Political Preferences and Migration of College Educated Workers

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Abstract

We study the consequence of political polarization along educational lines in the United States. First, we show an increase in the gap between college and non-college voters' policy views. Today, the average college graduate is far to the left of the average non-college voter on both economic and social issues, and to a degree much larger than 10 years ago. Next, we estimate the causal effects of a Republican governor on college graduates' choice of where to live. Republican governance reduces the in-migration flow of college-educated workers by about 13% per year over the four post-election years. This result changes over time in ways that closely mirror changes in political preferences, is robust to various identification strategies, and cannot be explained by labor demand. Finally, we extend a model of spatial sorting to allow workers to hold preferences over *political* amenities, and we calibrate the model to match our reduced form migration responses. We use the model to simulate various counterfactual changes in political control, with a particular focus on the inter-linkages between different states and the distributional consequences of the effects.

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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 of the fastest growing sectors are skill-intensive, and college graduates generate economic growth that raises the wages of all local workers (Moretti, 2004). 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 this spatial sorting can exacerbate political polarization (Bishop, 2009; Brown and Enos, 2021). At the same time, it also 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 this paper, we show how these phenomena - sorting by education and sorting by political attitudes - 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 patterns 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.

We next estimate the migration effects of transitioning from a Democratic governor to a Republican one, paying special attention to how these effects have changed over time. As we discuss below, we focus on governors because they are very salient and have a large influence on policy outcomes. During the recent period, amid heightened polarization across education lines, we find that a Democrat-to-Republican transition leads to a large (13% per year) decline in the inflow of college graduates into the state.[1] Put differently, in the average state, around 3% of the college-educated workforce lived in a different state during the previous year. Our estimates imply a 0.4 percentage point decline in the migration flow, which over four years implies a 1.6% decline in the stock of college-educated workers. This magnitude is roughly equal to one year of growth in the average state's college-educated workforce.

We find the same effects from an IV strategy based on the differential timing of gubernatorial elections across states, as well as from a regression discontinuity design, although both are less precise and not statistically significant. We find no effects on non-college workers (who are far less averse to Republican governance), and the pattern of effects that we find on job openings and hiring rule out a labor demand explanation. Remarkably, the over-time pattern we find for migration effects almost perfectly mirrors the non-monotonic over-time pattern that we find for college/non-college gaps in views on social/cultural issues.

Overall, then, we conclude that our migration effects reflect labor supply responses as college graduates are increasingly reticent to live under Republican governance. This idea is not new.[2] As noted by Moretti and Wilson (2017), conservative states spend millions advertising low taxes in liberal states. As one 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 young professionals like crazy. How much more can our state government make Indiana uninviting" (Eason, 2015). Our paper shows that this councilmember was correct, and characterizes the size, dynamics, and consequences of this phenomenon.[3]

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[1] These estimates are identified by states that switch their governors. Thus, they reflect local average treatment effects. They say little about how politics shapes the choice to move to the large set of states never observed switching gubernatorial partisanship during our period, but they are the ideal estimates to inform our structural counterfactuals, where we consider flips among relatively evenly contested swing states like these.

[2] Since the Supreme Court's *Dobbs* decision gave states more authority over abortion laws, significant media coverage has emphasized the challenges facing employers trying to recruit educated workers to conservative states (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.

[3] Survey data also suggests this margin is realistic for many. In 2017, Smith, Hibbing, and Hibbing (2019) find that 23% of respondents report that politics had led them to consider moving.

To this end, we analyze a structural model of migration that builds on Bryan and Morten (2019). We extend the model in two ways. First, we add two types of workers – college and non-college – who differ in wages, migration costs, and valued amenities, and are combined in state-level production. Second, we add a "political amenity" which differs for college and non-college workers and 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 of various states.

We use the model because we are interested in three general equilibrium forces. First, what are the state-level effects of reducing college-educated labor supply? This not only reduces the state's total output, but it also raises wage inequality within the state, both by pushing up the relative wage of college graduates and pushing down the real wage of non-graduates. Second, we aim to understand how these effects differ across states. The aggregate effects depend on how many workers are "on the margin" between this state and another state, which depends on where the state is in the amenity and productivity distributions. Our model allows us to characterize the considerable variation in the expected effects of a gubernatorial flip. Finally, we are interested in spillovers across states. Deterred would-be migrants choose to live somewhere else. Our model lets us quantify spillovers from the implicit subsidy that red states offer to blue states by pushing college educated workers towards them.

We find moderate general equilibrium effects, with most GDP effects ranging from .5% to 1.5%, though larger in some cases. We show that most of the heterogeneity emerges from differences in non-wage amenities. States with higher amenities are more attractive places to live, and we show why this makes them more sensitive to switches in governor partisanship. Finally, the spillover effects of one state changing its governor on another state tend to be smaller than the direct effects on the switching state. In certain circumstances, however, spillovers on neighboring states can be as large, in proportional terms, as the direct effects (such as the effects of Texas becoming Democratic on Oklahoma).

At the broadest level, our results illustrate 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 actual material well-being or economic activity.[4] As US politics grows more hostile and divisive (Boxell et al., 2020), it is important to understand the consequences for non-political outcomes. Our study shows the aggregate, spatial, and distributional consequences of polarization along educational lines.

Our results also contribute to work on the role of education in political divides. Recently,

[4]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.

Gethin et al. (2022) combine data from 70 years and 21 countries to show that increased college graduates' support for left-of-center parties is a part of a universal and long-run trend. This suggests that the forces we focus on are likely to continue in relevance, 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.[5]

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 lays out our model, Section 5 our strategy to identify and estimate key parameters, and Section 6 our results. Section 7 concludes.

2 Education polarization

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

2.1 Data and methods

Our main data is 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). Relative to other political surveys, the key feature that is important for our purposes is that the CES intentionally asks a large number of policy questions about diverse topics, with an eye towards comparability and standardization over time.

Using these policy questions, we create indices of policy preferences on social and economic policies. We follow the topic lists developed by Caughey and Warshaw (2018) to classify questions as social/cultural or economic policies. From its inception, the CES has asked 15-50 different policy questions during each wave. In the appendix, we present details about these questions, as well as our classification into economic and social policy issues.[6] While the specific issues covered in the CES changes from year to year, these issues are always chosen to represent the main issues central to political debates. Thus, trends in our indices don't necessarily reflect changes in views on the exact same questions, but changes in views on the questions that are important to political debate at the time. We see this as an advantage,[7] although it

[5]For instance, Brox and Krieger (2021) show that far-right protests in Dresden, Germany, led to a decline in in-migration of college graduates there.

[6]As a few examples, social policy issues include gun control, abortion, immigration, and policing; economic policy 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, regulation of mortgage lending and teaching of intelligent design in high school science classes

does mean that we cannot distinguish between changes in specific views as opposed to changes in the specific issues debated within the broad classification of "social" and "economic" policy 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 expected to be 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 of generating indices (such as principal component analysis), it generates comparable scores across respondents even when the set of questions asked changes over time, so long as there exist some "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 all survey waves. IRT estimation does not require researchers to take a stance on the sign of individual questions, but researchers must still normalize the sign of the final index. We follow the common practice of normalizing the index so that more negative values indicate more liberal (left-leaning) views and more positive numbers denote more conservative (right-leaning) views. Our IRT indices are estimated 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 have reasonable confidence that observed education is equal to final education.[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 that we display are similar when we focus on 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" (or the gap in policy views between college and non-college respondents) over time. Panels (a) and (b) show the average views of college graduates and non-college citizens on social and economic policy questions. Recall that the indices have been normalized to be mean zero and unit standard deviation, across all respondents (of all ages) over time.

On social issues, both college graduates and non-college have drifted left considerably over

were major debates in 2008. Views on these questions would say little about a person's political ideology in 2023, while views on pandemic preparedness or police funding would say little about a person's political ideology in 2008.

[8]Note that the only educational attainment we focus on is a Bachelor's degree, and we never aim to separate those with graduate degrees from the Bachelor-only population.

time, consistent with widely recognized changes in views on issues like divorce, gay marriage, and marijuana legalization. But while college graduates have always had more liberal views than non-college citizens, this gap has changed over time. 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.[9] From 2015 to 2020, it more than doubled from $.2\sigma$ to roughly $.5\sigma$.[10]

[Figure 1 about here.]

Panel (b) shows a somewhat different pattern on economic policy issues. Here, there was historically 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 $.4\sigma$.

One important insight from Figure 1 is that while educational gaps in views on social issues and economic issues started at somewhat different *levels*, the trends in both have similar and today gaps on both views are equally large. Moreover, Panel (d) shows that, across individuals, the correlation between these views has increased from roughly .5 in 2010 to roughly .75 in 2020.[11] For this reason, when we estimate migration responses to Republican governors, we never attempt to separate between economically and socially conservative governors, which is useful because there is no existing dataset that would allow us to do so.

While differences in *average* views are helpful, they are necessarily a simplifying summary. In Appendix Figure B3, 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 0.2σ further to the left, the share of college voters even further left than the median Democrat rose by 5 percentage points (12%), while the share of non-college further to the left fell by 5pp (18%). As a result, college graduates went from being 50% more likely than non-college to fall left of the median Democrat to being 120% more likely.

[9] We see similar trends when we look at the gap in *median* (rather than mean) views in Appendix Figure B2, 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 journalists (Yglesias, 2019) and scholars (Haidt and Lukianoff, 2021) have argued that changes in college-educated political views and campus political climate pre-dated Trump's campaign.

[11] This trend is seen for both college and non-college individuals. Excluding climate change from the economic policy questions lowers the level of the correlation by roughly .02, but does not change the trend.

2.3 Differences in policy preferences by earnings

In general, states care about attracting and building a high-skilled, high-productivity workforce, and education is correlated with measures of productivity like earnings. However, it is possible that the college/non-college gaps we see are driven by some subset of well-educated workers who are very left-leaning but have relatively low earnings. In this case, a conservative policymaker might be unconcerned about these workers away, and reasonable minds can disagree about the costs to economic growth. If, on the other hand, the gap in policy views broadly holds when comparing high-earners with low-earners, then virtually all policymakers would be concerned about the growth implications of taxing 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 wages on geography-by-time fixed effects and age-by-education-by-industry fixed effects:

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

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

[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.

[14] One advantage 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).

Figure B5 shows the basic patterns of earnings polarization. In Panel (a), we plot the coefficients on our earnings measure from regressing social and economic policy views on earnings (again for workers age 26-45). Earnings had little correlation with policy views through 2014, though in 2011 higher earners were slightly more conservative on economic policy issues. But since then, higher earners have become substantially more left-leaning, on both economic and social issues. By 2020, a 1σ shift in earnings capacity is associated with a $.12\sigma$ shift in policy views (on both sets of issues).

This relationship is entirely driven by education. Panel (b) presents coefficients that come from controlling for 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 (for age 26-45). 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 more liberal than all types of non-college workers, on both economic and social policy issues. The figure shows that between-education-group differences dramatically swamp any type of within-education-group differences 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.

2.4 Explaining education polarization

Our goal in this paper is not to explain the rise of education polarization. Nonetheless, some features of our data do speak to some potential explanations. In Appendix Section B.2.1 we provide suggestive evidence on three prominent explanations for the growth in the gaps between college and non-college voters.

[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 likely that within these cells, higher earnings college graduates are even further to the left of their peers, although we cannot test this in our data.

First, we show that some of the change is driven by a growth in the gaps between successive cohorts, which is consistent either with universities having stronger "treatment effects" over time, or with changes in selection into university attendance over time. However, these between-cohort effects are much smaller than the within-cohort over time changes, so these forces are not important for explaining the aggregate patterns. Second, one prominent explanation is that political debate has shifted from economic issues to social issues (Gethin et al., 2022). We show that views on social issues have become more important (and those on economic issues less important) for explaining partisan preferences. However, these effects are also fairly small, and above we showed that gaps in views themselves (and not simply the priority given to different types of views) have changed considerably. Third, we find some evidence that the states which saw the greatest growth in college share of the workforce also saw the largest change in education polarization, which is consistent with the idea that increased sorting of like-minded individuals reinforces polarization (Bishop, 2009), but this effect is also fairly weak.

Overall, then, we find some modest support for three common claims, although none offers a complete explanation. Indeed, we do not believe there *is* a singular explanation for the rise in education polarization. For this reason, we do not seek to model endogenous polarization.

2.5 Strength of policy preferences

We have shown large growing differences in the views of college and non-college voters. We have, thus far, said nothing about the *strength* of those views. It is possible that different groups of voters hold different views, but that these views are only weakly held and unlikely to shape actual behavior. While it is more difficult to quantitatively assess evidence on the strength of preferences, here we summarize a range of evidence illustrating that many US citizens, and particularly college-educated liberals in recent years, care deeply about politics.

First, Americans devote substantial time to politics. Standard time use datasets do not collect information on political activity or media consumption, but Hersh (2020) fielded a nationally representative survey in 2018 which found that one-third of Americans averaged more than two hours *per day* following politics. College-educated respondents and Democrats were both more likely to fall into this group. In a 2017 nationally representative survey, Smith, Hibbing, and Hibbing (2019) find that 26% of Americans spend more time thinking about politics than they would like, and 17% report their lives would be better if they focused on politics less.

Second, Americans spend substantial money on politics. In the 2020 CES, among college graduates aged 26-45, 29% report contributing to a political campaign, compared to 9% of non-college respondents.[16] One-quarter of these contributors spent more than $300. A classic perspective in scholarship on campaign finance is that individual contributions are "consumption" rather than any sort of investment (Ansolabehere, de Figueiredo, and Snyder, 2003). This appears even more true during the recent period. Candidates routinely raise millions of dollars – largely from out-of-state donors who cannot vote in the election – by challenging influential conservative senators, only to lose by large margins.[17]

Finally, Americans report that politics is having meaningful effects on their mental health. The 2017 Smith et al. (2019) survey found that politics has led 38% of respondents to experience stress, 18% to lose sleep, 20% to damage valuable friendships, 12% to experience adverse effects to physical health, and 4% to consider suicide. The battery of such health problems is more severe among younger, better educated, more left-leaning, and more politically engaged respondents. These are self-reported associations between politics and mental health, but experimental evidence shows that exposing participants to mainstream political news (the type frequently consumed by the Hersh (2020) respondents) causally increases stress and reduces psychological well-being (Ford et al., 2023). Accordingly, recent editions of the American Psychological Association's annual "Stress in America" report have focused extensively on politics.

Overall, politics appears to be a dominant force in the lives and mental health of many college graduates, and increasingly so. Next, we turn to whether these individuals respond to state politics in deciding where to move.

3 Migration responses

Our main interest is in how college educated workers respond to changes in the partisan control of state government, and how this response has changed over time. For this reason, we focus on the partisanship of the governor. It has long been recognized that citizens tend to know more about their governor than other elected officials, and that 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 between 45% and 60% for their Senators or House Representatives.

Moreover, governors have significant effects on state policy outcomes, and the size of these

[16] Bouton et al. (2022) estimate there were roughly 20 million independent individuals making federal campaign contributions in 2020 (roughly 10% of the adult population), with 8 million contributing more than $200.

[17] In 2018, Beto O'Rourke raised $80 million challenging Senator Ted Cruz in Texas and lost by 2.5 points. In 2020, Jamie Harrison raised $132 million challenging Senator Lindsey Graham in South Carolina and lost by 10.2 points. In 2020, Amy McGrath raised $96 million challenging Senator Mitch McConnell in Kentucky and lost by 19.6 points.

effects has grown over time (Caughey, Xu, and Warshaw, 2017).[18] In part, this is because governors face very few checks and balances or other constraints on authority and the prevalence of these checks has declined over time (Seifter, 2017), and in part it is because they are typically the de facto leaders of their political party within their state, giving them responsibility for setting agendas and driving legislation. Quantifying their relevance for policy, we use the difference-in-difference strategy discussed below and updated data from Caughey and Warshaw (2016) – who create policy indices based on 148 separate state-level policies – and we find that a Republican governor leads states' economic policies and social policies to become .2-.3σ more conservative (see Appendix Figure B6). At the same time, given the salience of governors and their high-profile stances on controversial issues, we do not expect effects to operate *solely* through policy. We make no attempt to separate between governors' policy effects and other high-salience political theatrics.

3.1 Data and methods

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, and we mostly focus on those holding at least a Bachelor's degree. 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.[19] 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 education. We define migration inflows into state s in year y as the number of respondents living in state s in year y who report living in a different state in year $y-1$. 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 or temporary. Thus, we always end our migration analysis window in 2019. Finally, our primary analyses focus on respondents who are employed in the

[18] In an influential paper on *mayors*, Ferreira and Gyourko (2009) use a regression discontinuity to show that partisanship does not matter for policy outcomes. Using updated and expanded data, de Benedictis-Kessner and Warshaw (2016) show that it does. Moreover, the same regression discontinuity design shows that *governor* partisanship is consequential for policy (Caughey et al., 2017).

[19] 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, and voter registration data only includes partisanship for around half the country.

private sector (and thus most relevant for economic activity), citizens (and thus more engaged in politics), and aged 26-45 (where inter-state migration is concentrated). We also present estimates using other samples, and the figure we use for calibrating our structural model is based on all private sector employees age 26 or older.

With these annual migration flows, we estimate the effects of gubernatorial transitions on migration. Our primary analyses use a difference-in-difference specification, although we show similar point estimates and time trends using a regression discontinuity specification and an instrumental variables specification where we instrument for outcomes using variation across states in the timing of their gubernatorial election cycle. Since gubernatorial terms in 48 of the 50 states are four years long, we focus on four-year treatment effect windows.

Switches in governor partisanship (what we call "flips") occur in different places at different times and are thus a form of "staggered rollout" problem that has received attention in the recent econometrics literature. We use a Callaway and Sant'Anna (2021) estimator, in which each state experiencing a flip is paired with other states sharing the same pre-flip partisanship, but which did not experience a flip during the five years before and after the treated state's flip. For example, in 2012 North Carolina elected Republican Governor Pat McCrory to replace the outgoing Democratic governor. McCrory took office in 2013. At the time, there were six other states which had Democratic governors in office during the full ten years surrounding this election (i.e., 2008-2017). We estimate the effects of McCrory taking office on migration into North Carolina by comparing the post-McCrory change in migration inflows into North Carolina with the simultaneous changes experienced in these six other states.

As Callaway and Sant'Anna (2021) show, this yields a consistent estimate of effects of North Carolina's 2013 flip, avoiding the documented concerns about negative weights in staggered rollout designs (e.g., De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021)). Adding other states experiencing flips during the same year yields a consistent estimate of the average treatment effect for all of those states, which Callaway and Sant'Anna call a "treatment cohort." Weighting the control states for each cohort so that their collective weight is proportional to the number of treated states in the cohort and then combining multiple treatment cohorts into the same regression yields the average treatment effect (ATE) of a given type of flip. Our main approach is to use this pooled estimator, combining different sets of treatment cohorts to study dynamic effects. Unless otherwise noted, we always cluster standard errors at the state level to account for serial correlation in migration and the fact that the same state-year can appear as controls for different treatment cohorts. Finally, we always weight state-year observations by the number of ACS respondents.

When we aim to summarize a single number, we regress log migration flows on a standard post-treatment dummy. That is, we use a standard two-way fixed effects estimate,[20] but after using the Callaway-Sant'Anna approach to ensure that we are only comparing treated states to untreated states. In general, however, we prefer to compare the levels of our outcome variables between treated states and control states. We normalize each state's level by dividing by its average level during the four pre-election years, and then plot normalized outcomes across event time. This allows a more transparent inspection of the dynamics of migration around the election.

Figure 3 shows the number of flips over time, separately for Republican-to-Democrat and Democrat-to-Republican transitions. One notable fact that shapes our analysis is that during recent years, D-to-R transitions have been much more common than R-to-D transitions. For instance, since 2015 (when we observe the dramatic increase in education polarization) there have been eight transitions from Democratic to Republican governors, and only three transitions the other way around. Thus, for recent years, we are naturally better powered to study D-to-R transitions than R-to-D transitions.

[Figure 3 about here.]

3.2 Main results

We begin by focusing on the effects of a transition from a Democratic to a Republican governor during the recent period where the increase in education polarization has been concentrated. Specifically, we consider flips where the new Republican governor took office in 2015-2017 (since we see migration data as unreliable from 2020 on).[21] Figure 4 shows that college graduates' migration into the state falls considerably upon the election of a Republican governor. Prior to the election, migration patterns were extremely similar between the eight treated "flip states" and the 11 states that remained Democrat-run. After the election, however, in-migration falls by nearly 20% in those states relative to the control states.

[Figure 4 about here.]

Interestingly, when we examine the source of migration, we find that the effects are completely driven by in-migration from Republican-led states. These results are shown in Figure 5. Leading up to the election, both treatment states and control states saw in-migration flows from these states increase by about 25%. When treated states elect a Republican governor,

[20] $\ln(Mig)_{s,t} = \alpha_s + \delta_t + \beta\big(Flip_s^{DR} \times Post_t\big) + \varepsilon_{s,t}$

[21] Technically, this is an unbalanced panel. As Figure 3 shows, this sample includes three flips in 2017 where, because we do not use mid-pandemic 2020 migration rates, we observe only three (2017, 2018, 2019) and not four post-election years.

this growth halts almost completely, while control states see it continue to rise by another 25% of the next four years. In the appendix, we present various additional results where we find no responses among non-college workers, no response of out-migration to gubernatorial flips, and no effects of Republican-to-Democrat flips.[22]

[Figure 5 about here.]

It is not unreasonable for a reader to be concerned about adverse economic shocks biasing our estimates. After all, economic shocks have effects on both election outcomes and migration. Below, we present a series of robustness checks to rule out various labor demand explanations for our results. Here, we simply note that *i*) 2015-2019 (our post-election window) was a period of broad economic growth around the country, shared by all regions and all demographic groups and *ii*) given the immediate timing of college graduates' response, these economic shocks would have had to precisely coincide with the election. Below, we present more systematic evidence.

First, however, we focus on changes in migration responses over time. Above, we showed that the size of the gap in political views between college and non-college has changed in important ways over the last 20 years. Here, we investigate whether migration behavior shows the same dynamics. To do so, we split our 15-year sample into five three-year periods: flips occurring 2003-2005, 2006-2008, 2009-2011, 2012-2014, and 2015-2017. For each period, we estimate the effects of a D-to-R flip on in-migration of college-educated workers. These period-specific estimates are plotted in Figure 6 along with the average level of education polarization along social issues during the post-election years.[23] The relationship is striking. In the mid-aughts, when the college/non-college gap in views was large, college graduates' migration responded strongly to a Republican's election as governor. During the middle three periods, when the college/non-college gap was relatively small and stable, migration did not respond to these elections. During the recent period, when education polarization has risen again, migration has again become quite responsive.[24]

[22] See Figure B8. It is difficult to say whether our null effects for non-college workers are because they care less about politics or because base rates of migration are much lower. It is also difficult to interpret our null findings on R-to-D transitions since we have far fewer R-to-D transitions than D-to-R transitions during this period (see Figure 3). We interpret our null effects on out-migration as evidence that elections do not affect individuals' decision about *whether* to move, only *where* to move, and note that Monras (2020) shows that economic shocks, in general, only affect in-migration and not out-migration.

[23] Our calculation of average education polarization accounts for the fact that years have different numbers of treated states. For instance, in the 2009-2011 period, there were 11 times as many DR transitions in 2011 (post-treatment years: 2011-2014) as in 2009 (post-treatment years: 2009-2012). Thus, in calculating average polarization during the period, we give 11 times as much weight to the level in 2011-2014 as the level in 2009-2012.

[24] Nonetheless, it is important to acknowledge that the labor market was healthier in the mid-aughts and post-2015 period than at any point from 2007-2014. Thus, a potential alternative explanation for our *heterogeneous* effects is that migration is only responsive to politics when the labor market is strong enough to afford workers

[Figure 6 about here.]

The above estimates are based on private sector employed citizens age 26-45. However, these workers are not the full set of a state's college-educated workforce, and for our structural model it is the total change in college-educated labor that is important. Thus, Table 1 estimates our main difference-in-difference specification for less restrictive samples of college-educated workers. In our calibration, we match our model-generated migration behavior to that of all private sector employees aged 26 and above (column 2), where we estimate a Republican governor reduces annual migration flows by 14 log points (13%). In the appendix, we show corresponding plots for our estimated effects for these alternative samples of workers (Figure B10) as well as for the full sample of years (Figure B9).

[Table 1 about here.]

How large are these effects? For the average state, roughly 3% of college-educated private sector workers have moved across state lines in the last year. Thus, the 13% decrease in annual flows that we estimate corresponds to a roughly .4 percentage point decline per year, which over four years accumulates to a 1.6% decline in the stock of college educated workers. This is roughly equal to the annual growth that the average state sees in its college educated workforce (see Appendix Figure B4), implying that four years under a Republican governor sets back the state's human capital accumulation by about one year.

3.3 Alternative identification strategies

It is not unreasonable for a reader to worry about reverse causality. We have shown that college-educated workers would generally prefer liberal candidates. Thus, any decrease in the stock of college educated workers should, all else equal, decrease Democratic candidates' vote shares and increase the probability of a Democrat-to-Republican gubernatorial transition. In principle, one would expect this to be reflected in the pre-election migration trends – which we do not see – but it is nonetheless a legitimate concern.

In Appendix Section B.2.3, we propose two alternative identification strategies. The first is based on an instrumental variables strategy based on pre-determined variation in the timing at which different states hold their gubernatorial elections. Nearly all states give governors four-year terms, but some states hold these elections during presidential years (e.g., 2008, 2012) while others hold them during off-presidential years (e.g., 2010, 2014). This means that

decent employment options. We stress that this is a possible alternative explanation for the treatment effect *heterogeneity* that we document, but cannot explain the main treatment effect that we estimate.

some governors were elected during 2008 (a record year for Democrats, according to Congressional election returns, in part because of the Obama's campaign's success), while others were elected during 2010 (a record year for Republicans who came to power on the backs of the right-wing Te4a Party wave). Our instrument shows that these national swings in partisan sentiment translate into large effects on gubernatorial election outcomes for governors who are lucky/unlucky enough to be running during these years. Our other identification strategy is a standard regression discontinuity design (RDD) based on close gubernatorial elections.

In both cases, we estimate substantially similar effects of a Democrat-to-Republican transition on in-migration of college educated workers. These effects are similar not only in their levels, but also in their trends over time. However, none of these estimates are statistically significant because they only use a relatively small share of the data. Thus, the standard errors are much larger than our preferred difference-in-difference estimates. We interpret this as evidence that our difference-in-difference approach does not suffer from biases that could potentially arise from the fact that election outcomes are endogenous with respect to demographic changes.

To us, this is conclusion is unsurprising. While it is clearly true that a change in the composition of the electoral will affect vote shares and election outcomes, electoral politics is an inherently noisy and chaotic process, and changes in the composition of the electorate are unlikely to be the main determinant of outcomes.[25] Moreover, while economic conditions have long been seen as a determinant of voters' support for the incumbent, Jones (2020) shows that the long-run increase in partisanship has dramatically weakened this relationship as Democrats and Republicans now hold radically different subjective perceptions of economic conditions (including their own economic circumstances).

Considering some recent transitions within our sample illustrates the importance of "random" shocks in modern electoral politics. In 2016, Missouri elected a Republican to replace term-limited Democrat Jay Nixon. Many argued at the time that Democrats performed very poorly in the election because of the 2014 murder of Michael Brown at the hands of police in Ferguson, and mistakes made by party leaders (including Nixon and the Democrat's 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. 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 members were facing obstruction of justice charges (Catanese, 2012) and the NC Democratic Party's senior staff was embroiled in sexual harassment proceedings

[25] A large literature in political science shows that voters' choices are significantly influenced by truly random events like college football matches (Healy, Malhotra, and Mo, 2010) and shark attacks (Achen and Bartels, 2017), though this literature is controversial (Fowler and Hall, 2018; Fowler and Montagnes, 2015).

(Bass, 2012). In 2015, Louisiana elected Democrat John Bel Edwards to replace term-limited Republican Bobby Jindahl. Many argued that Edward's defeat of Republican David Vitter was because of Vitter's earlier prostitution scandal (McAfee, 2015), which led key Republicans to either endorse Edwards or no one (Barnes, 2015).

Importantly, many of these races were not close, though they appear to be driven by events exogenous to migration incentives. Put differently, the set of election outcomes useful for identification is larger than only the close elections used by an RDD strategy. For this reason, in our model, we do not attempt to endogenize political outcomes, although we acknowledge that our model is fundamentally about endogenous shifts in the composition of the electorate.

Finally, it is worth noting that we are not the first to conclude that difference-in-difference estimates of gubernatorial transitions do well at replicating RDD estimates. In their study of the effects of governor partisanship on policy outcomes, Caughey, Xu, and Warshaw (2017) also find that difference-in-difference estimates are similar to the RDD estimates, both in levels and in over-time heterogeneity.

3.4 Alternative explanations

We have now established that college-educated workers' migration decline after a D-to-R gubernatorial transition is the causal effect of the gubernatorial election. This does not automatically imply, however, that it reflects those workers' political preferences, although we have shown that those workers do have strong preferences and that the over-time pattern of those preferences matches the over-time pattern of migration responses.[26] Nevertheless, it is important to consider other potential reasons why a Republican governor might reduce in-migration of college graduates. In the appendix, we consider two possibilities: *i*) effects on economic activities and the incentives to migrate and *ii*) effects on perceptions of citizens' political views.

First, rather than reducing labor supply of college graduates who disprefer conservative governance, Republican governors could reduce demand for college graduates via spending cuts that induce an economic contraction and reduce job opportunities.[27] In Appendix Figure B21, we test for effects on job openings and hiring using the Bureau of Labor Statistics' Job Openings and Labor Turnover Survey (JOLTS). We find no effects, suggesting that labor demand cannot explain our migration findings.

[26]As an additional test of our political preferences explanation, in Appendix Section B.2.2 we compare different types of majors depending on the average political lean of college seniors. We find stronger migration responses among graduates holder further left-leaning majors.

[27]We see this explanation as, *ex ante*, unlikely. First, economic contractions tend to be disproportionately experienced by non-college workers rather than college graduates (Patterson, 2020). Second, Republican governors tend to lead to more pro-growth business friendly policies (see Caughey et al. (2017) or Figure B6).

Second, Republican governors might reduce in-migration of college graduates not because those graduates are averse to conservative *policies*, but because the election signals the prevalence of conservative *voters*, and college graduates do not want to live near these conservative voters. This matters for policy because it suggests that the actual decisions of Republican governors about what to do once in office have no bearing.[28] To test for this, we test whether the six states that switched from voting for Barack Obama in 2012 to Donald Trump in 2016 saw decreased college in-migration, relative to the 20 Obama-Clinton states. Because all of these states had the same actual president, this switch does not affect policy, but likely does affect voters' perceptions of the states' residents. We find no migration effects of these placebo switches. We conclude that our estimated effects reflect labor supply responses and we model them accordingly.

4 Model setup

In this section, we build a static general equilibrium model of migration based on Bryan and Morten (2019).[29] College and non-college workers have different preferences over political amenities, in addition to the other amenities more standard in the migration literature.

There are N locations in the economy. Workers live in origin o initially and sort across destinations (denoted d) based on wages, amenities, migration costs, and partisanship of governors, as well as an idiosyncratic skill draw for each destination.

4.1 Utility

Let the utility of worker i from type g and origin o who chooses destination d be:

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

where c_i is her consumption and α is the amenity value of living in destination d for group g. We consider amenities to be fixed across individuals with the same education, but we allow all parameters to vary across education groups, denoted by $g = C$ for college-graduates and $g = N$ for non-college. Let τ^g_{do} be the moving cost of migrating from o to d. We assume there are no

[28]Although stated choice experiments over neighborhoods do show that voters prefer living near ideologically similar neighbors (Mummolo and Nall, 2017), these effects are fairly small, and it is important to note that all cities (even in conservative states) have liberal neighborhoods that residents can choose.

[29]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.

costs of not migrating (i.e., $\tau_{oo} = 0$) and that migration costs are symmetric (i.e., $\tau_{do} = \tau_{od}$).

The term $\gamma^g_{p(d)}$ captures a group-specific preference wedge emerging from group g's disutility from living under a governor of partisanship $p(d) \in \{\text{Republican}, \text{Democrat}\}$. When this wedge is positive, workers must be compensated with $1/(1 - \gamma^g_{p(d)})$ 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.[30]

Following Bryan and Morten (2019) and Hsieh et al. (2019), each group of worker gets a skill draw s^g_{id} from a multivariate Fréchet distribution:

$$F(s^g_1, s^g_2, ..., s^g_N) = exp\left(-\left[\sum_{d=1}^{N} s_d^{-\theta^g}\right]\right)$$

where each worker has a different draw for each possible working location d. This skill draw can be thought of as the worker's match-specific human capital for her employment opportunity in each state. The distribution of s^g_{id} 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 skills across locations. This parameter is key because it determines how close to indifferent workers are between the employment opportunities available in different states.

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

$$V^g_{ido} = \alpha^g_d(1 - \tau^g_{do})(1 - \gamma^g_{p(d)})w^g_d s^g_{id} \tag{2}$$

where w^g_d is the human capital price in d for group g. That is, if $w^g_d > w^g_{d'}$ 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: $s^g_{id} = s^g_{id'}$) will have higher earnings and consumption in d since it is a higher wage state.

It is convenient to define the overall returns of destination d for a worker of group g and

[30] 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 estimate in the reduced form section would have to be generated entirely by the χ share of liberal college graduates. This would require a larger value of the γ preference 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 not the GDP implications, which is the focus of our analysis.

origin o as $\tilde{w_{do}^g} = \alpha_d^g(1-\tau_{do}^g)(1-\gamma_{p(d)}^g)w_d^g$ so that $V_{ido}^g = \tilde{w_{do}^g} s_{id}^g$.

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 s_{id}^g. 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 can be written as:

$$\pi_{do}^g \equiv \frac{L_{do}^g}{L_o^g} = \frac{\tilde{w_{do}^g}^{\theta^g}}{\sum_{j=1}^N \tilde{w_{jo}^g}^{\theta^g}} \tag{3}$$

where L_o^g denotes the number of group-g workers from origin o, and L_{do}^g denotes the number who move to d. These migration flows are driven by relative returns, including non-wage returns in terms of amenities or migration costs.

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:

$$\mathbb{E}\left(s_d^g \mid \text{choose } d \text{ from } o\right) = \bar{\Gamma^g}\pi_{do}^{-\frac{1}{\theta^g}} \tag{4}$$

where $\Gamma(.)$ is the gamma function and $\bar{\Gamma^g} = \Gamma(\frac{\theta^g-1}{\theta^g})$. This property is important, as it allows us to infer unobserved human capital from observed migration rates.

4.2 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., skills drawn) by two groups using a Constant Elasticity of Substitution (CES) production function.

Total output in state d is given by:

$$Y_d = A_d\left[\left(H_d^C\right)^{\frac{\sigma-1}{\sigma}} + \left(H_d^N\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}} \tag{5}$$

where A_d 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_{jd}^g is the total efficiency units of labor employed by firm j, which is simply the sum of skill draws for workers working at firm j:

$$H_{jd}^g = \sum_{i\in P_j} s_{id}^g$$

where P_j denotes 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 w^g_{jd}. We assume that the labor market is perfectly competitive, and so 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.

4.3 General equilibrium

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^g_d clears labor market for each destination

5 Identification and estimation

In this section, we describe the procedure for estimating the parameters $\left\{\theta^g, w^g_d, \tau^g_{do}, \alpha^g_d, \gamma^C_{\text{Rep}}, \gamma^N_{\text{Dem}}, A_d\right\}$. 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. 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.

5.1 Identification

5.1.1 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. 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).[31]

5.1.2 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. As noted above in equation (4), 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 cross-state differences in the returns to human capital (w_d^g) and selection. 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.

Thus, a non-linear function of θ can be expressed as a non-linear function of the distribution of residual wages:

$$\frac{\text{Variance}(\tilde{s}_i^g)}{(\text{ Mean}(\tilde{s}_i^g))^2} = \frac{\Gamma\left(1 - \frac{2}{\theta^g}\right)}{\left(\Gamma\left(1 - \frac{1}{\theta^g}\right)\right)^2} - 1 \tag{6}$$

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

5.1.3 Moving cost: τ_{do}^g

As Bryan and Morten (2019) point out, moving costs are identified because they depress migration flows between two states *symmetrically*, while differences in wages or amenities would only depress flows in one direction. We take the log of equation (3):

$$\ln(\pi_{do}^g) = \theta^g \ln(\alpha_d^g) + \theta^g \ln(w_d^g) + \theta^g \ln(1 - \gamma_{p(d)}^g) + \theta^g \ln(1 - \tau_{do}^g) - \ln\left(\sum_{j=1}^{N} \tilde{w_{jo}^g}^{\theta^g}\right) \tag{7}$$

In this expression, most parameters are destination-specific, and so they cancel out when

[31] These results mirror historical discussions in labor economics about the wage inequality implications of changes in the aggregate supply of college graduates. See Acemoglu and Autor (2011) for some discussion, who calibrate a value of $\sigma = 2.9$ for the 1964-2008 period.

we compare migration from two different origins into the same destination. This includes when considering "migration" from origin d to destination d (i.e., the probability of staying in d), which is useful because d-to-d migration costs do not appear because of our assumption that there are no costs to *not* migrating (i.e., $\tau_{dd} = 0$). Thus, the difference between rates of o-to-d migration and rates of staying in d can be written as the migration costs and the difference in the logged sum of o-specific and d-specific returns to migration:

$$\ln \pi^g_{do} - \ln \pi^g_{dd} = \theta^g \ln(1 - \tau^g_{do}) - \ln \left(\sum_{j=1}^{N} \tilde{w^g_{jo}}^{\theta^g} \right) + \ln \left(\sum_{j=1}^{N} \tilde{w^g_{jd}}^{\theta^g} \right)$$

The same derivation can be done to calculate $\ln \pi_{od} - \ln \pi_{oo}$ (migration from d to o relative to the probability of staying in o), and then summing these terms, the differences in the sum of returns cancel out, leaving only:

$$[\ln \pi^g_{do} - \ln \pi^g_{dd}] + [\ln \pi^g_{od} - \ln \pi^g_{oo}] = \theta^g \ln(1 - \tau^g_{do}) + \theta^g \ln(1 - \tau^g_{od}) \tag{8}$$

Because we assume that migration costs are symmetric ($\tau_{od} = \tau_{do}$), this reduces to one equation and one unknown (conditional on the estimate of θ^g from above) per origin-destination pair. In this way, the matrix of observed migration flows and non-migration decisions identifies all migration costs. Because it is identified only by the *symmetry* of flows, relative differences in attractiveness between the states are not relevant, and identification does not depend on who is governor of which state.

5.1.4 Productivity and wages: A_d, w^g_d

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:

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

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 function as a function of wages and total output. Human capital supplied is simply the product of the equilibrium skill conditional on migration from o to d (derived from the Frechet above in equation (4)) and the number of workers migrating from o to d ($L^g_o \pi^g_{do}$), summed over all origins.

$$H^g_d(demand) = \left(\frac{A_d^{\frac{\sigma-1}{\sigma}}}{w^g_d} \right)^{\sigma} Y_d = \sum_{o=1}^{N} L^g_o \pi^g_{do} \bar{\Gamma}^g \pi_{do}^{-\frac{1}{\theta^g}} = H^g_d(supply) \tag{10}$$

Given the above estimate of θ, and observable output and migration, this delivers three equations (since equation (10) must hold for $g \in \{C, N\}$) and three unknowns (A_d, w_d^C, w_d^N) for each state.

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.

5.1.5 General amenities: α_d^g

When governors in d and o belong to the same party, politics will not affect workers' migration decisions. Considering equation (3) and taking the difference in migration between two same-governor destination states, we have

$$[\ln(\pi_{do}^g) - \ln(\pi_{d'o}^g)] = \theta^g \ln(\alpha_d^g/\alpha_{d'}^g) + \theta^g \ln(w_d^g/w_{d'}^g) + \theta^g \ln(1 - \tau_{do}^g/1 - \tau_{d'o}^g) \tag{11}$$

With θ^g, w_d^g, τ_{do}^g 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 (i.e., than one would expect given observed GDP per worker) and estimated migration costs.

5.1.6 Political amenities: $\gamma_{\mathbf{Rep}}^C$, $\gamma_{\mathbf{Dem}}^N$

To calculate our values of political amenities, we perform a grid search over different values for the pair γ_{Rep}^C and γ_{Dem}^N 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_{Rep}^C and γ_{Dem}^N, 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 – approximating $E\big[Y_d(Dem)|d \in S\big]$ – and where governors were as they were in 2017 – approximating $E\big[Y_d(Rep)|d \in S\big]$.

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:

$$\varphi^g = \frac{1}{|S|}\sum_{d \in S}\left[\underbrace{\ln\left(\sum_{o=1, o\neq d}^{N} L^g_o \pi^g_{do}(p(d)')\right)}_{Y_d(Rep)} - \underbrace{\ln\left(\sum_{o=1, o\neq d}^{N} L^g_o \pi^g_{do}(p(d))\right)}_{Y_d(Dem)}\right] \tag{12}$$

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 12.8%, 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 $\hat{\varphi^C}$ = -12.8% and $\hat{\varphi^N} = 0$.

5.2 Estimation

We provide a detailed explanation of estimating all the parameters in three steps. The first step involves estimating θ^g using equation (6) 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 Bureau Economic Analysis at year and state levels. Next, we use the estimated value of θ^g and calculate (8) based on the average migration flows, which determines the migration cost matrix. To capture the migration flow over four-year governor term, we sum the annual migration flow in ACS for each origin-destination pair from 2011 and 2019 and then weighted this sum by 4/9. By construction, $\pi_{dd} = 1 - \sum_{o \neq d} \pi_{do}$.

In cases where there was no migration from 2011 to 2019 for a particular origin-destination pair, we assign τ^g_{do} a value of 0.999. After estimating θ^g and τ^g_{do}, the following step is to recover A_d and w^g_d which are determined using the system of equations (9) and (10). For output Y_d, we first adjusted nominal GDP by Bureau Economic Analysis at year and state levels in the same way as adjusting workers' nominal earnings. We then calculate the average of real GDP from 2011 to 2019 at the state level to measure Y_d.

To determine α_d, it is necessary to select a reference state that experienced a change in governor party between 2011 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 both college-educated and non-college workers: North Carolina. Given this, we can recover α_d using equation (11).

The final step is to recover the last two parameters, γ^C_{Rep} and γ^N_{Dem} using 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 of 0.01. We select the values of γ^C_{Rep} and γ^N_{Dem} that produce the minimum sum of absolute errors when we solve for equation (12).

We present the estimation results in Table 2.[32] Our estimate of $\gamma^C_{d(Rep)}$ (college workers' preference against living under a Republican governor) is 0.06, which can be compared with both non-political amenities and migration costs, since all enter the indirect utility function in equation (2) together. Our estimate is only about 5% of the cross-state standard deviation in non-political amenities. For the average state, the standard deviation in migration costs is approximately .037 (roughly 60% of our estimated $\gamma^C_{d(Rep)}$) and the migration costs across different states range from approximately .86 to 1, so the range is roughly 2.5 times as large as $\gamma^C_{d(Rep)}$. Put differently, $\gamma^C_{d(Rep)}$ is not large. However, as we will show, it can generate meaningful effects on the college population and state-level output, in part because of the ways that it interacts with these other parameters.

[Table 2 about here.]

It is worth noting that we estimate a positive but small disutility for non-college workers living under a Democratic governor. Our estimate is only one-sixth as large as the college-educated workers' political wedge, but it is not zero. Above, we estimate no statistically or economically significant effects of governor's party on migration of these workers. As we show below, however, college-educated workers sorting away from Republican-governed states reduces output and wages there, which increases non-college workers' incentives to avoid those states, too. Thus, our model can only reconcile zero migration responses for non-college workers by concluding that non-college workers do have some preference for living under Republican governors, consistent with our descriptive facts about political opinions above.

6 Model-implied effects of governors

In this section, we use counterfactuals generated by our model to understand three general equilibrium effects of gubernatorial transitions which would be difficult or impossible to explore

[32]For brevity, we do not present extensive validations of our estimated parameters. Our estimates of γ^C and γ^N are global optima, our estimated migration costs are positively correlated with the distance between states (.49), and our estimated amenities are positively correlated with the estimates of others' (Albouy, 2016; Hsieh and Moretti, 2019).

using only reduced form methods. First, we study the magnitude and sources of heterogeneity in the effects of switching governor partisanship. Second, we study the effects on equilibrium output, wages, and inequality. Third, we study the spillover effects resulting from one state's switch changing the college population in another state.

In all cases, we compare steady state equilibria under one set of governors with that under another set. Since our estimation matches our reduced form estimates that take place over the four post-election years, we view these as relative short-run (four-year) effects. Thus, we do not attempt to model potential changes in TFP that might result from changes in industry structure or firm location, changes in amenities that might be endogenously influenced by the educational composition of the population (as in Diamond (2016)), or changes in future politics that might result from changes in the composition of the electorate. We view all of these as plausible long-run outcomes which could be influenced by the governor's party and its effects on migration, but where further empirical work would be needed to discipline a quantitative model.

6.1 Heterogeneity

A key lesson from our analysis is that the effects of gubernatorial partisanship are inherently heterogeneous. This is despite the fact that we use a single parameter for all Republican governors and for all college-educated workers. In spite of this, heterogeneity emerges in our model because the effects depend on how many workers are on the margin between moving to one state rather than another. As an extreme example, a state with infinite migration costs would be unaffected by changing the incentives to migrate there. More generally, the full vector of state characteristics that we estimate determines how many workers are near indifferent to that state and another.

We begin by describing the *amount* of heterogeneity that we estimate. We then examine whether the model-implied heterogeneity resembles the heterogeneity we see in the data. Finally, we turn to the sources of heterogeneity.

6.1.1 Levels of heterogeneity

First, we calculate 50 separate counterfactuals, each of which involves one single state flipping its governor individually. In each case, we calculate the size of the change in the college graduate workforce that our model predicts. Figure 7 maps these single-state implied effects. The blue states currently have Democratic governors (as of 2023), and therefore we are simulating a switch to a Republican governor, which reduces the stock of college graduates. The red states currently have Republican governors, and so our counterfactual of switching to a

Democratic increases the size of the college graduate workforce. The intensity of the coloration indicates the size of the effects (as a percentage change in the number of college graduates working in the state in equilibrium). Below we study the implications for GDP and for non-college workers, but here we focus only on implications for the stock of college-educated workers, since that is the proximate cause driving all other downstream effects.

[Figure 7 about here.]

Because of college graduates' disutility of living under a Republican governor, the *sign* of the effects is perfectly predicted by the change in partisanship we simulate. However, the *magnitudes* differ considerably. The average state with a Democratic governor would stand to lose 2.8% of its college-educated workforce (roughly equivalent to two year's growth in human capital stock), but states in the Midwest and Mid-Atlantic often show much smaller effects, sometimes as small as a .5% decrease (MI). At the other extreme, three states are predicted to see a decrease of 5% or more, with Hawaii seeing as large as an 8% decline.

For states currently governed by a Republican, both the average effects and the variation in effects are larger. For the average of these states, our model predicts switching to a Democratic governor would increase the college-educated workforce by 4.1%. Again, Midwestern and Mid-Atlantic states are predicted to see rather small effects (as small as a .7% increase in Indiana), while the effects can be as large as 10-11% in Alaska, Wyoming, and Idaho.

6.1.2 Validating model-implied heterogeneity

Before exploring what accounts for this heterogeneity, it is important to assess whether the heterogeneous effects implied by our model match reality at all. To do so, we focus on the eight states "treated" by a Democrat-to-Republican transition in 2015-2017, which are the eight treatment events that we use to estimate our political amenity (γ_R^C). Because these effects are a targeted moment, our model mechanically matches the average size of the effects. However, the heterogeneity across these eight treatment events is *not* a targeted moment, and lets us investigate whether our model-implied heterogeneity is realistic.

To do so, we use our Callaway and Sant'Anna (2021) estimator to estimate the migration effects of each switch individually. The control group is always the full set of states that had a Democratic governor for at least the previous and subsequent five years, and so these eight estimates are comparable to one another. In Appendix Figure B11 we show that the empirical heterogeneity matches the model-predicted heterogeneity reasonably well. The state with the smallest model-predicted effect also has the smallest empirical effect, the four states with the largest model-predicted effects generally have larger empirical effects, and for two of the three intermediate states, we estimate intermediate empirical effects. The clear outlier is

West Virginia, which saw a very large (nearly 40%) empirical effect that our model cannot capture. In general, however, the model predicted heterogeneity is seen in the data, despite the fact that we use a single preference parameter for all Republican governors and the entire college-educated population.[33] With this validation in mind, we turn to interpreting the sources of this model-implied heterogeneity.

6.1.3 Sources of model-implied heterogeneity

Having documented that our model-implied heterogeneity is reasonable, a key question is where this heterogeneity comes from. Recall that workers' log indirect utility can be written as:

$$\ln V_{ido}^{C} = \ln(\alpha_d^C) + \ln(1-\tau_{do}^C) + \ln(1-\gamma_{p(d)}^C) + \ln(w_d^C) + \ln(s_{id}^C)$$

where only the skill draw (drawn from the Fréchet distribution) varies across individual workers. If a worker's productivity draw for state d is high enough to "compensate" for d's amenities, migration costs, political wedge, and human capital price, then the worker will choose to move to state d instead of any other state. These other factors constitute deterministic (from the perspective of an individual worker) incentives to migrate, and these deterministic incentives are traded off against the idiosyncratic productivity draw.

When these deterministic incentives to migrate are very low – such as a state with low quality-of-life amenities (α_d^C), low wages (w_d^C), or high costs of migration (τ_{od}^C) – then workers' productivity draws must be very large to justify moving there. When these incentives are very high, the necessary productivity draw is lower. Recall that the Fréchet distribution is right-skewed. This means that the density is lower around high draws than around low draws. This means that for workers moving to high-incentive states, they are more likely to be doing so with a relatively low productivity draw, and because the density of the Fréchet distribution is higher there, they are more likely to have close substitutes available (in terms of very similar productivity draws in other states). For workers migrating to low-incentive states, however, this move must be justified by a very high productivity draw. Since the density is lower there, these workers are less likely to have close substitutes to this move.

The implication of this logic is that our model predicts that it is *the most attractive states* which have the most at stake from a gubernatorial transition. For workers to move to an unattractive state, the unique circumstances of their employment opportunity there (i.e., their productivity draw) must be so compelling that they are unlikely to have close substitutes available, and therefore they are expected to be less responsive to changes in political incentives

[33] It is notable that our largest empirical *underestimate* of the model-implied effects is in Massachusetts, where Republican Governor Charlie Baker was rated as the single most popular governor in the country (Leins, 2019).

to migrate. While this logic applies to all deterministic incentives to migrate, there is no reason to expect it to be equally important for each incentive. In particular, a state's TFP and human capital price only matter through the worker's earnings, which has an automatically stabilizing effect arising in general equilibrium: When college graduates become more scarce (plentiful) then equilibrium wages will rise (fall) in response, which will increase (decrease) the incentives to migrate and dampen the effects of any non-market change in incentives.

In Table 3, we explore the importance of deterministic migration incentives for explaining the heterogeneous effects that we document above. To do so, we take the 50 state-specific estimates that we obtain from simulating each individual state switching its governor. We take the absolute value of the implied change in the college-educated workforce so that the magnitudes are comparable between those switching from Democrats and those switching to Democrats. We then regress these state-specific estimates on states' amenities, migration costs averaged over origin states o, average migration costs weighted by the number of college graduates living in o, states' TFP, and states' human capital price (which is partly determined by TFP). All of these state characteristics have been normalized to have unit standard deviation across states to facilitate comparisons of coefficient magnitudes.

[Table 3 about here.]

We find that the amenities (α_d^C) dominate the role of other incentives.[34] Amenities alone explain 57% of the variation in model-implied effects. A one standard deviation change in amenities implies a 2 percentage point larger effect of a gubernatorial transition. When we consider other incentives, we also find a significant role for migration costs. However, when we control for all incentives in column 6, we find that the relationship between migration costs and model-implied effects is entirely explained by the correlation between amenities and migration costs. In the fully saturated regression, the R^2 is only marginally higher (.71), the coefficient on amenities actually grows (to 2.2), and the only other marginally significant coefficient suggests a smaller but not trivial role of TFP. Non-wage amenities are clearly the dominant explanation for variation in the effects of a state's gubernatorial switch.

6.2 Counterfactuals

Having established the degree of heterogeneity in the effects of gubernatorial flips, it is important that the counterfactuals we consider are realistic. The implied effects of Wyoming switching to a Democratic governor are large, but the last Democrat running for governor in Wyoming received only 17% of the vote, so these large effects are irrelevant for the real world.

[34]In Appendix Figure B12 we present a raw scatterplot of the relationship between amenities and the effects of a gubernatorial transition.

The basic framework we use to choose our counterfactuals recognizes that politics has increasingly "nationalized," so that governors are evaluated in the same terms that voters use to evaluate national politicians (Grumbach, 2022). This means that the relationship between national voting (e.g., for president) and gubernatorial voting is very strong, although outcomes are not always identical. Figure 8 plots the Democratic Party vote share from the most recent Presidential election, along with the most recent gubernatorial election. The two are strongly correlated (.73), and dropping Vermont makes the correlation even stronger (.86). 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 (similar to the IV strategy we developed above).

In total, we consider four counterfactuals, all of which are shown in Figure 8. First, we consider a "Weak Red wave," in which a modest tide of conservative national sentiment could flip the four states won by Donald Trump but which currently have Democratic governors. Second, we consider a "Strong Red wave" which also flips an additional four states that Biden won narrowly and which have Democratic governors. Similarly, we consider a "Weak Blue wave" that flips the five states won by Biden but represented by a Republican governor, and a "Strong Blue wave" that also flips four battleground states narrowly lost by Biden and represented by a Republican governor.

[Figure 8 about here.]

6.3 General equilibrium effects

Our first question is how these plausible "waves" of Republican and Democratic gubernatorial elections might affect equilibrium economic activity and inequality. In Table 4, we present the model-implied effects of these shifts in governor partisanship. We focus on the effects on the states which are, themselves, flipping and leave spillover effects on other states for the next subsection. For each counterfactual, we present the average effects, as well as the minimum and maximum effects across the states we simulate flipping.

We begin with changes in stock of college-educated workers. For the average state flipping to a Democrat in a blue wave, we estimate an increase in college-educated labor force of 1.3-1.9%. These are smaller than the average effects among *all* Republican governed states that we showed for our counterfactuals that flip one state at a time (the map in Figure 7). This is partly because "swing states" (where a gubernatorial flip is plausible) have smaller effects, on average, than politically one-sided states (see Appendix Figure B13), and partly because flipping multiple states at once leads to cross-state spillovers that cancel out some of the effects for the flipped states (as we show below). However, perhaps more noteworthy than the average effects are the *heterogeneous* effects, which range from a .4% increase in college graduates to a 4.5% increase.

We next consider the effects of "red waves" of Republican gubernatorial victories. These effects are somewhat smaller because of the states which are affected (see Appendix Figure B13). We estimate an average decline of 1.0-1.1%, which ranges from -.8% to -2.4%. Overall, these results highlight the importance of considering realistic flips and using a model to extrapolate heterogeneity to other states, since the results observed in one set of states might say little about other states that could flip.

[Table 4 about here.]

We next turn to effects on GDP per worker.[35] Here, we estimate that the small blue wave increases treated states' GDP by an average of .9% (ranging from .1% to 1.7%), while the big blue wave increases GDP by an average of .5% (ranging from -.2%-1.5%). The effects for the red waves are similar (average of -1.1% or -.8%, ranging from -.4% to -1.5%) but negative. In general, GDP effects that we estimate throughout the paper are roughly one-third as large as the effects on college-educated labor.

These are not large effects. A 1% decline in a state's equilibrium GDP per capita would not cripple a state's economy. Nonetheless, it would be 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 a consideration of inequality. The effects we estimate have two distinct effects on inequality. First, since declines in college in-migration make college graduates more scarce, they raise the relative wage of college graduates. Second, however, since college and non-college workers are q-complements in our CES production function, declines in the number of college graduates reduces the marginal return to non-college labor, pushing down equilibrium wages in absolute terms.[36] In Panel C of Table 4, we present the joint effects on inequality in human capital price. That is, we calculate how equilibrium $\ln w_d^C - \ln w_d^N$ changes when the governor changes. This is a model-consistent notion of inequality in wages since this is the earnings a worker commands per unit of productivity.

We find that the log wage gap falls by an average of about 10% (ranging from 2.2% to 24.5%) in response to a blue wave, while is rises by about 10% (ranging from 2.5% to 33.4%) in

[35]Effects are somewhat larger for GDP, but because they are driven by changes in the size of the workforce, this is partly mechanical. Our model does not include non-workers, so GDP per worker is the most comparable calculation to GDP per capita.

[36]Both of these are artifacts of our production function. However, the best available reduced form causal estimates verify that increasing the supply of college graduates reduces their relative wage (Fortin, 2006) and increases the real earnings of non-college workers (Moretti, 2004).

response to a red wave. In Appendix Table B1 we decompose this into changes in non-college wages and changes in college graduate wages. Across all four counterfactuals, we find that 40%-45% is explained by declining real wages of non-college workers, with the remainder explained by rising college graduate wages.

These estimates represent a model-consistent notion of wage inequality, by which we mean the returns to a given skill draw from the Fréchet distribution. However, when the attractiveness of a state changes, this also induces changes in the selection into that state. This means that the workers migrating into and out of the state will have different levels of productivity, and the composition of the workforce will change. This means that changes in inequality of earnings per worker (which accounts for a worker's idiosyncratic level of productivity) might be larger, smaller, or even opposite signed relative to changes in the human capital price that we study in Panel C.

Panel D presents our estimated effects on actual inequality in log earnings per worker between college and non-college workers. These effects are quite a bit smaller than the effects of inequality in human capital prices, and they are not consistently signed. While all states see wage inequality fall with a Democratic governor, some see earnings inequality *rise*. The implication is that even though the equilibrium predictions of changing the relative supplies of college graduates might be clear, because these changes induce selection that changes the composition of the workforce, the implications for measures of inequality that could actually be observed in the data are unclear.

6.4 Cross-state spillover effects

Finally, our model allows us to study the way in which changes in the governor in one state affect other states. All states are inherently 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.

In Figure 9, we map these spillovers. In Panel (a), we present the direct effects on college-educated workforce for the nine states we consider electing Democratic governors in a strong blue wave.[37] As noted above these effects vary, ranging from .5% (Ohio and Georgia) to 5% (Texas and New Hampshire), with fairly strong effects also in Virginia, Nevada, and Iowa.

In Panel (b), we then plot the indirect spillover effects on college-educated workforce for

[37]Throughout this section, we focus on the implied spillover effects on the size of the college-educated workforce. Since this is the proximate driver of all downstream equilibrium outcomes, we find this the most straightforward to interpret. In Appendix Table B2, we show that spillover effects on GDP are essentially proportional to effects on college-educated workers, and roughly one-third as large.

the states which *do not* change their governor. Most states show very small effects (less than 1%). However, states near the highly affected treated states (those around Texas, for instance) can show quite large effects. We predict that a Democratic governor in Texas could reduce the college graduate workforce in Oklahoma by 4% and in the other three states bordering it by 1-2%. We see similar dynamics in which Nevada, Iowa, and New Hampshire have appreciable effects on their neighboring states, although smaller than the Texas case partly because Texas is such a large state relative to its neighbors. Overall, we find that spillovers tend to be small, but when a state is particularly responsive to gubernatorial partisanship, then effects on its immediate neighbors can be quite large.

[Figure 9 about here.]

In Figure 10 we perform the same exercise to calculate the spillovers induced by a strong red wave. As noted above, these states are generally less sensitive to gubernatorial partisanship. Thus, the direct effects are generally smaller, and the indirect effects therefore tend to be smaller. However, Figure 10 also shows that small states can be significantly affected by changes in larger states, even when the direct effects on the larger state are modest. We predict that North and South Dakota would see 1.2% and 3.8% increases in their college-educated workforce, despite the fact that the nearby treated states (Michigan and Wisconsin) only show modest effects (-.4% to -.6% declines). This is because those states are much larger; in 2022, they had 13 times the college-educated population of the Dakotas, and so even modest shifts affecting them can have appreciable effects on nearby small states.

[Figure 10 about here.]

In conclusion, despite the fact that our estimates generate only a moderate disutility of politics (compared to the amenity distribution or to migration costs), we find that it is substantial enough for political swings to have meaningful effects on economic outcomes. Importantly, these effects change the total allocation of the college-educated workforce across the country, and spillover effects in other states can be as large as those in the directly affected states themselves. 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 substantially exacerbate the effects we estimate here. A doubling of $\gamma^C_{d(Rep)}$ (which is the same amount of change that we see within our sample period, from the times when migration was non-responsive to the strong responses we find post-2015) would roughly double the effects we calculate here. As such, political divides across educational lines are important to monitor in the future.

7 Conclusion

What might conservative politicians do to reduce the growth penalty caused by Republican governance? 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.

Instead, 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.[38] If anything, the sort of 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.

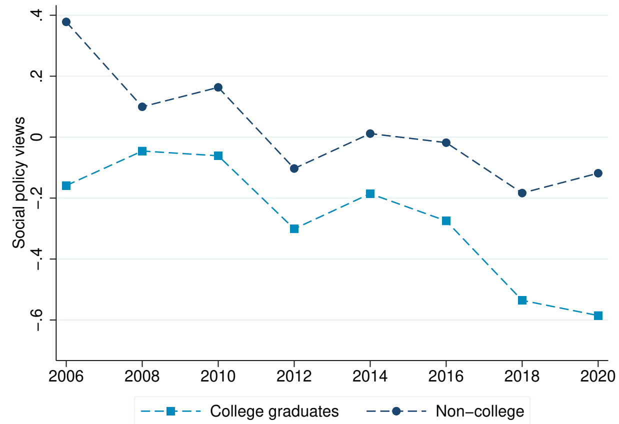
Finally, an open question is how the COVID-19 pandemic and its aftermath will interact with education polarization. We intentionally end our analysis in 2019 because we see migration data from 2020-2022 as being unreliable, but a salient feature of the post-pandemic United States is that working remotely 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,

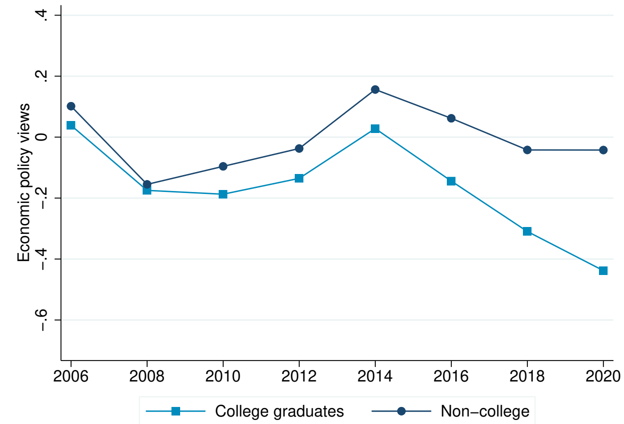
[38]For example, in announcing a controversial bill to reduce public universities' teaching of liberal views on race and gender issues, Florida Governor Ron DeSantis said "Nobody wants this crap. This is an elite-driven phenomenon being driven by bureaucratic elites, elites in universities, and elites in corporate America. And they're trying to shove it down the throats of the American people... They really want to tear at the fabric of our society." (Farrington, 2021)

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 plausibility that it continues to grow) when assessing the implications of post-pandemic work arrangements for heterogeneous growth in different regions of the country.

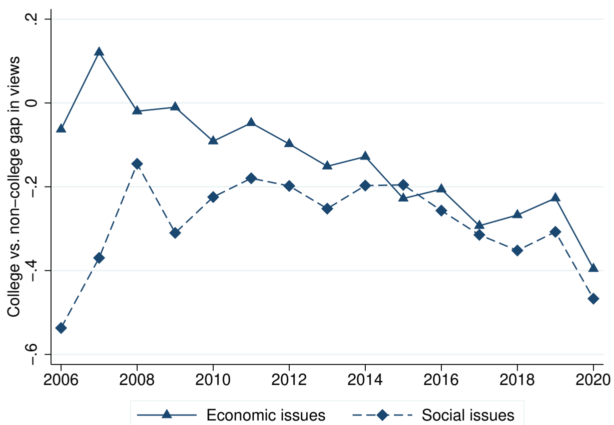
Figure 1: Differences in policy views by education



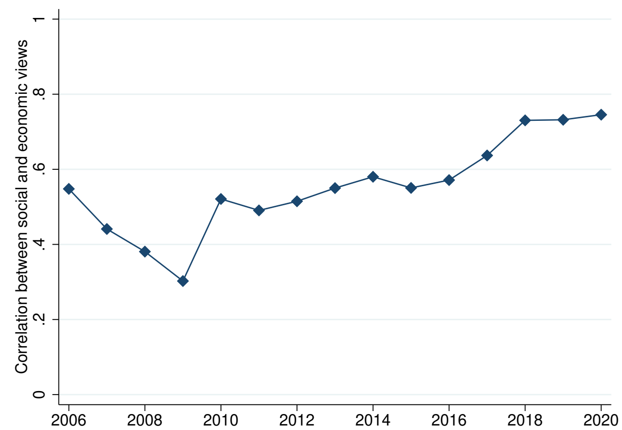
(a) Social policy views



(b) Economic policy views



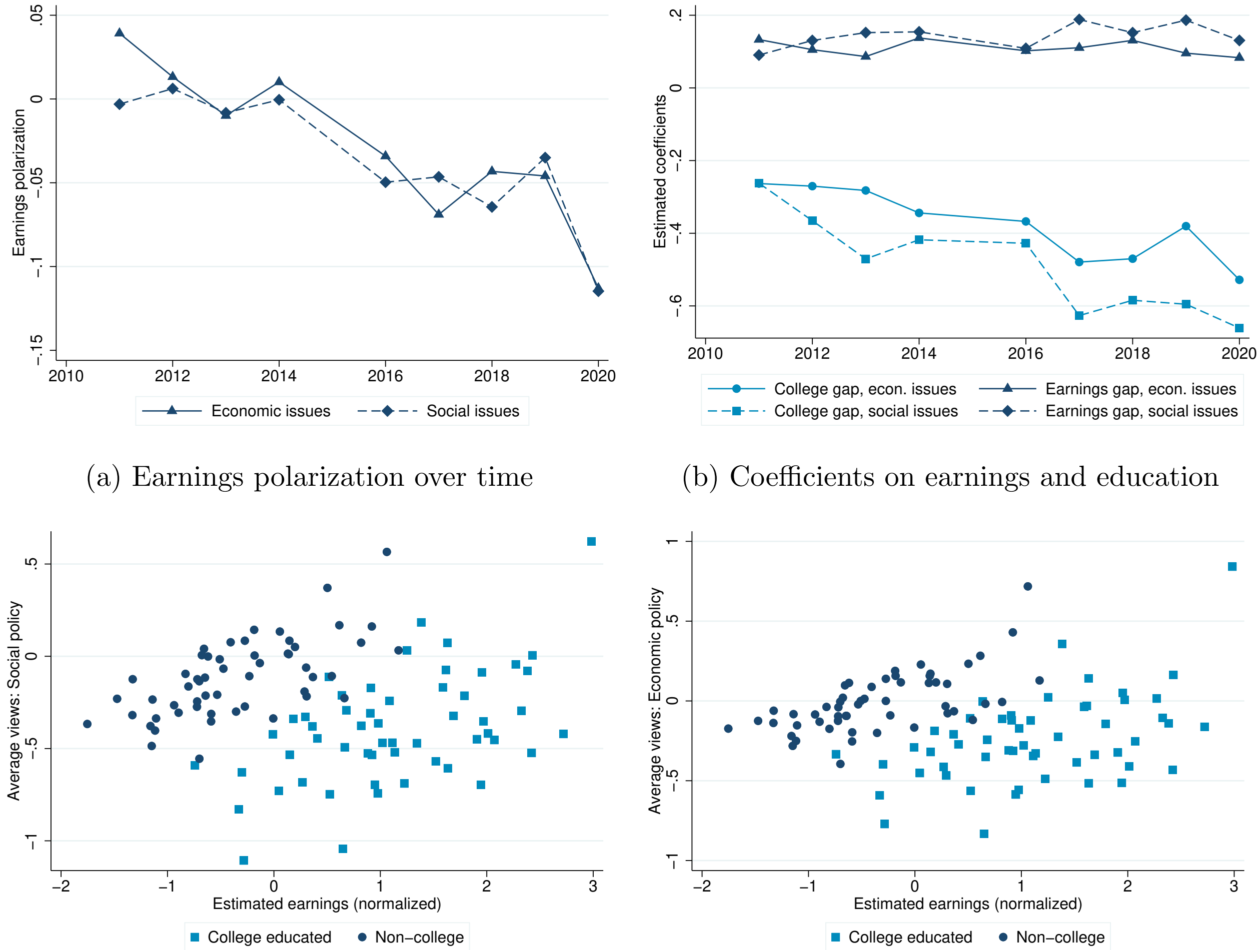
(c) College vs. Non-college gaps



(d) Correlation between social and economic views

Notes: All calculations 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 2: Differences in policy views by earnings



(a) Earnings polarization over time

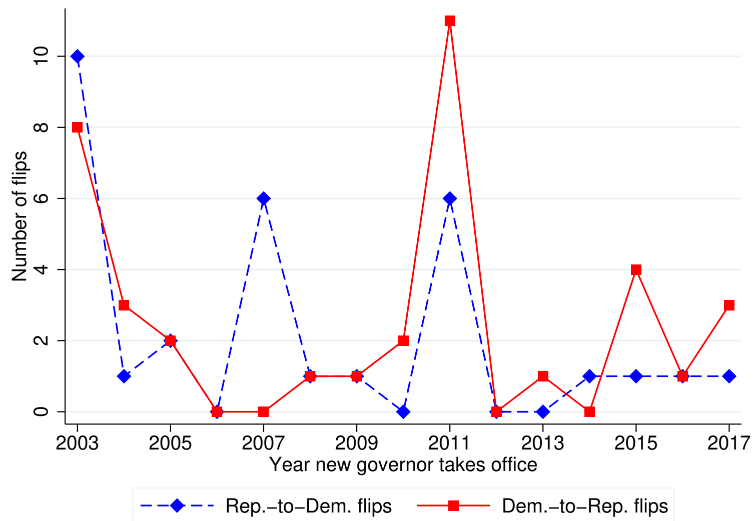
(b) Coefficients on earnings and education

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

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

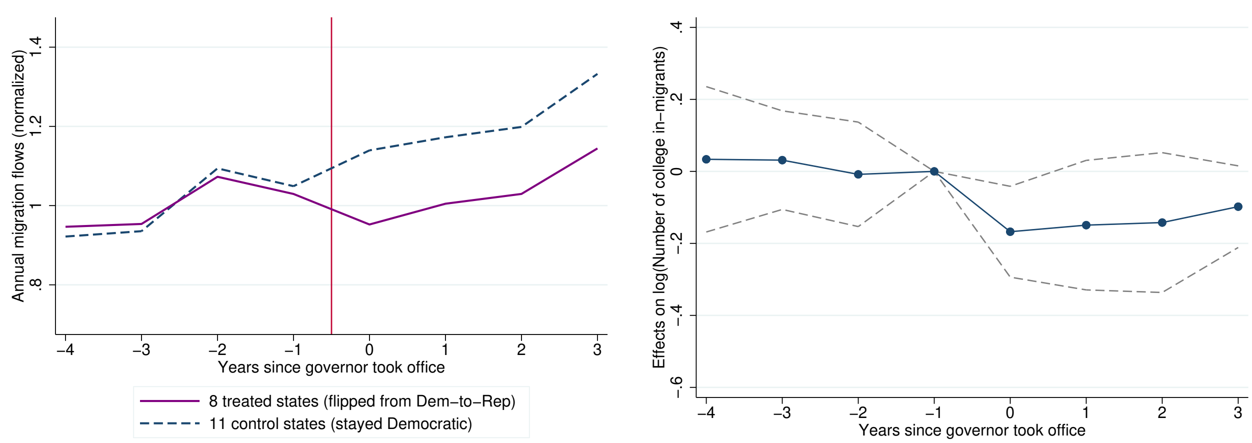
Notes: All calculations based on Cooperative Election Study (CES) respondents aged 26-45 and employed in the private sector. Earnings are imputed from the American Community Survey (ACS) based on age, education, and industry, using the approach proposed in the text that accounts for geographic pay differences (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: Number of gubernatorial party transitions over time



Notes: Figure plots the total number of gubernatorial transitions by type and year.

Figure 4: Democrat-to-Republican gubernatorial transitions and college in-migration

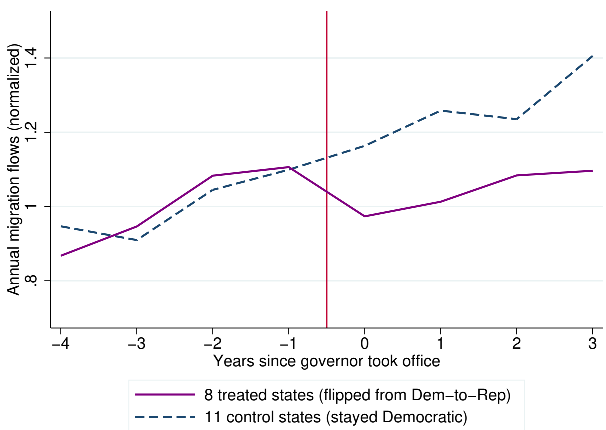


(a) Levels: Treated and control states

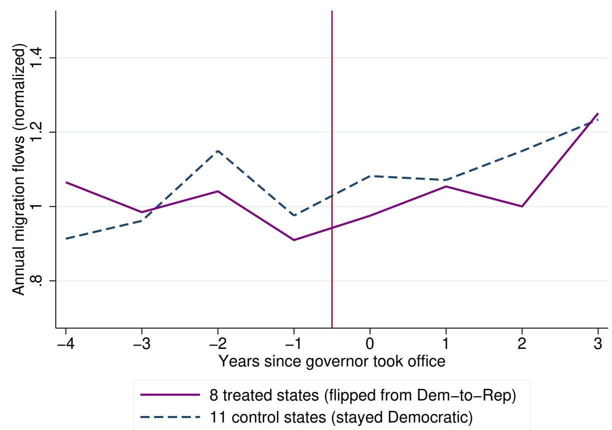
(b) Difference-in-difference estimates

Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican between 2015 and 2017) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). 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. Panel (a): Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. Panel (b): Dependent variable is log number of collage graduate in-migrants (measured in the ACS). See column 3 of Table 1 for estimates corresponding to Panel (b).

Figure 5: Migration responses by state-of-origin



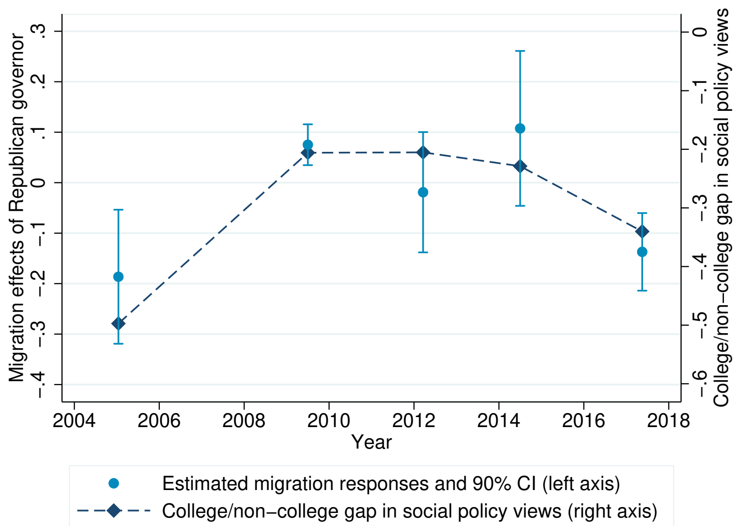
(a) In-migration from Red states



(b) In-migration from Blue states

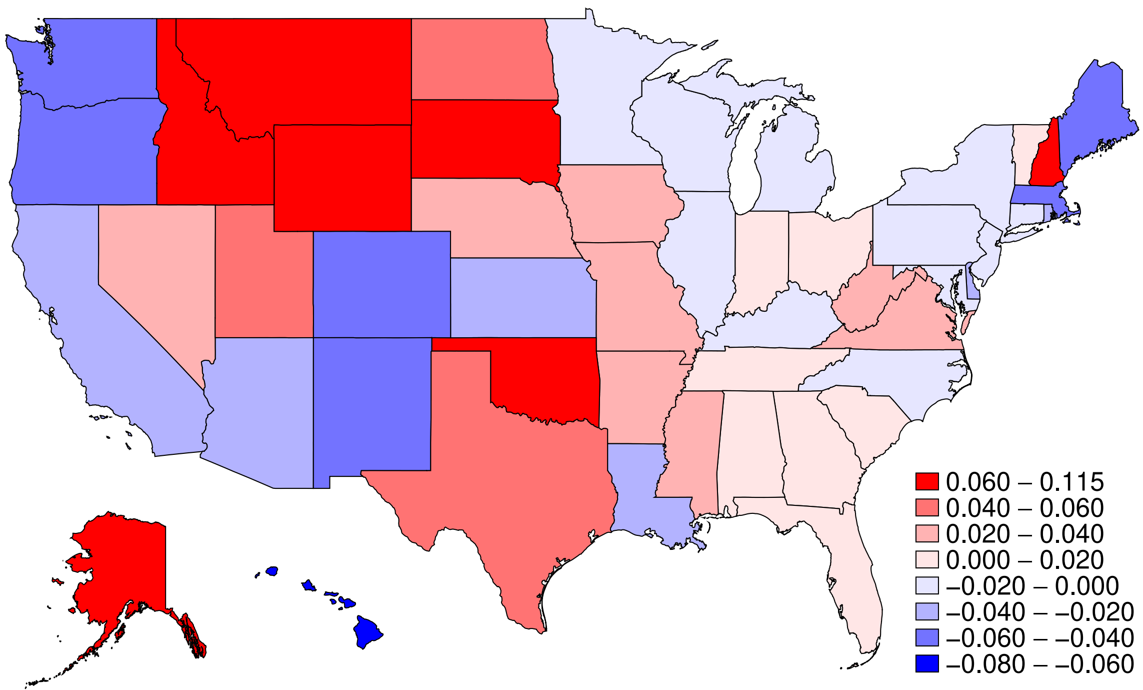
Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. 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. In-migration is calculated separately for migration from a state-year with a Republican governor (Panel (a)) and a state-year with a Democratic governor (Panel (b)). Migration from 2020 onwards is never used.

Figure 6: College graduates' migration responses and education polarization over time



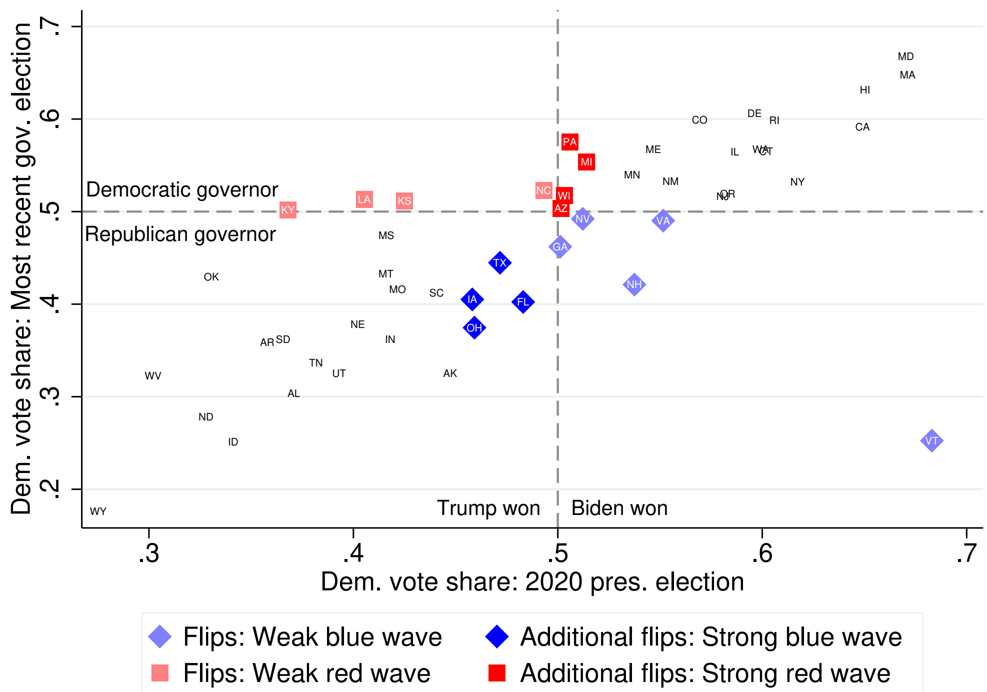
Notes: Figure plots difference-in-difference estimates (based on the Callaway-Sant'Anna-type estimator) for the effects of a Democrat-to-Republican gubernatorial transition on college graduate in-migration, separately for each three-year treatment cohort of our sample (i.e., 2003-2005, ..., 2015-2017), and the average gap in social policy views between college and non-college voters during those same years. Our calculation of average education polarization accounts for the fact that years have different numbers of treated states. For instance, in the 2009-2011 period, there were 11 times as many DR transitions in 2011 (post-treatment years: 2011-2014) as in 2009 (post-treatment years: 2009-2012). Thus, in calculating average polarization during the period, we give 11 times as much weight to the level in 2011-2014 as the level in 2009-2012. Migration data from 2020 onwards is never used.

Figure 7: Heterogeneous effects of single states switching governors



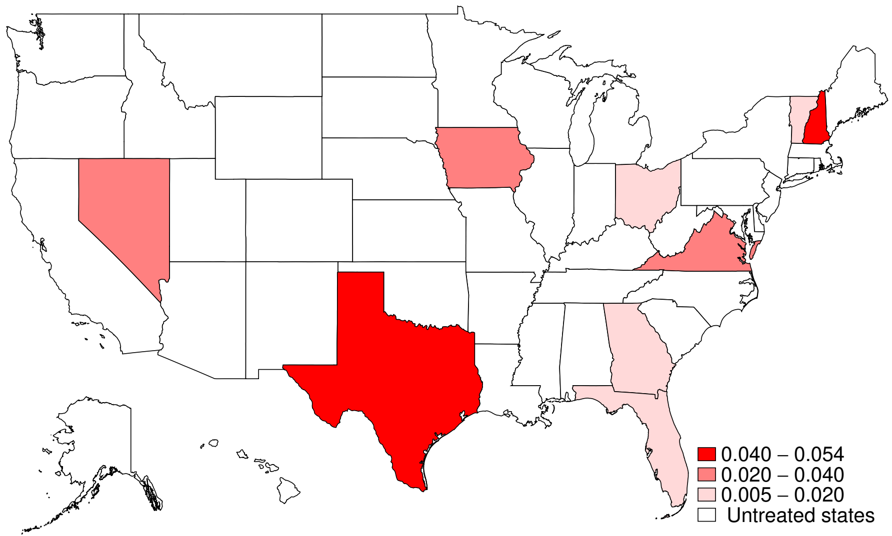
Notes: Map based on 50 separate counterfactuals, each of which flips only one state independently, relative to the set of governors in office in 2023. Red states have Republican governors in 2023, and so we simulate the effects of a Democrat taking office. Blue states have Democratic governors in 2023, and 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 8: Counterfactual switching states relative to 2023 gubernatorial partisanship

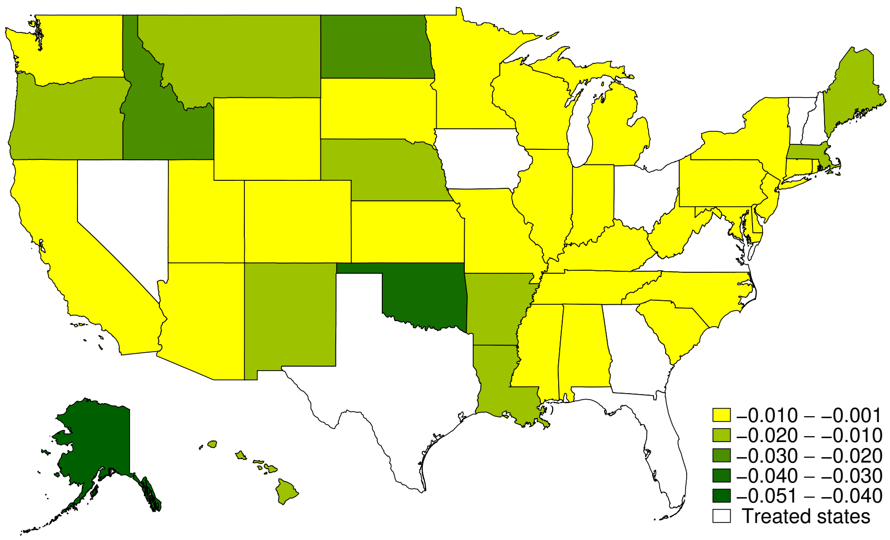


Notes: Figure plots the relationship between 2020 Presidential vote share and the most recent (as of 2023) gubernatorial election vote share. All vote shares based on two-party vote share. Figure identifies the 17 states we consider plausible flips in our four counterfactuals.

Figure 9: Direct and spillover effects of big blue wave



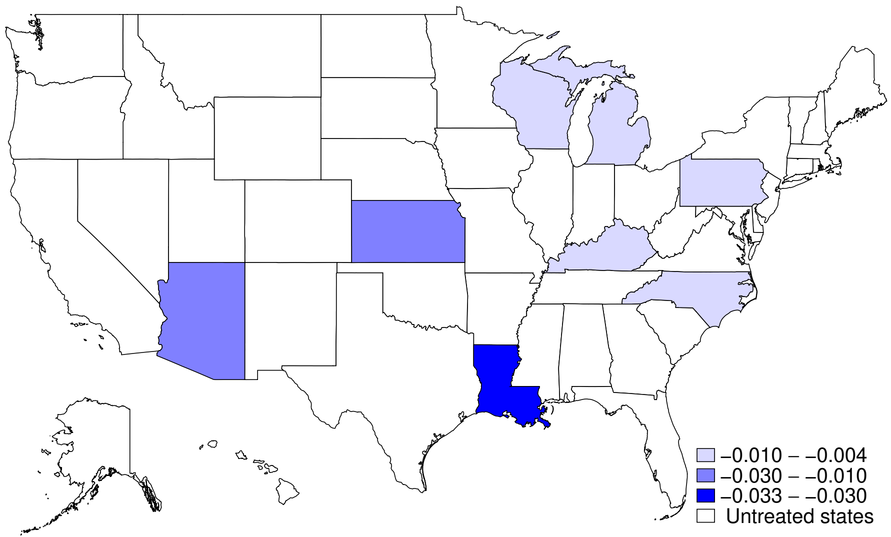
(a) Direct effects: Treated states



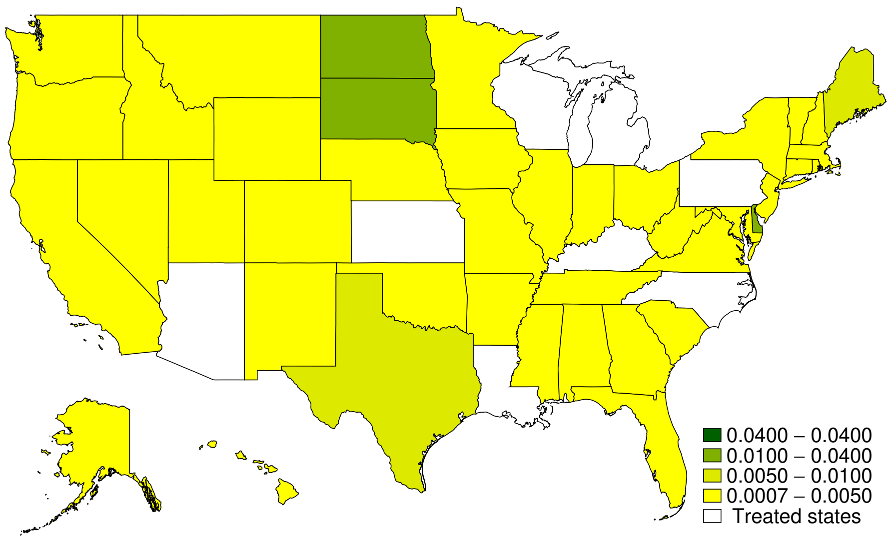
(b) Indirect spillover effects

Notes: Map based on the strong blue wave (see Figure 8) in which nine states governed by Republicans in 2023 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 10: 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 8) in which eight states governed by Democrats in 2023 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: Effects of Democrat-to-Republican transitions on college in-migration

	(1)	(2)	(3)	(4)	(5)	(6)
Flip years:		2015-2017			2003-2017	
Treat × Post	-0.105***	-0.137***	-0.157***	-0.077**	-0.083**	-0.117**
	(0.032)	(0.047)	(0.051)	(0.032)	(0.039)	(0.045)
R^2	0.987	0.983	0.980	0.976	0.965	0.955
N	274	274	274	853	853	853
Avg. migration rate	.027	.030	.040	.028	.033	.042
Number of states						
Treated		8			33	
Control		11			16	
Sample						
Employed	Yes	Yes	Yes	Yes	Yes	Yes
Age	18+	26+	26-45	18+	26+	26-45
Private sector		Yes	Yes		Yes	Yes
US Citizen			Yes			Yes

* $p < .10$, ** $p < .05$, *** $p < .01$. Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is log number of college graduate in-migrants (measured in the ACS). 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. Standard errors are clustered at the state level.

Table 2: Estimation results

Parameter	Description	Determination	Value	
			College	Non-college
σ	EoS across education group	Jerzmanowski-Tamura '20	2.6	
θ^g	Skill dispersion	Wage dispersion	2.85	2.96
τ^g_{do}	Moving cost	Migration portion difference	0.947 (0.14)	0.952 (0.139)
α^g_d	Amenity	Same party governors	1.86 (1.33)	1.61 (1.07)
w^g_d	Human capital price	Market clear + Common price	903291.02 (215634.86)	694500.96 (287623.69)
A^g_d	TFP	Market clear + Common price	502270.51 (156371.24)	
$\gamma^g_{p(d)}$	Political wedge	Reduced-form moments	0.06	0.01

This table reports the point estimates from structural estimation and calibration of the model.

Table 3: Sources of model-implied heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
Amenities (α^C_d)	2.02***					2.16***
	(0.25)					(0.42)
Average migration cost ($\bar{\tau}^C_d$)		1.08**				
		(0.41)				
Weighted avg. mig. cost ($\bar{\tau}^C_d$)			1.44***			0.03
			(0.39)			(0.37)
TFP (A_d)				0.67		1.22*
				(0.50)		(0.62)
Human capital price (w^C_d)					0.43	-0.20
					(0.51)	(0.61)
N	50	50	50	50	50	50
R^2	0.568	0.163	0.289	0.062	0.026	0.714

* $p < .10$, ** $p < .05$, *** $p < .01$. Regressions based on the results from 50 separate counterfactuals, each of which flips only one state independently, relative to the set of governors in office in 2023. For each counterfactual, we calculate the percentage change in the college graduate workforce predicted by our model for the treated state. The absolute value of these changes is regressed on various state characteristics estimated from our model.

Table 4: 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	4	8
Panel A: Total college-educated labor force				
Average effects	0.030	0.023	-0.020	-0.015
(min, max)	(0.009, 0.072)	(0.005, 0.054)	(-0.033, -0.009)	(-0.033, -0.004)
Panel B: GDP per worker				
Average effects	0.009	0.005	-0.011	-0.008
(min, max)	(0.001, 0.017)	(-0.002, 0.015)	(-0.015, -0.006)	(-0.014, -0.004)
Panel C: Unobservable wage inequality (human capital price)				
Average effects	-0.141	-0.080	0.138	0.087
(min, max)	(-0.245, -0.082)	(-0.211, -0.022)	(0.044, 0.334)	(0.025, 0.313)
Panel D: Observable wage inequality (college vs. non-college earnings)				
Average effects	-0.020	-0.018	-0.018	-0.013
(min, max)	(-0.142, 0.039)	(-0.131, 0.032)	(-0.036, -0.000)	(-0.033, 0.002)

Table presents changes in equilibrium outcomes for "treated" states (i.e., those changing their governors) in our four counterfactuals (Figure 8 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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A Data

[Table A1 about here.]

[Table A2 about here.]

Table A1: Social Policy Questions

Category	Number of questions ever	Number of questions each year	Most common question
Abortion and birth control	5	2.7	Do you support banning abortion after the 20th week of pregnancy?
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?
Crime and policing	12	1.1	Do you support eliminating mandatory minimum sentences for non-violent drug offenders?
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?
Gender and sexuality	7	1.5	Do you favor or oppose allowing gays and lesbians to marry legally?
Gun control	6	2.1	Do you support banning assault rifles?
Immigration	23	4.7	Do you support increasing the number of border patrols on the US-Mexico border?
Impeachment	2	0.1	Do you support removing President Trump from office for abuse of power?
Supreme Court	5	0.4	Do you support confirming Brett Kavanaugh to become a Justice of the Supreme Court?

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.

Table A2: Economic Policy Questions

Category	Number of questions ever	Number of questions each year	Most common question
Deregulation	2	0.3	Would you support an executive action requiring that with each new regulation enacted, two must be cut?
Environment	10	2.9	Do you support giving the Environmental Protection Agency the power to regulate Carbon Dioxide?
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?
Health care	14	2.3	Do you support repealing the Affordable Care Act?
Minimum wage	2	0.4	Do you support raising the minimum wage to $15 an hour?
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

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 Additional figures and tables

B.1.1 Descriptive facts

[Figure B1 about here.]

[Figure B2 about here.]

[Figure B3 about here.]

[Figure B4 about here.]

[Figure B5 about here.]

B.1.2 Causal effects

[Figure B6 about here.]

[Figure B7 about here.]

[Figure B8 about here.]

[Figure B9 about here.]

[Figure B10 about here.]

B.1.3 Structural analysis results

[Figure B11 about here.]

[Figure B12 about here.]

[Figure B13 about here.]

[Table B1 about here.]

[Table B2 about here.]

[Figure B14 about here.]

[Figure B15 about here.]

B.2 Further analysis

B.2.1 Explaining education polarization

Our goal in this paper is not to explain the rise of education polarization. Nonetheless, some features of our data do speak to some potential explanations.

First, many conservatives argue that universities are increasingly sites of indoctrination of leftist ideas. Thus, education polarization could reflect either a "treatment" effect of attending college, or a "selection" effect as conservatives are deterred from attending.[39] We find very limited support for this. In panel (a), we plot education polarization estimated separately by year and cohort, among cohorts turning 26 from 2008-2020.

[Figure B16 about here.]

For the period of 2012-2016, we find modest evidence supporting the "indoctination" claim: The college/non-college gap among new cohorts grew by roughly $.1\sigma$, while the gap among already-educated cohorts stayed constant. From 2016-2020, however, there is no support for this hypothesis. This period is where the dramatic increase in education polarization was concentrated, and that increase was identical between new cohorts and already-educated cohorts. During this period, each of the five cohorts that had already turned age 26 saw the college/non-college gap widen by roughly $.3\sigma$, much larger than the modest between-cohort effects from 2012-2016. Thus, while there is some support for either changes in universities' treatment effects or changes in selection into universities, these effects are modest relative to the overall growth in college/non-college gaps. Most of the growth (and all of the recent growth) instead reflects changes in the political environment.

One of the leading changes in the political environment that has garnered the most attention is changes in the relative importance of social and economic issues (Danieli et al., 2022; Gethin et al., 2022). Similar to Danieli et al. (2022), who focus on Europe, in panel (b) we predict whether a respondent identifies as a Republican (as opposed to a Democrat and excluding Independents) using our indices for social and economic policy views. We do this separately by education and year, and plot the resulting coefficients.

Consistent with this view, the relative importance of social issues for explaining partisan affiliation has increased, while the relative importance of economic issues has declined (for both college educated and non-college voters). In 2010, a 1σ increase in conservative views on economic issues increases the probability of identifying as a Republican by 22 percentage

[39] Little academic evidence has assessed this explanation because of both measurement and identification challenges. One important exception is Firoozi (2022), who estimates the causal effect of shifting from a less prestigious university to a more prestigious one, and finds sizeable impacts on political activity.

points, while a 1σ increase in social issues conservatism implies only a 11-13pp increase. By 2020, this had flipped, and a 1σ increase in economic issues implies only a 10-15pp increase in Republican identification, compared to a 20-22pp increase from a 1σ increase in social issue conservatism.[40] Thus, for both college educated and non-college voters, social issues have become more important and economic issues have become less important. Given that education polarization has always been larger for social issues than economic issues, this does lead to some widening in partisan preferences between the two groups. However, as we showed above, there has been widening polarization on both sets of issues independently and, moreover, the correlation between views has risen, making it less and less important to distinguish between them. Thus, while these shifts in priorities exacerbate education polarization, they do not explain the overall increase.

Finally, in an influential book, Bishop (2009) presents evidence that liberals and conservatives increasingly live in different places, and argues that a decline in interpersonal interactions drives polarization. Given the growth of sorting across different metropolitan areas by education (Diamond and Gaubert, 2022), as well as rising socioeconomic segregation at the neighborhood level (Mijs and Roe, 2021), it seems natural to connect these hypotheses. Indeed, segregation by political affiliation has increased for virtually any geography (Brown et al., 2022), and neighbors and communities do appear to have effects on individuals' political views (Cantoni and Pons, 2022; Martin and Webster, 2020; Perez-Truglia and Cruces, 2017; Perez-Truglia, 2018).

Consistent with this, panel (c) shows that the states with the highest college shares in 2005-2010 also saw the greatest growth in education polarization on social issues from 2010-2020. While this is consistent with an "echo chamber" effect, the magnitude is relatively small and only marginally significant, and the same effects do not show for polarization on economic issues, although this polarization has been equally extreme.

B.2.2 Heterogeneity by college major

As a "sanity check," it is useful to know whether the effects are driven by college graduates with more left-leaning majors. As we have emphasized throughout, the gap in average views between college and non-college voters is large, but there are meaningful variations within different types of college-educated voters.

To characterize these differences, we match the field of degree reported by college graduates in the ACS from 2009 to data on average political views by major from UCLA's Higher Education Research Institute (HERI). Specifically, we use the public use files from HERI's

[40] Interestingly, while college and non-college voters show nearly identical *levels* for the two issues in both 2010 and 2020, the timing of the change has been different. The shift from economic to social issues among non-college voters began following 2010, while for college graduates it began only after 2016 (and was very rapid).

College Senior Survey, which asks students "How would you characterize your political views?" and offers five options ranging from "Far left" to "Far right." For our purposes, there are two substantial limitations of the HERI data. First, publicly available data is from 1994-2008, which only modestly overlaps our main sample. Second, self-reported political identification is always relative to one's political context (in this case, other college majors at that specific point in time), which makes this a much more difficult metric to compare over time or with non-college respondents than our index above, which was based on actual policy views. Figure B17 characterizes some of the variation in political leanings across different college majors, as well as showing the relationship with median earnings (which is generally weak). Overall, many of the majors which lean furthest to the left and the right are intuitive examples, such as the strong left-lean of sociology, ethnic studies, and environmental sciences, or the far right lean of military studies, agricultural studies, and theology.

[Figure B17 about here.]

We split college majors into above and below median based on the share of HERI respondents who identify as either "Far left" or "Liberal." In Figure B18 we then study the migration effects for these two different types of degree holders. We find significant responses for both sets of college graduates (consistent with our arguments above that the *overall* differences in views are quite stark), but they are somewhat larger for graduates holding more left-leaning majors.

[Figure B18 about here.]

B.2.3 Alternative identification strategies

One potential concern is reverse causality; shocks to migration incentives might affect election outcomes, for instance by changing economic conditions or shifting the composition of the electorate. Here, we present two alternative identification strategies which are immune to these concerns.

We first use an instrumental variables (IV) strategy that builds on two insights: First, American politics is very "nationalized," and voters state and local vote choices largely reflect views on salient national controversies (Grumbach, 2022), which change over time. Second, states differ in the timing of their gubernatorial elections, which are largely pre-determined (barring deaths, mid-term resignations, etc.). As a result, states differ in the national political mood that happens to prevail at the time in which they hold their gubernatorial election, and this has effects on election outcomes.

The main intuition for our IV strategy is given in Figure B19, which shows *i*) the share of Democratic-led states holding an election during the year in which the Republican candidate wins, and *ii*) Republicans' share of the *Congressional* vote (i.e., not gubernatorial) in "purple" states during the year.[41] The over-time correlation between these two series is .78, suggesting that Republican gubernatorial candidates do well during the years when Republican Congressional candidates do well.[42] Our IV strategy takes advantage of the fact that while all states and districts elect their *Congressional* Representative every two years, 48 states elect their *Governor* only once every four years, and with staggered timing set well in advance. For example, six states with Democratic Governors held gubernatorial elections in 2008, a year in which Congressional Republicans were dominated by Democrats amidst a worsening recession (Republicans received less than 43% of the vote, the within-sample minimum). None of those six states flipped to a Republican governor. However, two years later, amid the "Tea Party wave," 19 states with Democratic governors held gubernatorial elections. Republicans captured 51% of the vote in purple states, and nearly 60% of Democrat-led states holding gubernatorial elections that year saw their state flip to Republicans. It is plausible that many of the Democratic governors winning in 2008 would have been defeated if they had been unlucky enough for their state's election to be in 2010 instead. Our IV strategy takes advantage of this insight.

[Figure B19 about here.]

Specifically, we instrument for a D-to-R flip using the interaction between the time-specific nationwide Republican vote share for Congressional candidates and an indicator for whether the state's regular gubernatorial election schedule fell in that year.[43] We control separately for year effects (absorbing the time-specific Congressional vote share) and whether the state had a regular gubernatorial election during that year, and so identification is driven solely by the above interaction; in other words, identification comes only from idiosyncratic cross-state variation in whether the regularly scheduled election fell on a "bad year" for Democrats:

$$Flip_{st}^{DR} = \delta_t + \beta_1 ElectYear_{st} + \beta_2 \big(ElectionYear_{st} \times RepVoteShareCongress_t\big) + \varepsilon_{st} \quad (13)$$

where again, $RepVoteShareCongress_t$ is the share of the Congressional vote going to Republicans during the year, among all "purple" states which experience both Democratic and Republican governors during the sample (i.e., the sample of "purple" states is fixed across time). The estimation sample is restricted to states with a Democratic governor at time t, since it is otherwise impossible to transition to a Republican.

[41] We define "purple" states as those we observe with both a Democratic and a Republican governor during our 20 year period. Thus, the definition of "purple" states does not change year-to-year.

[42] Interestingly, Congressional voting patterns among the same states are far less predictive of R-to-D transitions (also shown in Figure B19). Thus, we cannot instrument for those flips.

[43] Gubernatorial transitions and special elections can occur during other years because of deaths, resignations, etc. We focus on the years of regularly scheduled elections.

In the second stage, we estimate the change in migration rates, as a function of this instrumented D-to-R flip. Specifically, the second stage estimating equation is given by:

$$\Delta Mig_{st} = \theta_t + \gamma_1 ElectYear_{st} + \gamma_2 \hat{Flip}_{st}^{DR} + \nu_{st} \tag{14}$$

where ΔMig_{st} is the average annual log inflow of college educated workers during the next four years (i.e., post-flip) minus the average annual log inflow during the previous four years (i.e., pre-flip). In this way, our IV strategy still identifies only within-state changes in migration.

Our second alternative identification strategy is to use a standard regression discontinuity design (RDD), in which identification comes solely from close election outcomes. In this regression, we restrict only to state-years with a close election, and predict the pre/post change in migration as a function of the Republican candidate's vote share, whether the Republican won, and the interaction. This estimating equation is given by:

$$\begin{aligned} \Delta Mig_{st} = (\delta_t +)\ & \beta RepWins_{st} + \gamma_1 RepVoteShare_{st} \\ & + \gamma_2 \big(RepVoteShare_{st} \times RepWins_{st}\big) + \varepsilon_{st} \end{aligned} \tag{15}$$

where $RepVoteShare$ is "centered" at zero so that the core coefficient of interest, β, reflects the change in migration for an asymptotically close election won by a Republican. We use both the biased adjusted estimator from Calonico, Cattaneo, and Titiunik (2014) with a triangular kernel and no controls, as well as a standard OLS estimate on the Calonico et al. (2014)-selected bandwidth with a uniform kernel and year fixed effects.

Table B3 presents the results from our main difference-in-difference specification, as well as those from the IV specification and the RDD specifications. Panel A presents estimates from the full sample (all flips occurring between 2003 and 2017), while Panel B restricts to the most recent 10-year period (flips from 2008-2017). Unfortunately, we cannot restrict further to even more recent years because our IV strategy loses power in the first stage (see Figure B19) and the RDD sample would fall to too few close elections.

Overall, IV and RDD point estimates are very similar to our main specification. For the full sample period, the four estimates range from an 8 log point decline to an 11.7 log point decline.[44] The key difference is that these alternative identification strategies are far less precise, and none yield a statistically significant estimate. It is worth noting that each Panel A estimate would be significant if it had the same standard error as our main specification does. Our key emphasis, then, is that Table B19 does not seem to suggest that our primary identification strategy over-estimates the effects on college educated migration. Instead, it seems that the

[44] Above, we presented the difference-in-difference plot for the 2015-2017 flip period. Appendix Figure B9 shows the corresponding plot for all flips 2003-2017.

main difference between our preferred specification and these more conservative ones is that our preferred specification has much more statistical power.

[Table B3 about here.]

While our full-sample difference-in-difference estimate is slightly larger than that generated by the alternative identification strategies, this is no longer true in the more recent time period, where alternative strategies generate both larger and smaller estimated effects (again, none of which are statistically significant).

Beyond point estimates, it is also important to know whether the over-time patterns we identify hold for these alternative strategies. Because both strategies inherently have lower statistical power than our preferred difference-in-difference specification, we cannot estimate over-time changes as flexibly as we have done above (our instrument becomes very weak and the RDD sample becomes very small). Instead, in Figure B20, we present estimates for six 10-year rolling windows. In general, both the IV strategy and the RDD strategy generate an inverted-U-shaped pattern like the difference-in-difference specification does, with the exception of the RDD estimate in the final time period.

[Figure B20 about here.]

B.2.4 Alternative explanations

We interpret the decline in in-migration of college graduates after a Democrat-to-Republican gubernatorial transition as being driven by those workers' political preferences. Here, we consider two possibilities: *i*) effects on economic activities and the incentives to migrate and *ii*) effects on perceptions of citizens' political views.

First, we have argued that college graduates have a general preference against Republican Governors. This can reduce migration via a labor supply effect, and this is our primary interpretation. It is possible, instead, that Republican Governors reduce migration through a labor *demand* effect, such as through contractionary spending cuts. It is worth keeping in mind that economic contractions tend to be more experienced by non-college workers than college graduates (e.g., Patterson (2020)), and that Republican Governors tend to lead to more pro-growth business friendly policy Caughey et al. (2017). Thus, ex ante, one would expect a Republican Governor to *increase* labor demand, and one would also expect any economic incentives to be more pronounced for non-college workers than college graduates. Nonetheless, this is a concern worth exploring.

We use data from the Bureau of Labor Statistics' Job Openings and Labor Turnover Survey (JOLTS) to estimate effects on job openings and hiring, proxies for labor demand.[45] If Republican Governors implement contractionary spending cuts, we would expect job openings to go down. We do not find this. Panel (a) of Figure B21 shows that job openings are unaffected (if anything, rising in the very short-term). In panel (b), when we consider effects on hiring, we do find a small non-significant immediate decline in hiring. Recall that we found immediate migration responses during that year. Thus, our JOLTS results are consistent with a labor supply response in which fewer in-migrants make it more difficult to fill an unchanging number of vacancies.

[Figure B21 about here.]

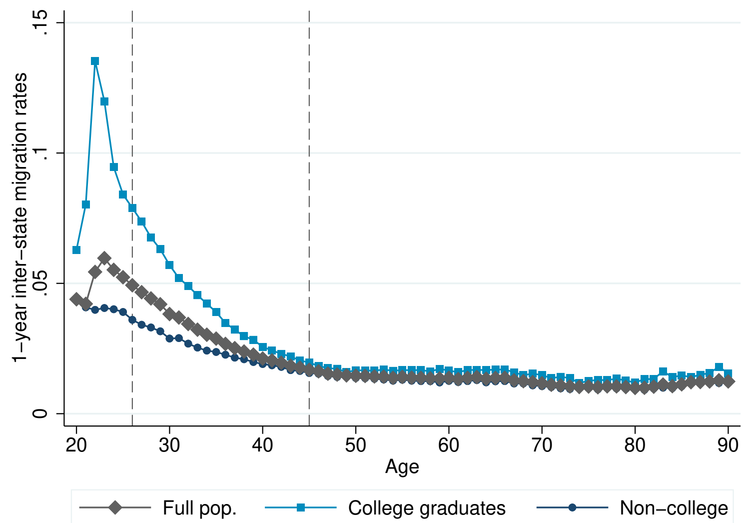
Second, it is possible that the election of a Republican governor reduces in-migration of college graduates not because those graduates are averse to conservative *policies*, but because the election signals the prevalence of conservative *voters*, and college graduates do not want to live near these conservative voters. This matters for policy because it suggests that the actual decisions of Republican governors about what to do once in office have no bearing, the penalty emerges simply from being elected. Stated choice experiments over neighborhoods do show that voters prefer living near those who are ideologically similar, and this could explain why we find effects on in-migration but not out-migration. Nonetheless, we are skeptical of this explanation, since estimated preferences for same-ideology neighbors have been fairly small (Mummolo and Nall, 2017) and anyway, most cities (even in very conservative states) have liberal neighborhoods that residents can choose.

Nonetheless, we can test for the effects of shifts in *perceptions* – rather than policy – by estimating the effects of flips in the Presidential vote. Since all states have the same President, a shift from supporting the Democrat to supporting the Republican does not actually differentially affect the policies in that state; all states are affected. However, these electoral outcomes are highly salient, so it should have the same signalling effects about the voters that electing a Republican governor does. In the 2016 Presidential election, Donald Trump flipped six states that Barack Obama had won in 2012. We estimate the effects of these "flips" on college workers' in-migration in Figure B22. We find no effects on college graduates' in-migration, though we note the $t = -3$ coefficient suggests some caution in interpreting the pre-trends. It is important to note that this specification as well as time frame are directly comparable to our main estimates of 2015-2017 flips presented above in Figure 4. Overall, then, we conclude that the evidence is more consistent with the importance of Republican *governors* who, as we have discussed, have meaningful influence on policy (Caughey et al., 2017; Seifter, 2017).

[45] Unfortunately, at the state level, the BLS does not make such data available separated by industry or any other proxy for education.

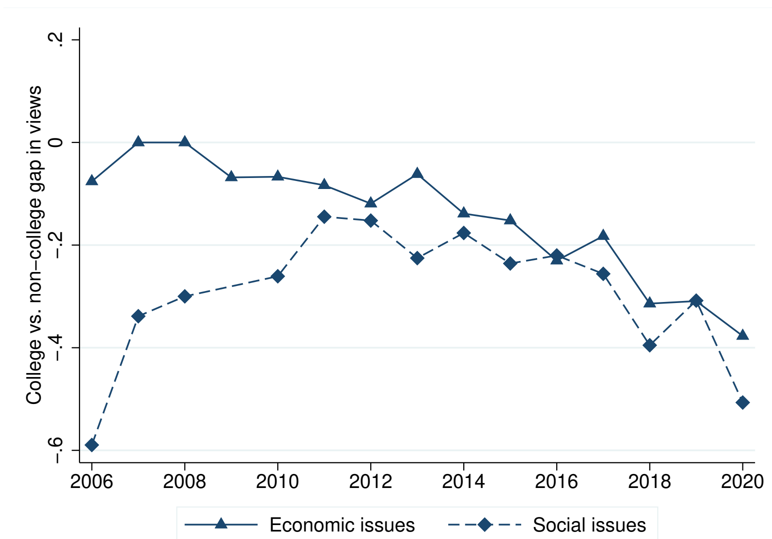
[Figure B22 about here.]

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



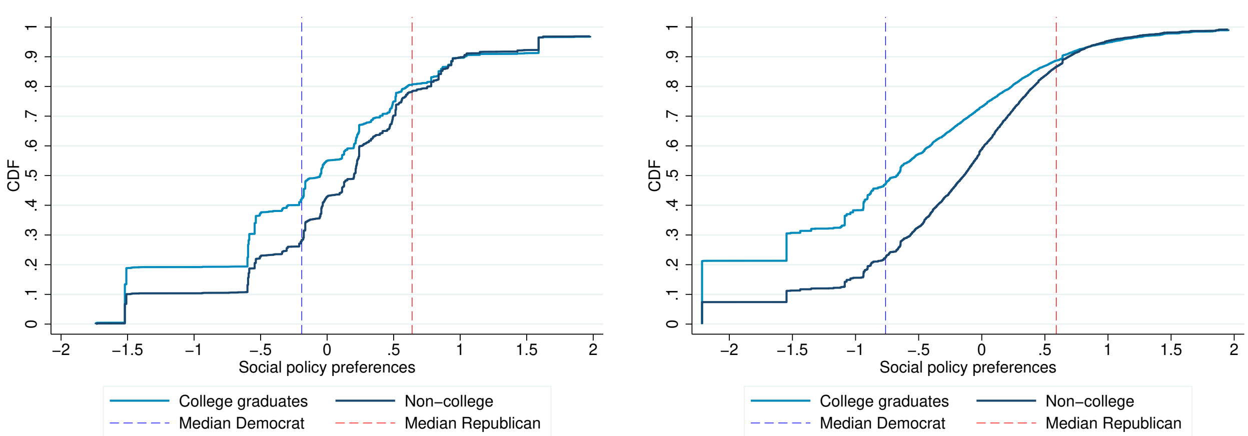
Notes: Calculations based on one-year inter-state migration rates from the American Community Survey (ACS). All calculations use sample weights.

Figure B2: College vs. Non-college gaps in median views



Notes: All calculations 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.

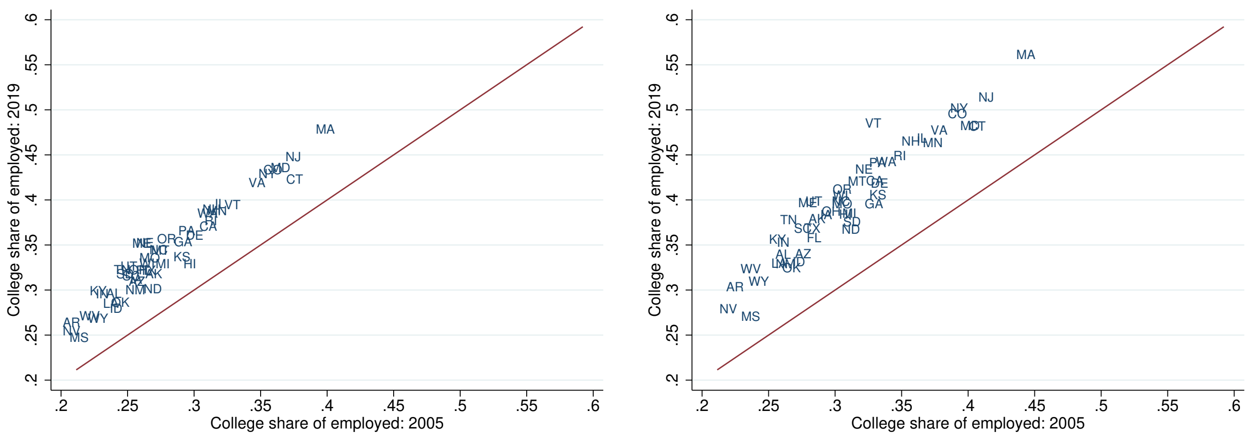
Figure B3: Distribution of social policy views: 2010-2020



(a) 2010 (b) 2020

Notes: All calculations 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 B4: Changes in college-shares by state over time

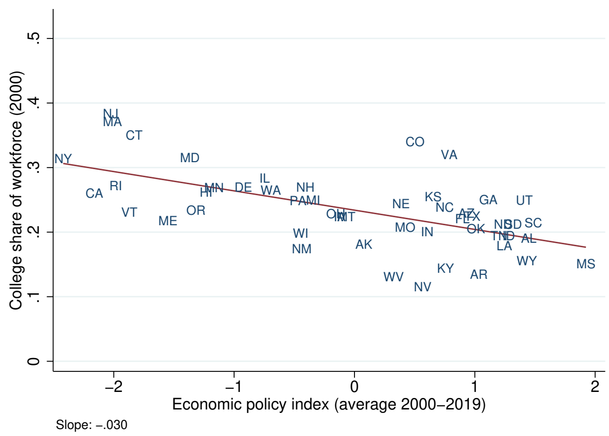


(a) Share of all employed

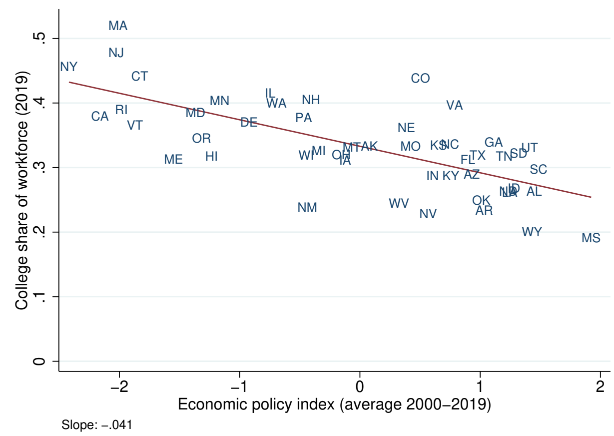
(b) Share of “young” (26-45) employed

Notes: Calculations based on American Community Survey (ACS). All calculations use sample weights.

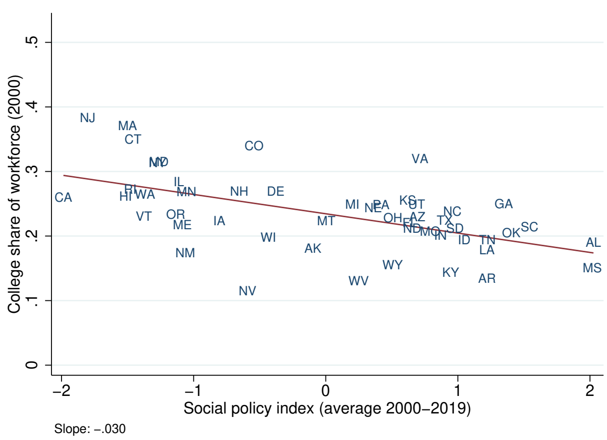
Figure B5: College share by state policy over time



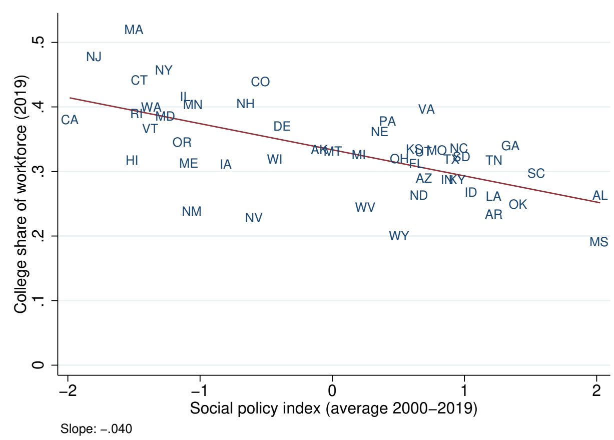
(a) By economic policy, 2000



(b) By economic policy, 2019



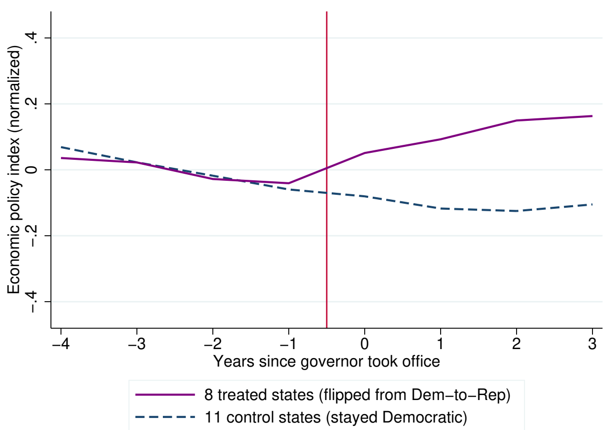
(c) By social policy, 2000



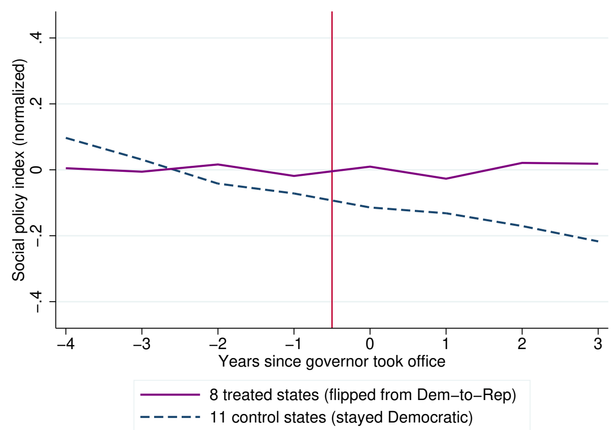
(d) By social policy, 2019

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

Figure B6: Policy effects of Democrat-to-Republican gubernatorial transitions



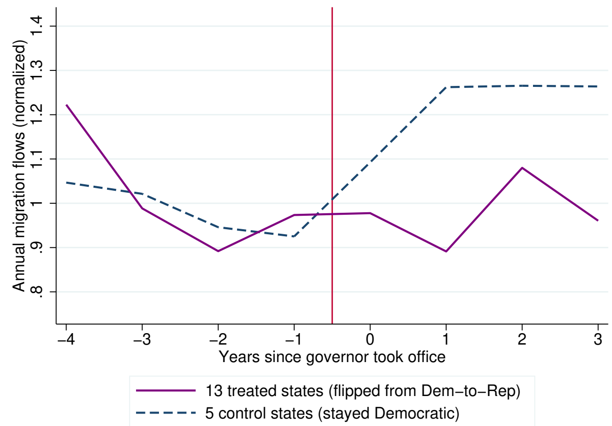
(a) Economic policy



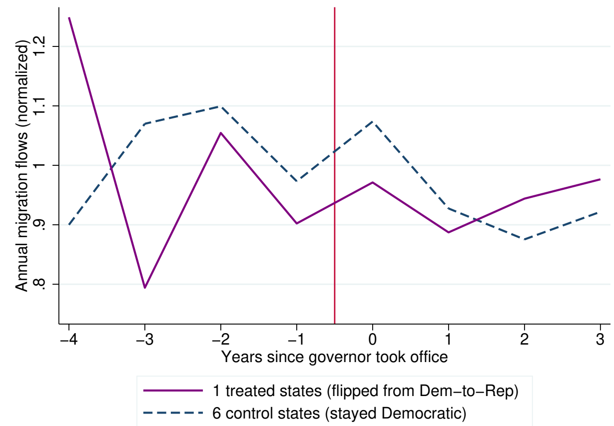
(b) Social policy

Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is index of policies in place at the state-year level, taken from Caughey and Warshaw (2016) and normalized to be mean-zero in the pre-period. Higher values indicate more conservative policies. All state-years are re-weighted to ensure that years are proportional to the number of treated states.

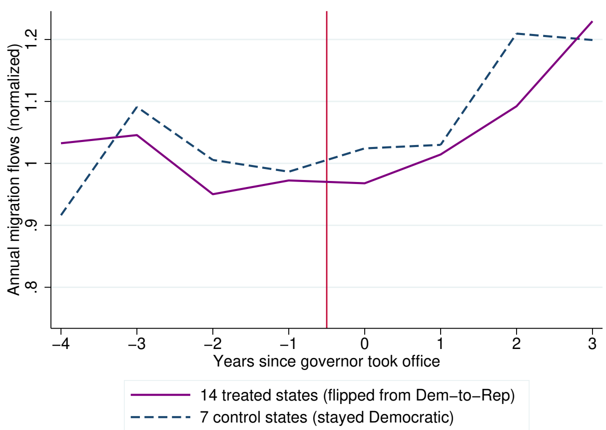
Figure B7: Effects on college in-migration by timing of D-to-R flip



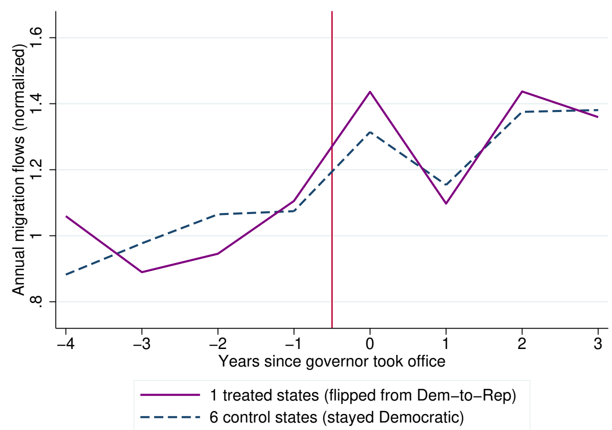
(a) 2003-2005



(b) 2006-2008



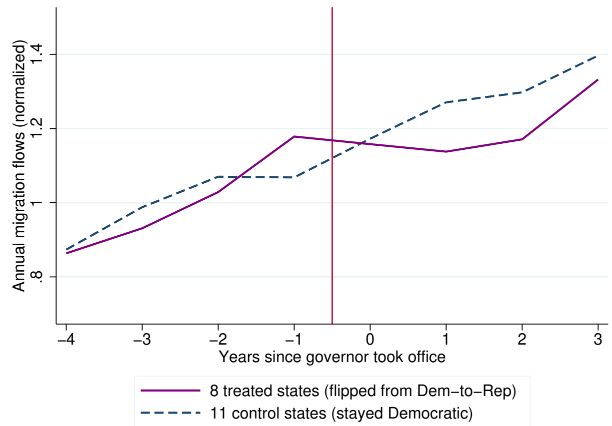
(c) 2009-2011



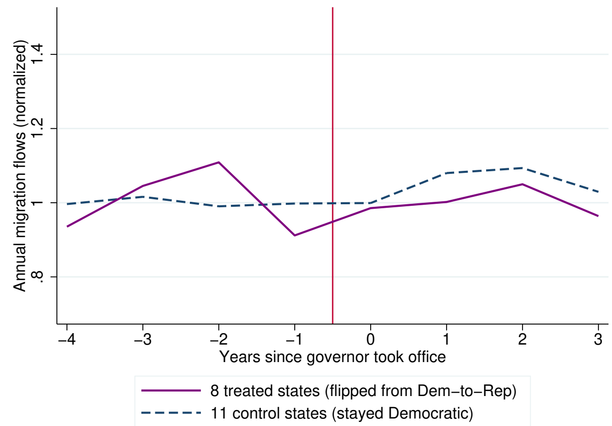
(d) 2012-2014

Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. 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.

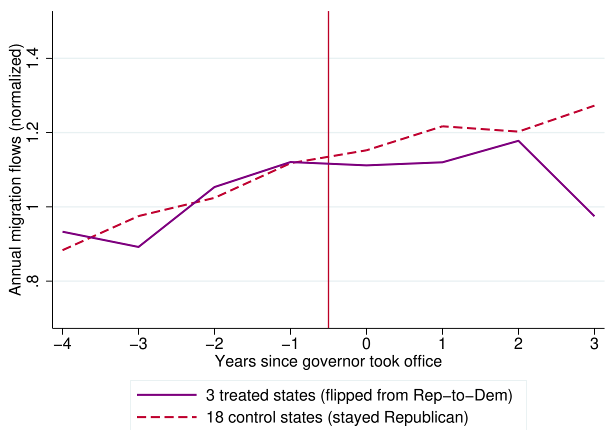
Figure B8: Other margins of migration adjustment



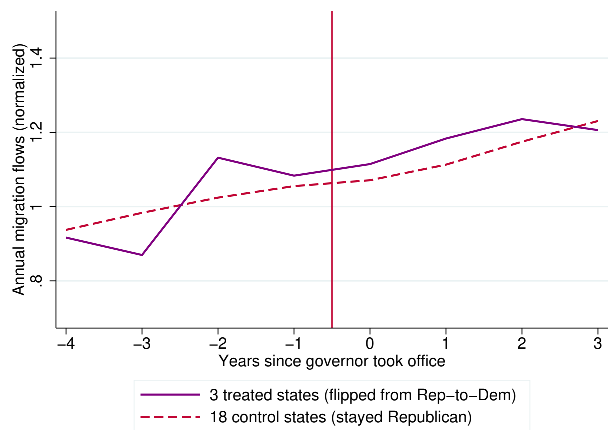
(a) D-to-R, college, out-migration



(b) D-to-R, non-college, in-migration



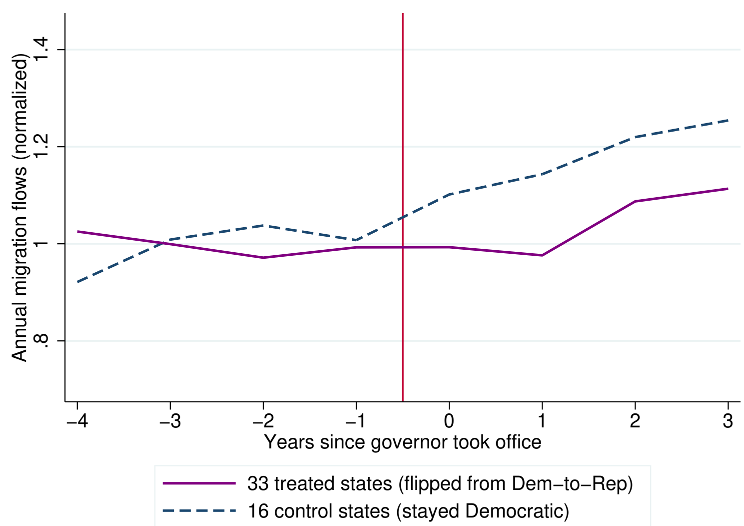
(c) R-to-D, college, in-migration



(d) R-to-D, college, out-migration

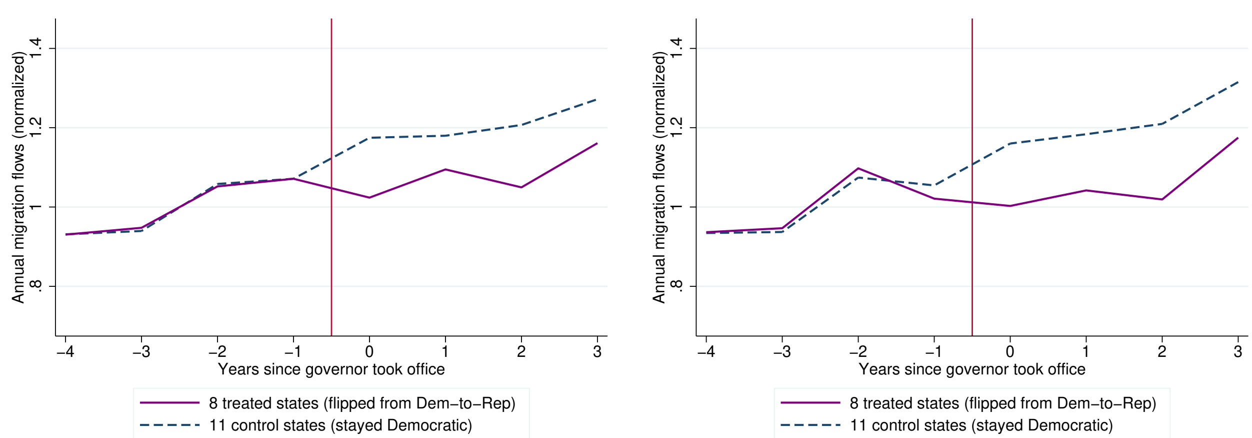
Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states are matched only with "never treated" states (which had the same pre-flip party of governor as the treated states and kept that party for the previous and subsequent 5 years). Dependent variable is normalized to be mean-one in the pre-period. 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.

Figure B9: Democrat-to-Republican gubernatorial transitions (all years)



Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. 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.

Figure B10: Migration responses for different samples of college-educated workers

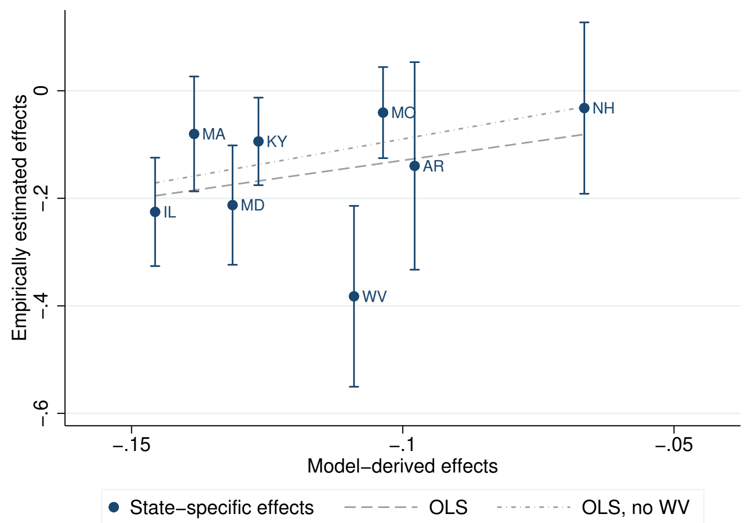


(a) All employed, age 18+

(b) Private sector employed, 26+

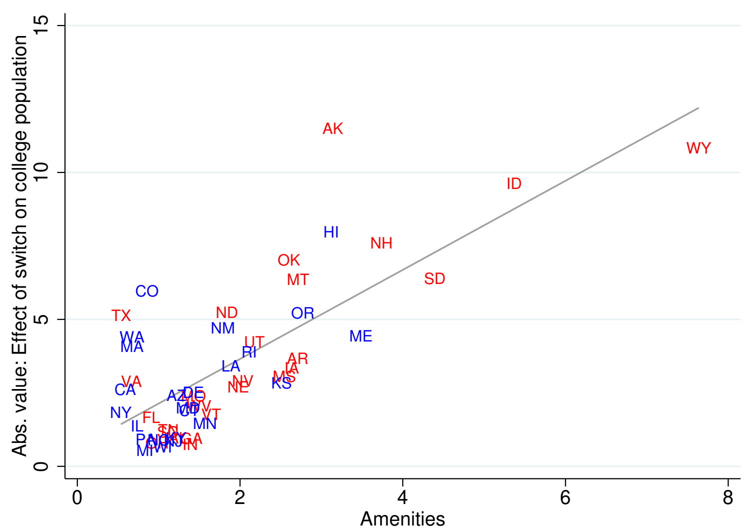
Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. 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.

Figure B11: Model-implied and empirical heterogeneity in D-to-R effects



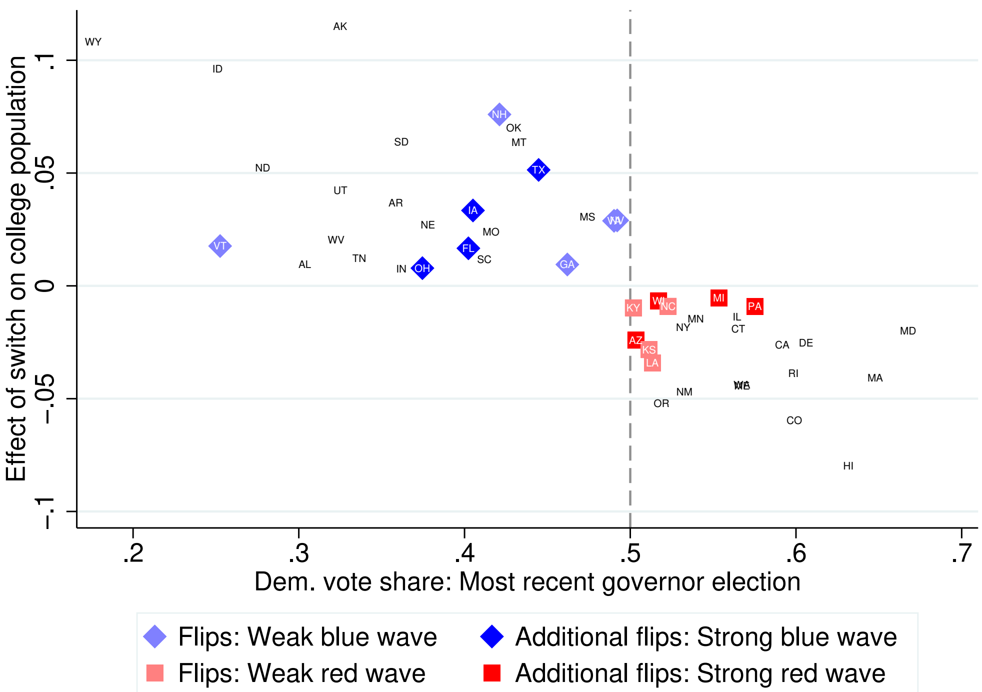
Notes: Empirical estimates (y-axis) are based on a Callaway-Sant'Anna-type estimator in which each "treated" state in individually compared to all "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). These state-specific estimates are generated for each of the eight states flipping from a Democrat to a Republican during 2015-2017 (our main sample). Model-implied effects (x-axis) are derived from our model based on simulating a counterfactual in which those eight states flipped simultaneously, and holding all other states' governors fixed as they were in 2014.

Figure B12: Amenities largely explain heterogeneous effects of a switch



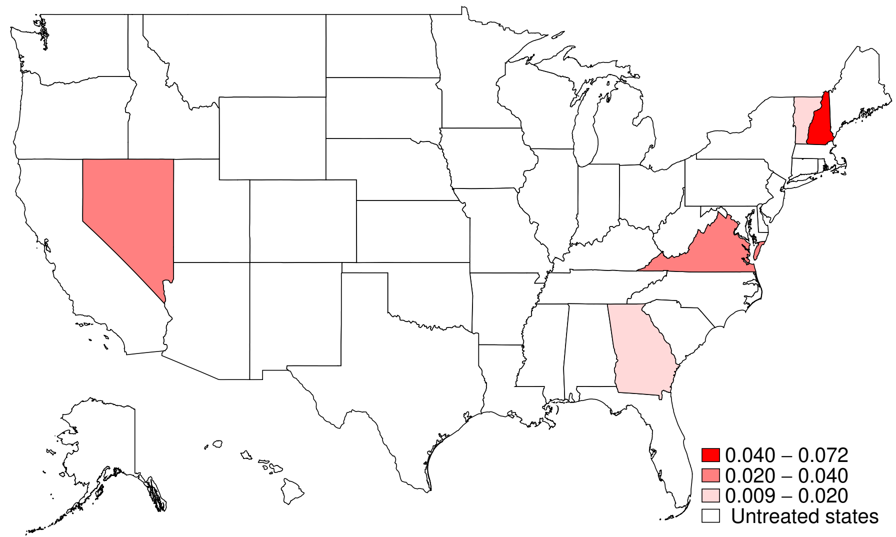
Notes: State-specific model-implied effects (y-axis) are based on 50 separate counterfactuals, each of which flips only one state independently, relative to the set of governors in office in 2023. We take the absolute value since the partisanship of the actual governor in 2023 (which determines the color of the state labels) determines the sign of the effects. Amenities (x-axis) are model-based estimates of α_d^C.

Figure B13: Swing states typically have smaller predicted effects

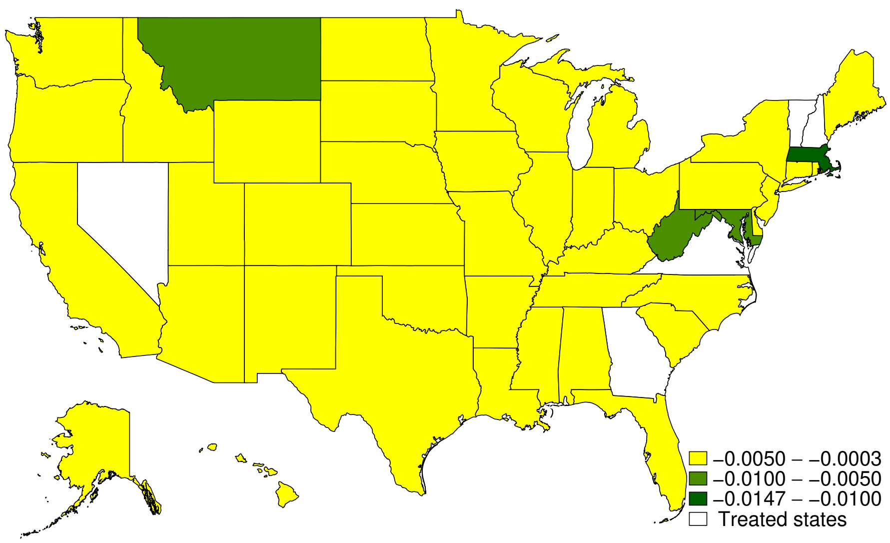


Notes: State-specific model-implied effects (y-axis) are based on 50 separate counterfactuals, each of which flips only one state independently, relative to the set of governors in office in 2023. Democratic vote share (x-axis) is based on the two-party vote share in the most recent gubernatorial election (as of 2023).

Figure B14: Direct and spillover effects of small blue wave



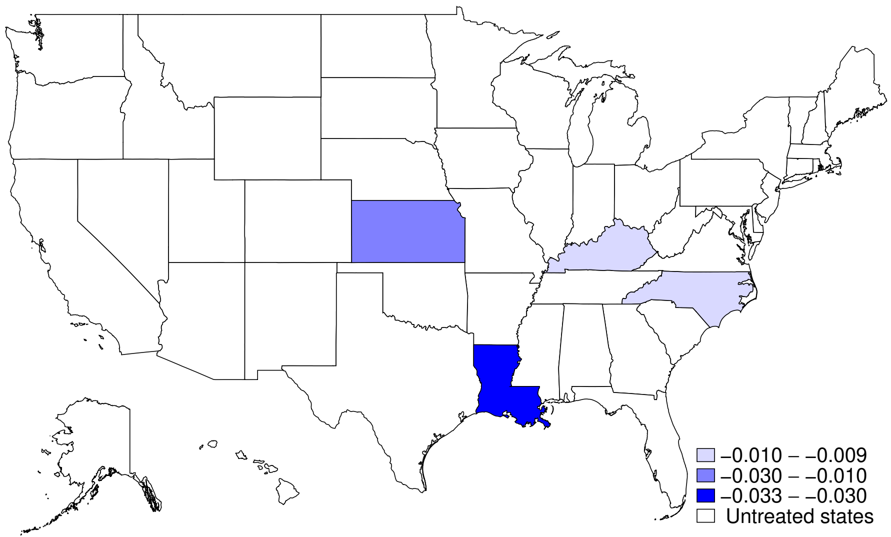
(a) Direct effects: Treated states



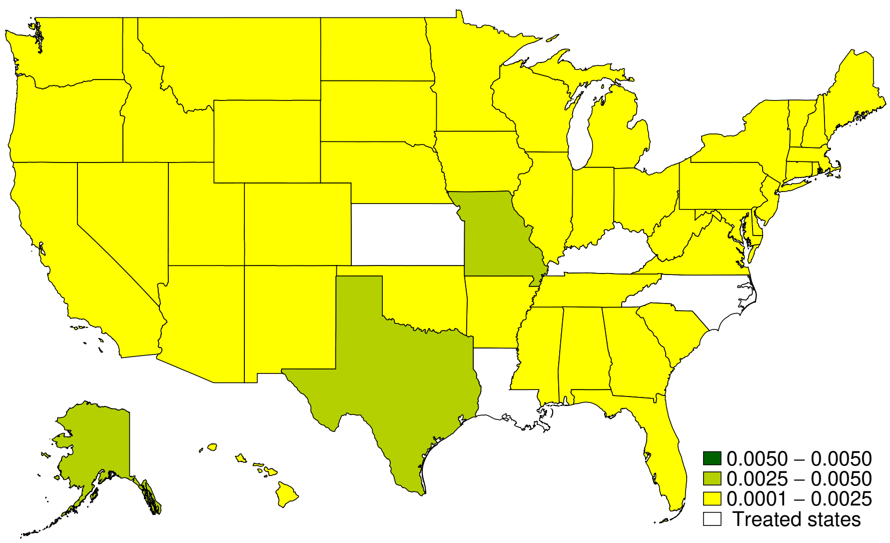
(b) Indirect spillover effects

Notes: Map based on the weak blue wave (see Figure 8) in which five states governed by Republicans in 2023 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 B15: Direct and spillover effects of small red wave



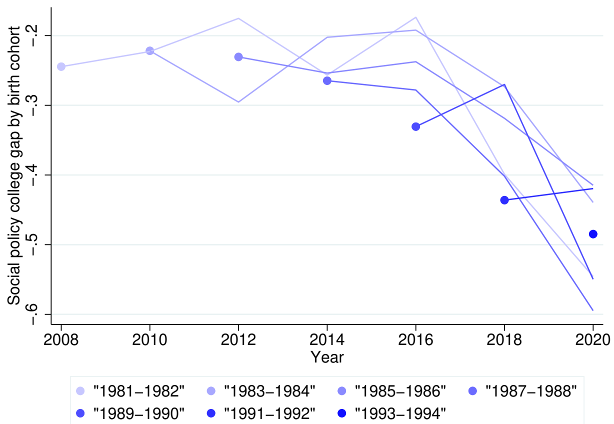
(a) Direct effects: Treated states



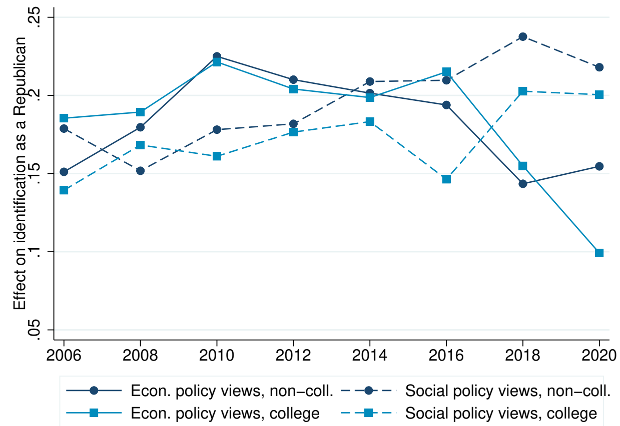
(b) Indirect spillover effects

Notes: Map based on the weak blue wave (see Figure 8) in which four states governed by Democrats in 2023 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).

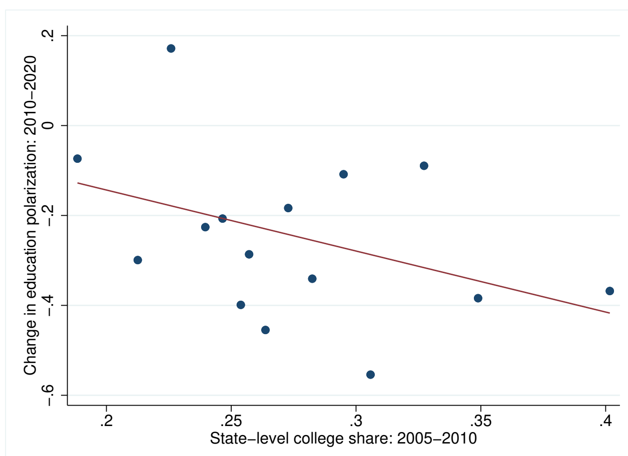
Figure B16: Assessing explanations for education polarization



(a) Education polarization by birth cohort



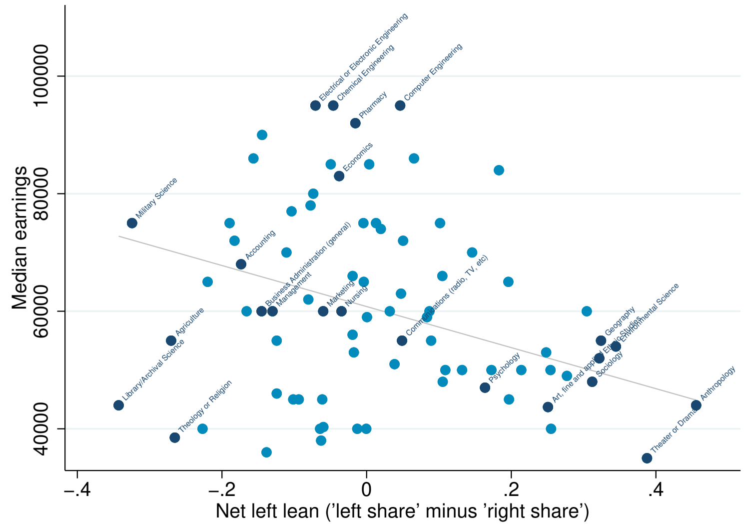
(b) Importance of social/economic policy



(c) Changes in polarization by state college share

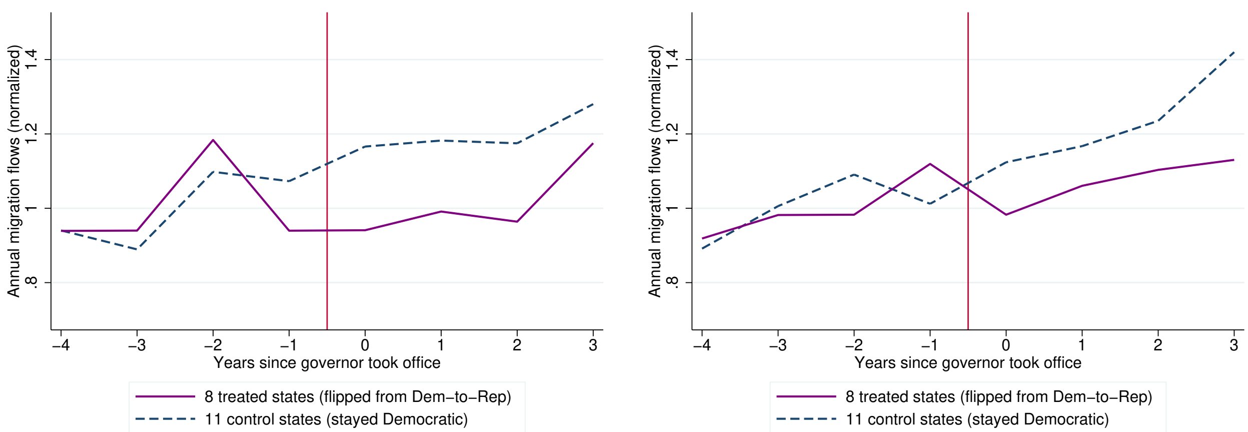
Notes: All calculations based on Cooperative Election Study (CES) respondents aged 26-45. Policy view indices are based on Item Response Theory estimates using the approach developed by Imai et al. (2016). Policy questions are divided into social and economic policy issues using the scheme developed 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 B17: Earnings and political views 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 Higher Education Research Institute (HERI) respondents who identify with "far left" or "liberal" political views minus the share who identify with "far right" or "conservative" political views.

Figure B18: Migration responses by political lean of major

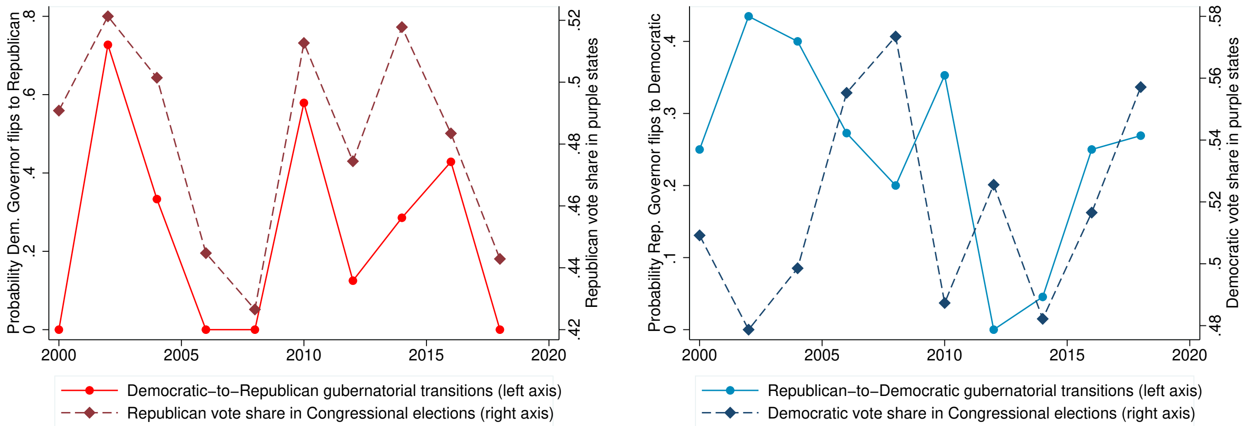


(a) More left-leaning majors

(b) Less left-leaning majors

Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. 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. In-migration is calculated separately as a function of field of degree (as recorded in the ACS). Fields are grouped based on the share of HERI respondents who identify with "far left" or "liberal" political views. They are split based on being above/below median, among the sample which migrates across state lines. Migration from 2020 onwards is never used.

Figure B19: IV intuition: National vote shares and gubernatorial election outcomes

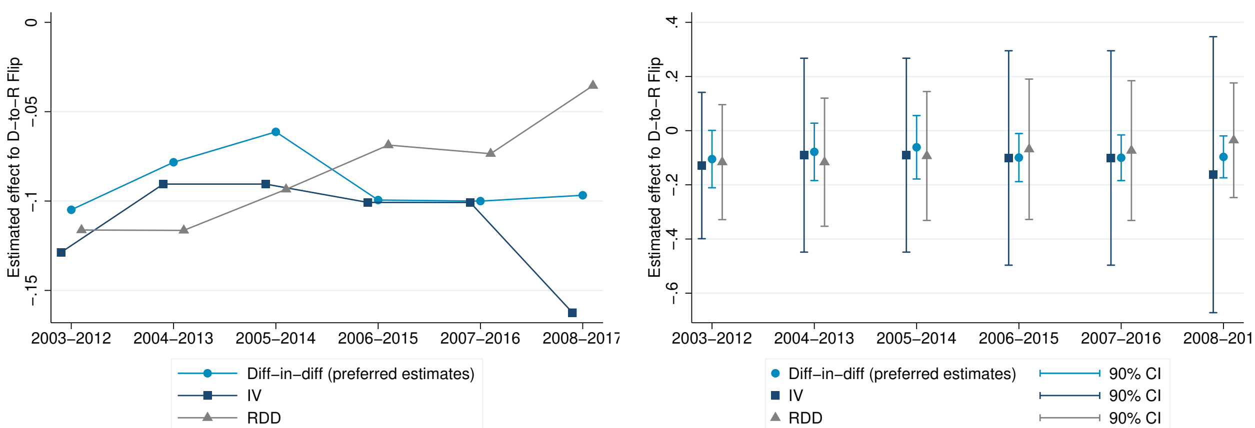


(a) D-to-R transitions

(b) R-to-D transitions

Notes: In Panel (a), the solid line displays the fraction of states *i*) with a Democratic governor and *ii*) holding a gubernatorial election, in which the Republican won; dashed line displays the Republican vote share in Congressional elections among the set of states ever observed with both Democratic and Republican governors during our sample period. In Panel (b), the solid line displays the fraction of states *i*) with a Republican governor and *ii*) holding a gubernatorial election, in which the Democrat won; dashed line displays the Democratic vote share in Congressional elections among the set of states ever observed with both Democratic and Republican governors during our sample period.

Figure B20: Alternative identification strategies by rolling 10-year window

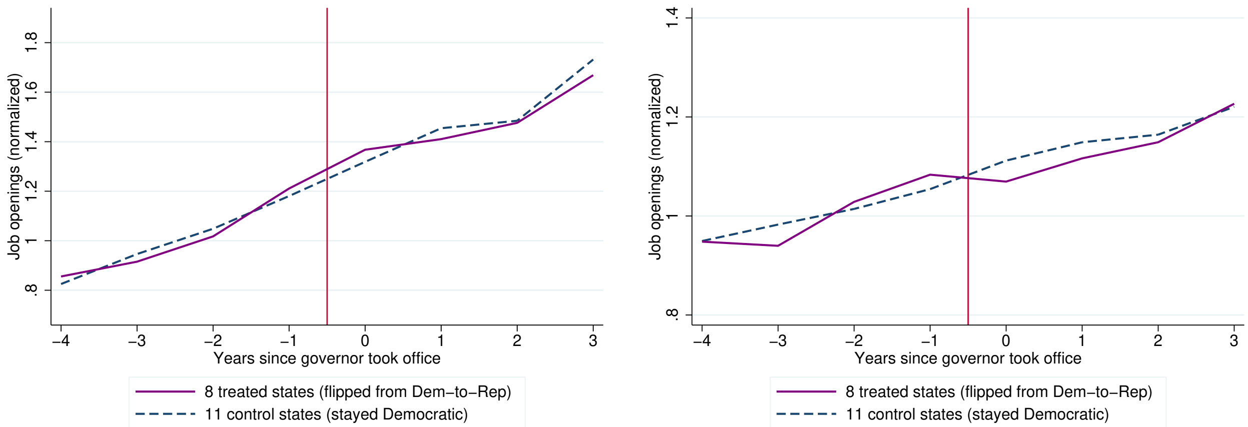


(a) No Confidence Intervals

(b) With Confidence Intervals

Notes: Diff-in-diff estimates are based on our Callaway-Sant'Anna-type estimator. IV estimates are based on the instrument discussed in equation (13) in the text based on changes in Congressional vote share. RDD estimates are based on the Calonico et al. (2014) estimate.

Figure B21: Placebo effects of Democrat-to-Republican transitions on labor demand

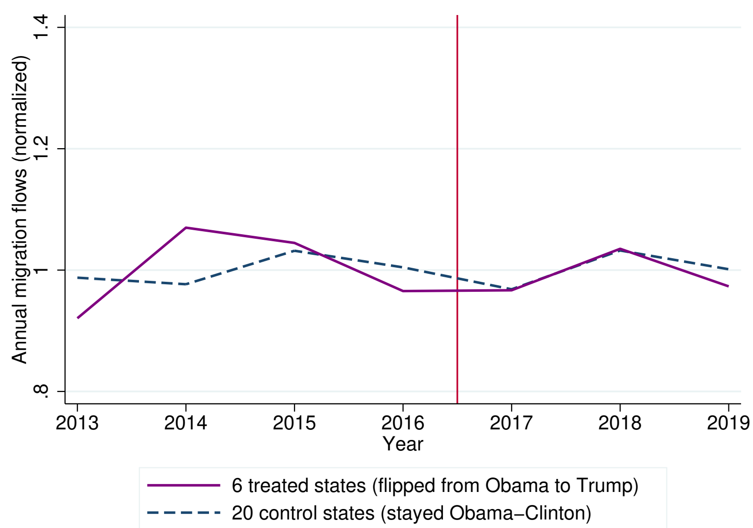


(a) Effects on job openings

(b) Effects on hiring

Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Democrat to Republican) are matched only with "never treated" states (states which had a Democratic governor for at least the previous and subsequent 5 years). Dependent variables are from JOLTS and are normalized to be mean-one in the pre-period. All state-years are re-weighted to ensure that years are proportional to the number of treated states. Openings and hiring from 2020 onwards are not used.

Figure B22: Placebo effects of 2017 Obama-Trump flips



Notes: Estimates based on a Callaway-Sant'Anna-type estimator in which "treated" states (flipping from Obama in 2012 to Trump in 2016) are matched only with "never treated" states (states which voted for Obama-2012 and Clinton-2016). Dependent variable is number of college graduate in-migrants (measured in the ACS), normalized to be mean-one in the pre-period. 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 not used.

Table B1: 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	4	8
Panel A: Human capital price, college-educated workers (w_d^C)				
Average effects	-0.009	-0.007	0.010	0.007
(min, max)	(-0.014, -0.006)	(-0.009, -0.004)	(0.005, 0.012)	(0.004, 0.012)
Panel B: Human capital price, non-college workers (w_d^N)				
Average effects	0.008	0.005	-0.007	-0.005
(min, max)	(0.005, 0.010)	(0.002, 0.008)	(-0.010, -0.004)	(-0.009, -0.003)
Panel C: Share attributable to non-college wages				
Share	.451	.433	.422	.418

Table presents changes in equilibrium outcomes for "treated" states (i.e., those changing their governors) in our four counterfactuals (Figure 8 presents the states we simulate as flipping in each counterfactual).

Table B2: GDP spillovers are proportional to college graduate spillovers

	(1)	(2)	(3)	(4)
Counterfactual	Small blue	Big blue	Small red	Big red
Effects on college workforce	0.324***	0.380***	0.305***	0.330***
	(0.044)	(0.079)	(0.049)	(0.099)
N	41	45	42	46
R^2	0.737	0.669	0.680	0.231
Mean of DV	-0.006	-0.001	0.002	0.001
SD of DV	0.004	0.001	0.002	0.001

* $p < .10$, ** $p < .05$, *** $p < .01$. Table presents changes in equilibrium outcomes for "un-treated" states (i.e., those not changing their governors) in our four counterfactuals (Figure 8 presents the states we simulate as flipping in each counterfactual). For each un-treated state, we calculate the percentage change in the college-educated workforce and the percentage change in GDP, and regress the change in GDP on the change in college graduates.

Table B3: Alternative identification strategies

	(1)	(2)	(3)	(4)
Identification:	Diff-in-diff	IV	RDD	RDD
Panel A: All flips (2003-2017)				
D-to-R Flip	-.117**	-.093	-.080	-.089
	(.045)	(.180)	(.098)	(.096)
N	853	213	91	91
R^2	.955	.142		.265
Year FE's	Yes	Yes		Yes
First stage F		46.2		
Estimator		2SLS	CCT-'14	OLS
Bandwidth			.049	.049
Panel B: Recent flips (2008-2017)				
D-to-R Flip	-.097**	-.162	-.035	-.115
	(.047)	(.311)	(.129)	(.109)
N	642	114	57	57
R^2	.957	.236		.258
Year FE's	Yes	Yes		Yes
First stage F		19.7		
Estimator		2SLS	CCT-'14	OLS
Bandwidth			.067	.067

* $p < .10$, ** $p < .05$, *** $p < .01$. Column (1) displays estimates from our preferred Callaway-Sant'Anna-type difference-in-difference estimator. Unit of observation is a state-year. Standard errors are clustered at the state level. Column (2) displays IV estimates using the instrument discussed in equation (13) in the text based on changes in Congressional vote share. Columns (3) and (4) are based on Regression Discontinuity estimates using a triangular kernel, no controls, and Calonico et al. (2014) estimated optimal bandwidth and standard errors (column (3)) and a rectangular kernel, year fixed effects, and the bandwidth chosen in the Calonico et al. (2014) estimate (column (4)). In columns (2)-(4), the dependent variable is the change in the log of average annual in-migration from the four pre-election years to the four post-election years.

C Model

C.1 The probability that an individual chooses one state

$$V_{ido} = \alpha_d^g(1-\gamma)w_d(1-\tau_{do}^g)\epsilon_{do}s_{id} = \tilde{w_{do}}s_{id}$$

Extreme value theory: U(.) is Frechet $\Rightarrow$ so is max u(.), which is V(.)
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 $F_1(.)$ 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 $\bar{T}_r \equiv -\sum_{s=1}^{N}(\frac{\tilde{w_{ro}}}{\tilde{w_{so}}})^{-\theta}$.

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

C.2 Average skill of workers

To calculate this conditional expectation, we use the extreme value magic of the Frechet distribution. Let $y_d = \tilde{w}_d s_d$ 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 distribution:

$$F(x) \equiv Pr[s^* < x] = exp\left\{-T^* x^{-\theta}\right\} \tag{16}$$

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

The expected skill of chosen d is

$$\begin{aligned}
E(s_d) &= \int_0^{\infty} sdF(s) \\
&= \int_0^{\infty} \theta T^* s^{(1-\theta)} e^{-T^* s^{-\theta}} ds
\end{aligned} \tag{17}$$

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

$$\begin{aligned}
E(s_d) =&(T^*)^{\frac{1}{\theta}} \int_0^{\infty} x^{-\frac{1}{\theta}} e^{-x} dx \\
&= (T^*)^{\frac{1}{\theta}} \Gamma(\frac{\theta-1}{\theta})
\end{aligned} \tag{18}$$

Applying this result to our equation, we have

$$
\begin{aligned}
E\left(s_d^{\theta} \mid choose\, d\, from\, o\right) &= (T^*)^{\frac{1}{\theta}}\Gamma(\frac{\theta-1}{\theta}) \\
&= \pi_{do}^{-\frac{1}{\theta}}\Gamma(\frac{\theta-1}{\theta})
\end{aligned}
\tag{19}
$$