Vice versa for migration from a liberal to a conservative state.

To focus on the change in migration brought about by gubernatorial elections in the origin, we assume that there was no migration before the election took place. This is the case if, ex-ante, the amenities in the destination net of migration costs are so low, relative to the origin amenities, that no one want to migrate, even if this would mean a better fit in terms of policy premium: $x_d - k_{od} < x_o + \mu_d - \mu_o$.

Now the election takes place and the winner of this election governs with a policy platform $\widetilde{\mu}_o' \in (\widetilde{\mu}_o, \mu_o)$. That is, the policies in the origin are now more conservative than before. The resulting ex-post migration decision follows straightforwardly. Individuals compare the utility of moving to the origin with that of staying in the destination, given the new political reality. The marginal individual is now described by

$$\nu_{od}^{*\prime} = \frac{x_o + k_{od} - x_d}{\mu_d - \widetilde{\mu}_o'} > \nu_{od}^*.$$

Using the distribution functions of political orientations $F_c(\nu_{ic})$, we can express change in the flow of migrants from county c to destination d as

$$\Delta M_{cd} = F_c(\nu_{od}^{*\prime}) - F_c(\nu_{od}^*). \tag{5}$$

Which counties experiences the greater increase in out-migration due to the election? Comparing the changes in migration out of both counties, we find that $\Delta M_{2d} > \Delta M_{1d} > 0$. Both counties experience an increase in migration, with the increase in the liberal county (county 2) greater than the increase in the conservative county (county 1).[9] This is equivalent to the positive treatment effect found in the empirical investigation. The model rationalizes this effect in a simple way: In the liberal county

[9]This result holds if the variance of political views in county 2 is sufficiently large relative to the county-1 variance.

live relatively more individuals which are negatively affected by the change to more conservative policies.

Interestingly, the model also offers a testable prediction for state-to-state migration. The distribution of political views in the origin is the convolution of both county-level distributions. We now assume that $x_d - k_{od} > x_o$, so that the amenities of the destination net of migration cost are greater than the amenities of the origin. Then more conservative policies in the origin county lead to *lower* migration from the origin to the destination:

$$\frac{\partial F_o(\nu^*_{od})}{\partial \widetilde{\mu}_o} = f_c(\nu^*_{od}) \frac{x_o + k_{od} - x_d}{(\mu_d - \widetilde{\mu}_o)^2} < 0.$$

The reason for this is straightforward: If $x_d - k_{od} > x_o$, then the amenities of the destination net of migration costs are large relative to the amenities in the origin, so that even conservatives from the origin want to tolerate liberal policies. We test this prediction in the gravity section.

4.3 Migration and polarization

From this simple migrant selection model, we can also derive predictions regarding political polarization across states. People move to the place where their policy premium, net of non-political amenities, is highest. This implies that liberal people move to liberally governed states, and conservative people to conservative states. Which in turn, shifts the average political view in both origin and destination state more to the extremes. In terms of federal election results, i.e. elections that are not contested by state-specific candidates (presidential elections), state-level vote margins should become larger and the number of states where the election outcome was close should decline.

Figure 7 offers descriptive evidence of this insight. Shown are US counties that are won by a landslide (margin $>$ 20%) by the Democratic (blue) or Republican (red) candidates. Panel a) shows the distribution of landslide county for the 1992 presidential election. Panel b) shows the same for the 2020 election. As can be seen, the number of counties that were not won by a landslide (white) decreased significantly. Moreover, most counties were won by the Republican candidate, whereas the number counties with overwhelming support of the democratic candidate has decreased. This shows growing political polarization of the US and the decrease of contested counties.

5 Migration and political distance

To examine how state-to-state migration flows are affected by political distance, we construct the variable $poldist_{odt}$, which captures the difference in average political views between states as revealed by presidential elections. Average political views are calculated as the vote-share weighted average conservatism of presidential candidates. We then estimate the effect of changes in political distance on bilateral migration using PPML.

5.1 Construction of political distance

We measure the political orientation of presidential candidates in the US using data from the manifesto project (Lehmann et al. 2023a).[10] This dataset contains information on the election manifestos of the most relevant parties in 67 countries. For US presidential elections, only the manifestos of candidates who have won at least 5 percent of the national vote are collected.

Lehmann et al. (2023a) define a wide range of variables that capture a candidate's stance on a range of political issues, for example, the military. They also construct a summary variable $rile_{jt}$ that captures the right-left position of candidate j at election t by aggregating his stance on conservative issues, for example, the national way of life and subtracting his aggregate stance on liberal issues, for example, labor unions. This variable is positive for candidates with an overall conservative election platform and negative for those with a liberal election platform.[11] In what follows, we refer to this measure simply as conservatism. Figure 8a shows the conservatism of Democratic and Republican presidential candidates since 1988. In every election, Republican candidates are more conservative than their Democratic opponents. The gap between Republican and Democratic candidates' conservatism remained relatively stable between 1988 and 2008. Since then, it increased steadily and was at a 30-year high in 2020.

Next, we provide some descriptive evidence on changes in conservatism across states and time. To that end, we calculate the average conservatism in state d in

[10] The dataset and its documentation are available at `https://doi.org/10.25522/manifesto.mpds.2023a`.

[11] See Lehmann et al. (2023b) for a full list of covered political issues and variable construction.

election year t by weighting the vote share of each candidate with their conservatism

$$polor_{dt} = \sum_j voteshare_{jdt} \times rile_{jt}. \tag{6}$$

Figures 8b and 8c show the spatial distribution of conservatism in the US in 1992 and 2020. In 1990, both presidential candidates were similarly conservative, which is also reflected in quite similar degrees of conservatism across states and counties. However, this picture looks dramatically different in 2020. Here, the political distance between presidential candidates is roughly doubled compared to 1992, which is also reflected in far larger differences in conservatism across states and counties. The least conservative regions are the coasts, whereas interior regions are more conservative.

We measure the bilateral political distance between the populations of two states as revealed by their voting choices as the absolute difference in their conservatism:

$$poldist_{odt} = |polor_{ot} - polor_{dt}|. \tag{7}$$

The red line in Figure 9 shows that the correlation between the political distance variable $poldist_{odt}$ and the number of migrants that moved from state o to state d in the year after the election $t + 1$.

The reason to look at the migrant flow in the year after the election accounts for two issues that arise when using the flow observed in the election year. First, presidential elections take place in November, so most of the moves likely took place before the election, which reveals the political distance between states. Second, movements before the election happens can affect election outcomes in the destination and origin.

5.2 Estimation framework

To estimate the effect of political distance on migrant flows, we aggregate county-level migration flows to the state level. It contains bilateral migration flows for 50 continental US states (excluding Alaska and Hawaii but including Washington DC) over the period 1992 to 2021. Since our measure of political orientations is only available for presidential elections, we have eight years where political orientations are revealed (1992, 1996, 2000, 2004, 2008, 2012, 2016, 2020). To account for potential endogeneity, we use the migrant flow in the year after the election took place (1993, 1997, 2001, 2005, 2009, 2013, 2017, 2021).

Our regression equation is

$$M_{odt+1} = \exp\left(\beta poldist_{odt} + \boldsymbol{\gamma X}_{odt} + \pi_{ot} + \pi_{dt}\right)\varepsilon_{odt}, \tag{8}$$

where M_{odt+1} is the flow of migrants from o to d in the year after an election $t+1$, $poldist_{od}$ is the political distance between states o and d as revealed by the presidential election in year t. The vector $\boldsymbol{X}_{odt}$ contains time-invariant measures of migration costs (the log distance between states and a dummy indicating contiguity) as well as a dummy indicating whether states o and d were won by the same candidate $winner\_pair_{odt}$ in election t. As is common in the migration literature, we include origin-time fixed effects π_{ot} in the regression. These account for the origin-specific push and pull factors (Bertoli and Fernández-Huertas Moraga 2015). Additionally, they ensure consistency with an underlying RUM model (Ortega and Peri 2013; Bertoli and Fernández-Huertas Moraga 2015). We also include destination-time fixed effects π_{dt}, which control for destination-specific pull factors, like employment opportunities or average incomes. Additionally, both sets of fixed effects control for state-level policies. For the estimation, we again rely on the PPML estimator.

5.3 Main results

Table 2 shows the baseline gravity regression results, where the dependent variable is always the number of migrants.

Column (1) shows the effect of the migration cost variables. A one-percent increase in the distance between two states leads to a 0.76 percent lower migrant flow. Migration is roughly twice as large between contiguous counties than between non-contiguous ones. The sizes and signs of these migration cost variables are robust across specifications. Generally, an increase in the distance between two states lowers migrant flows, whereas this flow is higher if the states border each other.

In Column (2), we add the political distance variable $poldist_{odt}$. We find a significant and negative effect of political distance on migrant flows. Since the political distance measure is related to the election outcomes, we use the $winner\_pair_{odt}$ dummy that indicates whether states o and d were won by the same candidate in election t. Column (3) reports the results when we include only the $winner\_pair_{odt}$ dummy but exclude the political distance variable. We find a significant and positive effect of the winner-pair dummy, indicating that there is more migration between states where a majority

Table 2: Migration and Political Distance – Baseline

	(1)	(2)	(3)	(4)
Log distance	-0.759^{***}	-0.746^{***}	-0.753^{***}	-0.746^{***}
	(0.025)	(0.025)	(0.025)	(0.025)
Contiguity	0.690^{***}	0.691^{***}	0.691^{***}	0.691^{***}
	(0.052)	(0.051)	(0.052)	(0.051)
Political Distance		-2.177^{***}		-2.205^{***}
		(0.501)		(0.497)
Winner-Pair			0.050^{***}	-0.002
			(0.019)	(0.017)
Observations	19600	19600	19600	19600
Origin-Time FE	✓	✓	✓	✓
Destination-Time FE	✓	✓	✓	✓

Notes: Standard errors in parentheses. Standard errors are clustered at the state-pair level.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

has voted for the same candidate. As a final test, we include the political distance variable as well as the winner-pair dummy simultaneously. The results are reported in Column (4). The coefficient of the political distance variable is slightly larger (in absolute value) than in Column (2) at the same level of statistical significance. The winner-pair variable, on the other hand, loses statistical significance and even turns negative. These results lead us to conclude that our political distance variable is a preferable measure of political similarities between populations of different states.

5.4 Robustness

To control for factors that might influence migrant flows as well as differences in political orientation, we add a variety of control variables into the vector $\boldsymbol{X}_{odt}$. Table 3 shows the results of the robustness exercises. The coefficients of the log distance and contiguity variables are robust across all specifications, with magnitudes similar to those reported in Table 2.

Our control variables include dummies indicating whether states o and d were opponents or allies during the Civil War. The motivation behind including these two dummies is that alignment during the Civil War might indicate historical factors or sets of beliefs ingrained into the populations that can shape current migration choices. For example, in our period of observation, Kansas and Texas are conservative (they score consistently positive values on the $polor_{dt}$ measure) and thus exhibit only a relatively small political distance. Yet, while Texas was a member of the Confederacy, Kansas was a Northern Union state. Now, if an individual from Texas decides to emigrate, she might opt for a conservative state (low political distance) that was aligned with Texas during the Civil War, like Alabama instead of Kansas. This might be because people exhibit "friendlier" attitudes towards states that were allied with their origin during the Civil War.

The results reported in Column (1) show the effect of being opponents in the Civil War. The coefficient is positive and significant, indicating that current migrant flows are greater between previously opposed states. Column (3) shows the effect of being allied in the Civil War. The coefficient is negative and statistically significant: Being allies during the Civil War lowers current migrant flows. The size and sign of the political-distance effect on migration is robust to the inclusion of these dummies.

In Column (2), we include directional fixed-effects indicating whether the origin was a Union (Confederate) state whereas the destination was a Confederate (Union) state. Both coefficients are positive and statistically significant. The size of the Union to Confederate dummy is larger, meaning that migrant flows are greater from Union to Confederate states than the other way around. The estimated effect of political distance on migration is similar in Columns (1) and (2): a greater political distance is associated with lower migrant flows.

In Columns (4), we include a directional decomposition of the ally-effect reported in Column (3). We find that the reduction in migration between allied states is greater among Union members than among members of the Confederation. The effect of political distance on migration is again, in size and significance, unaffected by the inclusion of these additional controls.

As a further robustness check, we also include a dummy variable that captures whether the origin and the destination are both coastal states. The idea behind this dummy is that the economies in coastal states are relatively similar, so that people from the east coast might have skills that are easily transferable to states located on the west coast. Since both regions are relatively liberal, it might be the case that the effect of low political distance is subject to omitted variable bias since the migration flow is higher because of skill transferability and not because of a low political distance.

Column (5) shows the results of including the coast-to-coast dummy. The sign of the coefficient indicates that migration between coastal states is greater than otherwise. The size of the political distance variable is halved, but it keeps its negative sign and statistical significance. These results show that our political orientation variable is robust across specifications, even after controlling for various confounding factors.

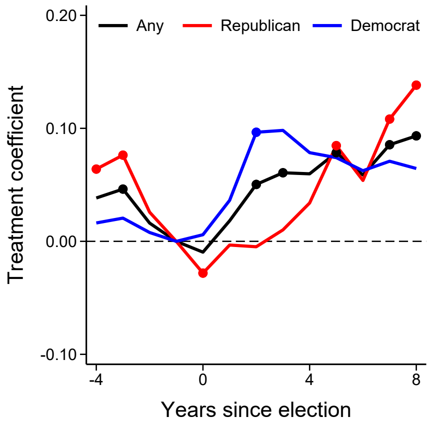
Table 3: Migration and Political Distance – Robustness

	(1)	(2)	(3)	(4)	(5)
Log distance	-0.754^{***}	-0.754^{***}	-0.757^{***}	-0.756^{***}	-0.716^{***}
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Contiguity	0.735^{***}	0.735^{***}	0.741^{***}	0.740^{***}	0.667^{***}
	(0.052)	(0.052)	(0.053)	(0.053)	(0.050)
Political Distance	-2.703^{***}	-2.703^{***}	-2.713^{***}	-2.703^{***}	-1.282^{**}
	(0.475)	(0.475)	(0.474)	(0.478)	(0.501)
Opponents in CW	0.189^{***}				
	(0.042)				
NU to Conf.		0.196^{***}			
		(0.072)			
Conf. to NU		0.180^{***}			
		(0.063)			
Allies in CW			-0.194^{***}		
			(0.044)		
NU to NU				-0.211^{***}	
				(0.067)	
Conf. to Conf.				-0.173^{***}	
				(0.065)	
Coast to coast					0.444^{***}
					(0.063)
Observations	19600	19600	19600	19600	19600
Origin-Time FE	✓	✓	✓	✓	✓
Destination-Time FE	✓	✓	✓	✓	✓
Pseudo R^2	0.926	0.926	0.926	0.926	0.929

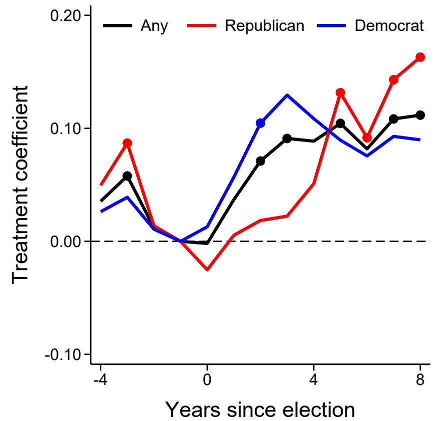
Notes: Standard errors in parentheses. Standard errors are clustered at the state-pair level.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Heterogeneity

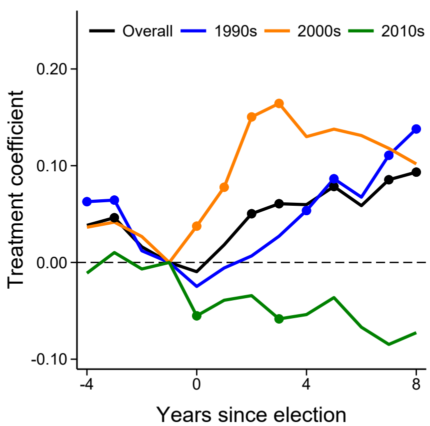
(a) By election winner, d=any



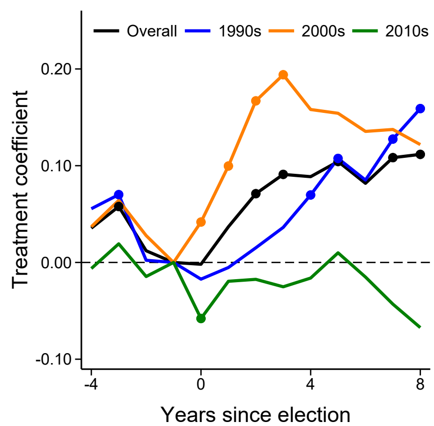
(b) By election winner, d =loser



(c) By decade, d = any



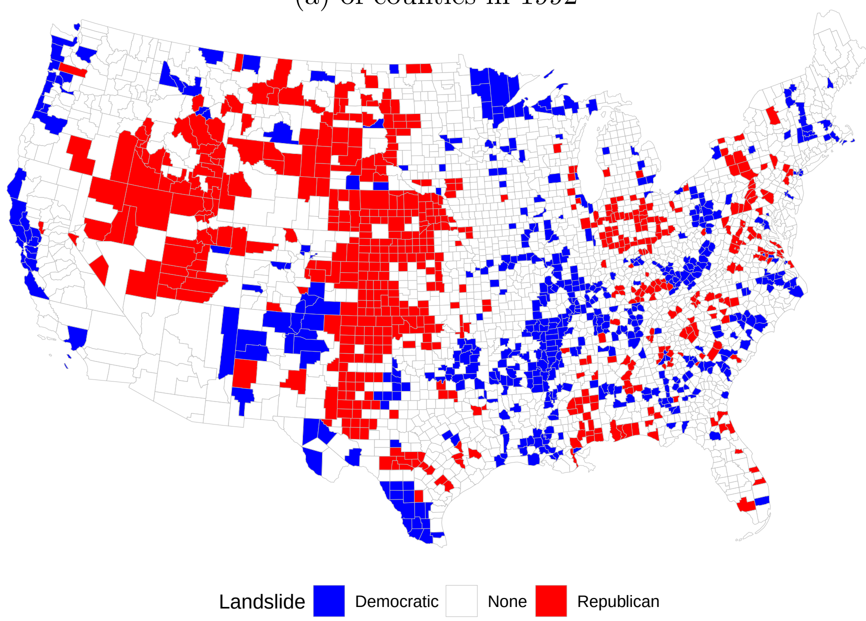
(d) By decade, d =loser



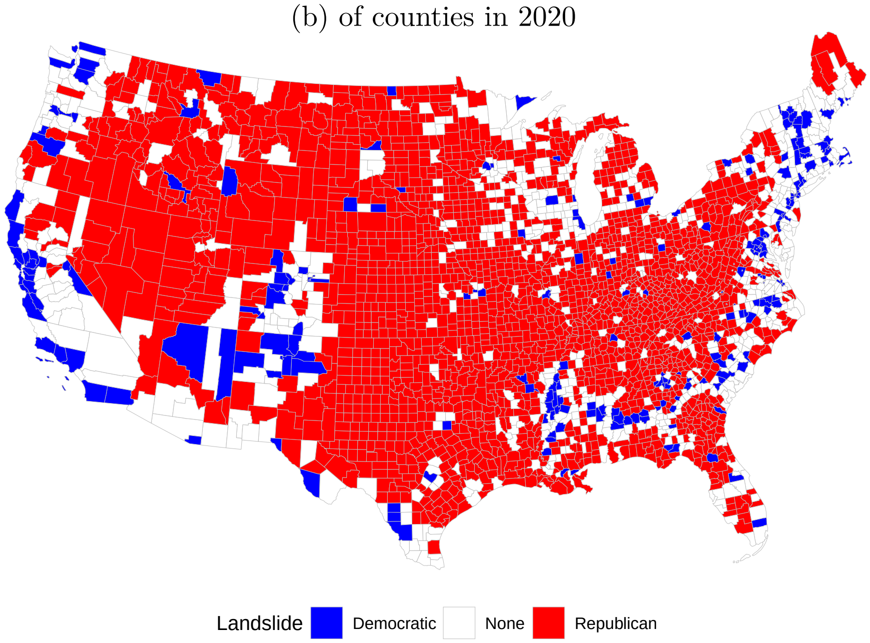
Notes: Baseline specification estimated on sample splits. Panels (a) and (b) split samples by election winner, with Panel (a) showing out-migration into any destination and Panel (b) showing outflows into strongholds of the election loser. Panels (a) and (b) split the sample by decade and again report results for any destination (Panel (c) and strongholds if the election winner (Panel (d)). All plots show point estimates connected by a line. Dots indicate estimates that are significant at the 5 % level.

Figure 7: Landslide election margins

(a) of counties in 1992



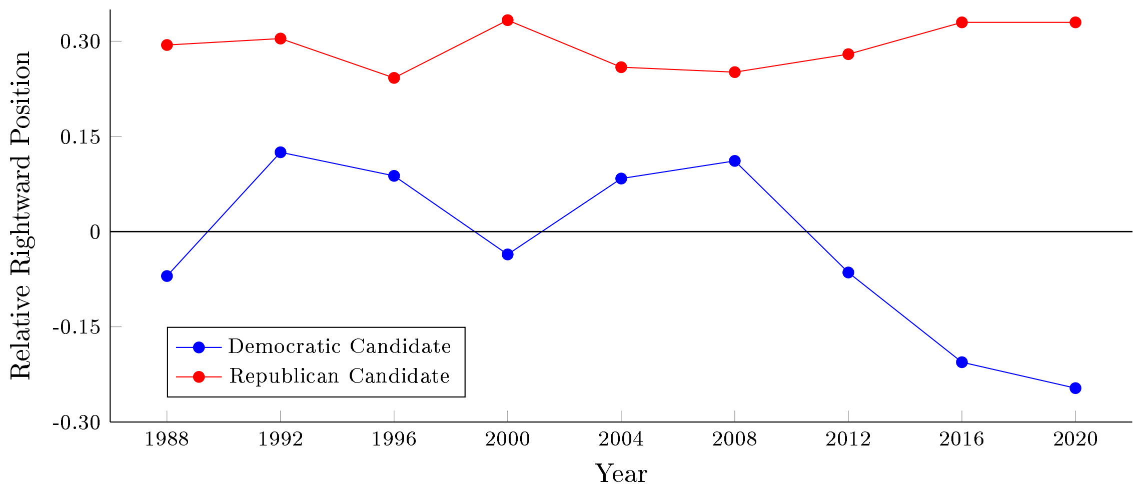
(b) of counties in 2020



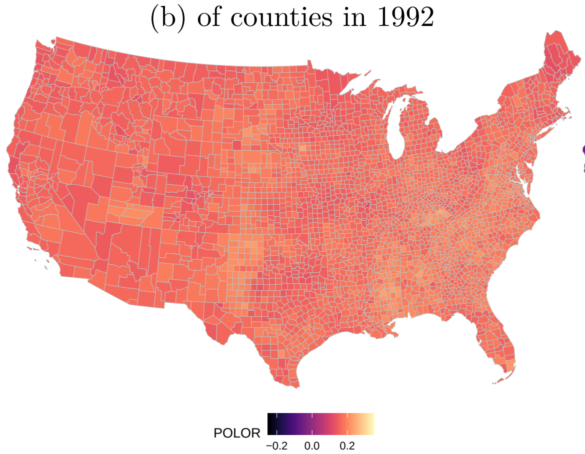
Notes: Figure shows counties won by a landslide. Blue indicates a Democratic, red a Republican landslide. Landslides are defined by a party winning by more than 20 percent.

Figure 8: Conservatism

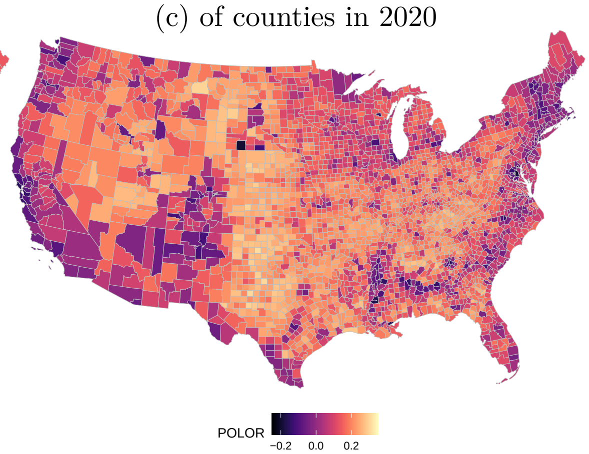
(a) of presidential candidates across time



(b) of counties in 1992

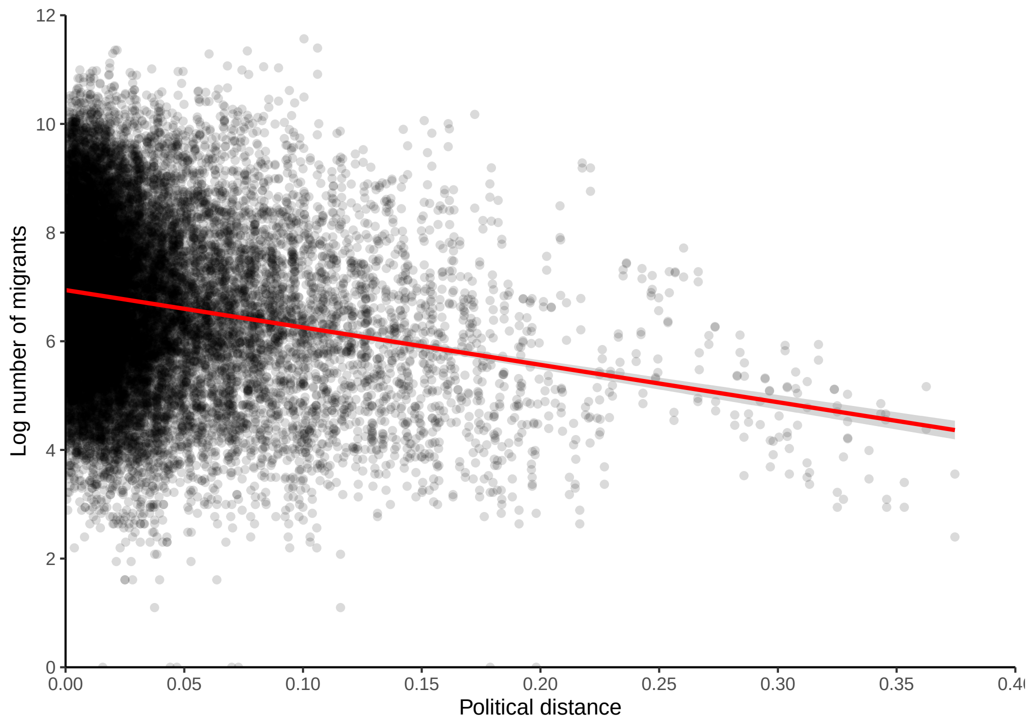


(c) of counties in 2020



Notes: The top panel shows the conservatism ($rile_{jt}$) of Democratic and Republican presidential candidate j in election t over the periods observed in Lehmann et al. (2023a). Independent candidates are not shown. The bottom two panels show the average political position of US counties in 1992 and 2020. To that end, we multiply, for every county, the vote share of each candidate with his $rile_{jt}$-score and sum up across candidates. Darker colors correspond to more left-leaning political positions.

Figure 9: Correlation between Migration and Political Distance



Notes: The figure shows log bilateral migration flows and political distance. Each dot is a state-pair-election observation. The red line shows fitted values from a simple linear regression of log migration on political distance.

6 Conclusion

In this paper, we asked if US out-migration responds to political shocks. To answer, we first consider a difference-in-difference study of out-migration at the county level before and after close gubernatorial elections (toss-ups) between counties with a majority for the winning candidate (control group) and those counties with a majority for the losing candidate (treatment group). For all toss-ups between 1994 and 2014, this treatment effect on out-migration is about 5 percent roughly two years after the toss-up result and turns insignificant again afterward. Additionally, we find that this treatment effect is strongest after Republican wins. Here, out-migration from Democratic counties increases by up to eight percent in the year of the election and two years after and remains significantly elevated even five years after. Next, we rationalize this evidence in a migration-selection model in which individuals' political views and state-level policies take center stage. The model implies that migration between states should be lower if their political distance increases. We confirm this result in a gravity estimation, controlling for distance and additional measures of migration costs.

In its current form, our analysis does not make full use of the bilateral micro-data on migration that is available from IRS. Instead, future research could use DiD framework Nagengast and Yotov (2025) proposed to estimate causal effects of trade agreements. Using bilateral data would additionally allow the inclusion of further control variables, such as unemployment, costs of living or gaps in wages and house price gaps as in Olney and Thompson (2024).[12]

[12]They compute the wage gap based on income data available in the IRS migration data that we use. For house prices, they use indices compiled by Bogin et al. (2019).

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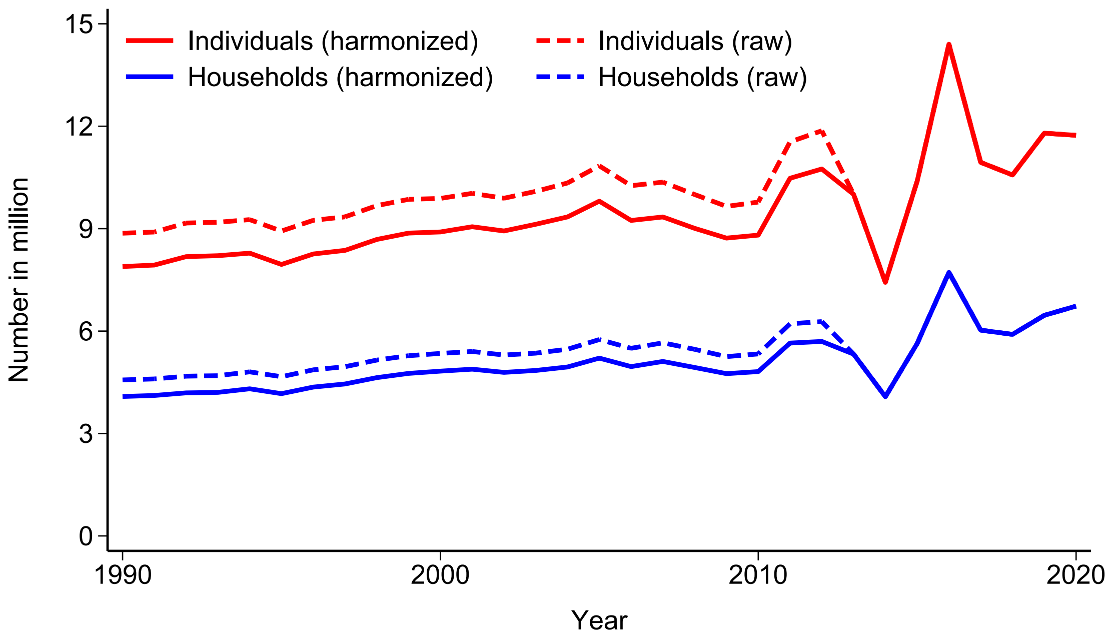
Appendix

A.1 Additional Figures

Figure A.1: Total internal migration in the US



Notes: The figure shows the total number of migrating individuals in the US. Red lines are for the number of individuals and blue lines for the number of households. In the underlying data, there is a change in the reporting threshold in 2010. Up until then, flows (in terms of individuals and households) were reported when they reflected at least 10 households. Since 2010, this threshold has increased to 20 households. For harmonization, we set the reporting threshold to 20 for the entire sample period and drop pre-2010 observations reflecting less than 20 households. The raw data (pre-harmonization) are shown as dashed lines and the harmonized data are solid lines.

Table A.1: Descriptive statistics of placebo elections

(a) 1994 to 2006

Year	State	Margin	Votes	Control counties	Treated counties
1994	ID	-8.4	0.4	36	8
1994	NM	-9.9	0.5	19	14
1994	OK	-17.3	1.0	45	32
1994	OR	8.5	1.2	11	25
1994	PA	-5.4	3.6	56	11
1994	SD	-14.8	0.3	59	7
1994	TN	-9.6	1.5	60	35
1998	AR	-21.1	0.7	66	9
1998	IA	5.8	1.0	48	51
1998	NE	-7.9	0.5	70	23
1998	NM	-9.1	0.5	22	11
1998	OH	-5.4	3.4	68	20
1998	SD	-31.2	0.3	59	7
1998	TX	-37.1	3.7	240	14
2002	AR	-6.1	0.8	42	33
2002	FL	-12.8	5.1	54	13
2002	GA	-5.2	2.0	118	40
2002	ID	-14.5	0.4	38	6
2002	IA	8.2	1.0	68	31
2002	KS	7.8	0.8	54	49
2002	ME	5.7	0.5	11	5
2002	NM	16.4	0.5	24	9
2002	OH	-19.5	3.2	82	6
2002	PA	9.0	3.6	18	49
2002	SC	-5.8	1.1	23	23
2002	SD	-14.8	0.3	55	11
2002	TX	-17.8	4.6	219	35
2006	AR	14.9	0.8	61	14
2006	CO	16.8	1.6	38	26
2006	GA	-19.7	2.1	130	29
2006	ID	-8.6	0.5	36	8
2006	IA	9.6	1.1	62	37
2006	KS	17.5	0.8	64	41
2006	PA	20.7	4.1	33	34
2006	TX	-9.2	4.4	217	37

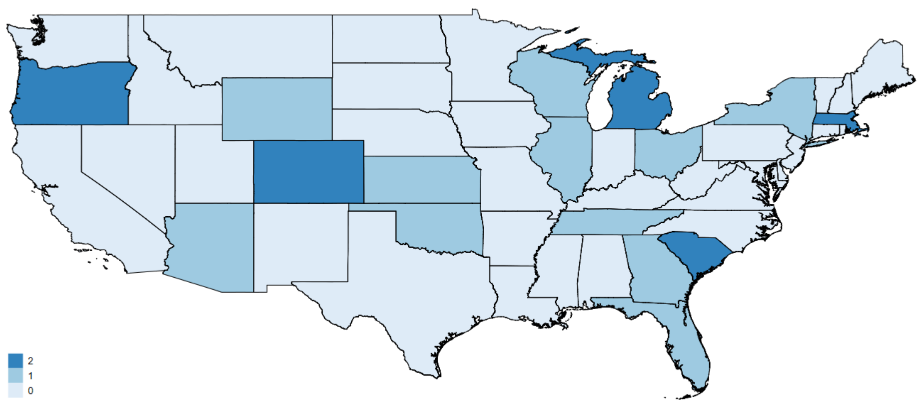
Notes: Year is the election year, and state is the abbreviation of the state where the election took place. Margin is the margin of the democratic candidate, votes is the total number of votes in the election (for any candidate), and control and treated counties give the number of treated and control counties in the election.

(b) 2010 to 2014

Year	State	Margin	Votes	Control counties	Treated counties
2010	AL	-15.7	1.5	47	20
2010	CA	12.9	10.1	23	35
2010	GA	-10.0	2.6	120	39
2010	IA	-9.6	1.1	90	9
2010	NM	-6.7	0.6	25	8
2010	NY	29.3	4.7	49	13
2010	SD	-23.0	0.3	56	10
2010	TN	-31.9	1.6	90	5
2010	TX	-12.7	5.0	226	28
2010	WI	-5.8	2.2	59	13
2014	AL	-27.3	1.2	54	13
2014	AR	-13.9	0.8	52	23
2014	CA	19.9	7.3	29	29
2014	ID	-15.0	0.4	39	5
2014	NM	-14.4	0.5	28	5
2014	NY	13.9	3.8	16	46
2014	OK	-14.8	0.8	71	6
2014	PA	9.9	3.5	24	43
2014	TX	-20.4	4.7	235	19
Sum			110.6	3,639	1,172
Average		-5.7	2.0	67	22

Notes: Year is the election year, and state is the abbreviation of the state where the election took place. Margin is the margin of the democratic candidate, votes is the total number of votes in the election (for any candidate), and control and treated counties give the number of treated and control counties in the election. Sum and average also consider elections listed in Panel (a).

Figure A.2: Close gubernatorial elections



Notes: Map indicating how many isolated surprise election results there were by state between 1994 and 2014. Hawaii and Alaska had no such events and are not shown. Furthermore, New Hampshire and Vermont are not considered, as governors there are elected for two-year terms only.

Table A.2: Full results for baseline specifications

	(1)	(2)	(3)	(4)
$t-4$	0.04*	0.03	0.05**	0.03
	(0.02)	(0.02)	(0.02)	(0.03)
$t-3$	0.05**	0.04**	0.04	0.06**
	(0.02)	(0.02)	(0.02)	(0.03)
$t-2$	0.02	0.00	0.03	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
t	−0.01	−0.02	−0.02	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)
$t+1$	0.02	−0.00	0.02	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)
$t+2$	0.05**	0.02	0.06**	0.07**
	(0.02)	(0.02)	(0.03)	(0.03)
$t+3$	0.06**	0.03	0.05	0.09**
	(0.03)	(0.02)	(0.04)	(0.04)
$t+4$	0.06	0.04	0.04	0.09*
	(0.04)	(0.03)	(0.04)	(0.05)
$t+5$	0.08**	0.07**	0.06	0.10**
	(0.03)	(0.03)	(0.04)	(0.05)
$t+6$	0.06*	0.04	0.05	0.08*
	(0.03)	(0.03)	(0.04)	(0.04)
$t+7$	0.09**	0.08***	0.06*	0.11**
	(0.04)	(0.03)	(0.04)	(0.04)
$t+8$	0.09***	0.08**	0.09**	0.11***
	(0.04)	(0.04)	(0.04)	(0.04)
N	13,218	8,903	9,366	10,058

Notes: Column (1) shows results for any destinations, Columns (2) to (4) for strongholds of the winner, swing states, and strongholds of the loser. Period $t-1$ is the baseline period. Standard errors are clustered by county and election, and they are reported in brackets below the point estimates.

Table A.3: Full results for specifications using the number of migrating individuals

	(1)	(2)	(3)	(4)
$t-4$	0.04*	0.04	0.05*	0.04
	(0.02)	(0.02)	(0.03)	(0.03)
$t-3$	0.04*	0.04*	0.04	0.06*
	(0.02)	(0.02)	(0.03)	(0.03)
$t-2$	0.01	−0.00	0.03	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
t	−0.01	−0.02*	−0.02	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)
$t+1$	0.01	−0.00	0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
$t+2$	0.05*	0.02	0.05*	0.07*
	(0.03)	(0.02)	(0.03)	(0.04)
$t+3$	0.05	0.03	0.04	0.08*
	(0.04)	(0.02)	(0.04)	(0.05)
$t+4$	0.05	0.03	0.03	0.08
	(0.05)	(0.03)	(0.05)	(0.07)
$t+5$	0.07*	0.06**	0.05	0.09*
	(0.04)	(0.03)	(0.04)	(0.05)
$t+6$	0.06	0.03	0.06	0.08
	(0.04)	(0.03)	(0.04)	(0.05)
$t+7$	0.09**	0.09***	0.06	0.11**
	(0.04)	(0.03)	(0.04)	(0.05)
$t+8$	0.11***	0.08**	0.10**	0.13***
	(0.04)	(0.04)	(0.04)	(0.04)
N	13,218	8,903	9,366	10,058

Notes: Column (1) shows results for any destinations, Columns (2) to (4) for strongholds of the winner, swing states, and strongholds of the loser. Period $t-1$ is the baseline period. Standard errors are clustered by county and election, and they are reported in brackets below the point estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Full results for specifications classifying destinations based on voting in presidential elections

	(1)	(2)	(3)	(4)
$t-4$	0.04*	0.02	0.06**	0.05**
	(0.02)	(0.02)	(0.03)	(0.02)
$t-3$	0.05**	0.03	0.06**	0.05**
	(0.02)	(0.02)	(0.03)	(0.02)
$t-2$	0.02	0.01	0.04*	−0.01
	(0.02)	(0.02)	(0.02)	(0.02)
t	−0.01	−0.01	0.00	−0.03**
	(0.01)	(0.01)	(0.01)	(0.01)
$t+1$	0.02	0.01	0.02	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
$t+2$	0.05**	0.04	0.05**	0.06**
	(0.02)	(0.03)	(0.03)	(0.02)
$t+3$	0.06**	0.05	0.07**	0.06*
	(0.03)	(0.03)	(0.03)	(0.03)
$t+4$	0.06	0.04	0.07*	0.07
	(0.04)	(0.03)	(0.04)	(0.04)
$t+5$	0.08**	0.05	0.10***	0.08**
	(0.03)	(0.03)	(0.04)	(0.04)
$t+6$	0.06*	0.04	0.09**	0.06*
	(0.03)	(0.04)	(0.04)	(0.03)
$t+7$	0.09**	0.06	0.12***	0.09**
	(0.04)	(0.04)	(0.04)	(0.04)
$t+8$	0.09***	0.06	0.14***	0.09**
	(0.04)	(0.04)	(0.04)	(0.04)
N	13,218	8,364	10,920	8,381

Notes: Column (1) shows results for any destinations, Columns (2) to (4) for strongholds of the winner, swing states, and strongholds of the loser. Period $t-1$ is the baseline period. Standard errors are clustered by county and election, and they are reported in brackets below the point estimates.

Table A.5: Full results for specifications using a broader definition of contested elections

	(1)	(2)	(3)	(4)
$t-4$	0.04**	0.05**	0.04**	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
$t-3$	0.03*	0.04**	0.02	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)
$t-2$	0.01	−0.01	0.02	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
t	−0.00	−0.01	−0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
$t+1$	0.05**	0.06**	0.05*	0.04**
	(0.02)	(0.03)	(0.03)	(0.02)
$t+2$	0.05**	0.04*	0.06**	0.07**
	(0.02)	(0.02)	(0.03)	(0.03)
$t+3$	0.09***	0.07***	0.08**	0.11***
	(0.03)	(0.02)	(0.03)	(0.04)
$t+4$	0.09**	0.08***	0.07*	0.11**
	(0.03)	(0.03)	(0.04)	(0.05)
$t+5$	0.10***	0.09***	0.08**	0.12***
	(0.03)	(0.02)	(0.03)	(0.04)
$t+6$	0.07**	0.06**	0.06*	0.08**
	(0.03)	(0.02)	(0.03)	(0.04)
$t+7$	0.10***	0.10***	0.08**	0.11***
	(0.03)	(0.03)	(0.03)	(0.04)
$t+8$	0.11***	0.10***	0.11***	0.12***
	(0.03)	(0.03)	(0.03)	(0.04)
N	16,539	11,741	10,959	11,990

Notes: Column (1) shows results for any destinations, Columns (2) to (4) for strongholds of the winner, swing states, and strongholds of the loser. Period $t-1$ is the baseline period. Standard errors are clustered by county and election, and they are reported in brackets below the point estimates.

Table A.6: Full results for placebo elections

	(1)	(2)
$t-4$	0.04	0.03
	(0.02)	(0.02)
$t-3$	0.02	0.01
	(0.02)	(0.02)
$t-2$	0.03^{*}	0.03^{**}
	(0.02)	(0.02)
t	0.07	0.03
	(0.06)	(0.04)
$t+1$	0.04	0.02
	(0.03)	(0.02)
$t+2$	0.01	0.01
	(0.02)	(0.02)
$t+3$	0.01	0.00
	(0.02)	(0.02)
$t+4$	0.05	0.03
	(0.04)	(0.03)
$t+5$	0.00	−0.00
	(0.03)	(0.02)
$t+6$	−0.01	0.00
	(0.02)	(0.02)
$t+7$	0.02	0.02
	(0.03)	(0.03)
$t+8$	0.09	0.09
	(0.09)	(0.08)
N	35,837	21,984

Notes: Column (1) shows results for any destinations, Columns (2) to (4) for strongholds of the winner, swing states, and strongholds of the loser. Period $t-1$ is the baseline period. Standard errors are clustered by county and election, and they are reported in brackets below the point estimates.