Political Preferences and Migration Decisions of College-Educated Workers

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Abstract

We study the consequence of political polarization along educational lines in the United States. Descriptively, we show that college graduates are now well to the left of non-college voters on economic and social issues and much more so than 15 years ago. We then estimate the causal effect of a Republican governor on college graduates' inter-state migration rates, finding that conservative governance substantially reduces the inflow of college-educated workers. Finally, we analyze a structural model of migration that quantifies the implications of plausible changes in political control for cross-state spillovers and college/non-college earnings inequality.

Keywords: Political polarization, migration, spatial sorting, inequality

JEL Classification Numbers: D31, D72, H31, H73, J61, P16

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1 Introduction

How do people choose where to live, and what are the consequences for the pattern of economic activity? Over the last 40 years, US college graduates have become increasingly concentrated in specific places (Moretti, 2012; Diamond and Gaubert, 2022). This is important because many high-growth sectors are skill-intensive, and college graduates generate economic spillovers that raise the wages of local non-graduates, too (Moretti, 2004; Giannone, 2022). Therefore, increased spatial sorting has contributed to the slowdown of regional convergence (Kleinman, Liu, and Redding, 2023) and substantially exacerbated inequality in welfare between college and non-college workers (Diamond, 2016).

A separate literature shows that Democrats and Republicans increasingly live in different places (Brown et al., 2022; Kaplan et al., 2022). This is important because neighbors and neighborhoods do affect individual attitudes (Cantoni and Pons, 2022; Martin and Webster, 2020; Perez-Truglia, 2018; Perez-Truglia and Cruces, 2017), and so spatial sorting can exacerbate political polarization (Bishop, 2009; Brown and Enos, 2021). At the same time, it reduces democratic competition in these places, which further exacerbates the election of political extremists (Hopkins, 2017) and undermines economic growth (Besley, Persson, and Sturm, 2010).

In summary, geographic sorting by education and geographic sorting by political attitudes are both important social, political, and economic phenomena. In this paper, we show how they are related. Divisions in political attitudes increasingly fall along educational lines. In 2020, the college/non-college gap in Biden/Trump voting was as large as the gap between New York and Mississippi. This is part of a long-running worldwide trend in the realignment of political coalitions (Gethin, Martínez-Toledano, and Piketty, 2022), but we show below that the recent growth has been dramatic. We also show that in this period, college graduates are increasingly reluctant to move into Republican-governed states, reducing the human capital stock of those states. Our main goal in this paper is to document this effect and quantify its consequences for the pattern of economic activity.

We begin by using state-of-the-art tools borrowed from political science to create comparable indices of policy views on economic and social/cultural issues over the last 15 years. We find that the gap in policy views between college and non-college voters has grown dramatically since 2010. On economic issues, as recently as 2010, there was *no* college/non-college gap in policy views, while today, the average college voter is .4 standard deviations to the left of the average non-college voter. On social and cultural issues, while the gap shrunk during the later years of the Bush Administration and was roughly stable from 2010-2015, it more than doubled from 2015-2020 (to .5 standard deviations). We show that this implies that higher earners are, on average, far to the left of lower-earning voters on both social and economic issues.

We next estimate the migration effects of transitioning from a Democratic governor to a Republican one. As we discuss below, we focus on governors because they are very salient and have a large influence on policy outcomes. Using a difference-in-difference strategy, we find that a Republican governor, relative to a Democratic one, reduces college graduates' in-migration by about 8% per year.[1] Put differently, in the average state, 3% of the college-educated workforce lived in a different state one year earlier. Our estimates imply a 0.25 percentage point decline in this flow, which over four years implies a 1% decline in the stock of college-educated workers. This is slightly smaller than one year of growth in the average state's college-educated workforce.

In interpreting this magnitude, it is important to emphasize that neither our empirical results nor our model implies that individuals are uprooting their families to flee Republican governors. Indeed, like most of the literature (Monras, 2020), we find no effects on out-migration. Instead, our results imply that other factors lead individuals to decide to move to a new state, and conditional on having decided to move, individuals take politics into account as one factor in choosing *where* to live.

We find larger and significant effects from a regression discontinuity design, though we prefer the difference-in-difference estimates. We also rule out the possibility that migrants are responding to economic conditions or beliefs about voters instead of the governor. Finally, using our specification to estimate effects on economic policies, social policies, and college residents' approval of the governor, as well as how these effects vary over time, we present suggestive evidence for both policy motives and more superficial symbolic motives.

Overall, then, we interpret our migration effects as labor supply responses reflecting that college graduates are reticent to live under Republican governance. This idea is not new.[2] Conservative states spend millions advertising low taxes in liberal states (Moretti and Wilson, 2017). As an example, Republican-controlled Indiana ran a billboard in neighboring Democratic-controlled Illinois asking whether residents were "Illinoised by higher taxes" and encouraging them to "Come to Indiana: A state that works." At the same time, however, Indiana Governor Mike Pence was signing into law one of the most controversial anti-LBGTQ laws in the nation, and one of Indianapolis' Democratic City Councilmembers noted, "The real harm, to all [of us], is that it makes us all look like backwater hicks. Indiana is losing jobs and

[1] Focusing on states that change gubernatorial partisanship is a limitation in the sense that we cannot speak to the full range of political variation in the United States since much of the US policy variation is a persistent difference between liberal and conservative states (Caughey and Warshaw, 2016). At the same time, however, drawing identification from "switchers" speaks directly to the effects of changes in partisanship, which we consider policy relevant.

[2] In a 2017 survey, Smith, Hibbing, and Hibbing (2019) find that one in four respondents report that politics had led them to consider moving. The challenge of attracting college graduates to conservative states has garnered considerable attention since the Supreme Court's *Dobbs* decision gave states more authority over abortion laws (Cain et al., 2022; Hagelgans and Basi, 2022; Keshner, 2021; Leonhardt, 2021). Our results show that this challenge is real but broader than abortion laws alone.

young professionals like crazy. How much more can our state government make Indiana uninviting (Eason, 2015)?" The rhetoric of making Indiana "uninviting" is notably similar to the rhetoric conservative policymakers use to discuss high taxes. In a sense, then, our paper shows that this councilmember was correct and that conservative policies are an implicit preference tax on the average college graduate.

To understand the consequences of this, we analyze a structural model of migration that builds on Bryan and Morten (2019). College and non-college workers choose where to live based on wages, migration costs, and general amenities, and are combined in state-level production. Our key innovation is adding a "political amenity" that depends on the partisanship of the governor. We estimate most parameters following the approach developed by Bryan and Morten (2019) and calibrate the political amenity to match our reduced form results. We use the calibrated model to simulate various counterfactuals reflecting plausible shifts in partisan control and focus on two types of general equilibrium forces that would be difficult or impossible to study using reduced form methods.

First, we study how these labor supply responses affect inequality, both because inequality is an important trend in the US economy (Acemoglu and Autor, 2011) and because it plays a key role in political polarization.[3] Reducing in-migration of college graduates has two primary effects on college/non-college earnings inequality. On the one hand, reducing the relative supply of college graduates raises their relative wages (a price effect). On the other hand, migrants are positively selected relative to non-migrants, since they must receive employment opportunities sufficient to justify the move. Thus, reducing the share of migrants among college graduates reduces the average productivity of college graduates and thereby reduces earnings inequality (a composition effect).[4]

Theoretically, it is ambiguous which force will be most important, but in our counterfactuals the composition effect dominates the price effect. As a result, Democratic control of a state attracts more college graduates but also raises their earnings relative to non-college workers. This occurs even without agglomeration forces or knowledge spillovers, but solely because cross-state migrant college graduates are more positively selected than in-state college graduates. This is an interesting observation because many of the most progressive areas of the country (e.g., coastal cities and states) have seen growing college workforces, increasing in-migration from other states, and rising earnings inequality, and these effects have often been attributed to

[3]In the words of James Madison, "the most common and durable source of factions has been the various and unequal distribution of property" (Gunderson, 2022). The political consequences of income inequality have been a major research area in economics (Guriev and Papaioannou, 2022), political science (McCarty, Poole, and Rosenthal, 2008; Iversen and Soskice, 2015), and psychology (Power, Madsen, and Morton, 2020).

[4]There is also a third channel that is quantitatively small. The marginal migrant deterred by a Republican governor is less positively selected than inframarginal migrants, and so deterring them raises the average human capital of college graduate in-migrants.

competitive productivity advantages or land regulations, rather than the selectivity of workers.

The second general equilibrium force we focus on is the cross-state spillovers inherent to migration. Deterred would-be migrants choose to live somewhere else, so changes in the incentives to move to one state ripple throughout other states. Our model lets us quantify these spillovers, which we find can be considerable.

At the broadest level, our results speak to the real-world consequences of political polarization. Despite the widely held view that polarization is an important social phenomenon, there is little evidence of its effects on tangible economic outcomes.[5] As US politics grows more hostile and divisive (Boxell, Gentzkow, and Shapiro, 2024), it is important to understand its consequences beyond purely political outcomes.

Our results also contribute to recent work exploring the role of education in political divides (Firoozi, 2022; Gethin, Martínez-Toledano, and Piketty, 2022; Kuziemko, Marx, and Naidu, 2023). Recently, Gethin et al. (2022) combined data from 70 years and 21 countries to show that increased college graduates' support for left-of-center parties is part of a universal and long-run trend. This suggests that the forces we focus on will likely continue to be relevant, regardless of short-term changes in the influence of specific candidates or issues. It also illustrates that these forces are relevant beyond the United States. For instance, Brox and Krieger (2021) show that anti-immigration protests in Germany led to declines in in-migration into affected cities which was driven by college graduates, and Pickard et al. (2022) show that voters opposed to Brexit began migrating away from pro-Brexit districts after the vote.

Finally, our results contribute to work on the role of politics in spatial sorting. A growing literature using stated preference experiments studies the role of politics in neighborhood choice (Mummolo and Nall, 2017; Koşar, Ransom, and Van der Klaauw, 2022) and the role of specific policies—like abortion laws (Baumle, Miller, and Gregory, 2023) or political reforms (Nelson and Witko, 2022)—in state choice. Our reduced form empirical results imply that the insights from these studies aggregate in two important ways. First, aversion to policies that are disproportionately enacted by Republican politicians implies aversion to the Republican Party. Second, a stronger aversion to these things among Democrats (who are disproportionately college-educated) implies a stronger aversion, on average, among college graduates. Our structural analysis then quantifies the consequences of these findings.

The rest of the paper is organized as follows. Section 2 documents our descriptive results. Section 3 estimates the causal effects of governors on migration. Section 4 presents our model, estimation, and counterfactual analysis. Section 5 concludes.

[5]A noteworthy exception is McConnell et al. (2018), who experimentally show that workers require significant compensating differentials to work for employers whose political views oppose their own.

2 Descriptive facts about education polarization

We begin by documenting trends in "education polarization," by which we mean the difference in policy preferences between college graduates and non-college graduates.

2.1 Data and methods

Our main data is from the Cooperative Election Study (CES) operated by Harvard University and conducted by YouGov. The CES began in 2006 with roughly 35,000 respondents and today includes roughly 60,000 respondents per even-numbered year (much larger than any similar survey). For our purposes, the key feature is that the CES asks a large number of policy questions about diverse topics, with an eye toward comparability over time. Since its inception, the CES has asked 15-50 different policy questions during each wave, far more than other political surveys.

Using these policy questions, we create indices of policy preferences on social and economic policies. We classify questions using the topic lists developed by Caughey and Warshaw (2018); Appendix Tables A1 and A2 contain details.[6] While the specific set of covered issues changes from year to year, questions are always chosen to capture the central issues in political debates. Thus, trends in our indices do not necessarily reflect changes in views on the exact same questions but changes in views on the questions that are important to political divides at the time. We see this as an advantage,[7] although it means we cannot distinguish between changes in specific views and the specific issues debated within the broad classification of "social" and "economic" issues.

To estimate indices, we follow the best practices in political science and estimate ideal points using Item Response Theory (IRT). IRT was developed for use in standardized testing, where not all test takers receive the identical test, and not all questions are equally informative about underlying ability. It is widely used in political opinion research because (1) it does not require the analyst to specify, ex-ante, which questions are more or less informative about ideology, and (2) unlike other methods (such as principal component analysis), it generates comparable scores across respondents even when the set of questions changes over time, so long as there

[6]Examples of social issues including gun control, abortion, immigration, and policing; examples of economic issues include financial regulation, environmental policy, taxation, and government spending. The average survey wave includes 13.5 social policy questions and 13.9 economic policy questions.

[7]For example, in 2006, there were major political divides over whether school science classes should teach intelligent design and whether social security should be privatized. By 2020, these issues were irrelevant; voters instead debated whether schools should require face masks and whether Medicare should be universal. Changing the questions over time is necessary for the index to reflect actual political disagreement. Of course, some of the more permanent divides in US politics (such as questions about tax rates and government spending) appear continuously in each CES wave.

are "bridge" questions that are continuous from one survey wave to the next (see Caughey and Warshaw (2015) for some discussion). For this reason, the CES intentionally includes bridge questions in each survey wave. IRT estimation does not require assumptions about the sign of individual questions, but researchers must normalize the final index. We follow the standard practice and normalize the index so that more negative values indicate more liberal (left-leaning) views and more positive numbers denote more conservative (right-leaning) views. The index is normalized to be mean zero and unit standard deviation across all respondents across all years. We estimate our indices using an Expectations Maximization algorithm (Imai, Lo, and Olmsted, 2016).

Because our interest is in education and migration, we focus on respondents aged 26-45. We focus on respondents over age 26 to ensure that college education has been completed.[8] We end our range at age 45 because of a steep decline in rates of inter-state migration (see Appendix Figure B1). In general, all of the trends we document are similar when we include the full population; older citizens have different *levels* of views, but the trends are similar.

2.2 Differences in policy preferences by education

Figure 1 presents our main results about "education polarization" (i.e., the gap in policy views between college and non-college respondents). Panels (a) and (b) show the evolution of college and non-college respondents' social and economic policy views over time.[9]

On social issues, both college graduates and non-college have drifted considerably left over time, consistent with widely recognized changes in views on issues like divorce, gay marriage, and marijuana legalization. However, while college graduates have always had more liberal views, the gap has not been constant. Panel (c) shows that the gap fell during the end of the Bush Administration (2006-2008) and was roughly stable from 2008-2015, though it has been growing steadily since the candidacy/election of Donald Trump.[10] From 2015 to 2020, it more than doubled from $.2\sigma$ to roughly $.5\sigma$.

[Figure 1 about here.]

[8]We only focus on Bachelor's degree completion and never aim to separate those with graduate degrees from the Bachelor-only population.

[9]We see similar trends when we look at the gap in *median* (rather than mean) views in Appendix Figure B4, which is important because the ability to distinguish extreme views has changed over time as more policy questions have been added to the CES.

[10]Our aim in this paper is not to determine the source of education polarization, but the coincidence with the timing of Donald Trump's presidential campaign is noteworthy. Various commentators have argued that changes in college-educated political views and campus political climate pre-dated Trump's campaign (Haidt and Lukianoff, 2021; Mounk, 2023; Yglesias, 2019) but were accelerated by it. Moreover, similar trends are seen in many other countries (Gethin et al., 2022).

Panel (b) shows a different pattern for economic policy issues. Historically, there was no gap in average views, but since 2010, college-educated citizens have moved considerably left (by $.2\sigma$-$.3\sigma$), while non-college voters tend to have moved modestly to the right. Panel (c) shows that the gap in views has grown to roughly $.4\sigma$. Thus, while educational gaps in views on social and economic issues started at somewhat different *levels*, the post-2010 trends in both are similar, and today, gaps in both are similar.

While differences in *average* views are helpful, they are necessarily a simplifying summary. In Appendix Figure B5, we plot the full distributions of social policy preferences for college and non-college respondents in 2010 and 2020. The change in distributions is striking. Over this period, although the median Democrat moved $.2\sigma$ to the left, there was a five percentage point (12%) increase in the share of college graduates left of the median Democrat and a 5pp (18%) decrease in the share of non-college voters who were. Thus, college graduates went from being 50% more likely than non-college to fall left of the median Democrat to being 120% more likely.

Finally, in panel (d), we consider whether college graduates' views of the two parties have changed. To do so, we turn to the American National Election Study (ANES), which is available for a longer time period but with a smaller sample. Following the standard approach from the literature (Boxell, Gentzkow, and Shapiro, 2024), we measure respondents' attitudes towards the parties using a 100-point scale called a "feeling thermometer." We find that for the average college graduate, the gap in their views of the parties has risen from a 2-point preference for the Democratic Party to a 9-point preference. This increase is about 40% as large as the growth in the gap between how Democrats feel about the two parties (an 18-point increase). Put differently, for the average college graduate (irrespective of partisanship), their preference for the Democratic Party has grown by nearly half as much as the average *Democrat's* preference for the party. Among college graduates who identify as Democrats (a growing share of graduates), the gap has risen from 34 points to 51 points.

Interestingly, panel (d) shows that college graduates' views of the Democratic Party have also worsened, simply by much less than views of the Republican Party.[11] This is even true for college graduates who identify as Democrats, although that 6-point decline has been modest. In Appendix Figure B6, we present trends in partisan identification among college graduates and views of the Democratic Party by partisan identification. While college-educated Republicans' views of the Democratic Party have fallen by 21 points, the share of college graduates who identify as Republicans has also fallen by 9 points. Thus, an important part of the decline comes

[11] One possible explanation we cannot assess is that many college graduates, particularly young ones, are disappointed by the Democratic Party's inability to make sufficient progress on key issues (Lawless and Fox, 2015).

from college graduates who identify as independents, which has been rising, and among whom we see a 16-point decline in views of the Democratic Party. In summary, then, college graduates' attitudes towards *both* parties have been worsening, though much more so for attitudes towards the Republican Party. We use this fact below to interpret some of our findings about the effects of Republican-to-Democrat gubernatorial transitions.

2.3 Differences in policy preferences by earnings

Most state policymakers care about attracting a skilled, productive workforce, which may not be the same as attracting college graduates. If the college/non-college gaps we see are driven by a subset of left-leaning graduates with low earnings, then a conservative policymaker might be unconcerned, and reasonable minds may disagree about the costs to economic growth. If, on the other hand, the gap in policy views holds broadly when comparing high- and low-earners, then virtually all policymakers would be concerned about the consequences of pushing away these workers.

One challenge in assessing this is that the CES does not collect information on individual-level earnings or income.[12] For most recent years, however, it does collect respondents' sector of employment (2-digit NAICS), in addition to education and age.[13] This information is sufficient to impute earnings with high accuracy. To do so, we rely on an approach that adjusts earnings for geography. Coastal areas and large cities are more liberal and have higher nominal wages, and we would not want to conflate political differences by earnings with a spurious correlation between regional earnings differences and local political dynamics. Using the American Community Survey (ACS), we regress log annual earnings on geography-by-time fixed effects and age-by-education-by-industry fixed effects:

$$\ln(y_i) = \alpha_{m,s,t} + \theta_{a,n,e} + \varepsilon_i \tag{1}$$

where i denotes individual, m denotes MSA, s denotes state, t denotes year, a denotes age range (25-32, 33-40, 41-55), n denotes industry (2-digit NAICS), and e denotes education (college graduate vs. non-college). The MSA-by-state-by-year fixed effects ($\alpha_{m,s,t}$) account for heterogeneous levels and trends in nominal earnings in a flexible way, and the age-by-industry-by-education fixed effects ($\theta_{a,n,e}$) therefore represent persistent earnings differences between different types of workers, regardless of where they live. We see these estimated $\hat{\theta}_{a,n,e}$ fixed effects as CES respondents' *earnings capacity*, which is relevant for a state attempting

[12]The CES *does* collect information on household income, but *i*) it is collected only for relatively broad income ranges, and *ii*) household income includes income from both partners in married or cohabiting families, and patterns of assortative matching and marriage rates differ by education and have changed over time.

[13]Industry is collected in 2011-2014 and 2016-2020.

to attract high-productivity workers.[14] In this way, we determine the earnings capacity for all employed CES respondents (ignoring non-employed respondents) and normalize our measure to have unit standard deviation across all employed respondents across all years.

Figure 2 shows the basic patterns of earnings polarization. In Panel (a), we plot the coefficients on our earnings measure from year-by-year regressions of social and economic policy views on earnings capacity (again for workers aged 26-45). Earnings had little correlation with policy views through 2014, though in 2011, higher earners were slightly more conservative on economic policy issues. However, since then, higher earners have become substantially more left-leaning on both social and economic issues. By 2020, a 1σ increase in earnings capacity is associated with a $.12\sigma$ shift left in policy views on both sets of issues.

This relationship is entirely driven by education. Panel (b) presents coefficients that come from regressing views on both earnings and education. Once we control for education, high earners are more conservative on both economic and social policy issues, and this relationship has not changed at all over time. The trends in "earnings polarization" arise entirely from the college/non-college gap, which, once we control for earnings, has increased from roughly $.25\sigma$ in 2011 to $.55\sigma$-$.65\sigma$ in 2020.

[Figure 2 about here.]

Finally, panels (c) and (d) show the distribution of earnings capacity and 2020 policy views across all age-education-industry cells. There is a striking separation between college and non-college workers, with almost zero overlap. At virtually any point in the earnings distribution, all types of college graduates are to the left of all groups of non-college workers on both economic and social policy issues. The figure shows that differences between education groups are dramatically larger than any differences within the college and non-college population, at least among groups that we can measure in the data.[15] For this reason, when we study migration responses to gubernatorial partisanship, we focus only on differences by education and do not seek to explore differences by age or industry.

[14] One advantage of this approach relative to simple average earnings is that changes over time in the relationship between policy views and earnings capacity reflect changes in different groups' views, holding fixed the earnings position of any given "group" (i.e., age-industry-education cell).

[15] Earnings and political views do, of course, differ within age-industry-education cell, and we can say nothing about the within-cell correlation in views. Existing causal evidence from the University of California's admissions policies, however, shows that attending a more prestigious university increases earnings (Bleemer, 2021) *and* pushes students further to the left (Firoozi, 2022). These effects are consistent with the correlation across all universities, which shows that students at more prestigious universities are generally further to the left (Firoozi, 2022). Thus, it is plausible that within these cells, higher-earning college graduates are even further to the left of their peers, although we cannot test this in our data. In other data, we can test differences across college majors, and there is very little correlation across majors between earnings and political views (see Appendix Figure B7).

2.4 Explaining education polarization

Although it is not our goal to explain education polarization, our data allows us to speak to several prominent explanations, and we find some evidence in favor of most. It is worth noting that these explanations aim to explain slightly different facts, such as the rise in affective polarization between Democrats and Republicans (Bishop, 2009; Klein, 2020), college vs. non-college support for the Democratic Party (Gethin et al., 2022; Kuziemko et al., 2023), or aggregate changes in views (Schumpeter, 1942). Nonetheless, all have some bearing on our results about changing views between college and non-college workers.

First, conservatives often allege that universities have become increasingly left-wing, increasing both the treatment effects (the college experience pushing students left) and selection effects (more left-wing students choosing to go to college) of higher education. In panel (a) of Figure 3, we separate education polarization into within-cohort and between-cohort changes. More recent birth cohorts indeed show larger college/non-college gaps at age 26. However, these trends are almost perfectly matched by widening gaps within previously educated cohorts. This is inconsistent with changes in the university experience and points instead towards changes in our political climate that college and non-college voters respond to differently.

[Figure 3 about here.]

One possible such change, suggested by Gethin, Martínez-Toledano, and Piketty (2022), is that the increasing importance of social issues rather than economic ones has led college graduates towards the Democratic Party since (as we showed above) college graduates have long been to the left of non-college voters on social issues. In panel (b), we predict party choice using social and economic policy views, separately for college and non-college voters, and plot the estimated coefficients over time. Consistent with Gethin et al. (2022), since 2010, the importance of social issues for explaining party choice has increased by one-third, while the importance of economic issues has fallen by half. All else equal, this shift will increase college graduates' support for the Democratic Party and non-graduates' support for the Republican Party. However, as we showed above, there are also dramatic shifts in views on these issues themselves, which this story cannot explain.

A very different explanation – advanced by Mason (2015) and popularized by Klein (2020) – is that the distinction between economic and social issues is increasingly unimportant as voters' political identities have collapsed into a single dimension. This can exacerbate affective polarization and hostility by eroding the common ground that voters of different groups might otherwise have shared on different sets of issues. Panel (c) is consistent with this, as both college and non-college voters show a rise in the correlation between social and economic policy views since 2009. However, without some additional theory for reinforcing pressures in opinion formation, this cannot explain why views themselves have diverged between college and non-college voters.

One possible type of reinforcing pressure is associated with Bishop (2009). He shows Democrats and Republicans increasingly live in different places and argues that if exposure to neighbors affects political views, then the clustering of like-minded voters can exacerbate polarization via an echo chamber-like effect.[16] Looking at social issues (where educational gaps are longstanding), panel (d) does show that states with more college graduates prior to 2010 saw greater divergence between college and non-college voters after 2010.

Another explanation aims to explain changes in views over time, specifically for economic policy. Kuziemko, Marx, and Naidu (2023) show that college graduates tend to support redistribution (taxes and transfers) while non-college voters support "predistribution" (market intervention and regulation). They argue that as the Democratic Party has increasingly prioritized redistribution over predistribution, this has pulled in college graduates and pushed away non-graduates. If political debates about economic issues have also shifted from predistribution to redistribution, then our indices of economic policy views (which capture the main content of political debates at the time) would show a divergence between college and non-college voters, even if there were no changes in underlying views on fixed issues. In panel (e), we present college and non-college support for increasing taxes (redistribution) and the minimum wage (predistribution). Consistent with Kuziemko et al. (2023), non-college voters are more likely to support increasing the minimum wage, while college voters are more likely to support tax increases. Thus, holding views fixed, these shifts in the Democratic Party's priorities will attract college graduates and repel non-graduates. However, the views have not been fixed over time. Comparing 2020 to the end of the Bush Administration, non-college voters have shifted to the right on both predistribution and redistribution, eliminating the college/non-college gap in support for the minimum wage and doubling the gap in support for tax increases.

Finally, over 80 years ago, Austrian economist Joseph Schumpeter (1942) predicted that economic growth would lead to a class of intellectuals, journalists, and bureaucrats who are sufficiently far removed from production, innovation, and market competition to abandon capitalism for socialism.[17] Panel (f) presents trends in the economic policy attitudes of college graduates working in select industries, measured relative to the nationwide average non-college

[16] While the book was controversial at the time (Abrams and Fiorina, 2012), more recent evidence has largely supported both the stylized fact of a modest increase in partisan sorting (Kaplan, Spenkuch, and Sullivan, 2022), as well as his key mechanism that exposure to neighbors affects political attitudes (Perez-Truglia and Cruces, 2017; Cantoni and Pons, 2022).

[17] See Sobel (2021) for some helpful discussion. At the time (1942), Schumpeter argued that this trend had already begun (without quantitative evidence). More recently, using data back to the late 1940s, Gethin et al. (2022) show that the shift of better-educated voters towards the Democratic Party had indeed begun as early as their data can measure.

voter during the same year. Perhaps consistent with Schumpeter's prediction, college graduates working in education and arts/entertainment have long been well to the left of non-college voters and have trended substantially further. However, in sectors like information and professional and business services (key sectors driving innovation and productivity growth in recent years), we see the same trends despite starting to the right of non-college voters. Even in finance – where workers surely continue to think about capitalism, economic growth, and market competition – college graduates have drifted from being $.1\sigma$ to the right of non-college voters in 2012 to being roughly equal by 2020. Only in manufacturing (the quintessential production sector) do we see steady levels of economic policy views relative to non-college voters.

Indeed, then, we find support for five common claims, although none offers a complete explanation for the patterns we document. Indeed, we do not believe there *is* a singular explanation for the rise in education polarization. For this reason, below, we do not model endogenous polarization.

3 Reduced form estimates of migration responses

Our main interest is how college-educated workers respond to changes in the partisan control of state government. For this reason, we focus on the partisanship of the governor. It has long been recognized that citizens know more about their governor than other elected officials, and governors garner more media attention (Hinckley, Hofstetter, and Kessel, 1974; Squire and Fastnow, 1994). Even today, although politics has nationalized, 78% of 2020 CES respondents could correctly identify the partisanship of their governor, compared to only 45-60% for their senators or representatives in the House.

Moreover, governors have significant effects on state policy outcomes, and the size of these effects has grown over time (Caughey, Xu, and Warshaw, 2017). In part, this is because governors face few checks and balances or constraints on authority (Seifter, 2017); in part, it is because they use their veto power much more actively than presidents do; in part, it is because they are typically the *de facto* leaders of their political party within the state, letting them set agendas and drive legislation. Below, we show that gubernatorial transitions have large policy effects. At the same time, however, we recognize that governors also become well-known for controversial statements, acts, and political performances, and these, too, might affect migration behavior. In Section 3.4, we aim to distinguish between these two explanations.

3.1 Data

Our primary migration data is from the American Community Survey (ACS), a nationally representative 1% sample of the US population conducted by the Census Bureau, accessed via IPUMS (Ruggles et al., 2022). Microdata includes respondents' state-of-residence, age, education, employment details, and last year's state-of-residence. This allows us to calculate annual inter-state migration rates by education. Relative to studies using state of birth and residence to measure migration (e.g., Bryan and Morten (2019)), a key advantage is that we observe the specific timing of the move.[18] We use the ACS back to 2000 (when it was introduced), although from 2000-2004, the sample size was considerably smaller than the 1% sample design used since 2005.

Our primary focus is on migration flows, calculated separately by whether the respondent has at least a Bachelor's degree. We define in-migration rates in state s in year t as the fraction of respondents living in state s in year t who report living in a different state in year $t-1$. We always use ACS sample weights when calculating migration flows. Throughout the paper, we never analyze migration from 2020 onwards because we are concerned that during the main pandemic years, respondents' state-of-residence is either not well defined, not well measured, or is only temporary. Thus, we always end our migration analysis window in 2019. Finally, our primary analyses focus on respondents who are at least age 26 (so that educational attainment has been completed) and employed in the private sector (and thus most relevant for economic activity), although we show the results are similar for more restrictive and less restrictive samples in Appendix Table B2.

We observe migration from 2000-2019. Since all elections occur in November and governors take office in January, we treat migration during the election year as pre-election and the year after the election as the first post-election year. For our regression discontinuity, we use elections 2000-2016 (governors take office 2001-2017) so that we have at least three post-election years to measure migration outcomes. For our difference-in-difference estimators, we restrict further and only use elections occurring 2002-2016 (governors take office 2003-2017) so that we also have at least three pre-election years to test pre-trends.

3.2 Methods

At a broad level, we use two identification strategies to estimate the effects of governor partisanship on migration: a regression discontinuity design (RDD) based on close elections and

[18]Some migration studies use the IRS migration files. This data does not contain education, and the college/non-college earnings distributions are not sufficiently distinct to impute it. Other recent work studies migration using voter registration data or change of address data (Brown et al., 2023). This data also does not contain education.

a staggered rollout difference-in-difference (DiD) estimator. Below, we discuss the details of our approaches, including separating the effects of Democrat-to-Republican (D-to-R) transitions and Republican-to-Democrat (R-to-D) transitions. Throughout the paper, we focus on four-year windows (since gubernatorial terms are four years in 48 of 50 states). Readers interested in which states and election years drive identification in these various specifications may refer to Appendix Figure B8.

3.2.1 Regression discontinuity design

Our RDD approach is standard. Our main outcome variable is the mean migration rate in the state, averaged across the four post-election years that comprise a typical gubernatorial term. Letting t denote an election year and $v_{s,t}$ denote the Republican share of the two-party vote minus one-half (i.e., centered at zero so that a Republican wins when $v_{s,t} > 0$), then our primary estimating equation is:

$$\bar{Y}_{s,t}^{post} = \alpha_0 + \alpha_1 v_{s,t} + \alpha_2 \big(v_{s,t} \times 1\{v_{s,t} > 0\}\big) + \beta 1\{v_{s,t} > 0\} + \varepsilon_{s,t} \tag{2}$$

where $1\{\cdot\}$ is the indicator function and β is the coefficient of interest, reflecting the discontinuous change in post-election outcomes when a Republican wins by an asymptotically close margin. We use linear polynomials and always use the Calonico, Cattaneo, and Titiunik (2014) estimator and optimal bandwidth. As discussed below, we also control for pre-election migration rates in our primary specification.

3.2.2 Difference-in-difference

The RDD approach is usually considered the gold standard for estimating the effects of election outcomes but uses very few elections. Therefore, it is imprecise and not well suited for studying how effects vary over time or across different types of flips that further divide our sample. Moreover, as we discuss below, we believe that close elections are not representative of the average gubernatorial flip.

For this reason, we also use a DiD approach to estimate the effects of a gubernatorial flip relative to keeping the same gubernatorial party. Because different states elect different governors at different times, this is a staggered rollout problem. Thus, we use a stacked DiD estimator (as proposed by Cengiz et al. (2019)) and adjust the weights (as proposed by Callaway and Sant'Anna (2021)) to yield a consistent estimator of the Average Treatment Effect on the Treated (ATT).

As a concrete example, in 2012, North Carolina elected a Republican governor to replace the outgoing Democrat. This governor took office in January 2013. At the time, there were six other states that had Democratic governors in office during the entire ten years surrounding this election (i.e., 2008-2017). We estimate the effects of this flip by comparing the change in migration inflows into North Carolina from 2013 to 2016 with the simultaneous changes experienced in these six other states.

Dube and Zipperer (2014), Cengiz et al. (2019), and Callaway and Sant'Anna (2021) propose this as an estimator for the treatment effect of the gubernatorial flip in North Carolina, which avoids the problems with using a two-way fixed effects (TWFE) estimator (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). Callaway and Sant'Anna (2021) show that averaging many such estimates yields a consistent estimator for the ATT. Following Cengiz et al. (2019), we estimate this ATT by stacking all treatment events together (in comparable treatment time) within one regression. This means that many state-years will appear multiple times (serving as controls for different treatments in different states at different times), so we cluster our standard errors at the state level. Also, as shown by Callaway and Sant'Anna (2021) and Wing, Freedman, and Hollingsworth (2024), this only yields a consistent estimator for the ATT when using adjusted weights such that each state-year's weight is proportional to the number of states that were treated during that particular treatment cohort.

Thus, letting $\tau \in \{2003, ..., 2017\}$ denote a treatment cohort (i.e., the year in which the flip occurred), we estimate the following stacked DiD regression:

$$Y_{s,t,\tau} = \alpha_{s,\tau} + \delta_{t,\tau} + \sum_{k \neq -1} \beta_k \big(Flip_{s,\tau} \times 1\{t = k\}\big) + \varepsilon_{s,t,\tau} \tag{3}$$

where s denotes state, $t \equiv y - \tau$ denotes treatment time (where y is the calendar year), state-years are weighted by the number of ACS respondents but with the adjustment to be proportional to the number of treated states in the treatment cohort,[19] $1\{t = k\}$ is an indicator variable that $t = k$, and $Flip_{s,\tau}$ is an indicator that state s's governor is a different party in year τ ($t = 0$) than they were in year $\tau - 1$ ($t = -1$). Our sample includes any treatment cohorts that include both treatment and control states (in practice, there are typically many control states per treatment state), all treated states from that cohort, all control states that had stable governor partisanship from $t = -5$ to $t = 4$ (i.e., five years pre-treatment and five years post-treatment, at a minimum), and all state-years four years before or after treatment (i.e., $-4 \leq t \leq 3$).

We estimate this separately for samples where treatment and control states both had Democratic governors at $t = -1$ and therefore $Flip_{s,\tau}$ reflects a flip to a Republican governor, as well

[19] Formally, let $p_{s,t}$ be the number of ACS respondents in the state-year. Let $N_{T=0,\tau}$ be the number of control states for treatment cohort τ and $N_{T=1,\tau}$ be the number of treated states. Then the weights for any observation denoted by s, t, τ are given by $p_{s,t}$ if s is treated in cohort τ and $\frac{N_{T=1,\tau}}{N_{T=0,\tau}} p_{s,t}$ if state s is a control in cohort τ.

as when both had Republicans at $t=-1$ and $Flip_{s,\tau}$ denotes a switch to a Democrat. We refer to these as the effects of D-to-R and R-to-D transitions, respectively. We sometimes present dynamic coefficient plots and sometimes pool all post-treatment coefficients into a single estimate using the following simplified version of Equation (3):

$$Y_{s,t,\tau} = \alpha_{s,\tau} + \delta_{t,\tau} + \sum_{k<-1} \beta_k \big(Flip_{s,\tau} \times 1\{t=k\}\big) + \beta_{post}\big(Flip_{s,\tau} \times 1\{t \geq 0\}\big) + \varepsilon_{s,t,\tau} \tag{4}$$

which assumes a single constant treatment effect: $\beta_{post} = \beta_0 = \beta_1 = \beta_2 = \beta_3$.

The time effects $\delta_{t,\tau}$ are important because migration patterns vary over time, including in response to the large recession in the middle of our sample (Jia et al., 2022). We assume that these temporal fluctuations are proportional,[20] and therefore use log migration rates in our DiD specifications (Roth and Sant'Anna, 2023).

We use the regressions above – equations (3) and (4) – to estimate the treatment effect of gubernatorial flips and to test whether pre-trends are parallel between treated and control states. However, it is also useful to visualize these changes over time to understand whether migration (or some other outcome) was increasing or decreasing leading up to the flip. To do so, we present the raw plots of levels over time for treatment and control states by normalizing levels by the pre-treatment mean: $\tilde{Y}_{s,t,\tau} = Y_{s,t,\tau}/\bar{Y}_{s,pre,\tau}$ where $\bar{Y}_{s,pre,\tau} = \frac{1}{4}\big(Y_{s,-4,\tau} + Y_{s,-3,\tau} + Y_{s,-2,\tau} + Y_{s,-1,\tau}\big)$.[21]

We also estimate a modified version of these regressions that pools both types of treatment effects, imposing the assumption that they have opposite signed effects:

$$Y_{s,t,\tau,F} = \alpha_{s,\tau,F} + \delta_{t,\tau,F} + \sum_{k<-1} \beta_{F,k} \big(Flip_{s,\tau,F} \times 1\{t=k\}\big) + \beta_R Republican_{s,t,\tau} + \varepsilon_{s,t,\tau,F} \tag{5}$$

where $F \in \{DR, RD\}$ indicates whether the treatment event is a D-to-R transition or an R-to-D transition, and both types of treatment events are pooled into the same sample. Comparing (5) to (4), we need no additional identification assumptions (because we allow the set of fixed effects to differ by type of treatment) but impose symmetry for estimated election effects because $Republican_{s,t,\tau} = Flip_{s,\tau} \times 1\{t \geq 0\}$ for D-to-R flips, while $Republican_{s,t,\tau} = 1 - Flip_{s,\tau} \times 1\{t \geq 0\}$ for R-to-D flips.

[20] For instance, we assume that a recession might reduce all states' in-migration rates by half, and find this more plausible than assuming that a recession might reduce in-migration rates by one percentage point equally for states with baseline migration rates of 2% per year and 5% per year (in which case we might estimate specifications with migration rates in levels). In practice, when we estimate our specifications with migration rates in levels, estimated treatment effects are similar and significant, but pre-trends are sometimes worse.

[21] Sometimes we analyze outcomes that have no natural units, such as policy indices, which are mean zero by construction. In this case, we normalize by subtracting, rather than dividing by, the pre-treatment mean.

3.3 Main results

We begin with estimates from a regression discontinuity design (RDD). Figure 4 presents a binned scatterplot showing the relationship between the Republican vote share in the gubernatorial election (on the x-axis) and the fraction of college graduates living in the state who report having moved within the last year (on the y-axis), which we define as the college in-migration rate. When a Republican is elected governor instead of a Democrat (i.e., when the Republican share of the two-party vote exceeds 50%), there is a clear and pronounced almost one percentage point drop in the average annual in-migration rate during the next four years (from a baseline of around 4% per year).

[Figure 4 about here.]

Figure 4 presents migration rates in levels and without controls. It is, therefore, the most straightforward representation of discontinuous in-migration as a function of gubernatorial election outcomes. However, in our DiD models, we prefer to use log migration rates, and when we test for discontinuities in pre-election in-migration, we find a small and non-significant discontinuity that might inflate our estimated effects. Therefore, our preferred RDD specification (which is used below) uses log in-migration rates (to be comparable to the DiD) and controls for the log in-migration rate during the year leading up to the regression (to ensure that the non-significant discontinuity does not inflate our estimates). In Figure 5, we show that the RDD estimates are very stable and robust, regardless of whether we use migration rates in logs or levels, whether or not we control for pre-election in-migration, and regardless of the bandwidth. Looking at the specification in logs, which will be our main specification, we see a roughly 20% decline in in-migration flows.

[Figure 5 about here.]

The RDD estimates are appealing because they rely on very weak assumptions. However, such weak assumptions only allow us to use a small share of the data. We believe that a broader set of elections — and not only those that are very close — are useful for estimating the causal effects of governor partisanship on migration behavior. To illustrate this, consider three examples of recent elections when the partisanship of the governor changed for reasons that seem to be exogenous to migration incentives, even though the elections were not necessarily close.

In 2016, Missouri replaced its term-limited Democratic governor, Jay Nixon, with a Republican (who won by 6 points). Many argued at the time that poor Democratic Party performance was due to the 2014 murder of Michael Brown at the hands of police in Ferguson and mistakes made by party leaders (including Nixon and the Democrats' 2016 gubernatorial candidate, who had been the Attorney General at the time) in responding to the protests (Ortiz, 2016). In 2012, North Carolina elected a Republican governor for the first time since 1988 (who won by 11 points). The Democratic candidate was the incumbent Lieutenant Governor, and commentary at the time focused on the challenges he faced campaigning at the same time that several of the Governor's staff were facing obstruction of justice charges (Catanese, 2012) and the Democratic Party's senior staff was embroiled in sexual harassment proceedings (Bass, 2012). In 2015, Louisiana replaced the term-limited Republican governor with Democrat John Bel Edwards (who won by 12 points). Many argued that Edwards' defeat of Republican David Vitter was because of Vitter's recent prostitution scandal (McAfee, 2015), which led key Republicans to either endorse Edwards or no one (Barnes, 2015). These events appear to have driven (non-close) election outcomes but were likely exogenous to college graduates' migration incentives.

In order to use all gubernatorial transitions, we use a staggered rollout difference-in-difference estimator, as discussed above. We estimate the in-migration effects of a Democrat-to-Republican (D-to-R) transition relative to states that kept Democratic governors, as well as those of a Republican-to-Democrat (R-to-D) transition relative to states that kept Republican governors. Figure 6 shows the progression of in-migration rates (normalized by the levels in the pre-election years) for these groups of states. For both D-to-R and R-to-D transitions, we observe quite similar pre-trends between treated and control states, followed by a substantial differential decline in in-migration among states switching from a Democrat to a Republican and a modest differential increase for states switching from a Republican to a Democrat.

[Figure 6 about here.]

In Table 1, we present these estimated treatment effects numerically. The table presents the estimated effect of a Republican governor instead of a Democrat from the RDD (column 1) and the DiD (column 4), as well as separating the effects of D-to-R transitions (columns 2 and 5) from R-to-D transitions (columns 3 and 6). Both specifications estimate a statistically significant decline in in-migration resulting from a Republican governor, although the RDD estimate is only significant at the 10% level (conventional p-value: .052, bootstrapped p-value: .064). The DiD estimates are far more precise, which is the virtue of using more variation. It is notable, however, that those estimates are substantially smaller (roughly half the size of the RDD estimates).

Looking at the RDD plot in Figure 4, there appears to be somewhat elevated migration when the Democrat's win was very narrow, along with depressed in-migration when the Republican's win was very narrow. One interpretation is that college graduates are particularly put off by very narrow Republican wins that might be seen as illegitimate (as when Brian Kemp controversially defeated Stacey Abrams by less than 1.5 points [Georgia, 2018] while serving as the Secretary of State overseeing election administration) or are particularly enthusiastic about narrow Democratic wins which might be seen as turning points for the state (such as when the Democrat won in North Carolina in 2016 by only 0.2 points after the state was propelled to national controversy in response to its 2016 "transgender bathroom law"). For this reason, we believe that the magnitudes of the DiD estimates better reflect the average effects of a gubernatorial transition rather than the RDD estimates that reflect the local average treatment effect only for very close elections.

[Table 1 about here.]

The magnitudes presented in Table 1 imply that a Republican governor reduces college in-migration rates by about 8% per year, with slightly larger negative effects from D-to-R transitions than the positive effects of R-to-D transitions. How should we interpret this magnitude? The baseline migration rate in the sample is roughly 3.2%, meaning that for the average state year, 3.2% of its college graduates lived in a different state last year. Thus, an 8% decline is roughly equal to a .25 percentage point decline each year, aggregating to a 1% decline in the number of college graduates at the end of a typical governor's four-year term. For reference, the average state sees its college educated population grow by about 1.5% per year, implying that four years under a Republican governor sets a state's human capital accumulation back by almost one year. Another way to interpret this magnitude is to compare it to estimates from Monras (2020), which estimates how in-migration responds to wage changes. The migration response we estimate is roughly equal to the implied response from a 1.2% decline in wages. Thus, these magnitudes are large enough to be meaningful but, in our view, not implausibly large.

In the appendix, we show that these results are robust to both less restrictive (not conditioning on private sector employment) and more restrictive (also conditioning on citizenship and the age range we use for our descriptive analyses) samples. We also find no effects on non-college workers' migration. Point estimates are smaller and non-significant. One interpretation is that non-college workers care less about politics (which is true of self-reported political interest in surveys), although it is also true that those workers show lower baseline migration rates (see Appendix Figure B1) and might have weaker employment opportunities that do not offer the luxury of acting according to their political preferences (Enke et al., 2022).

Finally, we also find no effects for out-migration (i.e., the decision to leave the state). At least with the DiD, estimates are the opposite sign of the in-migration effects, but the magnitudes are much smaller and never statistically significant. This is consistent with existing evidence that in-migration responds to changing incentives to live in a state, while out-migration responds either not at all (Monras, 2020) or by much less (Howard, 2020). In our context, the most natural interpretation of this finding is that politics does not influence *whether* individuals decide to move to a new state. Instead, a person's decision to move to a new state is likely based on factors like completing graduate studies, deciding to change jobs, key school transitions of one's children, or "shocks" like these for one's spouse.[22] Thus, our evidence does not suggest that politics is a major reason to migrate, but instead plays a role in deciding where to migrate after changes in life circumstances have led one to decide to do so. Our model in Section 4 matches this intuition by introducing an exogenous shock in the decision to move, similar to Monras (2020). However, before introducing the model, we aim to unpack the estimates we have presented here.

3.4 Mechanisms for politically responsive migration

Thus far, we have focused on the average effect of a Republican or Democratic governor. Here, we begin by asking how that effect varies over time and then use those results to understand why migration responds to governors' partisanship.

There are several reasons to believe that effects might be substantially different during the middle years of our sample and that this might help us separate various mechanisms behind the average effects we estimate. First, from roughly 2008 to 2015, the United States had elevated unemployment and a depressed labor market (Cunningham, 2018). Enke, Polborn, and Wu (2022) present a model in which political values are luxury goods, which become more important to voters when incomes are higher. In this case, voters might be more responsive to political incentives when the labor market is strong enough to offer a decent set of employment options.

Second, from 2009-2016, the United States had a Democratic President. As US political views are increasingly shaped by national debates (Hopkins, 2018), a key piece of citizens' views of governors comes from tensions around whether states cooperate with controversial federal policies (e.g., education reform and the No Child Left Behind Act during the Bush Administration, healthcare reform and the Affordable Care Act during the Obama Administration, refugee resettlement during the Trump Administration). Thus, college graduates' reactions to governors might be fundamentally different when it comes to enacting the agenda of a Republican versus a Democratic President.

Finally, thus far, we have emphasized the results from Figure 1 showing that college grad-

[22] In the CPS Annual Survey of Economic Conditions (ASEC) respondents are asked *why* they moved. Among employed college graduates moving across state lines, the three most common reasons are for a new job or transfer (49%), family reasons other than marriage (9%), or attending or completing higher education (5%).

uates have gradually been moving left *relative to non-college voters*. However, from 2008-2014, all voters were shifting to the right on economic issues, college graduates simply by less so than non-college voters. Thus, even though college graduates were further left than non-college workers during this period, the reaction to conservative economic policies might have been different than during the earlier and later periods during which they were continuing to drift left.

For these reasons, we present results in which we separate the middle seven sample years (flips occurring 2007-2013) from the first four and last four (2003-2006, 2014-2017). We refer to these middle years as "recession years," although there are clearly other ways in which they differ, and the post-treatment years of these samples obviously overlap.

The DiD plots in Figure 7 show how college in-migration responds to D-to-R and R-to-D transitions during non-recession and recession years. Above, we found that migration was more responsive to D-to-R transition than R-to-D transitions. Here, we find that there is actually very little difference in the size of the response, but the timing is very different. A Republican governor substantially reduces in-migration outside of recession years but has no effect during recession years, while a Democratic governor substantially increases in-migration during recession years but has no effect outside of them.

[Figure 7 about here.]

Table 2 presents point estimates for these migration responses by time period, as well as a series of additional outcomes that might help us understand *why* Republican governors reduce in-migration. Specifically, there are two classes of explanations that are consistent with our evidence and arguments. First, Republican politicians generally enact Republican policies, and as we showed above, college graduates increasingly disprefer these policies. At the same time, however, governors are often salient and garner attention and controversy in the media. Sometimes, this is related to a specific policy (such as Mike Pence's support for a religious freedom law that critics argued would legalize discrimination on sexual orientation), but sometimes, it is not (such as Florida's Ron DeSantis' decision to fly asylum seekers from Texas to Martha's Vineyard). We call these two explanations policy motives and symbolic motives, respectively.

In order to separate the two, we examine three primary outcomes (corresponding DiD plots are included in Appendix Figures B9-B12, which mostly show parallel pre-trends). First, we consider indices of economic and social policies that Caughey and Warshaw (2018) construct based on roughly 150 different policies in place at the state level. These aggregate all of the high-profile, salient policies debated in modern politics, and their categorization matches the views we presented above because we used their classification of issues in constructing our indices. Second, we consider the approval rating of the governor. We do this using the 5-point gubernatorial approval scale in the CES, and we calculate this only among college graduates to reflect our population of interest.

We see all three of these measures as reflecting the substantive changes enacted by the governor, in the sense that the policy indices are based on actually enacted policies, and the gubernatorial approval ratings (since they are calculated among those living in the state) are more likely to reflect the actual changes being made by the governor, rather than the simplistic associations that we suspect are the basis of the views college graduates in other states hold. Both the policy indices and the gubernatorial approval scales have been normalized to have a standard deviation of one across state-years in our sample.[23]

[Table 2 about here.]

Given the plausibility that political values are luxury goods (Enke et al., 2022) and that workers only respond to political differences when they are afforded a decent set of employment options, it is perhaps unsurprising that Republican governors do not disincentivize college migration during recessions. Interestingly, however, outside of recession years, Republican governors substantially deter college graduates' in-migration despite having relatively small effects on policy and the approval of college graduates already living there. Such large migration effects, even amidst modest policy effects, suggest symbolic motives, at least during this period, particularly as college graduates' views of Republicans were steadily worsening (Figure 1).

However, if political values are a luxury good, then how are Democratic governors able to attract college graduates even during recessions? Table 2 shows that these transitions have particularly large effects on social policy, shifting it $.38\sigma$ to the left, substantially larger than any other effects we see. This magnitude is roughly equal to the 2020 difference between Pennsylvania and Kansas. Why, then, might Democratic governors be unable to attract college graduates outside of this period despite non-trivial leftward shifts in policy and a significant improvement in gubernatorial approval? One possibility is the continuous worsening of attitudes towards the Democratic Party among college graduates (including Democrats but especially the growing number of independents). As perceptions of the Democratic Party have deteriorated, it is possible that a state that replaces its Republican Governor with a Democrat might not, on its own, be sufficient to attract college graduates and that the types of massive social policy swings seen during recession years are necessary to pull them in.

In summary, then, our results provide some support for both policy and symbolic motives. We think the large migration responses to modest policy shifts in D-to-R transitions outside

[23] Unfortunately, the CES only began in 2006, so it is not available for our full sample of years. Thus, we restrict those analyses to flips occurring in 2008 or later, which provides us with at least two pre-treatment years.

of recessions are likely due to symbolic motives and that these same motives dampen support for R-to-D transitions outside of recessions, even though those transitions led to meaningful policy shifts. However, we think the increased in-migration due to R-to-D transitions during recessions is best explained by the very large leftward social policy moves that these transitions brought.

Finally, we consider two potential explanations for why governors might have a causal effect on in-migration but which are *not* consistent with our overall argument. First, it is possible that governors affect economic conditions and that college graduates respond to those conditions without intrinsically caring about politics or the partisanship of the governor. The final column of Table 2 presents effects on unemployment among college graduates calculated within the ACS. We find no effects on unemployment, and our confidence intervals rule out fairly small effects. In the DiD plots in the appendix, it is clear that this is not because these unemployment rates do not fluctuate over time; it is because large shifts among treated states are almost perfectly mirrored by the same shifts among control states.[24]

Second, it is possible that college graduates react to gubernatorial elections because of what those elections suggest about *voters* in the state. This is similar to our core argument but, importantly, different because it suggests that college graduates would respond to new information about voters regardless of what Republican governors actually do while in office. To test for this, Figure 8 tests whether we find migration effects among states that flipped from voting for Barack Obama in 2012 to Donald Trump in 2016 and compares these estimates with states that switched from Democratic to Republican governors during roughly the same period (flips from 2014-2017). While we find large and pronounced declines in in-migration after switching to Republican governors, we find no visual or statistical evidence that migration declined after states switched their presidential choice, even though such switches were very salient. This is consistent with our argument that college graduates care about the identity of the politician in charge of the state (since Donald Trump was in charge of *all* states), but it is difficult to explain as a result of them drawing inferences about the state's voters.

[Figure 8 about here.]

[24]Looking at the DiD plots in Figures B9-B12, to the extent that there are effects on unemployment, they are the opposite of what would threaten our results. If anything, a D-to-R transition during recessions reduces graduates' unemployment, and an R-to-D transition outside of recessions increases it, though neither effect is statistically significant. If such effects were real and were salient, then this could be a straightforward explanation of why D-to-R transitions do not reduce in-migration during recessions and R-to-D transitions do not increase it outside of them. However, we hesitate to assign such a strong interpretation to non-significant results.

4 Structural analysis

4.1 Model setup

In this section, we briefly describe our model of migration, which is based on Bryan and Morten (2019).[25] The model is developed in detail in Appendix C, but here we discuss the key elements, how features of the model interact to explain migration and its consequences, and the intuition for how we estimate and identify the parameters.

There are two types of agents: College-educated workers and non-college workers. These two types will have different wage rates, productivity, appreciation of amenities, migration costs, and political preferences. However, within education-type, all workers are *ex ante* identical, although they will realize different productivity draws and migration choices.

4.1.1 Worker decisions

Workers are infinitely-lived agents who, during each period, statically maximize their utility each period. Each period (one year) consists of two sub-periods. First, the worker draws a stochastic, idiosyncratic shock which determines whether she will migrate to a new state. This shock reflects idiosyncratic events like job dissatisfaction that leads a worker to look for new employment opportunities; a spouse's beginning or finishing graduate school; children facing key transitions into, out of, or between schools; etc. We allow the probability of realizing this "migration shock" to differ across states of residence to match the empirical fact that different states see very different out-migration rates. This shock is helpful because it allows our model to generate asymmetric responses in which in-migration responds, but out-migration does not, which standard models do not generate (Monras, 2020), but which is a salient feature of our empirical results above.

Second, workers who receive the "migration shock" (and will move to a new state) draw a vector of idiosyncratic human capital opportunities available in these new states, excluding the one where they currently live. We see this as reflecting potential employment opportunities across those states. Like much of the migration literature, we gain substantial tractability by assuming that these idiosyncratic draws are independent across locations and follow a Fréchet distribution (discussed below).

A human capital draw reflects the idiosyncratic productivity of a worker in a particular state. Each draw is valued at the local wage rate in that state, which will, of course, depend

[25] There are other models of spatial sorting (e.g., Diamond (2016)). An advantage of the Bryan-Morton model is that identification and estimation are based on observed migration flows, whereas some other models infer migration behavior from relative changes in city size over long periods. Given that our reduced form estimates focus specifically on migration flows, this model provides a more natural framework to incorporate those results.

on productivity and relative skills supplies (discussed below). In addition to wages, a worker considering moving from state o (origin) to d (destination) considers three additional factors. First, she considers one factor unique to the o-d pair. This reflects migration costs (as is standard in the literature), but also idiosyncratic features of d making it attractive to those moving from o (such as cultural similarities). Thus, our model allows residents from liberal states, for instance, to prefer moving to other liberal states for general cultural reasons – rather than solely due to the partisanship of the governor – in order to avoid misattributing broader cultural similarities to partisan politics.

Second, she considers the general amenities in the state, which could reflect how attractive the state is in terms of weather, local restaurants, crime rates, school quality, or natural attractions. Amenities like these are standard in the literature and have been shown to be important determinants of migration decisions (Diamond and Gaubert, 2022). We allow college and non-college workers to have different valuations of the amenities in the state but hold these amenities fixed across all workers within each education-type, regardless of their origin state.

Finally, she considers a political amenity, which depends on the partisanship of the governor. This parameter is our key conceptual departure from the previous literature.

With these ingredients, the indirect utility of worker i belonging to education group $g \in \{C, N\}$ moving from state o to d can be expressed as:

$$V^g_{ido,t} = \alpha^g_d \eta^g_{do}(1 - \gamma^g_{p(d,t)}) w^g_{d,t} s^g_{id,t} \tag{6}$$

where α^g_d is the general amenity, η^g_{do} is the differential utility living in d which is idiosyncratic to origin o-migrants, $\gamma^g_{p(d,t)}$ is the disutility of living under a governor belonging to party p, and $w^g_{d,t}$ is the endogenously determined wage rate that one unit of human capital commands in location d (i.e., the human capital price). Note that all of these parameters and variables are g-specific and, therefore, allowed to differ between college and non-college agents.

The only component in the indirect utility expression that is idiosyncratic to agent i is her human capital draw in state d: $s^g_{id,t}$. We assume that these human capital draws are independent realizations from a multivariate Fréchet distribution with parameter θ^g (which is the inverse variance of the draws across states). As pointed out by Hsieh et al. (2019), this distributional assumption is key because it implies that flows from o to d in t are proportional to non-stochastic parameters in the indirect utility expression:

$$\pi^g_{do,t} \equiv \frac{L^g_{do,t}}{\sum_{j \neq o} L^g_{jo,t}} = \frac{\left(\alpha^g_d \eta^g_{do}(1 - \gamma^g_{p(d,t)}) w^g_{d,t}\right)^{\theta^g}}{\sum_{j \neq o} \left(\alpha^g_j \eta^g_{dj}(1 - \gamma^g_{p(j,t)}) w_{j,t}\right)^{\theta^g}} \tag{7}$$

where $L^g_{do,t}$ is the number of workers moving from o to d in t. This expression implies that all key parameters are log-linear in the observable migration flows, which is key for identification. Specifically, the log of the observable migration flows from o to d can be rewritten as:

$$\ln \pi^g_{do,t} = \psi^g_{o,t} + \theta^g \ln \alpha^g_d + \theta^g \ln \eta^g_{do} + \theta^g \ln(1 - \gamma_{p(d,t)^g}) + \theta^g \ln w^g_{d,t} \tag{8}$$

where $\psi^g_{o,t} = -\ln\left(\sum_{j\neq o}\left(\alpha^g_j \eta^g_{dj}(1-\gamma^g_{p(j,t)})w^g_{j,t}\right)^{\theta^g}\right)$ is the structural interpretation of an origin-by-time fixed.

4.1.2 Production

Production is standard. Perfectly competitive firms produce a single final costlessly traded good, which is chosen as the numeraire ($p = 1$). They combine the effective labor (i.e., total human capital) of the two education groups using a Constant Elasticity of Substitution (CES) production function, so total output in state d is given by:

$$Y_d = A_d\left[\left(H^C_d\right)^{\frac{\sigma-1}{\sigma}} + \left(H^N_d\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \tag{9}$$

where A_d is the exogenous location-specific total factor productivity (TFP), $\sigma \geq 1$ is the elasticity of substitution between college and non-college workers, and $H^g_{jd} \equiv \sum_{i\in P_j} s^g_{id}$ is the total efficiency units of labor employed by firm j (i.e., the sum of the human capital draws of its workers, including new migrants into d and stayers in d from the previous period). P_j is the set of workers employed at firm j.

The profits for a representative firm in location d are given by:

$$\Pi_{jd} = Y_{jd} - w^C_{jd}H^C_{jd} - w^N_{jd}H^N_{jd}$$

where w_{jd} is the wage paid per effective unit of labor by firm j. In this economy, firms compete for each type of worker by setting wages. Since the labor market is perfectly competitive, in equilibrium, $\Pi_{jd} = 0$ and $w^g_{jd} = w^g_d \ \forall j$. This prevailing wage (or the price of human capital) is the same w^g_d discussed in the worker's problem above.

4.1.3 Equilibrium and initial conditions

A competitive equilibrium in this economy consists of destination choices, total efficiency of labor in each destination H^g_d, and a wage w^g_d such that:

- Workers choose the workplace that maximizes their utility
- Firms choose efficient labor H^g_d to maximize profit

- w_d^g clears the labor market for each destination

We assume all workers are born in the initial period $t = 0$ and receive a state-specific human capital skill draw $s_{id,t=0}^g$. They migrate to the state that maximizes their utility based on equation (6). Starting from the next period $t \geq 1$, workers who get a migration shock will obtain a new vector of $s_{id,t}^g$ and decide where they will move.

4.2 Identification and estimation

A key advantage of our model is that the proof of identification is constructive and illustrates exactly which moments of the data identify which parameters. Here, we briefly summarize our approach and its intuition, while Appendix C presents it in detail. Our calibrations are based on data from 2011-2019, while our counterfactual simulations are based on the most recent available data on population counts (from 2022) and governor partisanship (from 2024).

First, we take the elasticity of substitution between college and non-college workers from the literature and use $\sigma = 2.6$ from Jerzmanowski and Tamura (2020). This estimate is greater than one (as is well-accepted, Acemoglu and Autor (2011)), which implies that a decrease in the supply of college graduates will both raise the *relative* wage of college graduates in the state, as well as reduce the real wage of non-college workers.

Next, we calibrate the variance of the Fréchet distribution that governs human capital draws. To do so, we draw on the insight from Hsieh et al. (2019) that this variance can be calibrated based on the observable within-state variance of (residual) wages. This is intuitive: In our model, all workers of the same type in the same state face the same wage rate, so differences in pay reflect differences in their human capital draws.

Third, in our model, exogenous shocks lead workers to decide *whether* to move. We can pin down the probability of the "migration shock" by observing the annual share of people who move out of each state.

Fourth, we can recover the benefits and costs of migration that are specific to a pair of states by observing residual log migration flows between these states after accounting for origin-state fixed effects (which includes the probability of realizing the migration shock), destination-state fixed effects (which include wage rates and amenities), and governor partisanship.

Fifth, we can jointly identify productivity and wages using two conditions. First, our assumptions about the product market imply that the price of a state's output (which is constant across states) is equal to the marginal costs of production, and our assumptions about the production function tell us what these costs are as a function of wages, TFP, and the elasticity of substitution. Second, because we assume that *i*) the only idiosyncratic factor that varies across individuals is the Frechet-distributed human capital draw and *ii*) migrating individuals choose their state to maximize their utility, our model tells us the degree of selection on human capital draws (and thereby aggregate human capital) as a function of observable migration flows. These two conditions imply that we can back out TFP and wages from observed output (GDP) and observed migration since the workers' endogenous selection allows us to infer aggregate human capital stocks from that migration.

Sixth, given estimates of origin-destination-pair-specific migration incentives, TFP, and wage rates, we can infer general amenities from residual migration into the state from *all* destinations (since we assume that the valuation of these general amenities does not depend on the state-of-origin).

Finally, given all other parameters, we calibrate the value of the political wedge to match the empirical migration responses we estimated in Section 3 above. Since we always use the most recent information available for our simulations, we also choose to calibrate the political wedge to match the most recent of the estimates we provide. Specifically, we match the in-migration response to the D-to-R flips from 2014-2017, which are presented in Figure 8. We simulate gubernatorial switches for the same set of treated states used in those estimates and choose the γ values so that the model-implied average effects match the empirical estimates we recovered there.

Estimated parameter values are summarized in Appendix Table B4. Our key parameter is γ^C_{Rep}: college workers' disutility from living under a Republican governor. Since political preferences and general amenities both enter the indirect utility function together, we can directly compare the magnitude of our estimated political preferences with the magnitude of the general, common-value amenities emphasized in the migration literature (Albouy, 2016; Albouy and Stuart, 2020; Diamond and Gaubert, 2022; Jia et al., 2022). Our estimate of college graduates' disutility of Republican governance is only one-fifth the cross-state standard deviation of non-political amenities. To be indifferent to Republican governance, a college graduate would require a 5.3% wage increase, which is about one-fifth as large as the wage effect of a one standard deviation increase in TFP and one-third as large as the effect of a one standard deviation increase in the human capital price. Put differently, our estimated disutility of Republican governance is small compared to other determinants of migration. However, as we will show, even this modest disutility of Republican governance can generate meaningful effects on the college population and state-level output.

4.3 Counterfactuals

In order to understand the consequences of governor partisanship predicted by our model, we begin with a set of 50 counterfactuals, each of which flips only a single state. For each flip, we calculate the equilibrium number of college graduates four years later,[26] and Figure 9 maps the percent change in the number of college graduates. Blue states have Democratic governors in 2024, and so we are simulating the effects of a Republican governor who reduces the number of college graduates. Red states currently have a Republican governor, so we are simulating the effects of a Democratic governor. In both cases, darker colors reflect larger predicted effects.

[Figure 9 about here.]

An interesting result from our analyses is that our model implies significant heterogeneity in the predicted effects of a gubernatorial transition. This is despite the fact that we impose substantial homogeneity by using one disutility parameter that applies to *all* college graduates and *all* Republican governors. The decrease implied by a Republican governor varies from .4% (over four years) to 4%, while the implied increase from a Democrat ranges from .7% to 6.6%. This heterogeneity comes from an intuitive source: differences in the baseline fraction of college graduates who come from other states. States that rely on in-migration to generate their college-educated workforce are obviously more susceptible to the effects of changing migration incentives. This generates an interesting pattern in Figure 9, in which the states with the smallest predicted effects are those with very strong public university systems,[27] even though our estimation approach did not use this information. This highlights an important policy implication of our results. Republican governors interested in blunting these effects might invest in universities to produce "home-grown" college-educated workers, although, in practice, Republican governors and legislatures tend to reduce university funding.

Because different states show different implied effects, it is important that our counterfactuals reflect realistic scenarios. For example, Wyoming shows among the largest effects of a Democratic governor, but the last Democrat running for governor there received only 17% of the vote, so these large effects are irrelevant. We choose our counterfactuals by recognizing that voters have become less likely to vote for different parties for different offices and increasingly make state-level political choices based on national-level political debates, which Hopkins

[26] We see these four-year effects as short-run effects and make no attempt to model changes in industry structure or firm location, endogenous changes in common-value amenities, or changes in future political conditions or attitudes, even though all of these may be affected by a change in the educational composition of the population. We see all of these as plausible long-run outcomes, but where further empirical work would be needed to discipline a quantitative model.

[27] Republican-governed states like Texas and Ohio and Democrat-governed states like California, Minnesota, Wisconsin, Michigan, and Pennsylvania.

(2018) calls the “nationalization” of state politics. Thus, we consider counterfactuals in which the most recent Presidential election implies that a different state-level gubernatorial outcome is plausible.

Figure 10 plots the Democratic Party vote share from the most recent Presidential election (2020), along with the most recent gubernatorial election. The two are strongly correlated (.74), and dropping Vermont makes the correlation even stronger (.87). However, the arbitrariness inherent in winner-take-all elections means that outcomes differ, and since many elections were close, modest shifts in public sentiment could easily sway the outcomes in many of these states.

[Figure 10 about here.]

We consider four counterfactuals, all shown in Figure 10. First, we consider a “weak red wave,” in which a modest tide of conservative national sentiment could flip the three states won by Donald Trump but which currently have Democratic governors (top left quadrant). Second, we consider a “strong red wave” which also flips an additional four states that Biden won narrowly and which have Democratic governors (top right). Similarly, we consider a “weak blue wave” that flips the five states won by Biden but represented by a Republican governor (bottom right) and a “strong blue wave” that also flips four battleground states narrowly lost by Biden and represented by a Republican governor (bottom left).

4.4 General equilibrium effects

Our first question is how these plausible “waves” of Republican and Democratic gubernatorial elections might affect equilibrium economic activity and inequality. Panel A of Table 3 summarizes our model’s prediction for how the college-educated workforce would change in the states experiencing a gubernatorial flip. For all four counterfactuals, the average state seeing a flip is predicted to see a 1.3 to 2.1% change in its college population. These magnitudes are similar to our reduced form estimates from above because we used those estimates in our calibration. Of course, as shown in Figure 9, there is substantial heterogeneity around this average effect.

[Table 3 about here.]

In Panel B, we then ask what these effects imply for GDP per worker.[28] Across estimates, the elasticity of GDP per worker with respect to the size of the college-educated workforce is about .9, meaning that the 1.8% average decline in college workforce we simulate in our “small red wave” counterfactual implies a 1.6% average decline in GDP per worker. These effects are

[28]Our model does not include non-workers, so GDP per worker is equivalent to GDP per capita.

modest but meaningful. The median state saw average annual GDP growth of 1.25% during the 2005-2019 period, so our estimated effects roughly correspond to one year of lost growth.

We next turn to inequality, both because it is substantively important in its own right but also because of its role in political dynamics, where rising inequality often fuels support for controversial populist candidates (Guriev and Papaioannou, 2022). Intuitively, the inequality implications of our results are straightforward. If a Republican governor makes college graduates more scarce, then decreasing the relative supply of college graduates should increase their relative price, and college/non-college inequality should rise (with the converse effects for a Democratic governor).

In Panel C, we present these effects by summarizing the change in $\ln w_d^C - \ln w_d^N$, which is the gap in the log "wage" (i.e., earnings per unit of human capital) of college and non-college workers. Consistent with the basic logic of supply and demand above, the counterfactuals that increase the relative supply of college graduates reduce the relative price of their labor, and vice versa for the counterfactuals that decrease their relative supply.[29]

However, this simple intuition could be misleading because changes in the migration incentives also change the composition of college graduates. Workers are not randomly sorted across states, but choose their state after realizing idiosyncratic productivity draws for all states. This selection means that migrants will tend to have higher human capital draws than non-migrants because their idiosyncratic realization must have been sufficient to draw them to this state over the other states. Thus, while increasing the relative supply of college graduates reduces their wage *per unit of human capital* (the price effect shown in Panel C), it also increases the average human capital of the college graduates who do live there (a composition effect from increasing the migrant share of the college population). Thus, the difference in *earnings* between college and non-college workers (which include wages per unit of human capital and differences in the level of human capital) might go up or down.

In Panel D, we find that across the counterfactuals we consider, the composition effect dominates the price effect, and switching to a Democratic governor simultaneously increases the number of college graduates *and* their relative earnings, while switching to a Republican reduces both.[30] These magnitudes are modest on average, ranging from .4 to .6 log points, but can be large. In the largest case, our "small blue wave" counterfactual implies that a Democratic

[29]Because we assume that the elasticity of substitution in production in equation (13) is greater than one, decreasing the supply of college graduates also reduces the real earnings of non-college workers (as in Moretti (2004) and Giannone (2022)). In Appendix Table B5, we summarize changes in log wage for college and non-college. Across the counterfactuals, about 40% of the increase in inequality arises from the change in non-college wages, with the other 60% arising from the change in college wages.

[30]This is not always true. Appendix Figure B13 shows the inequality implications for the 50 single-state counterfactuals we simulated above. In two of the counterfactuals that imply the largest increase in the number of college graduates, the price effect dominates the composition effect, and college/non-college inequality falls.

governor would increase the college/non-college earnings gap by 1.4 log points in Vermont. How large is this? Acemoglu and Autor (2011) estimate that from 1980-2008—widely seen as a period of dramatically increasing inequality and college premia—the college/non-college earnings gap increased by roughly 1 log point per year. Thus, an additional 1.4 log point increase over four years, on top of any secular trends in inequality, would likely be substantial enough to ignite some resentment among non-college voters, particularly when combined with a visible increase in the number of college graduates.

These results are interesting partly because including endogenous selection among migrants in our model overturns the standard intuition about the effects of a supply shock (the only response in our model).[31] But they are also interesting because many of the most liberal and progressive areas in the country (e.g., California, Seattle, New York) have seen rising shares of college graduates at the same time as rising inequality. Our model can reconcile this even without agglomeration effects or housing market distortions, with the mechanism instead emerging from inequality driven by college graduates moving into the state with particularly high-paying job offers.

4.5 Cross-state spillover effects

Finally, our model lets us study how changes in the governor in one state affect other states. All states are linked through the migration decision since workers are deciding between different states in choosing where to live. Thus, changes in the incentives to live in one state might push those workers towards another state or pull workers away from that state.

We map these spillovers in Figure 11. In panel (a), we present the direct effects on the college-educated workforce for the nine states we consider electing Democratic governors in a strong blue wave. These effects vary, but it is worth noting that we estimate reasonably strong 1-2% increases in the three Southeastern states that we consider plausible flips (Florida, Georgia, and Virginia) and that these are large states. In panel (b), we then plot the indirect spillover effects on the college-educated workforce in states that *do not* change their governor. Most states show small effects, but not always. For instance, in South Carolina (a relatively small state bordering Georgia and having high excess migration with Virginia and Florida), we estimate a 0.7% decline in the college graduate population. Thus, while spillovers are typically small, when several large states in the same area all flip at once, it can cause a substantial ripple effect through the region.

[31] Fortin (2006) presents reduced form empirical evidence for the "price effect" discussed above (wherein a larger supply of college graduates reduces college/non-college inequality). However, Fortin's identification came from state-level policy changes in university funding and access. Thus, the marginal college graduates driving identification in Fortin's analysis were primarily from the state itself and not cross-state migrants. This means that Fortin's results did not include the composition channel that we discuss.

[Figure 11 about here.]

In Figure 12, we perform the same exercise to calculate the spillovers induced by a strong red wave. Because these states are not concentrated in a single area, they are not amplified into a large regional effect. However, some states are highly exposed to one or two particularly large states that we simulate flipping. Thus, we predict that Arizona flipping will have large effects on its smaller neighbor Nevada, particularly because Nevada's college graduate population relies heavily on cross-state migrants.

[Figure 12 about here.]

In conclusion, our estimates suggest only a modest disutility of politics. It is not the dominant force driving migration decisions, paling in comparison to the distribution of non-wage amenities, migration costs, wages, or TFP. Nonetheless, it is substantial enough for political swings to have meaningful effects on economic outcomes, not only on the states experiencing a change but also on neighboring states. In our simulations, we have explored the effects of changing college-educated workers' disutility parameter, and have found that effects are close to linear in $\gamma^C_{d(Rep)}$. This implies that a continuation of recent patterns in education polarization can exacerbate the effects we estimate here, so political divides across educational lines are important to monitor in the future.

5 Conclusion

College education is increasingly the fulcrum of political disagreement in the United States. On both social and economic issues, college graduates are well to the left of non-college voters and to a degree much greater than just 15 years ago. As a result, Republican governance deters college graduates from moving to a state, driving down total human capital in the state and reducing economic growth. Of course, to the extent that Republican policies themselves are pro-growth, these migration responses offset some of those gains.

What might conservative politicians do to reduce this growth penalty? One option is to increase support for universities in order to increase the stock of "home-grown" college graduates (as in Fortin (2006) or Kennan (2015)) who would presumably show greater sympathies for the policies preferred by local voters. Instead, however, Republican governors and legislatures tend to cut funding and support for universities, exacerbating the local supply shortages induced by lower in-migration.

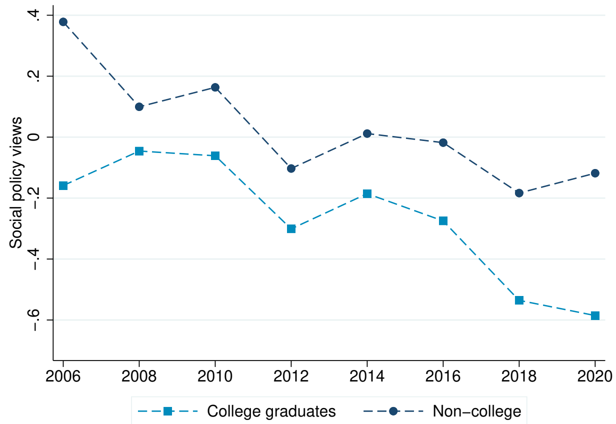
Alternatively, conservative politicians might aim to win back the college-educated electorate. Thus far, this does not appear to be a priority. Influential party leaders tend to use "college-educated," "liberal," and "elites" as almost interchangeable terms to refer to their enemies.[32] If anything, the regional inequality exacerbated by education polarization and migration appears to be a rhetorical victory for conservative politicians, who frequently emphasize these gaps to win votes. Moreover, much of the conservative appeal to college-educated voters in the past derived from pro-market, low-tax policy positions. With social and economic attitudes increasingly correlated, it is unclear whether libertarian economic policies can still appeal to college graduates.

Finally, an open question is how the COVID-19 pandemic and its aftermath will interact with education polarization. We intentionally end our analysis in 2019 because we see migration data from 2020-2022 as unreliable. However, a salient feature of the post-pandemic United States is that working remotely will be common, particularly for better-educated workers (Barrero, Bloom, and Davis, 2021). This is likely to exacerbate our findings since it allows college-educated workers to live in the states they prefer while holding jobs in conservative states where the firm is located, without the general equilibrium wage pressures to offset the political migration incentives.

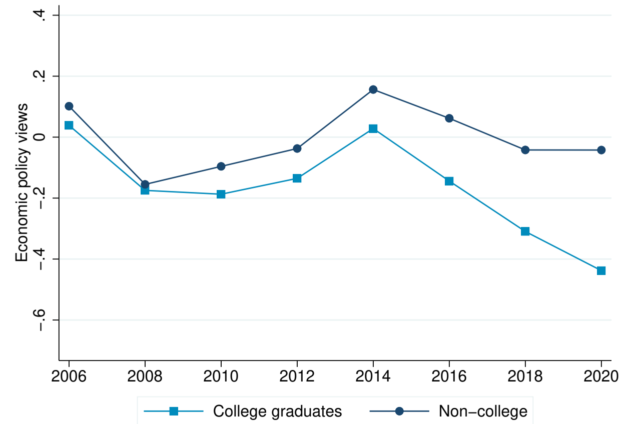
The implications for Republican-led states could be dramatic. Conservative states tend to raise more revenue through sales taxes and typically have low income and corporate tax rates, while liberal states rely more on income taxation for revenue. Thus, even if the firm locates in a conservative state, the state will receive little revenue from taxing the company and little revenue from sales taxes since many of the state's workers do not actually live and consume within the state. Liberal states reliant on income taxes, on the other hand, will essentially benefit from a revenue windfall as more high-earning workers relocate there than otherwise would, given the relatively high corporate tax rates disincentivizing firms from locating there. With this in mind, future research should account for education polarization (and the possibility that it continues to grow) when assessing the implications of post-pandemic work arrangements for heterogeneous growth in different regions of the country.

[32]For instance, Ohio Senator JD Vance has said, "If any of us wants to do the things that we want to do for our country, and for the people who live in it, we have to honestly and aggressively attack the universities in this country," which he describes as "very hostile" to conservative aims and dedicated to "deceit and lies, not the truth" (Knott, 2024).

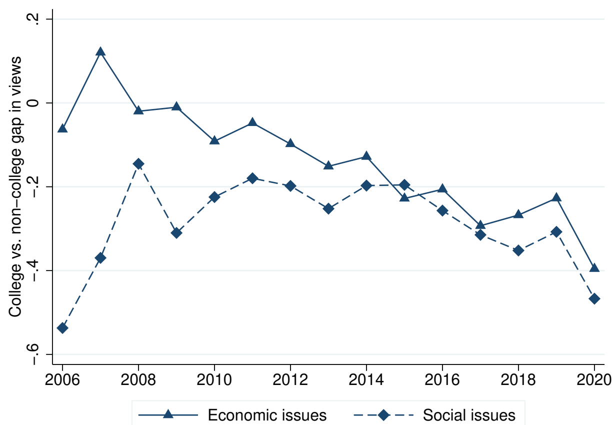
Figure 1: Differences in policy views by education



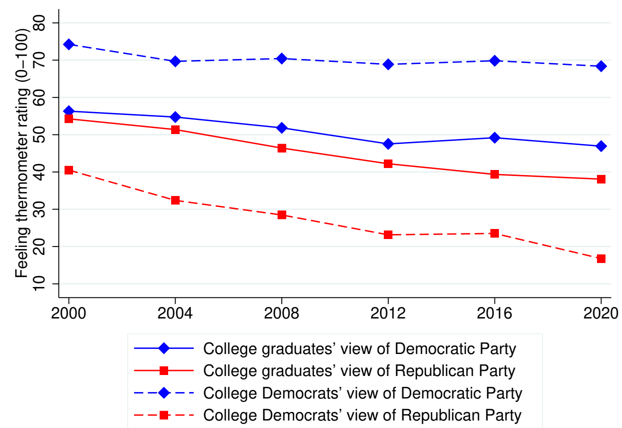
(a) Social policy views



(b) Economic policy views



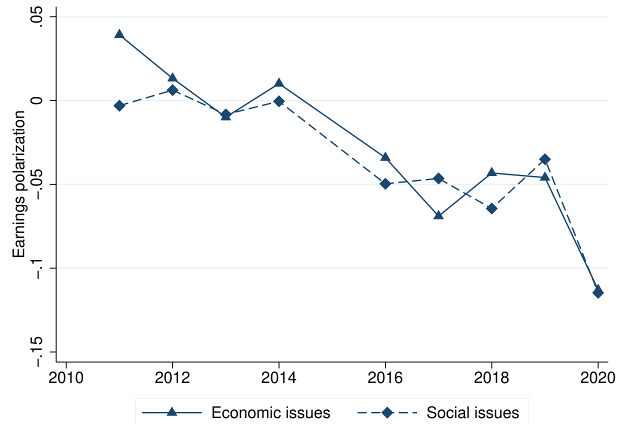
(c) College vs. Non-college gaps



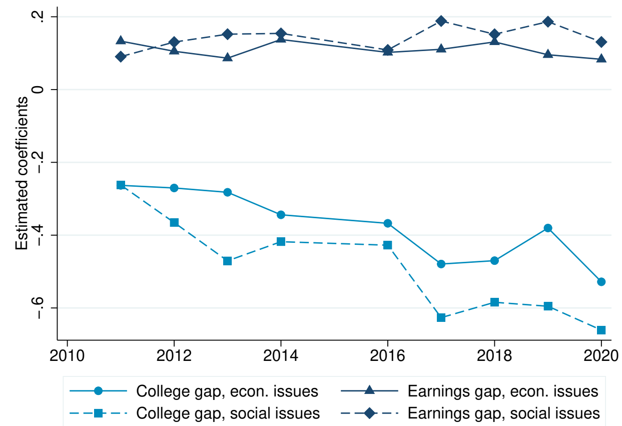
(d) College graduates' affective polarization

Notes: Calculations in panels (a)-(c) based on Cooperative Election Study (CES) respondents aged 26-45. Policy view indices are based on Item Response Theory estimates using the approach developed by Imai et al. (2016). Policy questions are divided into social and economic policy issues using the scheme developed by Caughey and Warshaw (2018). Indices are normalized to have mean zero and unit standard deviation across all respondents across all years, with more positive values indicating more conservative views. Calculations in panel (d) based on college graduates in the American National Election Study (ANES), where respondents report a "feeling thermometer" rating (0-100) on both parties. All calculations use sample weights.

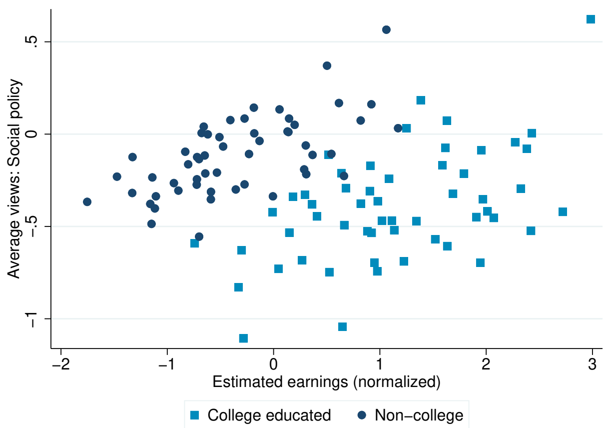
Figure 2: Differences in policy views by earnings



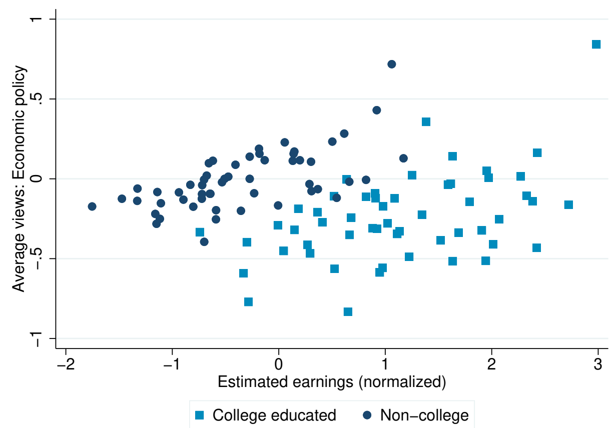
(a) Earnings polarization over time



(b) Coefficients on earnings and education



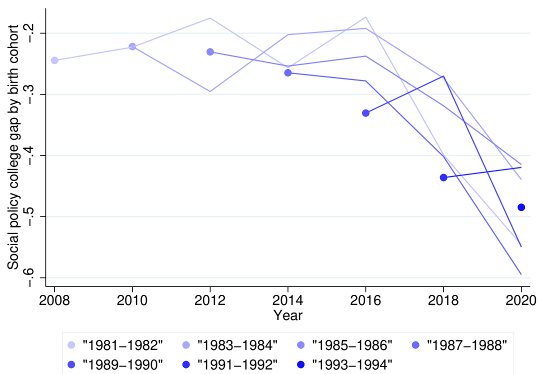
(c) Social policy views by age-industry-college



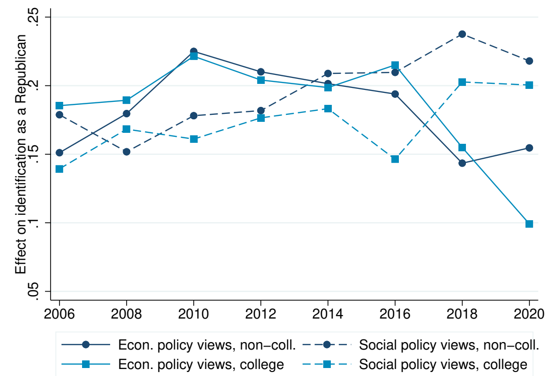
(d) Economic policy views by age-industry-college

Notes: All calculations based on Cooperative Election Study (CES) respondents aged 26-45 and employed in the private sector. Earnings are imputed from the American Community Survey (ACS) based on age, education, and industry, using the approach proposed in the text that accounts for geographic pay differences (see equation (1)). Policy view indices are based on Item Response Theory estimates using the approach developed by Imai et al. (2016). Policy questions are divided into social and economic policy issues using the scheme developed by Caughey and Warshaw (2018). Indices are normalized to have mean zero and unit standard deviation across all respondents across all years, with more positive values indicating more conservative views. All calculations use sample weights.

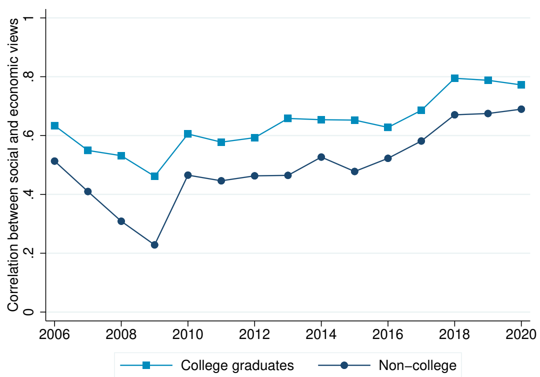
Figure 3: Assessing explanations for education polarization



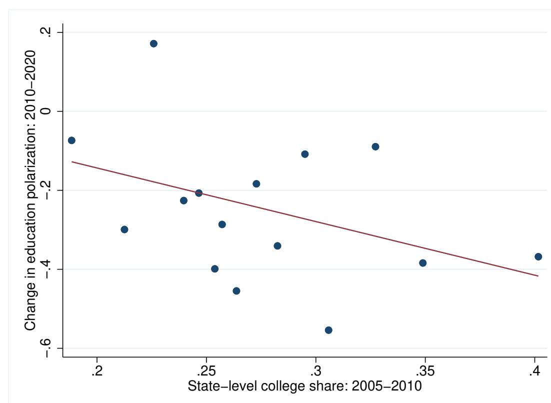
(a) Within- vs. between-cohort changes



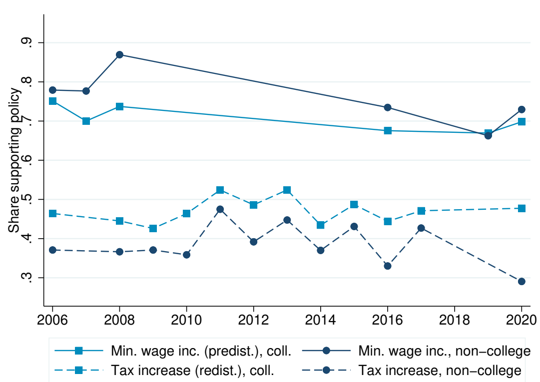
(b) Social vs. economic policy importance



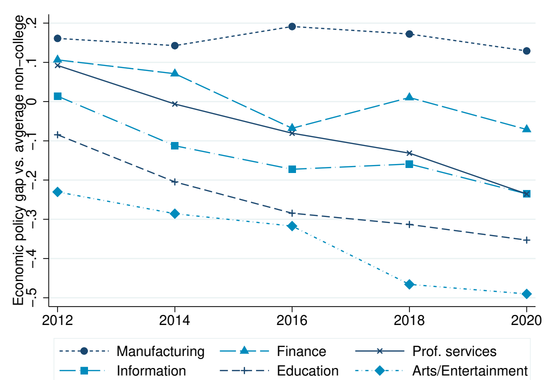
(c) Social and economic policy correlation



(d) Polarization and echo chambers



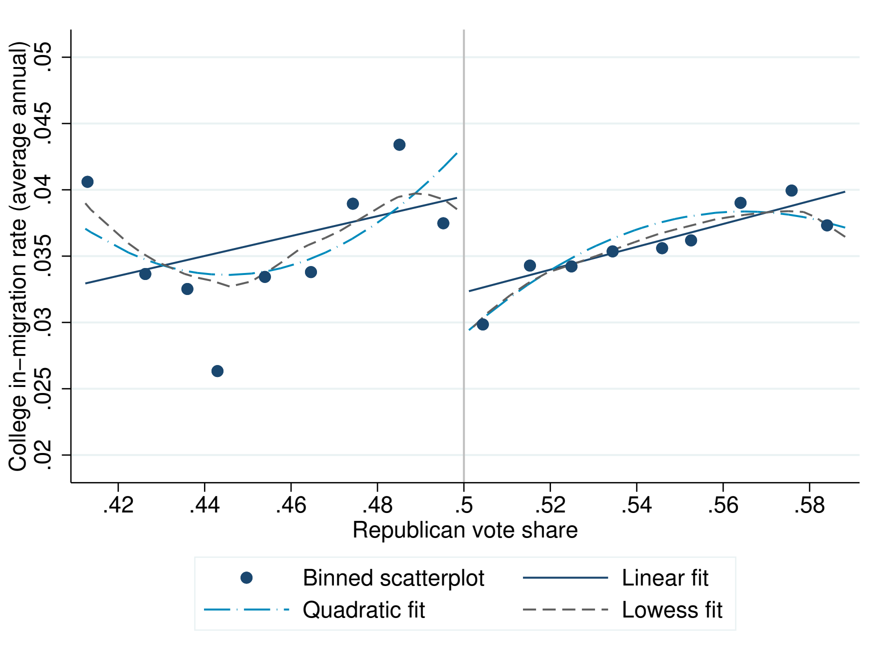
(e) Redistribution vs. Predistribution



(f) Trends by industry

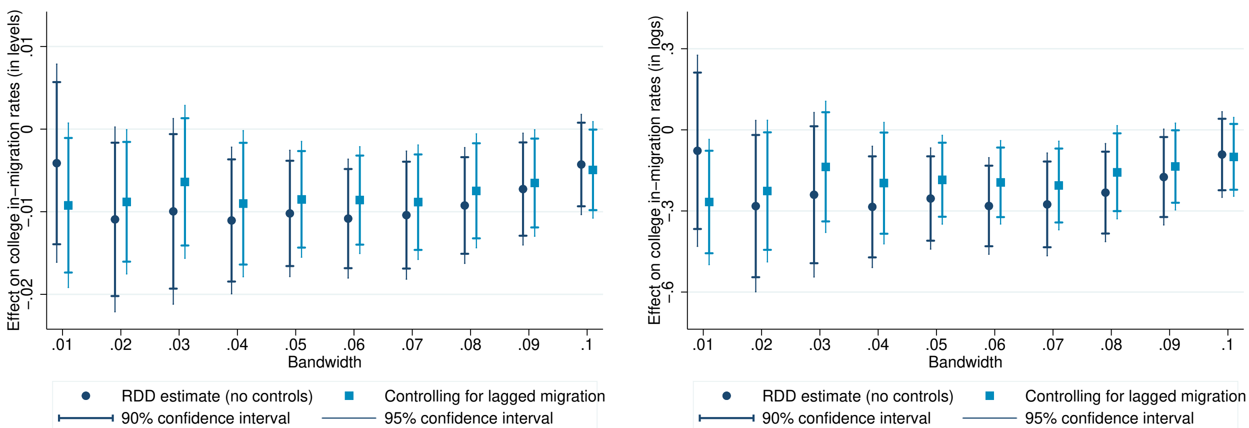
Notes: All calculations are based on Cooperative Election Study (CES) respondents aged 26-45. Policy view indices are based on Item Response Theory estimates using the approach developed by Imai et al. (2016). Policy questions are divided into social and economic policy issues using the scheme developed by Caughey and Warshaw (2018). Indices are normalized to have mean zero and unit standard deviation across all respondents across all years, with more positive values indicating more conservative views. Because of some small industry-level samples, panel (f) pools consecutive years together. All calculations use sample weights.

Figure 4: Regression discontinuity estimates of partisanship and college in-migration



Notes: Figure presents a binned scatterplot showing college graduates' average in-migration rates (the fraction of college graduates who lived in a different state the prior year) during the four post-election years as a function of the Republican vote share in the election.

Figure 5: Robustness of regression discontinuity estimates to controls and bandwidth choices

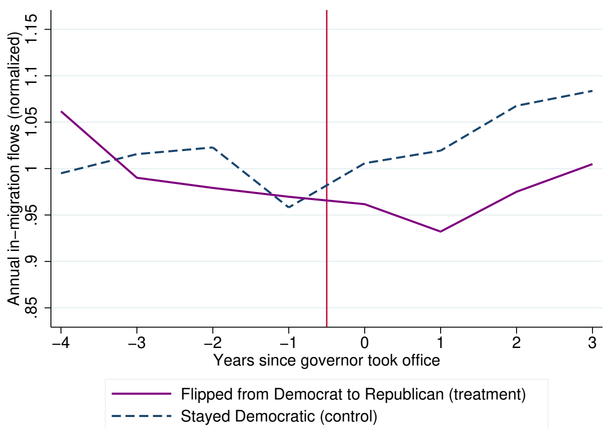


(a) DV: In-migration rate in levels

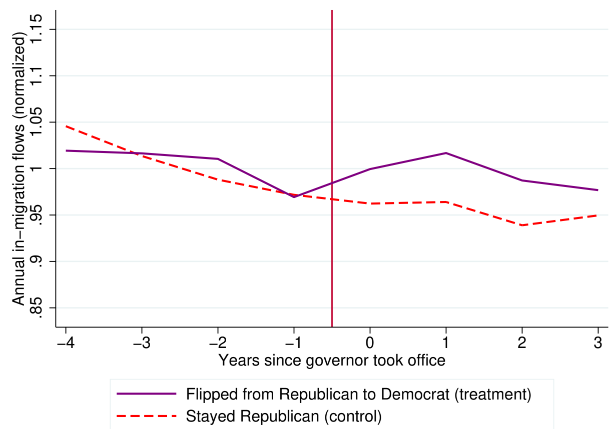
(b) DV: In-migration rate in logs

Notes: Figure displays point estimates (with 90% and 95% confidence intervals) for regression discontinuity estimates of the effect of a Republican win on college graduates' average in-migration rates (the fraction of college graduates who lived in a different state the prior year) during the four post-election years. The x-axis depicts integer bandwidths ranging from .01 (i.e., including only elections where the Republican got between 49% and 51% of the two-party vote share) to .10. In panel (a), the dependent variable is the in-migration rate; in panel (b) the dependent variable is the log in-migration rate. Dark blue circles do not include controls; light blue squares control for the in-migration rate, panel (a), or log in-migration rate, panel (b), during the election year (i.e., the last pre-election year). All specifications use linear polynomials and uniform kernels.

Figure 6: College graduates' in-migration responses to gubernatorial transitions



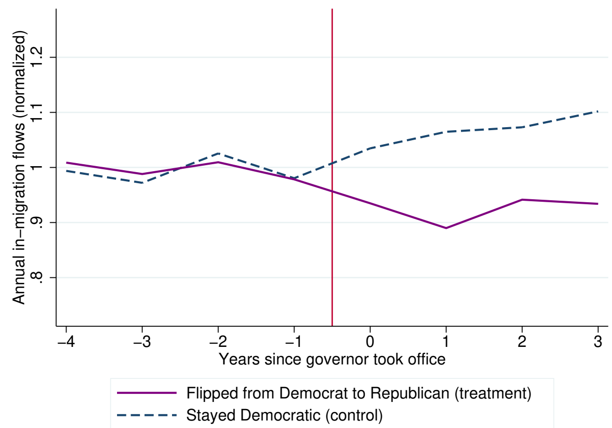
(a) Democrat-to-Republican (all years)



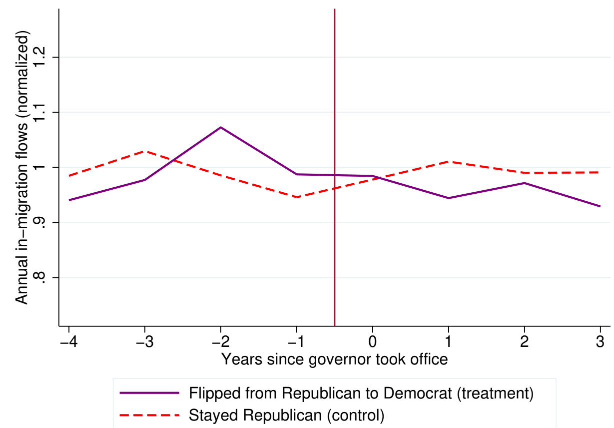
(b) Republican-to-Democrat (all years)

Notes: Figure presents the raw in-migration levels that underlie our stacked DiD estimates (see Section 3.2 and equation (3) for more). This compares "treated" states (switching governor partisanship) to "control" states that had the same pre-treatment governor partisanship but maintained it consistently during the ten years surrounding the treatment event. All state-years are weighted by the number of ACS respondents, in addition to the Callaway-Sant'Anna re-weighting to ensure that years are proportional to the number of treated states. Migration from 2020 onwards is never used. Dependent variable is college graduates' in-migration rates, normalized to be mean-one during the four pre-treatment years.

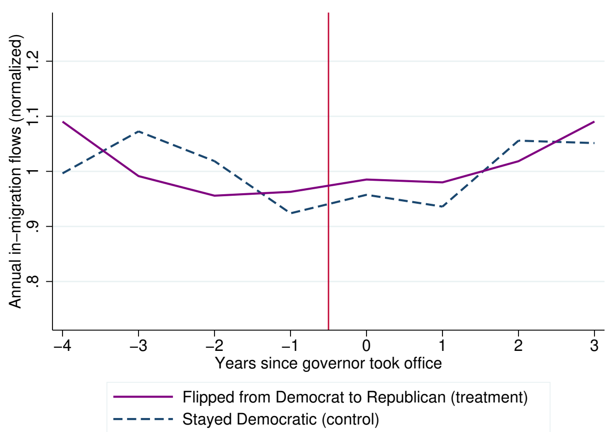
Figure 7: College in-migration responses to gubernatorial transitions by type and timing



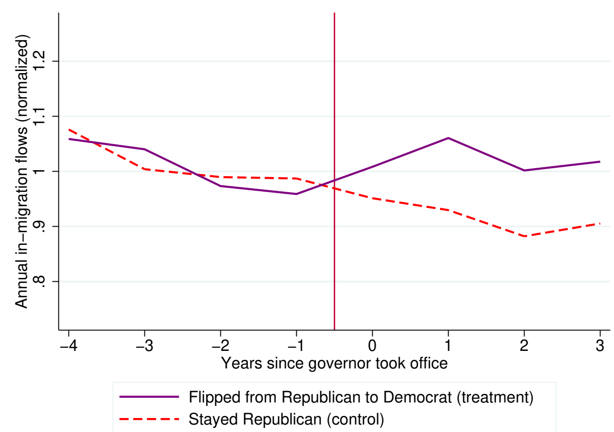
(a) D-to-R, non-recession years



(b) R-to-D, non-recession years



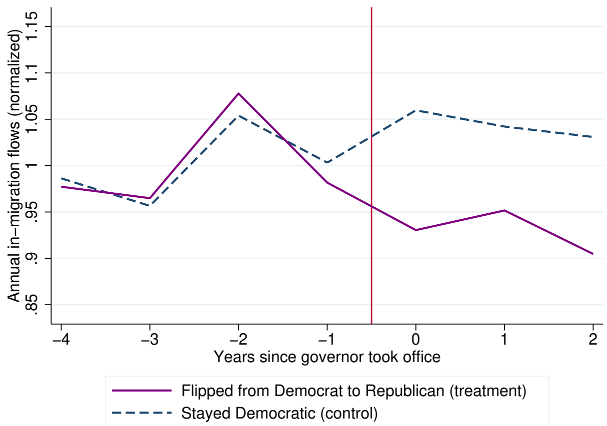
(c) D-to-R, recession years



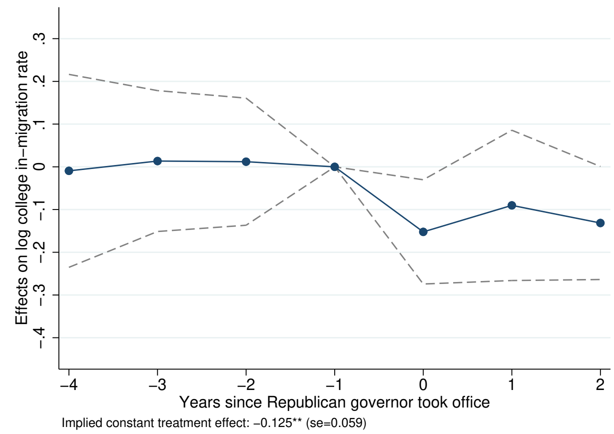
(d) R-to-D, recession years

Notes: Figure presents the raw in-migration levels that underlie our stacked DiD estimates (see Section 3.2 and equation (3) for more). "Non-recession years" refers to gubernatorial transitions taking place 2003-2006 or 2014-2017, "recession years" refers to gubernatorial transitions taking place 2007-2013. Migration from 2020 onwards is never used. Dependent variable is college graduates' in-migration rates, normalized to be mean-one during the four pre-treatment years.

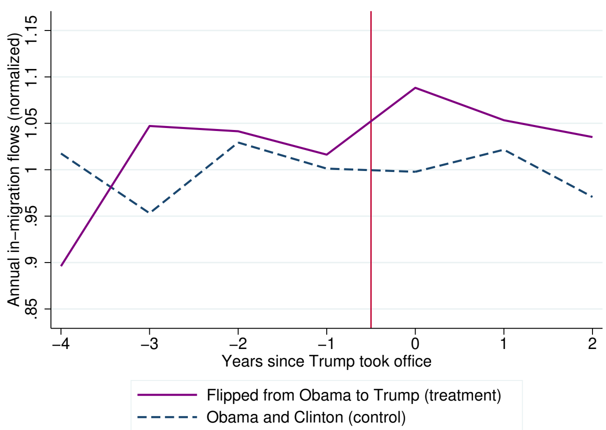
Figure 8: Migration responses to Gubernatorial vs. Presidential flips



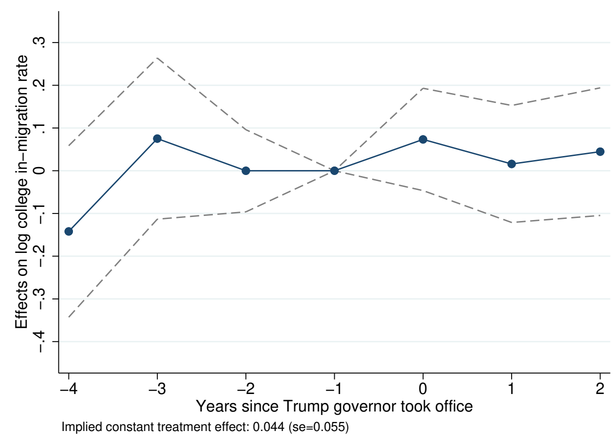
(a) D-to-R Governor flips 2014-2017 (DiD)



(b) D-to-R Governor flips 2014-2017 (coefficients)



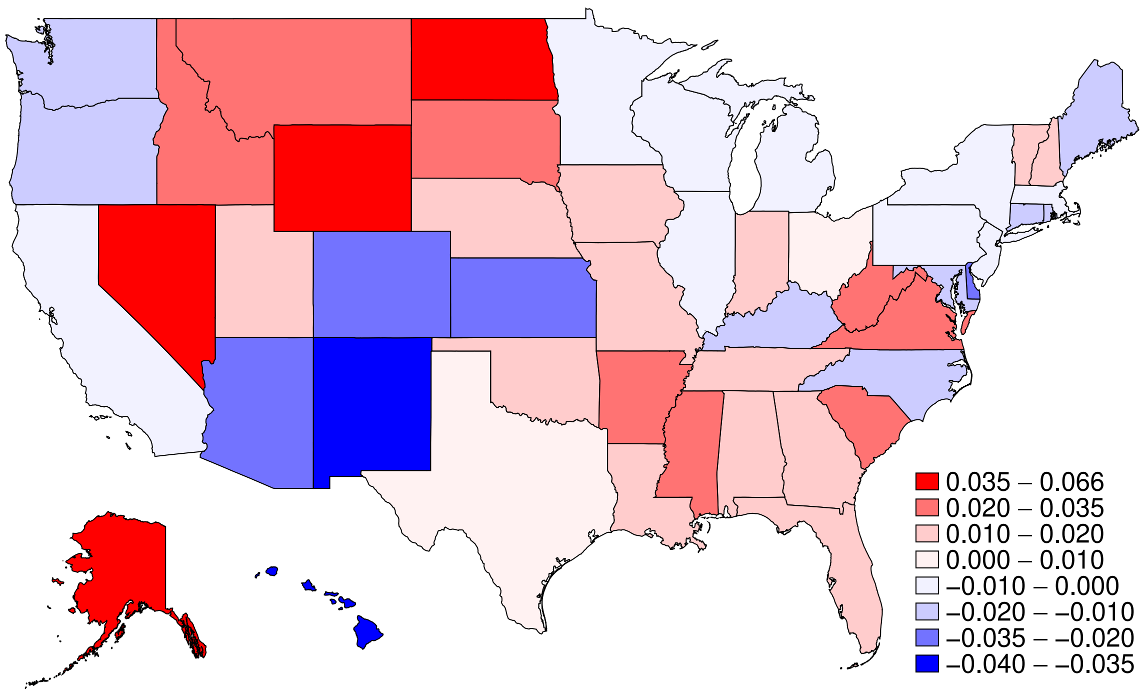
(c) Obama-Trump voting flips (DiD)



(d) Obama-Trump voting flips (coefficients)

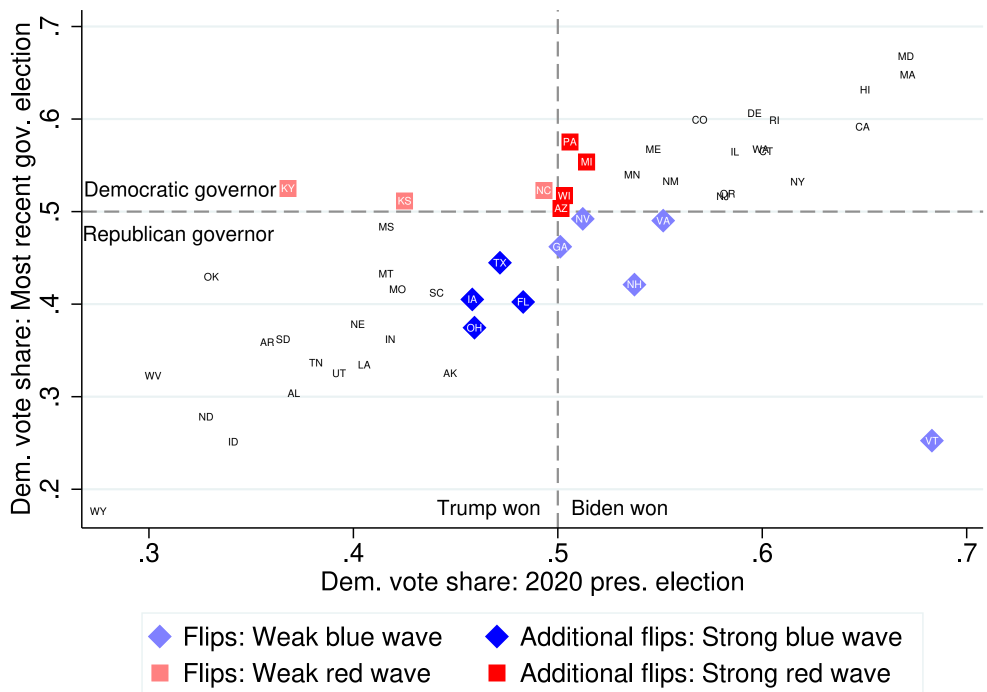
Notes: Panels (a) and (c) presents raw in-migration levels (normalized to be mean-one before treatment), and panels (b) and (d) present coefficient estimates from our stacked DiD estimates (see Section 3.2 and equation (3) for more). In panels (a) and (b), treatment is switching from a Democratic to a Republican governor, and we restrict to flips that happened 2014-2017. In panels (c) and (d), treatment is switching from voting for Barack Obama in 2012 to Donald Trump in 2016, and 2017 is the first post-treatment year for all events. Migration from 2020 onwards is never used. Dependent variable is college graduates' in-migration rates.

Figure 9: Heterogeneous effects of single states switching governors



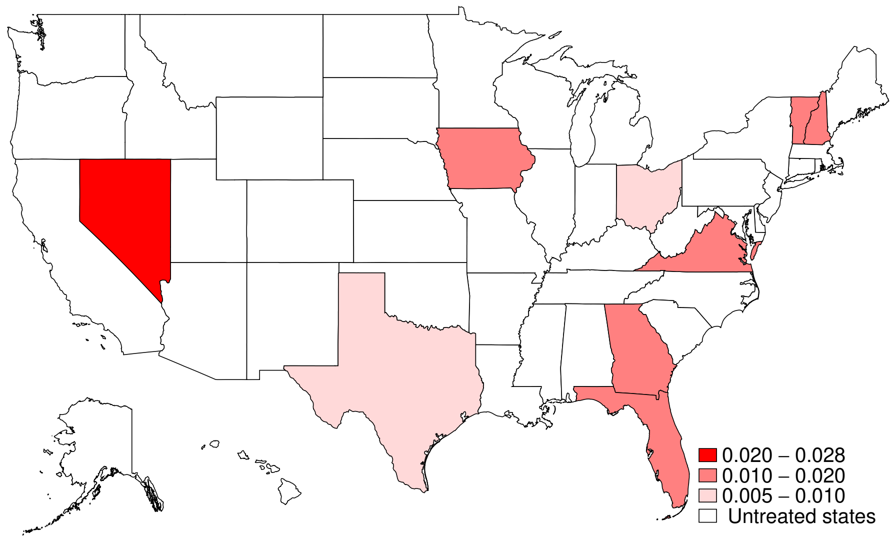
Notes: Map based on 50 separate counterfactuals, each of which flips only one state independently, relative to the set of governors in office in 2024. Red states have Republican governors in 2024, so we simulate the effects of a Democrat taking office. Blue states have Democratic governors in 2024, so we simulate the effects of a Republican taking office. Darker colors indicate a larger percentage change in the college graduate workforce predicted by our model.

Figure 10: Counterfactual switching states relative to 2024 gubernatorial partisanship

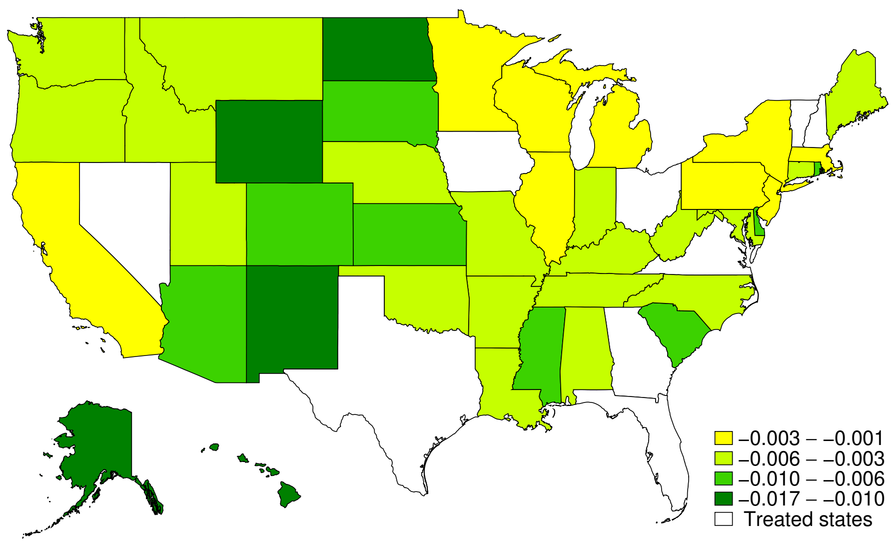


Notes: Figure plots the relationship between the 2020 Presidential vote share and the most recent (as of January 2024) gubernatorial election vote share. All vote shares are based on two-party vote share. Figure identifies the 16 states we consider plausible flips in our four counterfactuals.

Figure 11: Direct and spillover effects of big blue wave



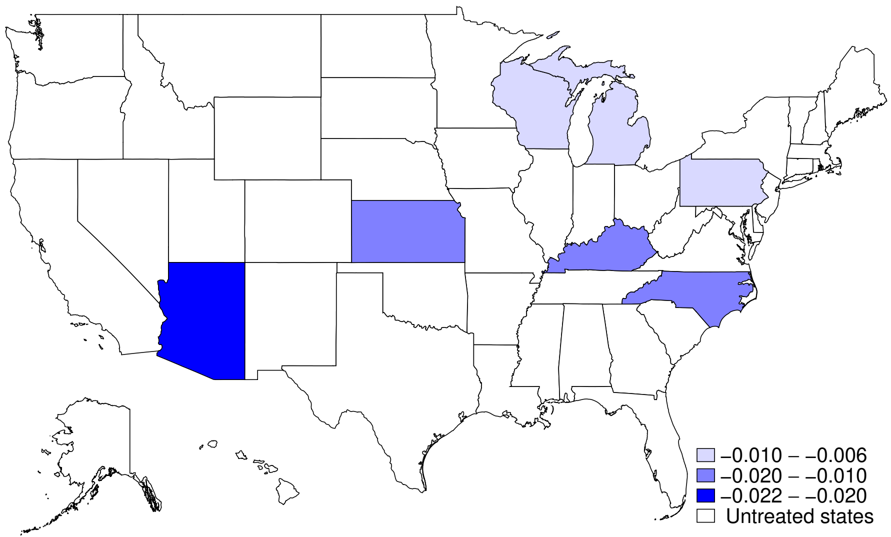
(a) Direct effects: Treated states



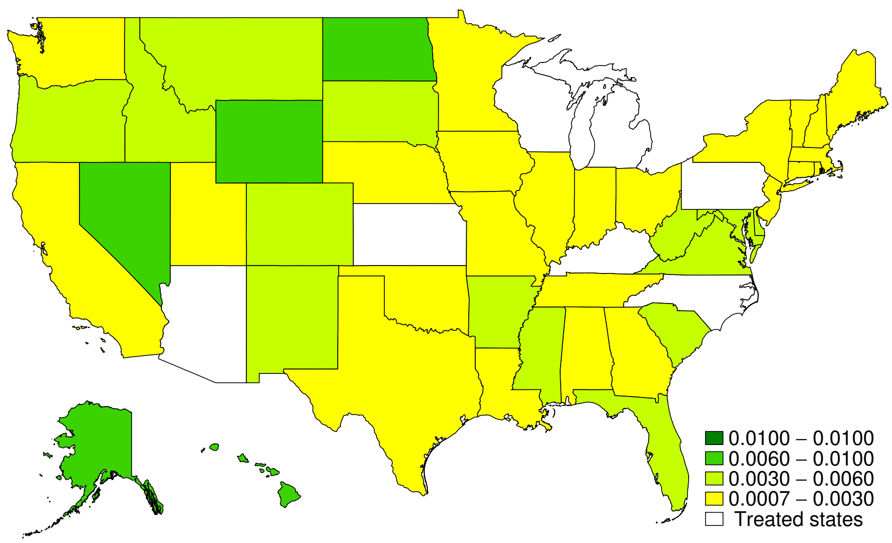
(b) Indirect spillover effects

Notes: Map based on the strong blue wave (see Figure 10) in which nine states governed by Republicans in 2024 are simulated as flipping to Democratic governors. Percentage changes in the college-educated workforce for the treated states are mapped in Panel (a), and percentage changes for un-treated states are mapped in Panel (b).

Figure 12: Direct and spillover effects of big red wave



(a) Direct effects: Treated states



(b) Indirect spillover effects

Notes: Map based on the strong red wave (see Figure 10) in which seven states governed by Democrats in 2024 are simulated as flipping to Republican governors. Percentage changes in the college-educated workforce for the treated states are mapped in Panel (a), and percentage changes for un-treated states are mapped in Panel (b).

Table 1: College graduates' in-migration responses to gubernatorial transitions

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Regression discontinuity			Difference-in-difference		
Identifying elections	All close	Close, D incumb.	Close, R incumb.	All flips	Flips, D-to-R	Flips, R-to-D
Republican Governor	-0.173*	-0.126		-0.079***	-0.103***	
	(0.089)	(0.098)		(0.024)	(0.029)	
	[p: .064]	[p: .302]		[p: .004]	[p: .006]	
Democratic Governor			0.224			0.056*
			(0.150)			(0.032)
			[p: .099]			[p: .133]
N. state-years				2,174	853	1,321
N. states (clusters)	45	32	31	50	42	44
N. close elections	104	59	43			
N. gubernatorial flips	43	24	19	68	37	31

* $p < .10$, ** $p < .05$, *** $p < .01$. *Columns 1-3*: Estimates are regression discontinuity estimates using the Calonico, Cattaneo, and Titiunik (2014)—CCT14—procedure with a triangular kernel and linear polynomial. A unit of observation is a state-by-election-year; the outcome variable is the unweighted average of the log of the in-migration rate during the four post-election years. We use the CCT14 procedure to choose the optimal bandwidth in the first column, and use that same bandwidth (.06) in the next two columns, which differ in whether they restrict to states with a Democratic incumbent (column 2, in which treatment effects therefore reflect D-to-R transitions) or a Republican incumbent. *Columns 4-6*: Estimates are based on a staggered rollout different-in-difference estimator described in Section 3.2 and equations (4) and (5). A unit of observation is a state-by-treatment-event-by-year, all specifications include state-by-treatment-event and treatment-event-by-year fixed effects, and the outcome variable is the log of the in-migration rate. State-years are weighted by the number of ACS respondents, adjusted using the Callaway-Sant'Anna adjustment so that the estimand is the ATT (see Section 3.2). We restrict to elections 2002-2016 to have at least three pre-treatment and post-treatment years in our 2000-2019 migration sample. *All columns*: Standard errors in parentheses are clustered at the state-level and p-values in brackets are from a cluster wild bootstrapped test of the null of zero treatment effect (Roodman et al., 2019), since we sometimes have relatively few states in the samples. "In-migration rates" refer to the fraction of private sector employed college graduates age 26+ who report living in a different state last year.

Table 2: Governor effects by type and timing of partisan flip

	(1)	(2)	(3)	(4)	(5)
Outcome:	Log of college in-migration rate	Economic policy index (SD = 1)	Social policy index (SD = 1)	Gov. approval among coll. (SD = 1)	College unemp. rate (range: 0-100)
	Panel A: D-to-R transitions during non-recession years				
Republican gov.	-0.140***	0.189**	0.125*	0.066	-0.001
	(0.048)	(0.080)	(0.071)	(0.291)	(0.152)
	[p: 0.021]	[p: 0.036]	[p: 0.153]	[p: 0.844]	[p: 0.992]
	Panel B: D-to-R transitions during recession years				
Republican gov.	-0.059	0.237***	0.139***	-0.062	-0.004
	(0.068)	(0.051)	(0.042)	(0.292)	(0.173)
	[p: 0.407]	[p: 0.001]	[p: 0.015]	[p: 0.833]	[p: 0.982]
	Panel C: R-to-D transitions during non-recession years				
Democratic gov.	-0.096*	-0.186***	-0.249***	1.117***	0.130
	(0.051)	(0.043)	(0.064)	(0.211)	(0.160)
	[p: 0.115]	[p: 0.004]	[p: 0.000]	[p: 0.001]	[p: 0.484]
	Panel D: R-to-D transitions during recession years				
Democratic gov.	0.149***	-0.151**	-0.376***	1.333***	-0.143
	(0.049)	(0.062)	(0.105)	(0.392)	(0.199)
	[p: 0.002]	[p: 0.044]	[p: 0.029]	[p: 0.011]	[p: 0.587]

* $p < .10$, ** $p < .05$, *** $p < .01$. Estimates are based on a staggered rollout different-in-difference estimator described in Section 3.2 and equation (4). A unit of observation is a state-by-treatment-event-by-year, all specifications include state-by-treatment-event and treatment-event-by-year fixed effects, and the outcome variable is the log of the in-migration rate. State-years are weighted by the number of ACS respondents, adjusted using the Callaway-Sant'Anna adjustment so that the estimand is the ATT (see Section 3.2). "Non-recession years" refers to gubernatorial transitions taking place 2003-2006 or 2014-2017, "recession years" refers to gubernatorial transitions taking place 2007-2013. Migration from 2020 onwards is never used. Standard errors in parentheses are clustered at the state-level and p-values in brackets are from a cluster wild bootstrapped test of the null of zero treatment effect (Roodman et al., 2019), since we sometimes have relatively few states in the samples. In-migration rates refer to the fraction of private sector employed college graduates age 26+ who report living in a different state last year. Both the economic and the social policy indices are created by Caughey and Warshaw (2018) to summarize 150 different policies. Gubernatorial approval is based on a 5-point scale, measured in the CES among college graduates living in the state. Both indices and gubernatorial approval are normalized to have a standard deviation of one across state years. College unemployment rate ranges from 0 to 100%, and is measured among college graduates in the ACS. Figures B9-B12 present pre-trends.

Table 3: General equilibrium effects of counterfactual gubernatorial switches

	(1)	(2)	(3)	(4)
Counterfactual	Small blue	Big blue	Small red	Big red
Number of states	5	9	3	7
Panel A: Total college-educated labor force				
Average effects	2.1%	1.3%	-1.8%	-1.3%
(min, max)	(1.5, 3.6)	(0.5, 2.8)	(-2.1, -1.5)	(-2.2, -0.6)
Panel B: GDP per worker				
Average effects	1.9%	1.2%	-1.6%	-1.1%
(min, max)	(1.3, 2.5)	(0.5, 2.0)	(-1.9, -1.4)	(-1.7, -0.6)
Panel C: Unobservable wage inequality (human capital price)				
Average effects	-1.7 log points	-1.0 pts.	1.4 pts.	1.1 pts.
(min, max)	(-2.4, -1.2)	(-1.9, -0.5)	(1.3, 1.7)	(0.6, 1.5)
Panel D: Observable wage inequality (college vs. non-college earnings)				
Average effects	0.6 log points	0.4 pts.	-0.6 pts.	-0.5 pts.
(min, max)	(0.2, 1.4)	(0.1, 1.1)	(-0.7, -0.3)	(-0.7, -0.3)

Table presents changes in equilibrium outcomes for "treated" states (i.e., those changing their governors) in our four counterfactuals (Figure 10 presents the states we simulate as flipping in each counterfactual). Panels A and B present percent changes in the college-educated workforce and GDP per worker, respectively. Panels C and D present changes in the college/non-college difference in log human capital price and log average earnings per worker, respectively.

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Downey & Liu

Online Appendix

A Data

[Table A1 about here.]

[Table A2 about here.]

Table A1: Social Policy Questions

Category	N. questions ever	N. questions each year	Most common question
Immigration	23	4.7	Do you support increasing the number of border patrols on the US-Mexico border?
Abortion and birth control	5	2.7	Do you support banning abortion after the 20th week of pregnancy?
Gun control	6	2.1	Do you support banning assault rifles?
Gender and sexuality	7	1.5	Do you favor or oppose allowing gays and lesbians to marry legally?
Crime and policing	12	1.1	Do you support eliminating mandatory minimum sentences for non-violent drug offenders?
Affirmative action	1	0.7	Do you support affirmative action programs that give preference to racial minorities in employment and college admissions in order to correct for past discrimination?
Supreme Court	5	0.4	Do you support confirming Brett Kavanaugh to become a Justice of the Supreme Court?
Education	3	0.2	Do you support the Student Success Act, which would end more than 70 federal education programs and decentralize education decision-making?
Impeachment	2	0.1	Do you support removing President Trump from office for abuse of power?

Table displays characteristics of economic policy questions. Topics are classified as economic policy based on scheme developed by Caughey and Warshaw (2018). Many of the questions appear in grid format, where the literal question itself is simply the last part of a longer sentence (the first part being shown above). We have shortened and re-worded the questions we present here into standard language (also sometimes eliminating preambles before the questions). It is sometimes difficult to cleanly separate questions across these categories (e.g., a question related to taxes *and* government spending, or to immigration *and* crime), however our analysis does not use these category labels *within* social/economic policy groups. It is typically simple to determine whether a question belongs to the social group or the economic group. "Number of questions ever" refers to the total number of unique questions appearing about the topic between 2006 and 2020. "Number of questions each year" refers to the average number of questions asked about the topic during any given year, 2006-2020.

Table A2: Economic Policy Questions

Category	N. questions ever	N. questions each year	Most common question
Government spending	23	4.7	If your state were to have a budget deficit this year, what would you prefer more, raising taxes or cutting spending?
Environment	10	2.9	Do you support giving the Environmental Protection Agency the power to regulate Carbon Dioxide?
Health care	14	2.3	Do you support repealing the Affordable Care Act?
Taxes	5	2.1	If the state had to raise taxes, what share of the tax increase should come from increased income taxes and what share from increased sales taxes?
Trade	8	1.2	Should the United States withdraw from the Trans-Pacific Partnership
Minimum wage	2	0.4	Do you support raising the minimum wage to $15 an hour?
Deregulation	2	0.3	Would you support an executive action requiring that with each new regulation enacted, two must be cut?

Table displays characteristics of economic policy questions. Topics are classified as economic policy based on scheme developed by Caughey and Warshaw (2018). Many of the questions appear in grid format, where the literal question itself is simply the last part of a longer sentence (the first part being shown above). We have shortened and re-worded the questions we present here into standard language (also sometimes eliminating preambles before the questions). It is sometimes difficult to cleanly separate questions across these categories (e.g., a question related to taxes *and* government spending, or to immigration *and* crime), however our analysis does not use these category labels *within* social/economic policy groups. It is typically simple to determine whether a question belongs to the social group or the economic group. “Number of questions ever” refers to the total number of questions about the topic ever asked from 2006-2020. “Number of questions each year” refers to the average number of questions asked about the topic during any given year, 2006-2020.

B Additional results

B.1 Descriptive facts about migration and population change

[Figure B1 about here.]

[Figure B2 about here.]

[Figure B3 about here.]

B.2 Descriptive facts about political attitudes

[Figure B4 about here.]

[Figure B5 about here.]

[Figure B6 about here.]

[Figure B7 about here.]

B.3 Reduced form estimates

[Figure B8 about here.]

[Table B1 about here.]

[Table B2 about here.]

[Table B3 about here.]

[Figure B9 about here.]

[Figure B10 about here.]

[Figure B11 about here.]

[Figure B12 about here.]

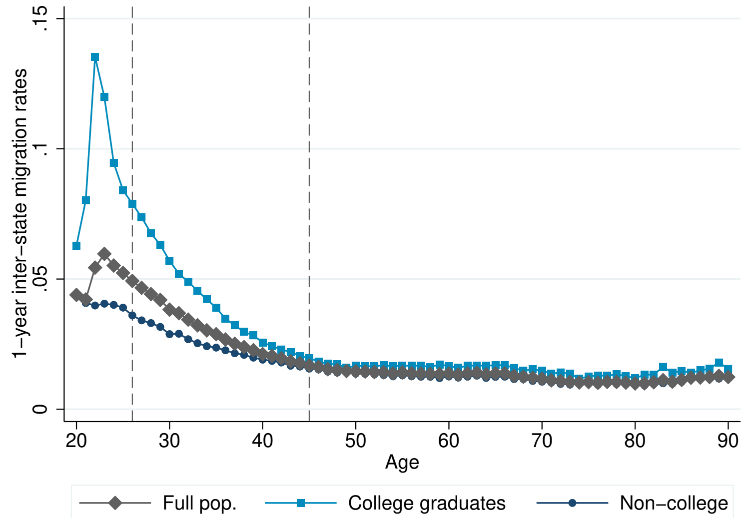
B.4 Structural analysis results

[Table B4 about here.]

[Table B5 about here.]

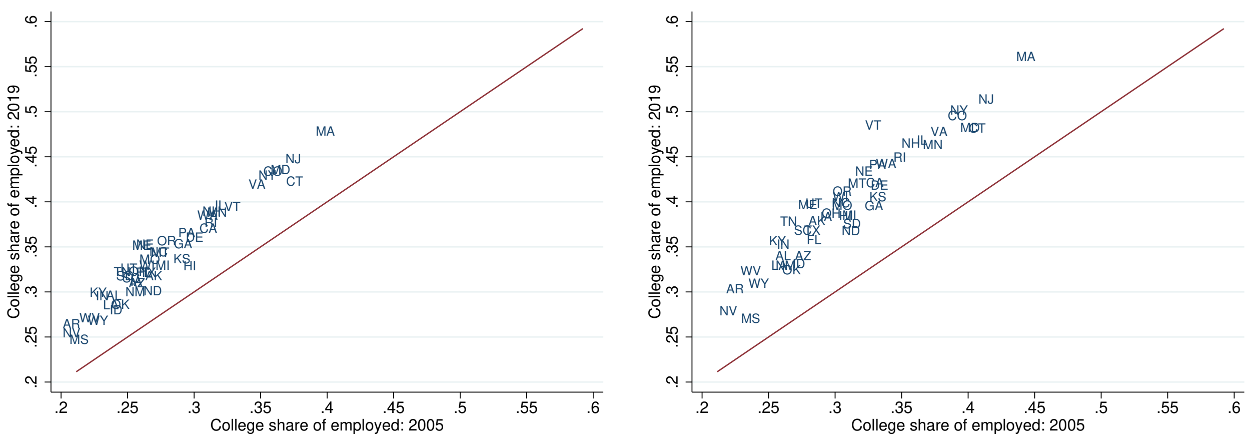
[Figure B13 about here.]

Figure B1: Inter-state migration rates by age and education (2005-2019)



Notes: Calculations based on one-year inter-state migration rates from the American Community Survey (ACS). All calculations use sample weights.

Figure B2: Changes in college-shares by state over time

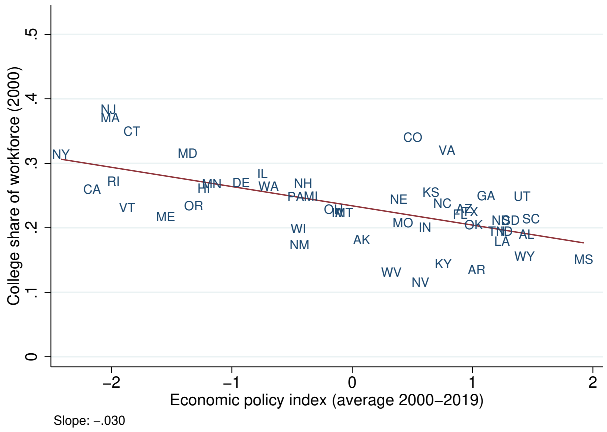


(a) Share of all employed

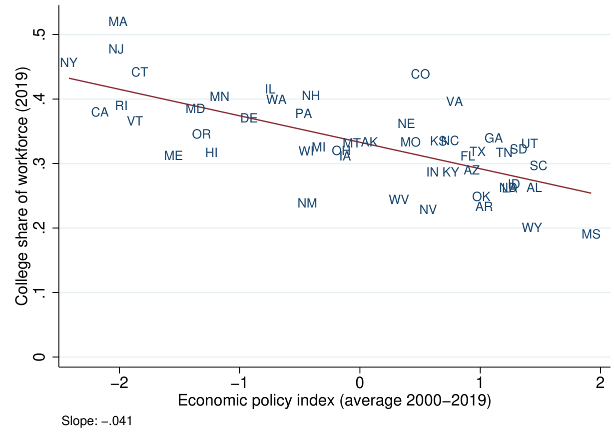
(b) Share of "young" (26-45) employed

Notes: Calculations based on American Community Survey (ACS). All calculations use sample weights. Red line is a 45-degree line.

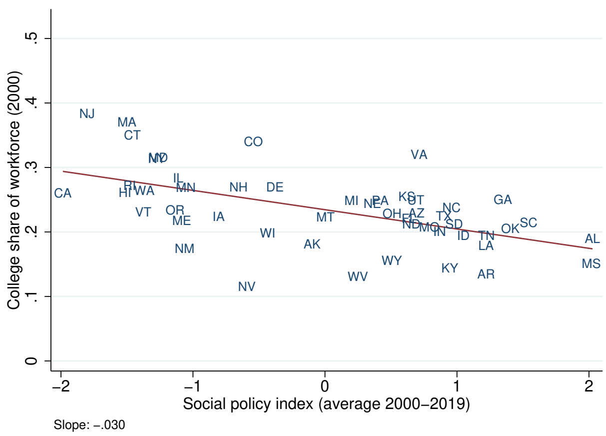
Figure B3: College share by state policy over time



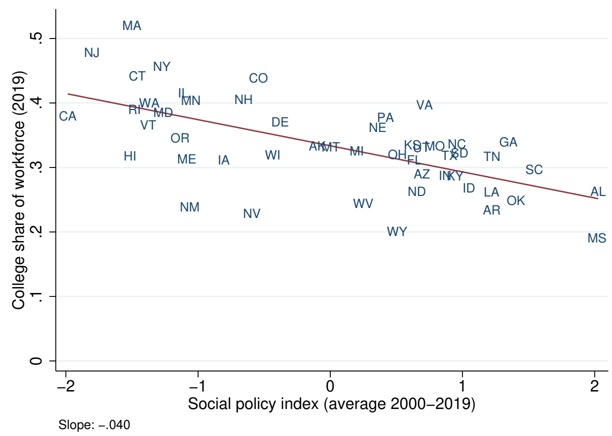
(a) By economic policy, 2000



(b) By economic policy, 2019



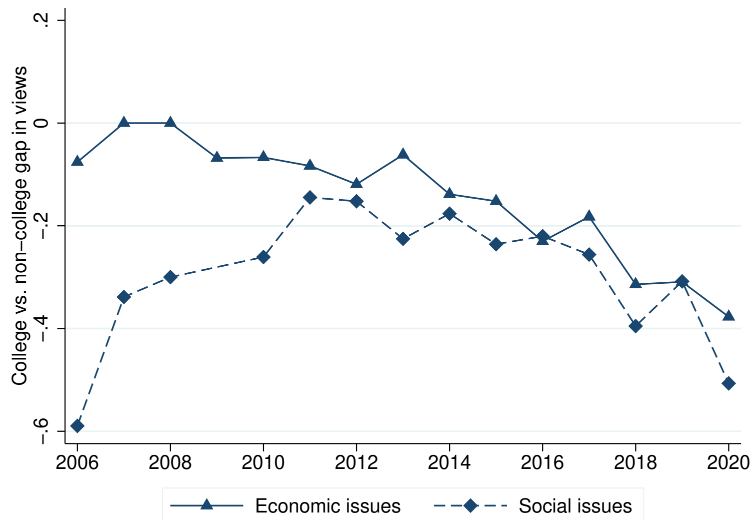
(c) By social policy, 2000



(d) By social policy, 2019

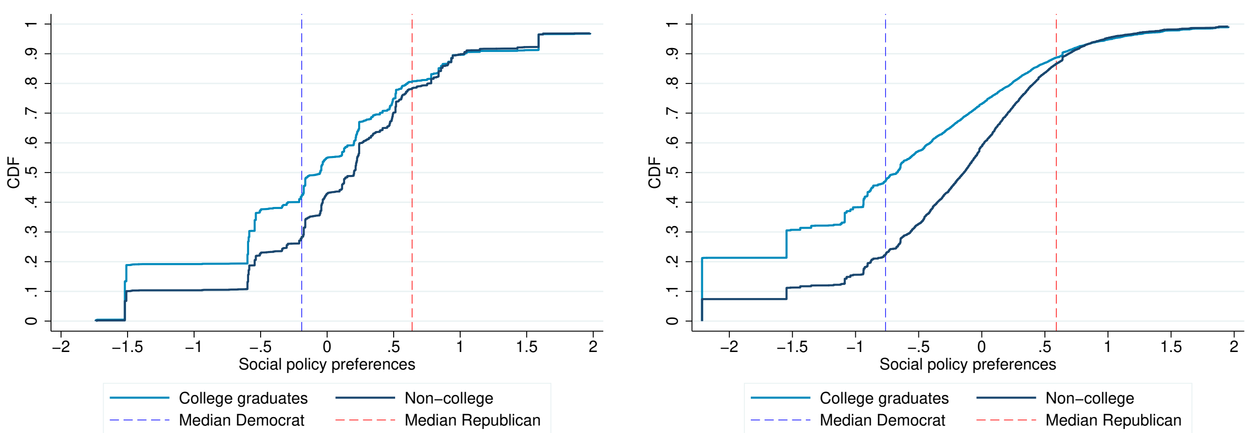
Notes: Calculations based on American Community Survey (ACS). All calculations use sample weights. Policy indices are drawn from Caughey and Warshaw (2016).

Figure B4: College vs. Non-college gaps in median views



Notes: All calculations are based on Cooperative Election Study (CES) respondents aged 26-45. Policy view indices are based on Item Response Theory estimates using the approach developed by Imai et al. (2016). Policy questions are divided into social and economic policy issues using the scheme developed by Caughey and Warshaw (2018). Indices are normalized to have mean zero and unit standard deviation across all respondents across all years, with more positive values indicating more conservative views. All calculations use sample weights.

Figure B5: Distribution of social policy views: 2010-2020

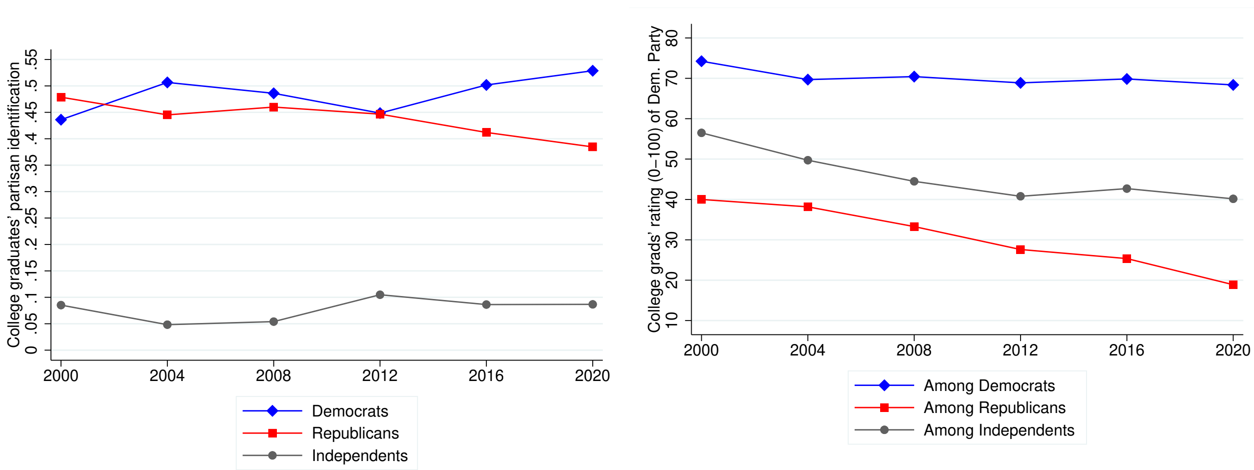


(a) 2010

(b) 2020

Notes: All calculations are based on Cooperative Election Study (CES) respondents aged 26-45. Policy view indices are based on Item Response Theory estimates using the approach developed by Imai et al. (2016). Policy questions are divided into social and economic policy issues using the scheme developed by Caughey and Warshaw (2018). Indices are normalized to have mean zero and unit standard deviation across all respondents across all years, with more positive values indicating more conservative views. All calculations use sample weights.

Figure B6: College graduates' party identification and attitudes towards the Democratic Party

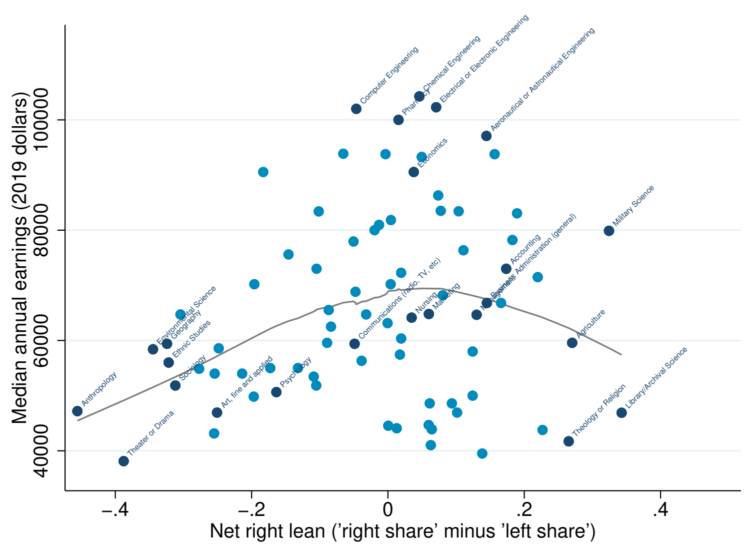


(a) Party identification

(b) Attitudes towards Dem. Party by identification

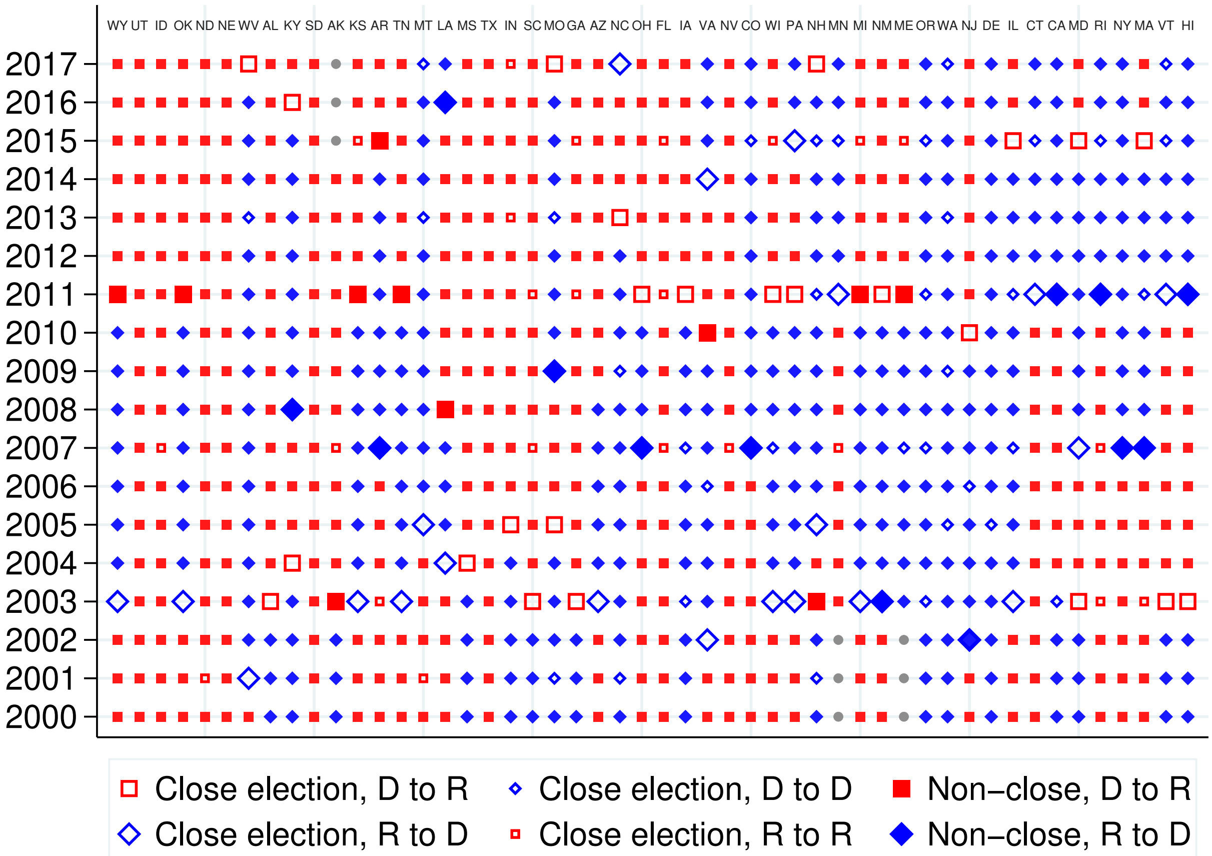
Notes: All calculations are based on American National Election Study (ANES), where respondents report a "feeling thermometer" rating (0-100) on both parties. All calculations use sample weights and are restricted only to college graduates.

Figure B7: Self-described political leanings and earnings by college major



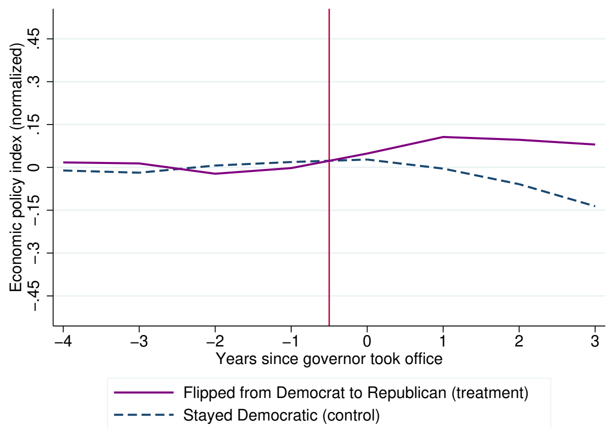
Notes: Median earnings (y-axis) are based on American Community Survey (ACS) respondents from 2009-2019. Net left lean (x-axis) is the difference between the share of UCLA's Higher Education Research Institute (HERI) survey respondents who identify with "far left" or "liberal" political views minus the share who identify with "far right" or "conservative" political views. Line represents a Lowess non-parametric fit.

Figure B8: Identifying variation: Close elections and partisan flips

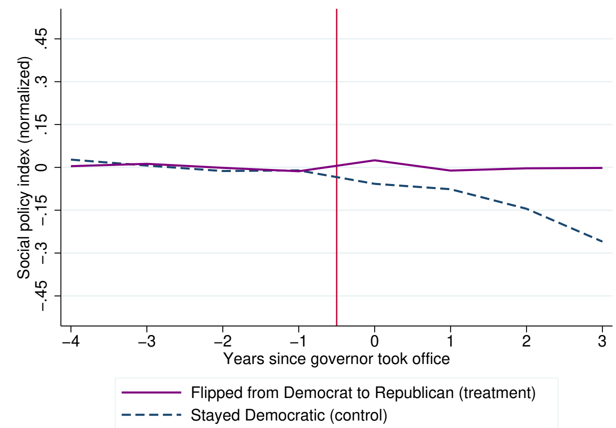


Notes: Figure shows the partisanship of the governor of each state over time, with states ordered by increasing Democratic vote share in presidential elections 2000-2020. Blue diamonds indicate Democratic governors, red squares indicate Republicans, and gray circles indicate independents. Large shapes indicate the first year of a flip from one party to the other, and hollow shapes indicate that the last year saw a close election (regardless of whether or not the state flipped). Our core RDD approach compares hollow red squares (narrowly elected Republicans) to hollow blue diamonds (narrowly elected Democrats), regardless of whether the election induced a gubernatorial flip (i.e., regardless of the size of the shape). When we estimate the RDD separately by the incumbent governor's party, we compare transitions driven by close elections to close elections that did not result in a transition. For instance, when we condition on a Democratic incumbent, we are comparing close-election-induced D-to-R transitions to close election D-to-D stable partisanship, which compares hollow large red squares to hollow small blue diamonds. Our core DiD approach compares states that flipped (large shapes, regardless of whether the election was close or not) to states that had the same pre-flip partisanship and remained stable over time. For instance, the 2009 R-to-D flip in Missouri (MO) is compared to South Carolina (SC) and Georgia (GA), which show very similar voting in presidential elections and the same gubernatorial partisanship prior to 2009, but retained Republican governors beyond that year.

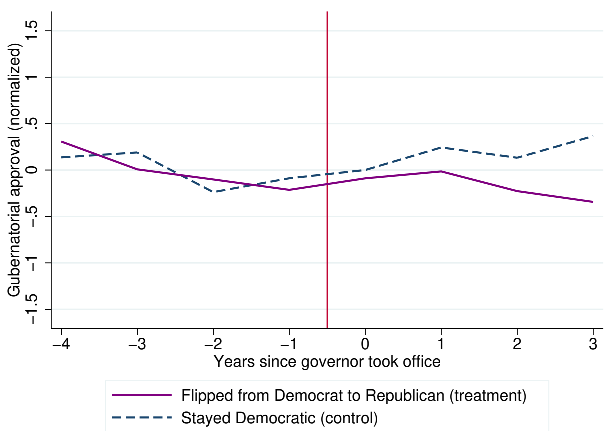
Figure B9: Other effects of Democrat-to-Republican transitions (non-recession years)



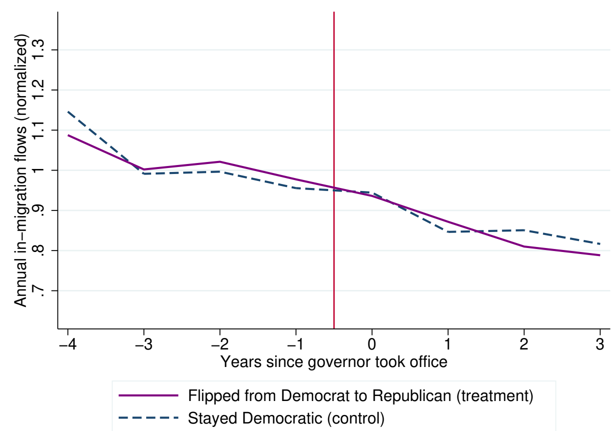
(a) Economic policy



(b) Social policy



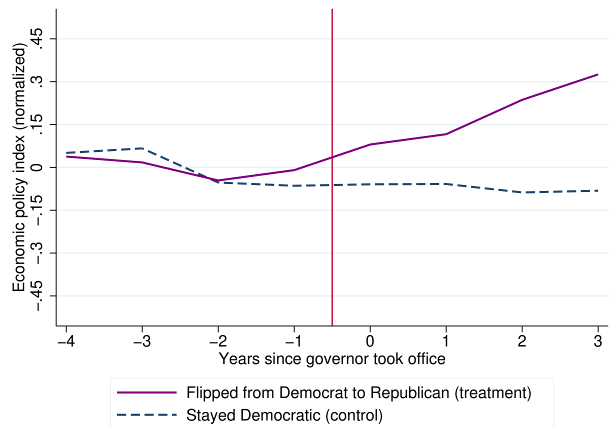
(c) College residents' gubernatorial approval



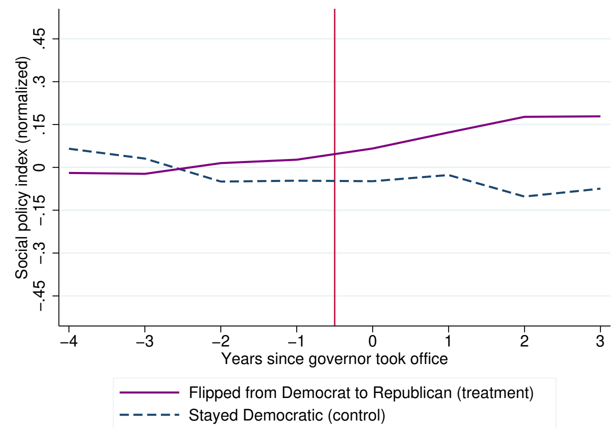
(d) College graduates' unemployment rate

Notes: Figure presents the raw levels of the variables that underlie our stacked DiD estimates in Panel A Table 2 (see Section 3.2 and equation (3) for more). These refer to Democrat-to-Republican transitions occurring 2003-2006 or 2014-2017. Both the economic and the social policy indices are created by Caughey and Warshaw (2018) to summarize 150 different policies. Gubernatorial approval is based on a 5-point scale, measured in the CES among college graduates living in the state. Both indices and gubernatorial approval are normalized to have a standard deviation of one across state years. College unemployment rate ranges from 0 to 100%, and is measured among college graduates in the ACS.

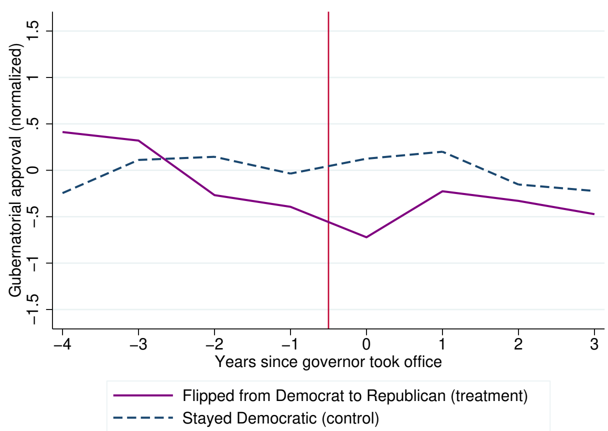
Figure B10: Other effects of Democrat-to-Republican transitions (recession years)



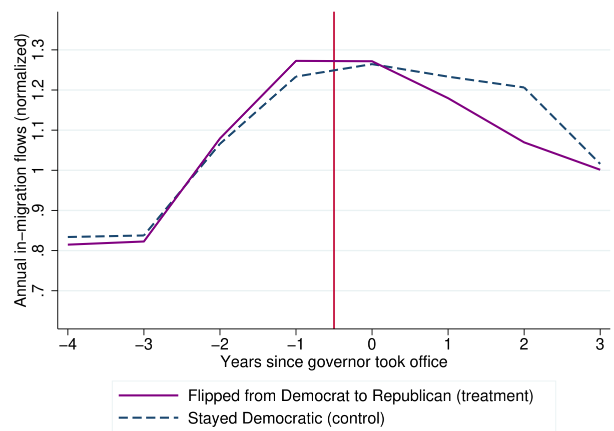
(a) Economic policy



(b) Social policy



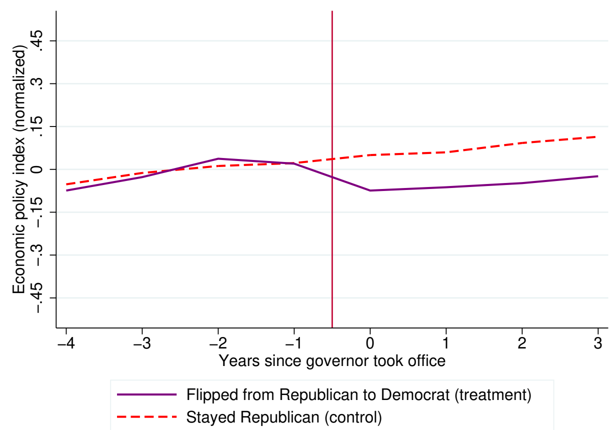
(c) College residents' gubernatorial approval



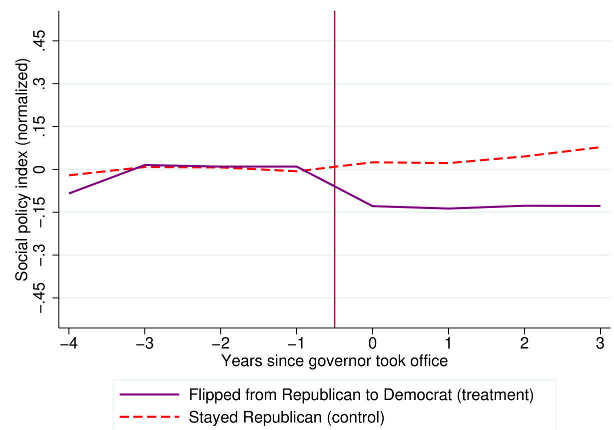
(d) College graduates' unemployment rate

Notes: Figure presents the raw levels of the variables that underlie our stacked DiD estimates in Panel B Table 2 (see Section 3.2 and equation (3) for more). These refer to Democrat-to-Republican transitions occurring 2007-2013. Both the economic and the social policy indices are created by Caughey and Warshaw (2018) to summarize 150 different policies. Gubernatorial approval is based on a 5-point scale, measured in the CES among college graduates living in the state. Both indices and gubernatorial approval are normalized to have a standard deviation of one across state years. College unemployment rate ranges from 0 to 100%, and is measured among college graduates in the ACS.

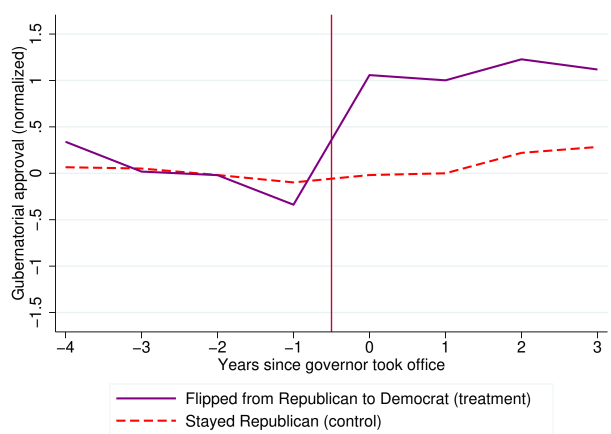
Figure B11: Other effects of Republican-to-Democrat transitions (non-recession years)



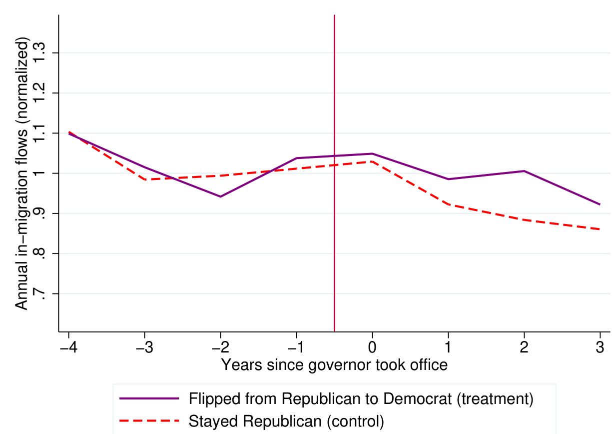
(a) Economic policy



(b) Social policy



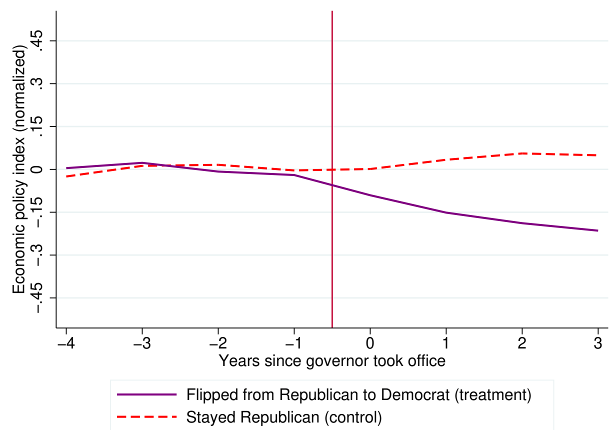
(c) College residents' gubernatorial approval



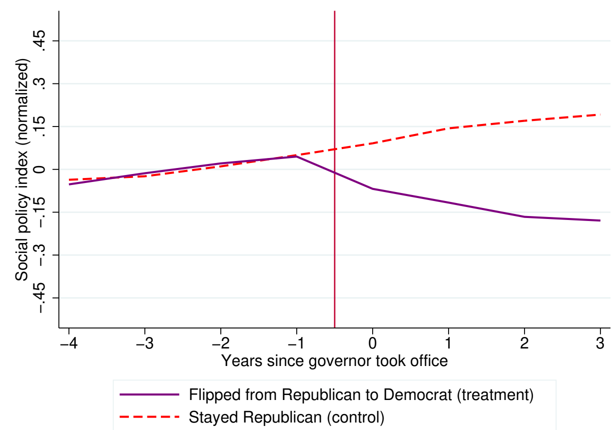
(d) College graduates' unemployment rate

Notes: Figure presents the raw levels of the variables that underlie our stacked DiD estimates in Panel C Table 2 (see Section 3.2 and equation (3) for more). These refer to Republican-to-Democrat transitions occurring 2003-2006 or 2014-2017. Both the economic and the social policy indices are created by Caughey and Warshaw (2018) to summarize 150 different policies. Gubernatorial approval is based on a 5-point scale, measured in the CES among college graduates living in the state. Both indices and gubernatorial approval are normalized to have a standard deviation of one across state years. College unemployment rate ranges from 0 to 100%, and is measured among college graduates in the ACS.

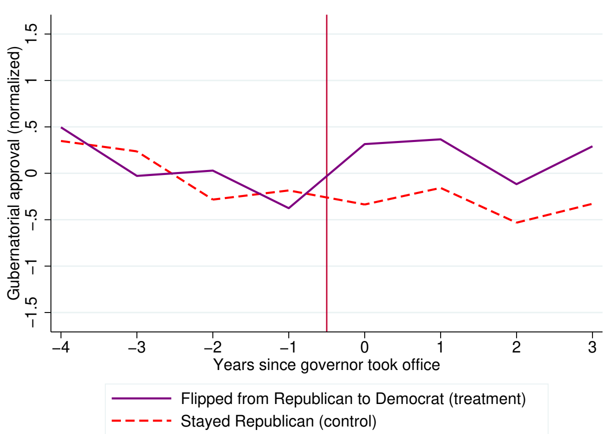
Figure B12: Other effects of Republican-to-Democrat transitions (recession years)



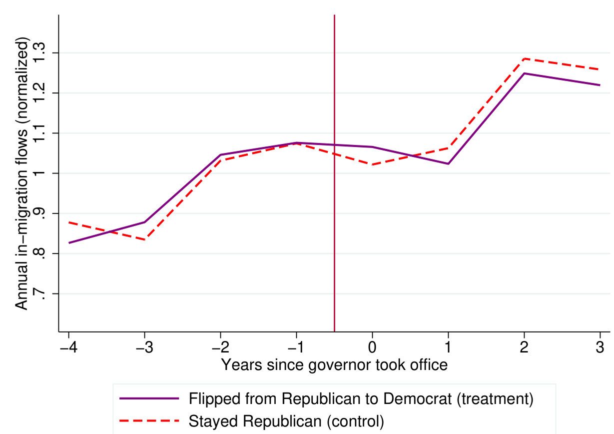
(a) Economic policy



(b) Social policy



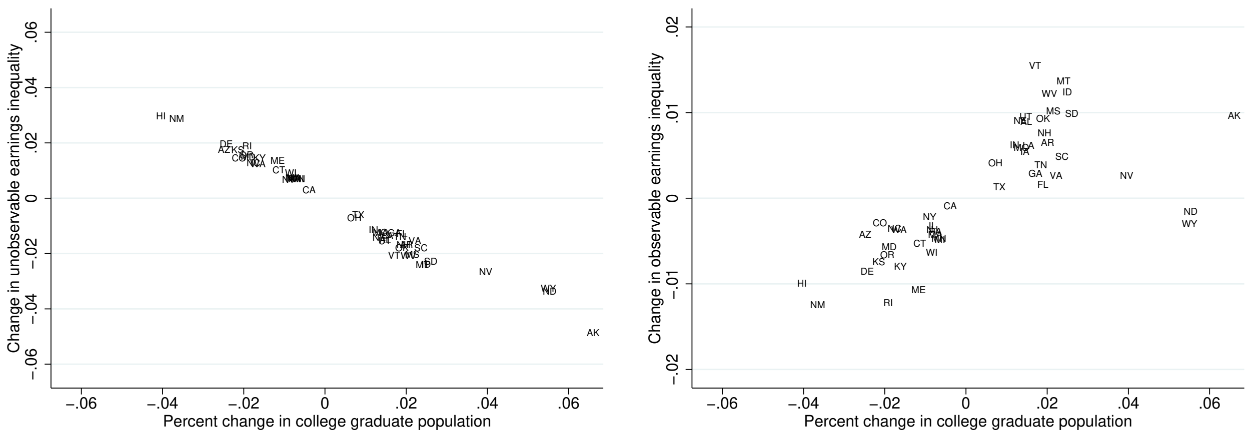
(c) College residents' gubernatorial approval



(d) College graduates' unemployment rate

Notes: Figure presents the raw levels of the variables that underlie our stacked DiD estimates in Panel D Table 2 (see Section 3.2 and equation (3) for more). These refer to Republican-to-Democrat transitions occurring 2007-2013. Both the economic and the social policy indices are created by Caughey and Warshaw (2018) to summarize 150 different policies. Gubernatorial approval is based on a 5-point scale, measured in the CES among college graduates living in the state. Both indices and gubernatorial approval are normalized to have a standard deviation of one across state years. College unemployment rate ranges from 0 to 100%, and is measured among college graduates in the ACS.

Figure B13: Inequality effects of single states switching governors



(a) Unobservable earnings inequality

(b) Observable earnings inequality

Notes: Figure based on 50 separate counterfactuals, each of which flips only one state independently, relative to the set of governors in office in 2024. In both panels, the x-axis refers to the model-implied percentage change in the size of the college graduate workforce. In panel (a), the y-axis refers to the change in the college/non-college gap in the log human capital price; in panel (b), the y-axis refers to the change in the college/non-college gap in log earnings per worker (see Table 3).

Table B1: College graduates' out-migration responses to gubernatorial transitions

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Regression discontinuity			Difference-in-difference		
Identifying elections	All close	Close, D incumb.	Close, R incumb.	All flips	Flips, D-to-R	Flips, R-to-D
Republican Governor	-0.095	-0.070		0.009	0.009	
	(0.071)	(0.071)		(0.026)	(0.041)	
Democratic Governor			.131			-.008
			(0.122)			(0.035)

* $p < .10$, ** $p < .05$, *** $p < .01$. *Columns 1-3*: Estimates are regression discontinuity estimates using the Calonico, Cattaneo, and Titiunik (2014)—CCT14—procedure with a triangular kernel and linear polynomial. A unit of observation is a state-by-election-year, and the outcome variable is the unweighted average of the log of the out-migration rate during the four post-election years. We use the CCT14 procedure to choose the optimal bandwidth in the first column of Table 1, and use that same bandwidth (.06) in this table. Column 2 and 3 restrict to states with a Democratic incumbent (in which treatment effects therefore reflect D-to-R transitions) or a Republican incumbent, respectively. *Columns 4-6*: Estimates are based on a staggered rollout different-in-difference estimator described in Section 3.2. A unit of observation is a state-year, and the outcome variable is the log of the out-migration rate. State-years are weighted by the number of ACS respondents, adjusted using the Callaway-Sant'Anna adjustment so that the estimand is the ATT (see Section 3.2). We restrict to elections 2002-2016 to have at least three pre-treatment and post-treatment years in our 2000-2019 migration sample. *All columns*: Standard errors in parentheses are clustered at the state-level. "Out-migration rates" refer to the fraction of private sector employed college graduates age 26+ who report living in the state last year and now live in a different state.

Table B2: College graduates' in-migration responses among different samples

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Regression discontinuity			Difference-in-difference		
Identifying elections	All close	Close, D incumb.	Close, R incumb.	All flips	Flips, D-to-R	Flips, R-to-D
	Panel A: Private sector, citizen, college, age 26-45 (more restrictive)					
Republican Governor	-0.196*	-0.228		-.095***	-0.129***	
	(0.116)	(0.140)		(0.029)	(0.040)	
Democratic Governor			0.201			0.062
			(0.185)			(0.037)
	Panel B: Employed, college, age 26+ (less restrictive)					
Republican Governor	-0.112	-0.136		-0.064***	-0.095***	
	(0.099)	(0.130)		(0.020)	(0.024)	
Democratic Governor			0.110			0.036
			(0.165)			(0.026)

* $p < .10$, ** $p < .05$, *** $p < .01$. *Columns 1-3*: Estimates are regression discontinuity estimates using the Calonico, Cattaneo, and Titiunik (2014)—CCT14—procedure with a triangular kernel and linear polynomial. A unit of observation is a state-by-election-year, and the outcome variable is the unweighted average of the log of the in-migration rate during the four post-election years. We use the CCT14 procedure to choose the optimal bandwidth in the first column, and use that same bandwidth (.06) in the next two columns, which differ in whether they restrict to states with a Democratic incumbent (column 2, in which treatment effects therefore reflect D-to-R transitions) or a Republican incumbent. *Columns 4-6*: Estimates are based on a staggered rollout different-in-difference estimator described in Section 3.2 and equations (4) and (5). A unit of observation is a state-by-treatment-event-by-year, all specifications include state-by-treatment-event and treatment-event-by-year fixed effects, and the outcome variable is the log of the in-migration rate. State-years are weighted by the number of ACS respondents, adjusted using the Callaway-Sant'Anna adjustment so that the estimand is the ATT (see Section 3.2). We restrict to elections 2002-2016 to have at least three pre-treatment and post-treatment years in our 2000-2019 migration sample. *All columns*: Standard errors in parentheses are clustered at the state-level and p-values in brackets are from a cluster wild bootstrapped test of the null of zero treatment effect (Roodman et al., 2019), since we sometimes have relatively few states in the samples. "In-migration rates" refer to the fraction of college graduates who report living in a different state last year. Panel A is restricted to college graduates with private sector employment who are age 26-45 and US citizens. Panel B is restricted to college graduates employed in any sector and age 26+.

Table B3: Non-college in-migration responses to gubernatorial transitions

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator	Regression discontinuity			Difference-in-difference		
Identifying elections	All close	Close, D incumb.	Close, R incumb.	All flips	Flips, D-to-R	Flips, R-to-D
Republican Governor	-0.018	0.126		-0.029	-0.028	
	(0.113)	(0.118)		(0.029)	(0.031)	
Democratic Governor			0.149			0.030
			(0.171)			(0.050)

* $p < .10$, ** $p < .05$, *** $p < .01$. *Columns 1-3*: Estimates are regression discontinuity estimates using the Calonico, Cattaneo, and Titiunik (2014)—CCT14—procedure with a triangular kernel and linear polynomial. A unit of observation is a state-by-election-year, and the outcome variable is the unweighted average of the log of the in-migration rate during the four post-election years. We use the CCT14 procedure to choose the optimal bandwidth in the first column, and use that same bandwidth (.06) in the next two columns, which differ in whether they restrict to states with a Democratic incumbent (column 2, in which treatment effects therefore reflect D-to-R transitions) or a Republican incumbent. *Columns 4-6*: Estimates are based on a staggered rollout different-in-difference estimator described in Section 3.2. A unit of observation is a state-year, and the outcome variable is the log of the in-migration rate. State-years are weighted by the number of ACS respondents, adjusted using the Callaway-Sant'Anna adjustment so that the estimand is the ATT (see Section 3.2). We restrict to elections 2002-2016 to have at least three pre-treatment and post-treatment years in our 2000-2019 migration sample. *All columns*: Standard errors in parentheses are clustered at the state-level and p-values in brackets are from a cluster wild bootstrapped test of the null of zero treatment effect (Roodman et al., 2019), since we sometimes have relatively few states in the samples. "In-migration rates" refer to the fraction of private sector employed non-college workers age 26+ who report living in a different state last year.

Table B4: Parameter values

Description	Determination	Parameter	Mean	Standard Deviation
			Value	
EoS across groups	Jerzmanowski-Tamura '20	σ	2.6	
Skill dispersion	Wage dispersion	θ^C	2.85	
		θ^N	2.96	
Political wedge	Reduced-form moments	$\gamma^c_{p(d)}$	0.051	
		$\gamma^N_{p(d)}$	0.000	
Migration shock	Migration flow	μ^C_{do}	0.04	0.02
		μ^N_{do}	0.02	0.01
Migration cost	Migration flow difference	η^C_{do}	1.05	0.39
		η^N_{do}	1.06	0.42
Amenities	Same party governors	α^C_d	0.70	0.25
		α^N_d	0.75	0.27
Human capital price	Market clear + common price	w^C_d	665.257	115.725
		w^N_d	533.784	180.013
TFP	Market clear + common price	A_d	380.630	105.293

This table reports the parameter values for the model. σ is from the literature and the rest parameters are estimated from target moments. Human capital price and TFP are reported in thousands of dollars.

Table B5: Decomposing effects on human capital price inequality

	(1)	(2)	(3)	(4)
Counterfactual	Small blue	Big blue	Small red	Big red
Number of states	5	9	3	7
Panel A: Change in log human capital price for college-educated workers: $\ln(w_d^C)$				
Average effects	-0.010	-0.006	0.008	0.006
(min, max)	(-0.015, -0.007)	(-0.012, -0.003)	(0.007, 0.010)	(0.004, 0.009)
Panel B: Change in log human capital price for non-college workers: $\ln(w_d^N)$				
Average effects	0.007	0.004	-0.006	-0.004
(min, max)	(0.005, 0.009)	(0.002, 0.007)	(-0.007, -0.005)	(-0.006, -0.002)
Panel C: Share attributable to non-college wages				
Share	.421	.412	.406	.405

Table presents changes in log human capital prices for "treated" states (i.e., those changing their governors) in our four counterfactuals (Figure 10 presents the states we simulate as flipping in each counterfactual). At the bottom, "share attributable to non-college wages" presents the share of the growth in college/non-college inequality that comes from falling real non-college wages, for the average treated state in the counterfactual.

C Structural analysis

C.1 Setup

The **timing** of the model is as follows: there are K states and in period $t = 0$, all workers are born in origin o . Each worker receives a destination-specific skill draw $s_{id,t=0}$ for each destination d and migrates to a state $d^*_{i,t=0}$ maximizing her utility based on wage rates, productivity, appreciation of amenities, migration costs, political preference. From $t \geq 1$, origin-specific migration shock μ^g_o activates. Workers who do not receive the shock will stay in the previous location $d^*_{i,t-1}$; workers who receive the shock will get a new skill draw $s_{id,t}$, for $d \neq o$, and migrate based only on the utility in t.

C.1.1 Utility

Let the utility of worker i belonging to education group g and moving from origin o to destination d in period $t = 0$ be given by:

$$U^g_{ido,t} = c^g_{id,t} \alpha^g_d \eta^g_{do} (1 - \gamma^g_{p(d,t)})$$

where $c^g_{id,t}$ is her consumption and α^g_d is the general amenity in d for education group $g \in \{C, N\}$. We consider these amenities fixed across all individuals with the same education over time, but we allow all parameters to vary across education groups, denoted by $g = C$ for college graduates and $g = N$ for non-college. Define η^g_{do} to be both the migration costs of moving from o to d, as well as idiosyncratic features of d that make it particularly attractive to those moving from o.

The term $\gamma^g_{p(d,t)}$ captures a group-specific preference wedge emerging from group g's disutility of living under a governor of partisanship $p(d, t) \in \{\text{Republican}, \text{Democrat}\}$. When this wedge is positive, workers must be compensated with $1/(1 - \gamma^g_{p(d,t)})$ times greater consumption in order to be indifferent. Since college-educated workers are to the left of non-college ones, we normalize $\gamma^C_{Dem} = 0$ and $\gamma^N_{Rep} = 0$ so that γ^C_{Rep} captures college graduates' disutility of Republican governance, relative to Democratic governance, and conversely for non-college workers' disutility of Democratic governance.[33]

[33] This parameter varies across education groups but not across individuals within the group. It is obviously stark and unrealistic to assume that all individuals of the same education have the same political preferences. More realistically, we could assume that only some share $\chi < 1$ of college graduates are liberal. But with this formulation, the decline in college in-migration that we estimated above would have been generated entirely by this χ share of liberal college graduates. This would require a larger value of the γ disutility parameter, since this subset's behavioral responses would have to be larger, but would not change the aggregate size of the migration response (since this is a targeted moment). This would change the welfare implications of our model (since it would increase the intensity and heterogeneity in preferences) but likely would not affect other implications, which is where our analysis focuses.

Workers who get the migration shock and will move across states receive one idiosyncratic skill draw for each potential destination state. Our representation of exogenous migration shocks is similar to Monras (2020), while our representation of idiosyncratic human capital draws is similar to Bryan and Morten (2019) and Hsieh et al. (2019). We think of this as a match-specific human capital draw reflecting the best employment opportunity available in that state. We assume these idiosyncratic opportunities, denoted $s^g_{id,t}$, are drawn from a multivariate Fréchet distribution:

$$F(s^g_{1,t}, s^g_{2,t}, ..., s^g_{K,t}) = exp\left(-\left[\sum_{d=1}^{K} s_{d,t}^{-\theta^g}\right]\right)$$

The distribution of $s^g_{id,t}$ is governed by the scale parameter θ^g. A higher value of θ^g implies less skill dispersion across locations, such as would be the case if all states afforded the worker an equally good employment match. As θ^g decreases, there is a greater difference between the match-specific human capital realizations a worker sees in different locations. This parameter is key because it determines how close to indifferent workers are between the employment opportunities available in different states. Note that this formulation assumes each worker's draws are independent across states, although state-specific factor supplies and wage rates will lead to a correlation in *earnings* across different workers within the same state.

We derive the indirect utility function for worker i in group g who moved from o to d in period $t = 0$ as:

$$V^g_{ido,t} = \alpha^g_d \eta^g_{do}(1 - \gamma^g_{p(d,t)}) w^g_{d,t} s^g_{id,t} \tag{10}$$

where $w^g_{d,t}$ is the human capital price in d for group g in period t. That is, if $w^g_{d,t} > w^g_{d',t}$, then d has higher "wages" (human capital prices) than d': a worker with the same quality of employment opportunity in both d and d' (i.e., the same human capital draw: $s^g_{id,t} = s^g_{id',t}$) will have higher earnings and consumption in d since it is a higher wage state.

It is convenient to define the overall utility returns of destination d for a worker of group g and origin o as $\tilde{w}^g_{do,t} = \alpha^g_d \eta^g_{do}(1 - \gamma^g_{p(d,t)}) w^g_{d,t}$ so that $V^g_{ido,t} = \tilde{w}^g_{do,t} s^g_{id,t}$.

Our key assumption is that workers choose the location that yields the highest indirect utility. Note that the only idiosyncratic component that varies across individuals is their vector of skill draws. Since these are Fréchet distributed, a property of the Fréchet distribution is that the share of individuals from origin o who choose to work in destination d in period $t = 0$ can be written as:

$$\hat{\pi}^g_{do,t} \equiv \frac{L^g_{do,t}}{L^g_{o,t}} = \frac{\tilde{w}^{g\theta^g}_{do,t}}{\sum_{j=1}^{N} \tilde{w}^{g\theta^g}_{jo,t}} \tag{11}$$

where $L^g_{o,t}$ denotes the number of group-g workers from origin o in period t, and $L^g_{do,t}$ denotes the number who move to d in the same priod. Relative returns, including non-wage returns in terms of higher amenities or lower migration costs drive these migration flows.

A property of the Fréchet distribution is that conditional on the workers' optimal choice of where to live, the average skill of workers choosing to move from o to d can be written as:

$$\mathbb{E}\left(s_{d,t}^{g} \mid \text{choose } d \text{ from } o\right) = \bar{\Gamma^{g}} \left(\frac{1}{\hat{\pi}_{do,t}^{g}}\right)^{\frac{1}{\theta^{g}}} \tag{12}$$

where $\bar{\Gamma^{g}} = \Gamma(\frac{\theta^{g}-1}{\theta^{g}})$ and $\Gamma(.)$ is the gamma function. This property is important, as it allows us to infer unobserved human capital from observed migration rates. Human capital supplied is the product of the skill conditional on migrating from and the number of workers migrating from o to d summed over all origins

$$H_{d,t}^{g}(supply) = \sum_{o=1}^{N} L_{o,t}^{g} \hat{\pi}_{do,t}^{g} \bar{\Gamma^{g}} \left(\frac{1}{\hat{\pi}_{do,t}^{g}}\right)^{\frac{1}{\theta^{g}}} \tag{13}$$

Starting from $t = 1$, workers who receive migration shock, which occurs with probability μ_{o}^{g}, will migrate to other states, and the rest will stay in the previous location. The indirect utility function in the period of $t \geq 1$ is :

$$V_{ido,t}^{g} = \left(1 - \mu_{o}^{g}\right) V_{ido,t-1}^{g} + \max_{d, d \neq o} \left\{\mu_{o}^{g}(\tilde{w}_{do,t}^{g} s_{id,t}^{g})\right\}$$

The share of individuals from origin o who choose to work in destination d in $t \geq 1$ is:

$$\pi_{do,t}^{g} \equiv \frac{L_{do,t}^{g}}{\mu_{o}^{g} L_{o,t-1}^{g}} = \frac{\tilde{w}_{do,t}^{g\theta^{g}}}{\sum_{j \neq o} \tilde{w}_{jo,t}^{g\theta^{g}}} \tag{14}$$

The average skill of workers choosing to move from o to d in t is

$$\mathbb{E}\left(s_{d,t}^{g} \mid \text{choose } d \text{ from } o\right) = \bar{\Gamma^{g}} \left(\frac{1}{\pi_{do,t}^{g}}\right)^{\frac{1}{\theta^{g}}}$$

Total efficiency units of labor supply in $t \geq 1$ is

$$H_{d,t}^{g}(supply) = \underbrace{(1 - \mu_{d}^{g}) H_{d,t-1}^{g}(supply)}_{\text{stayers}} + \underbrace{\sum_{o=1}^{N} L_{o,t-1}^{g} \mu_{o}^{g} \pi_{do,t}^{g} \bar{\Gamma^{g}} \left(\frac{1}{\pi_{do,t}^{g}}\right)^{\frac{1}{\theta^{g}}}}_{\text{movers}}$$

C.1.2 Production

Firms produce a single final good, which is costlessly traded and is chosen as the numeraire ($p = 1$). Output is produced by perfectly competitive firms. They combine the effective labor (i.e., total human capital) of the two education groups using a Constant Elasticity of Substitution (CES) production function.

Total output in state d in period t is given by:

$$Y_{d,t} = A_{d,t}\left[\left(H^C_{d,t}\right)^{\frac{\sigma-1}{\sigma}} + \left(H^N_{d,t}\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}}$$

where $A_{d,t}$ is the exogenous location-specific total factor productivity (TFP) and $\sigma \geq 1$ is the elasticity of substitution between college and non-college workers. $H^g_{jd,t}$ is the total efficiency units of labor employed by firm j in time t, which is simply the sum of skill draws for workers working at firm j:

$$H^g_{jd,t} = \sum_{i \in P_{j,t}} s^g_{id,t}$$

where $P_{j,t}$ denotes the set of workers employed at firm j.

The profits for a representative firm in location d are given by:

$$\Pi_{jd,t} = Y_{jd,t} - w^C_{jd,t}H^C_{jd,t} - w^N_{jd,t}H^N_{jd,t}$$

where $w_{jd,t}$ is the wage paid per effective unit of labor by firm j. In this economy, firms compete for each type of worker by setting wages. Since the labor market is perfectly competitive, in equilibrium, $\Pi_{jd,t} = 0$ and $w^g_{jd,t} = w^g_{d,t} \;\; \forall j$. This prevailing wage (or the price of human capital) is the same $w^g_{d,t}$ discussed in the worker's problem above.

C.1.3 General equilibrium

A competitive equilibrium in this economy consists of destination choices, total efficiency of labor in each destination $H^g_{d,t}$, and a wage $w^g_{d,t}$ such that in each period t:

- Workers choose the workplace that maximizes their utility
- Firms choose efficient labor $H^g_{d,t}$ to maximize profit
- $w^g_{d,t}$ clears labor market for each destination

C.2 Additional derivations

C.2.1 The probability that an individual chooses one state

$$V_{ido} = \alpha_d^g(1-\gamma)w_{d,t}\eta_{do}^g\epsilon_{do}s_{id} = \tilde{w_{do}}s_{id}$$

By extreme value theory, if $U(\cdot)$ is Frechet then so is $\max u(\cdot)$, which is $V(\cdot)$. Without loss of generality, consider the probability that worker chooses destination 1 and denote this by π_{1o}:

$$\begin{aligned}
\pi_{1o} &= Pr\left[\tilde{w_{1o}}s_{i1} \geq \tilde{w_{so}}s_{is}\right]\forall s \neq 1 \\
&= Pr\left[s_{is} \leq \frac{\tilde{w_{1o}}s_{i1}}{\tilde{w_{so}}}\right]\forall s \neq 1 \\
&= \int F_1(s_i, T_2 s_i, ..., T_N s_i)ds_i
\end{aligned}$$

where $F_1(\cdot)$ is the derivative of cdf with respect to its first argument and $T_l \equiv \frac{\tilde{w_{1o}}}{\tilde{w_{lo}}}$. Recall that

$$F(s_1, s_2, ..., s_N) = exp\left(-\left[\sum_{d=1}^{N} s_d^{-\theta}\right]\right)$$

Taking the derivative with respect to s_1 gives

$$F_1(s_i, T_2 s_i, ..., T_N s_i) = \theta s_i^{-\theta-1}exp(-\bar{T}_r s_i^{-\theta})$$

where $\bar{T}_r \equiv -\sum_{s=1}^{N}(\frac{\tilde{w_{ro}}}{\tilde{w_{so}}})^{-\theta}$.

$$\begin{aligned}
\pi_{1o} &= \int F_1(s_i, T_2 s_i, ..., T_N s_i)ds_i \\
&= \frac{1}{\bar{T}_r}\int \bar{T}_r\theta s_i^{-\theta-1}exp(-\bar{T}_r s_i^{-\theta})dFs_i \\
&= \frac{1}{\bar{T}_r}\int dFs_i \\
&= \frac{1}{\bar{T}_r} \\
&= \frac{\tilde{w_{ro}}^{\theta}}{\sum_{s=1}^{N}\tilde{w_{so}}^{\theta}}
\end{aligned}$$

C.2.2 Average skill of workers

To calculate this conditional expectation, we use the extreme value properties of the Frechet distribution. Let $y_d = \tilde{w}_{d,t} s_d$ denote the key destination choice term.

$$y^* \equiv max_s \{y_s\} = max_s \{s_s/\zeta_s\} = s^*/\zeta^*.$$

$$\begin{aligned}
Pr[y^* < z] &= \prod_{s=1}^{N} Pr[y_s < z] \\
&= \prod_{s=1}^{N} Pr[\tilde{w}_s s_s < z] \\
&= \prod_{s=1}^{N} Pr[s_s < z/\tilde{w}_s] \\
&= \prod_{s=1}^{N} exp\left[-(z/\tilde{w}_s)^{-\theta}\right] \\
&= exp\left\{-\sum_{s=1}^{N}(z/\tilde{w}_s)^{-\theta}\right\} \\
&= exp\left\{-\bar{T}z^{-\theta}\right\}
\end{aligned}$$

where $\bar{T} \equiv \sum_{s=1}^{N} \tilde{w}_s{}^{\theta}$

The ability of people in their chosen place is also Frechet distributed:

$$F(x) \equiv Pr[s^* < x] = exp\left\{-T^* x^{-\theta}\right\} \tag{15}$$

where $T^* \equiv \sum_{s=1}^{N}(\frac{\tilde{w_s}}{\tilde{w^*}})^{\theta} = \frac{1}{\pi}$.

The expected skill of the chosen d is

$$\begin{aligned}
E(s_d) &= \int_0^{\infty} s dF(s) \\
&= \int_0^{\infty} \theta T^* s^{(1-\theta)} e^{-T^* s^{-\theta}} ds
\end{aligned} \tag{16}$$

Recall the Gamma function is $\Gamma(\alpha) \equiv \int_0^{\infty} x^{\alpha-1} e^{-x} dx$. We replace x $= T^* s^{-\theta}$, $dx = \theta T^* s^{(-\theta-1)} ds$, $s = (\frac{T^*}{x})^{\frac{1}{\theta}}$ We can show that

$$\begin{aligned}
E(s_d) =& (T^*)^{\frac{1}{\theta}} \int_0^{\infty} x^{-\frac{1}{\theta}} e^{-x} dx \\
&= (T^*)^{\frac{1}{\theta}} \Gamma(\frac{\theta-1}{\theta})
\end{aligned} \tag{17}$$

Applying this result to our equation, we have

$$
\begin{aligned}
E\left(s_d^{\theta} \mid choose\, d\, from\, o\right) &= (T^{*})^{\frac{1}{\theta}}\Gamma(\frac{\theta-1}{\theta}) \\
&= \pi_{do}^{-\frac{1}{\theta}}\Gamma(\frac{\theta-1}{\theta})
\end{aligned}
\tag{18}
$$

C.3 Identification

C.3.1 Elasticity of skills substitution: σ

A large literature estimates the elasticity of substitution between skills. We use $\sigma = 2.6$, which is the estimate from Jerzmanowski and Tamura (2020) for college and non-college workers. Acemoglu and Autor (2011) for some discussion, who calibrate a value of $\sigma = 2.9$ for the 1964-2008 period. It is worth noting that we use a value such that $\sigma > 1$. This implies that a decrease in the supply of college-educated workers will increase inequality through two forces. First, it will raise the relative wages of college graduates (i.e., college and non-college workers are gross substitutes). Second, it will reduce the real wage of non-college workers (i.e., they are q-complements).

C.3.2 Fréchet parameter: θ^g

Workers' earnings are determined by state-specific human capital prices (the wage rate paid per effective unit of labor) and the individual human capital draw with which workers move to the state. Looking at equation (12), once one accounts for the cross-state variation in the returns to human capital and variation across *od* in the degree of worker self-selection, the remaining variation in earnings is informative about the underlying dispersion of human capital draws. This insight was pointed out by Hsieh et al. (2019), and our approach to estimation follows theirs.

Specifically, we regress log annual earnings in the ACS on destination-by-origin fixed effects, while also controlling for age, gender, and fixed effects for year, occupation, and industry. We do this separately for college and non-college workers and calculate residual log earnings.

Since we control for destination-by-origin fixed effects, these residualized earnings have been purged of differential cross-state selection and heterogeneous returns to human capital. Since we control for individual-level covariates, the remaining dispersion reflects the variation in place-adjusted earnings that one worker could plausibly see across different potential states of residence. In this way, we can write a non-linear function of θ as a non-linear function of the distribution of residual wages:

$$\frac{\text{Variance}(\tilde{s}_i^g)}{(\text{ Mean}(\tilde{s}_i^g))^2} = \frac{\Gamma\left(1-\frac{2}{\theta^g}\right)}{\left(\Gamma\left(1-\frac{1}{\theta^g}\right)\right)^2} - 1 \tag{19}$$

where $\tilde{s}_i^g$ is individual i's residual log earnings, exponentiated.[34]

C.3.3 Migration shock: μ_o^g

Since "migration shock" starts from $t \geq 1$, we precisely match the group-origin migration outflows from the data to calculate μ_o^g, the average of $\mu_{o,t}^g$ where

$$\mu_{o,t}^g = \frac{\sum_{j\neq o} L_{jo,t}^g}{L_{o,t-1}^g} \tag{20}$$

C.3.4 Moving cost: η_{do}^g

Taking the log of equation (14), we have

$$\ln(\pi_{do,t}^g) = \underbrace{\theta^g \ln(\alpha_d^g)}_{\text{destination fixed effect}} + \underbrace{\theta^g \ln(w_{d,t}^g)}_{\text{destination-time fixed effect}} + \underbrace{\theta^g \ln(1-\gamma_{p(d,t)}^g)}_{\text{party-time fixed effect}} - \underbrace{\ln\left(\sum_{j=1,j\neq o}^{N} w_{jo,t}^{\tilde{g}}{}^{\theta^g}\right)}_{\text{origin-time fixed effect}} + \theta^g \ln \eta_{do}^g \tag{21}$$

After controlling for origin, destination, time, and party fixed effects, migration costs act as residuals that match the migration inflows.

C.3.5 Productivity and wages: $A_{d,t}, w_{d,t}^g$

We jointly identify productivity and wages using two conditions. First, the assumption of perfect competition in the product market and the law of one price implies that price equals marginal cost, which yields:

$$\left(\frac{w_{d,t}^C}{A_{d,t}}\right)^{1-\sigma} + \left(\frac{w_{d,t}^N}{A_{d,t}}\right)^{1-\sigma} = 1 \tag{22}$$

Second, labor market clearing implies that total human capital demanded equals total human capital supplied. Human capital demanded can be written from the CES production

[34] Residual log earnings are mechanically mean zero across od, but exponentiated residual log earnings are not mean one because the exponential function is non-linear.

function as a function of wages and total output.

$$H^g_{d,t}(demand) = \left(\frac{A_{d,t}^{\frac{\sigma-1}{\sigma}}}{w^g_{d,t}}\right)^{\sigma} Y_{d,t} = H^g_{d,t}(supply) \tag{23}$$

Given the above estimate of θ^g, and observable output and migration, this delivers three equations (since equation (23) must hold for $g \in \{C, N\}$) and three unknowns $(A_{d,t}, w^C_{d,t}, w^N_{d,t})$ for each state in each period.

Fundamentally, TFP and wages are pinned down by variation in GDP. Given an estimate of skill dispersion, our model tells us how human capital stocks can be determined by population size and migration (since migration entails selection: higher rates of in-migration crowd-in marginally lower human capital draws). Thus, surprisingly high levels of GDP (given the human capital stock) imply a high total factor productivity, and because all firms are assumed to compete in the same product market and face the same production function, there is a stable relationship between productivity and wages across states. Thus, variation in productivity combined with the available supply of human capital determines the wages.

C.3.6 General amenities: α^g_d

When governors in d and d' belong to the same party, politics will not affect workers' decisions about whether to migrate from o to d as opposed to o to d'. Considering equation (14) and taking the difference in migration between two same-governor destination states, we have

$$\left[\ln(\pi^g_{do,t}) - \ln(\pi^g_{d'o,t})\right] = \theta^g \ln(\alpha^g_d/\alpha^g_{d'}) + \theta^g \ln(w^g_{d,t}/w^g_{d'}) + \theta^g \ln\left(\eta^g_{do}/(1-\eta^g_{d'o})\right) \tag{24}$$

With θ^g, $w^g_{d,t}$, η^g_{do} at hand, we can identify relative amenities, relative to some reference state d' (discussed below). Essentially, state d's amenities are identified by higher migration into d and lower out-migration out of d than one would expect given wages and estimated migration costs.

C.3.7 Political amenities: γ^C_{Rep}, γ^N_{Dem}

To calculate our values of political amenities, we perform a grid search over different values for the pair γ^C_{Rep} and γ^N_{Dem} such that our model-implied effects of switching governors match our reduced form estimates. More specifically, our primary reduced form estimates are identified from eight states switching from a Democratic to a Republican governor between 2015 and 2017. For each value pair of values for γ^C_{Rep} and γ^N_{Dem}, we simulate two counterfactuals from our model. First, we simulate outcomes where all governors are held fixed as they were in 2014. Second, we simulate outcomes where all governors are held fixed as they were in 2017.

Let S denote the set of states that switched from a Democratic to a Republican governor between 2015 and 2017. The purpose of difference-in-difference is to identify the expected difference in potential outcomes between treated states under treatment and treated states in the absence of treatment: $E\big[Y_d(Rep) - Y_d(Dem)|d \in S\big]$, which can be rewritten as $E\big[Y_d(Rep)|d \in S\big] - E\big[Y_d(Dem)|d \in S\big]$. Given some choice of the parameters γ^C_{Rep} and γ^N_{Dem}, our two simulations allow us to recover these two quantities by averaging (over the eight treated states that identify our reduced form estimates) the change in log in-migration rates between the counterfactual where governors are as they were in 2014 – an estimate of $E\big[Y_d(Dem)|d \in S\big]$ – and where governors were as they were in 2017 – an estimate of $E\big[Y_d(Rep)|d \in S\big]$.

For each education group g and each choice of γ^C_{Rep} and γ^N_{Dem}, these two counterfactuals are used to generate one moment:

$$\varphi^g = \frac{1}{|S|}\sum_{d\in S}\left[\underbrace{\ln\left(\sum_{o=1,o\neq d}^{N} L^g_{o,t-1}\pi^g_{do,t}(p(d,t)')\right)}_{Y_d(Rep)} - \underbrace{\ln\left(\sum_{o=1,o\neq d}^{N} L^g_{o,t-1}\pi^g_{do,t}(p(d,t))\right)}_{Y_d(Dem)}\right] \tag{25}$$

According to our difference-in-difference analysis, when a Democratic governor is replaced by a Republican governor, the migration flow of college-educated workers decreases by 12.5%, while the migration flow of non-college-educated workers does not show a significant change. Therefore, we choose γ^C_{Rep} and γ^N_{Dem} to match the target of $\hat{\varphi^C}$ = -12.5% and $\hat{\varphi^N} = 0$.

C.4 Estimation

C.4.1 Adjusting migration flows

The matrix of migration flows is central to our estimation procedure. Using the American Community Survey (ACS), we calculate the matrix of cross-state flows, year-by-year, separately for college and non-college workers. However, the ACS is only a 1% sample and so flows are measured with error. This causes a problem when flows are low, in which case there is a chance that the 1% sample misses the people who moved from o to d, and we observe zero flows. This is an issue because many key moments derive from the *log* of migration flows. As is well-known, ignoring these missing flows can bias our estimates, and so we build on the approach suggested by Silva and Tenreyro (2006) and assume that the realized number of college graduates observed in the ACS migrating from o to d in year t is drawn from a Poisson process. We can then estimate the expected number of true flows by imposing a functional form assumption on how these flows relate to state characteristics.

Key to this approach is that we have administrative data made publicly available by the Internal Revenue Service (IRS) on the total number of tax filers who moved from origin o to destination d in year t. This data is not suitable for our main reduced form regressions because it includes no demographic information (such as education), but it can be used to infer flows for our structural estimates. To do so for each origin-destination pair in each year, we assume that the *ratio* of college graduate movers to IRS movers is a function of an origin-fixed effect, a destination-fixed effect, a time-fixed effect, the log distance between o and d, and the ratios of per capita GDP, group-specific wages, and college populations between the two states. This is a very flexible approach that assumes that we can describe very well the *relationship* between college migration and total tax-filer migration and then imposes no assumptions or structure on the realizations of tax-filer migration across pairs or over time. Under these assumptions, we can write the expected number of college migrants per tax-filing migrant as:

$$\frac{E\left[L^g_{dot}\right]}{IRS_{dot}} = exp\left(\theta_o + \eta_d + t + \beta_1 \log(distance_{do}) + \beta_2 \frac{GDPpc_d}{GDPpc_o} + \beta_3 \frac{Wage^g_{dt}}{Wage^g_{ot}} + \beta_4 \frac{L^g_{dt}}{L^g_{ot}} + \epsilon_{dot}\right) \quad (26)$$

where IRS_{dot} refers to observed migration in the IRS administrative data. Equation (26) can be estimated using a Poisson pseudo-maximum likelihood procedure (Correia, Guimarães, and Zylkin, 2019, 2020), and we can calculate the expected number of college graduate migrants $\tilde{L}^g_{dot}$ from the observed IRS migrants (where we do not know education).

It is important to note that our reduced form estimates are not based on the transformed migration data, but use only ACS estimates.

C.4.2 Estimation procedure

In this section, we describe the procedure for estimating the parameters $\left\{\theta^g, \mu^g_o, w^g_{d,t}, \eta^g_{do}, \alpha^g_d, \gamma^C_{\text{Rep}}, \gamma^N_{\text{Dem}}, A_{d,t}\right\}$. Unless otherwise specified, all calculations are based on the sample of ACS respondents from 2011-2019, age 26 or older, and employed in the private sector. We consider 2011 as the initial period $t = 0$. In all cases, o refers to state of residence last year, and d refers to current state of residence. In all calculations based on worker earnings, we restrict to full-time, full-year employees since ACS earnings are reported annually.

We provide a detailed explanation of estimating all the parameters in six steps. In all of these steps, we use the estimated migration flows based on the two adjustments to the raw ACS flows described in the previous section.

The first step involves estimating θ^g using equation (19) based on the dispersion of residual wages. We use workers' real earnings, which refers to the nominal wage obtained from ACS divided by the price deflators estimated by the Bureau of Economic Analysis at year and state levels. We calculate the wage residuals from a cross-sectional regression of log annual wage on age, age squared, gender, marital status, education, race and fixed effects for year, occupation, and industry.

Next, we estimate migration shock μ_o^g by taking the average of the migration outflows for each group-origin from 2012 to 2019, using equation (20).

The third step is to estimate the matrix of migration costs, η_{do}^g, using equation (21). This process relies on the previously estimated value of θ^g and the matrix of migration flows.

After estimating θ^g,μ_o^g and η_{do}^g, the following step is to recover A_d and w_d^g which are determined using the system of equations (22) and (23). For output $Y_{d,t}$, we adjust nominal state-level GDP using the year-level and state-level price deflators we used to adjust workers' nominal earnings.

To determine α_d, it is necessary to select a reference state that experienced a change in the governor's party between 2012 and 2019, as α_d can only be estimated if the governors of d and the reference state belong to the same party. We compile a list of states that have experienced such a switch and select as a reference state the state with the highest migration flows for college-educated and non-college workers: North Carolina. Given this, we can recover α_d using equation (24). For any state d, it is often the case that multiple years exist in which the same governor serves with the reference state. In such situations, we compute the average of $\hat{\alpha_{dt}}$ values to recover α_d.

The final step is to recover the last two parameters, γ_{Rep}^C and γ_{Dem}^N, which are chosen to match the reduced form results. We use a grid search method that allows each γ to take on values ranging from -0.5 to 0.5, with a grid size 0.001. We estimate γ_{Rep}^C and γ_{Dem}^N that produce the minimum sum of absolute errors when we solve for equation (25).