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Does Context Outweigh Individual Characteristics in Driving Voting Behavior? Evidence from Relocations within the United States[†]

By ENRICO CANTONI AND VINCENT PONS*

We measure the overall influence of contextual versus individual factors (e.g., voting rules and media as opposed to race and education) on voter behavior, and explore underlying mechanisms. Using a US-wide voter-level panel, 2008–2018, we examine voters who relocate across state and county lines, tracking changes in registration, turnout, and party affiliation to estimate location and individual fixed effects in a value-added model. Location explains 37 percent of the cross-state variation in turnout (to 63 percent for individual characteristics) and an only slightly smaller share of variation in party affiliation. Place effects are larger for young and White voters. (JEL D12, D72, I20, J15, L82, R23)

Which is a more powerful driver of vote choice: who you are, or where you live? Rival traditions in social science attribute variations in voter preferences and behavior primarily to voters' characteristics or, instead, to the context in which they live. On one hand, large discrepancies in participation (e.g., Wolfinger and Rosenstone 1980) and policy preferences (e.g., Alesina and La Ferrara 2005) across voters of different age, race, education, income, and religiosity point to the explanatory role of individual sociodemographic factors. On the other hand, stark differences in partisanship and turnout across countries and states (e.g., Powell 1986) suggest that

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contextual factors such as voter registration and voting rules, electoral campaigns, the media environment, and the rate of economic growth also shape the behavior of voters.

Since the groundbreaking work by Siegfried (1913) and Gosnell (1930) in the early twentieth century, researchers have built a store of evidence on voting behavior differences across groups and locations. Yet, we still do not know how much individual and contextual factors matter overall. The difficulty comes from the strong correlation between these two sets of factors: due to geographic segregation by ethnicity, age, and income, states and counties vary both in their institutions and their demography, such as racial mix, average age, or affluence (Enos 2017). To be sure, recent studies have provided compelling causal evidence on the impact on participation and partisanship of *specific* factors, from individual stock ownership to voting technology and the media landscape, by exploiting naturally occurring or experimental variation in the presence of these factors (e.g., DellaVigna and Kaplan 2007; Fujiwara 2015; Jha and Shayo 2019). However, this strategy is not well suited to assess the *overall* importance of the individual and the context. Indeed, individual factors influencing voter behavior may reinforce or weaken each other, and it is impossible to pinpoint all of them; the same goes for contextual factors.

In this paper, we estimate the relative influence of individual and contextual drivers of voter behavior. Using individual-level panel data covering the vast majority of the US voting-age population from 2008 to 2018, we follow voters who relocate (henceforth *movers*) as they cross state or county lines, and test whether and to what extent their likelihood to register, vote, and affiliate with the Republican or Democratic Party adjusts to their destination's context. The size of the post-move adjustment reveals the importance of the context.

We make three distinct contributions. First, we decompose the large differences in participation and partisanship across counties and states in the shares due to individual versus contextual factors. Second, we provide evidence on the main components of place and individual effects. Third, we investigate which groups of voters are the most influenced by the context in which they live and shed new light on two longstanding political economy questions: what explains the low participation of ethnic minorities, and what shapes the behavior and preferences of young voters.

By identifying the drivers of citizens' partisanship and their decision to vote or abstain, our results also shed light on important determinants of election outcomes and, consequently, of public policies. Indeed, the distribution of partisan preferences among voters directly affects the pool of competing candidates, their policy platforms, and their vote shares. Participation decisions can be equally consequential: turnout differences across groups may result in the election of candidates who differ substantially from the preferences of the majority of the population (Meltzer and Richard 1981),¹ and low average turnout can weaken the legitimacy of elected officials and, consequently, constrain policymaking. Once elected, politicians are likely

¹ While preferences stated in surveys by nonvoters tend to be relatively similar to those of voters (Highton and Wolfinger 2001), large shifts in vote shares and policies have typically ensued from higher and more equal turnout (e.g., Bechtel, Hangartner, and Schmid 2016), particularly after the enfranchisement of women (Miller 2008), ethnic minorities (Cascio and Washington 2014), or less educated citizens (Fujiwara 2015).

to favor places and communities that vote at higher rates (Cascio and Washington 2014) or are politically aligned with them (e.g., Berry, Burden, and Howell 2010).

Our analysis relies on the largest individual-level dataset ever assembled to study voter participation and partisanship. These novel administrative data, collected and maintained by the political data vendor Catalist, LLC (2019) cover the vast majority of voting-age individuals in the United States from 2008 to 2018 and track them over time, resulting in a total of over 1.5 billion observations (approximately 250 million observations per election times six general elections).

The first part of our results focuses on voter turnout. Using our full sample, we estimate a value-added model regressing individual participation on voter, state, and election fixed effects. Our ability to jointly estimate the sets of voter and state fixed effects owes to the presence of voters observed in multiple states over time: the movers. We also estimate an event-study equivalent of our model to directly track changes in movers' turnout after move. If geographic heterogeneity in participation is entirely driven by individual characteristics, then post-move changes in turnout will be uncorrelated with differences in average participation across states of origin and destination. Conversely, if this heterogeneity is attributable to contextual factors, then movers' turnout will, after move, converge toward the average in the destination state. We find that movers' turnout jumps by 0.40 (or about 40 percent of the difference in average participation between origin and destination) after moving. In line with the event study, a decomposition based on the estimates of the value-added model obtains that state characteristics explain about 37 percent of the observed variation in voter turnout between states with above- and below-median voter turnout, and voter characteristics the residual 63 percent. A second decomposition, estimating the shares of cross-state variance in voter turnout due to voter and place characteristics, delivers the same insight: while the influence of contextual factors is considerable, it is dominated by the impact of individual factors.

Our estimates rely on two important assumptions. The first regards identification. The set of voter fixed effects included in our equations captures any difference in turnout *levels* across individuals. In particular, it allows the participation of movers to be arbitrarily different from nonmovers, and accounts for the possibility that movers with a high propensity to vote may sort to different states (e.g., states with higher average participation) than those less likely to vote.² However, we do need to assume that *changes* in individual drivers of turnout for movers do not correlate systematically with differences in average participation between their states of origin and destination—for instance, that voters who become more inclined to vote over time do not systematically move to states with higher turnout. The event studies do not show any pre-trend in movers' turnout before their move, supporting this assumption. Second, our model assumes that voter behavior is additively separable in its voter- and place-specific components. We do show that our results are robust to alleviating this assumption and allowing the state fixed effects to differ across voters of different age, gender, or race.

²We also include fixed effects for election relative to move, to permit differential average trends between movers and nonmovers such as a systematic decrease in turnout after movers arrive to their destination state, due to the need to reregister. See Section IIA for more details.

We go beyond the overall decomposition of turnout variation between voter and place effects by exploring the main components of these effects. We distinguish two broad types of contextual factors: the influence of peers on one hand, and of institutions as well as the economic and political environment on the other. Our event study shows a sharp jump immediately after move, indicating that the adjustment of movers' participation is complete by the first post-move election. Peer effects would likely take more time to materialize, suggesting that they do not contribute much to the place effects on turnout. We provide further support for this interpretation by studying within-state, cross-county moves to decompose cross-county differences in turnout between voter and county (not state) effects. Two counties of the same state share similar institutions, including identical voting rules, but their sociodemographic makeup can be very different, generating important differences in the composition of a voter's peers before and after move. Yet, the share of cross-county variation in turnout due to county effects is lower than the share of cross-state variation due to state effects, suggesting again that institutional and macro factors are responsible for most of the place effects. To identify which specific contextual factors are the most important, we go a step further and regress the state fixed effects on observable state characteristics. The strongest observable correlates of the state effects are electoral competitiveness, along with the availability of same-day registration and no-excuse absentee voting. Meanwhile, the voter characteristic that most strongly correlates with average voter turnout effects is the average level of education in the state.

The second part of the paper shifts focus to the second main dimension of voter behavior: not *whether* people vote, but *whom* they tend to vote for. While the choices of individual voters are not recorded in administrative data, of course, our data enable us to study a close proxy: individual-level party affiliation, which has been found repeatedly to be one of the strongest predictors of a person's likelihood to vote for the Republican or Democrat candidate (e.g., Bartels 2000).

A large body of evidence suggests that people's political views and partisan preferences are highly persistent and difficult to change (e.g., Campbell et al. 1960; Green, Palmquist, and Schickler 2002). Yet, our decomposition of cross-state variation in the likelihood to be affiliated with the Democratic Party, the Republican Party, or either of these two parties, reveals that place effects explain 22 to 44 percent of the observed difference between states above and below the median of these outcomes. The magnitude of the jumps we observe in the event studies tracking movers' party affiliation outcomes is consistent with these estimates.

We address two possible concerns in the interpretation of the results on party affiliation. First, some changes in this outcome may be driven by differences in primary election voting rules across the states of origin and destination rather than by actual changes in partisan leaning. However, the magnitude of the post-move changes in party affiliation only decreases slightly when we restrict the sample to moves between pairs of states with identical rules. Second, we only observe party affiliation conditional on people being registered and define the corresponding outcomes to zero for unregistered voters, to avoid sample selection issues. As a result, effects on party affiliation outcomes may capture effects on registration itself. We circumvent this issue by proposing a bounding strategy inspired from Lee (2009) to measure place effects on an outcome that is as close as possible to

partisanship: being affiliated with the Democratic Party (or with the Republican Party), conditional on being registered and on being affiliated with either of the two major parties. We find that place effects also exert a large influence on conditional party affiliation.

While contextual factors explain party affiliation and participation to a similar extent overall, the detailed picture differs. In the event-study graphs, we observe reassuringly flat pre-move patterns for both outcomes. However, while turnout jumps sharply post-move, party affiliation changes more gradually, especially if we restrict the sample to states with identical primary rules. In addition, the fraction of cross-county differences explained by county fixed effects is much higher for party affiliation than for turnout. Furthermore, state effects for Democratic Party affiliation and for affiliation with either major party correlate with a sociodemographic characteristic—average household income—while state effects from turnout and registration decompositions do not. Taken together, these different pieces of evidence suggest that peer effects contribute to the impact of place on partisanship more than on participation. On the other hand, education remains one of the most important correlates of average voter effects on Democratic and Republican Party affiliation, similarly as for turnout, along with median age and “universalist” versus “communal” values.³

Finally, we compare the relative influence of contextual and individual factors for voters of different ages, genders, and races. Place effects explain a larger share of the cross-state variation in registration, turnout, and, to a lesser extent, party affiliation, for younger relative to older voters. This result is in line with the impressionable years hypothesis, which posits that the first years after young voters come of age critically shape their behavior (Mannheim 1952). The decomposition of cross-state variation in party affiliation between individual and contextual factors is very similar across Whites and non-Whites, but place effects explain a larger share of cross-state differences in voter registration for ethnic minorities and, perhaps surprisingly, a *smaller* share of the differences in their average turnout. A common explanation for the low political participation of minority voters points to disenfranchising rules such as strict ID laws, felony disenfranchisement laws, and regulations allowing extensive voter roll purges, which can disadvantage these voters and are sometimes implemented more stringently against them (e.g., White, Nathan, and Faller 2015). Yet, these rules are only present in a subset of states. Therefore, the modest difference in average minority turnout between states above versus below the median that is due to place effects suggests that contextual factors present across all states and individual factors are at least as important, and that they deserve more attention than they receive today. In contrast to the heterogeneity we find across age and race, the decomposition of cross-state variation in voter behavior is very similar for men and women.

Our paper contributes to the literature studying the determinants of voter behavior by exploring this question from an entirely new perspective. Existing studies, including some of our own, tend to focus on a *specific* contextual factor, and they use variation in time, across space, or across individuals, to estimate its impact. Some papers exploit state-level changes in voting rules, including voter registration

³ Universalist values include individual rights and impartial fairness, and communal values include community, loyalty, and tradition (Haidt 2012).

laws, compulsory voting, early voting, election day registration, or voter ID requirements (e.g., Hoffman, León, and Lombardi 2017; Kaplan and Yuan 2019; Cantoni and Pons 2021). Others leverage variation—either naturally occurring or introduced by experimental manipulation—in localities and individuals' exposure to some of the aforementioned voting rules as well as other contextual factors, including voting technologies, electoral campaigns, the media, favorable economic context, and neighborhood composition (e.g., Gerber and Green 2000; Adena et al. 2015; Braconnier, Dormagen, and Pons 2017; Perez-Truglia 2018; Pons 2018). A complementary strand of the literature uses similar forms of experimental or quasi-experimental variation to document the effects of voter characteristics such as education, income, stock ownership, or religiosity (e.g., Milligan, Moretti, and Oreopoulos 2004; Gerber, Gruber, and Hungerman 2016; Kaustia, Knüpfer, and Torstila 2016).

Our goal is diametrically different. Instead of focusing on a specific dimension, we assess the *overall* importance of all relevant place and voter-driven factors. Previous research was not equipped to address this question, for three reasons. First, factors co-move: states may adopt a bundle of new policies in an effort to facilitate participation, and richer individuals tend to also be older and more educated. Single-factor studies see these correlations as a threat. In fact, they are primarily judged based on their ability to isolate the impact of one factor from the influence of correlated variables. In contrast, the place effects that we measure capture these co-movements. Second, separate factors may reinforce or weaken each other. We measure the total net effect of these interactions, which existing studies only explore partially. Third, quasi-experimental designs leveraging exogenous differences in the presence of a factor cannot assess the influence of determinants that are unobserved or that do not offer such variation: voting rules that differ across space but not over time, for example, or factors such as race, gender, or age, which unlike income or education are inherent to an individual. On the other hand, multivariate regressions of participation or partisanship can control for these factors, but they measure correlations that are not necessarily causal and they remain silent about unobserved dimensions. Instead, our estimates based on changes in movers' behavior compare the combined influence of *all* factors varying across states and across individuals, and we provide evidence supporting a causal interpretation of the results.

Additionally, we contribute to strands of the literature that have focused on particular groups of voters, including work investigating specific forces such as civic education and peers' influence that shape the choices of young voters and may affect their behavior in the long run (Neundorff and Smets 2017), and disenfranchising laws and other causes responsible for the low political participation of ethnic minorities (e.g., Filer, Kenny, and Morton 1991; Cantoni 2020). We shed new light on these questions by implementing our decompositions for voters of different ages, genders, and races, and by comparing the overall influence of the context and the individual across groups.

The evidence we provide on the most significant correlates of place and individual effects improves on multivariate regressions of voter turnout and partisanship (e.g., Verba, Schlozman, and Brady 1995) by using the state and voter components of these outcomes on the left-hand side. While this part of our results does not necessarily warrant a causal interpretation, an important strength is that we are able to estimate and compare the influence of a large number of factors in a unique

setting, in contrast to the individual single-factor studies above, which obtain their results in a large patchwork of different places and times. Our setting is also unusually broad, since it covers the close-to-entire population of the world's richest country over the entire last decade.

Methodologically, we draw on value-added models estimated in other settings, in particular on a number of recent studies that, like ours, track movers across states, companies, or schools to investigate the sources of spatial variation in health care utilization (Finkelstein, Gentzkow, and Williams 2016), intergenerational mobility (Chetty and Hendren 2018), and brands' market shares (Bronnenberg, Dubé, and Gentzkow 2012), wage differences across workers and companies (Abowd, Kramarz, and Margolis 1999; Card, Heining, and Kline 2013), and variations in students' outcomes (Chetty, Friedman, and Rockoff 2014). We are particularly indebted to the empirical framework laid out in Finkelstein, Gentzkow, and Williams (2016). To the best of our knowledge, we are the first to study voter behavior using this empirical strategy. We also extend the method by proposing a way to measure effects on outcomes (here, party affiliation) that are only observed conditional on movers adopting a certain behavior (here, registering to vote).

Finally, while our focus on movers is primarily driven by the goal to disentangle the influence of individual- and state-level factors, it allows us to make a substantial contribution to the literature on the political motives (e.g., Hirschman 1970; Bishop 2009) and effects (e.g., Squire, Wolfinger, and Glass 1987; Gay 2012) of spatial mobility: we do not find evidence that spatial sorting across states is driven by gradual changes in movers' level of political participation but we do observe a systematic drop in participation after the move.

The remainder of the paper is organized as follows. Section I provides more information on Catalist's voter-level panel data and Section II lays out the empirical specifications. Section III presents the results for turnout and Section IV the results for registration and party affiliation. Section V compares the importance of contextual factors for voters of different age, gender, and race, and Section VI concludes.

I. Data

A. *Catalist's Voter-Level Panel Data*

Our empirical strategy requires observing both individual voter behavior (registration, turnout, and party affiliation) and place of residence for the universe of the US voting-eligible population at multiple elections to track movers' behavior as they cross state (or county) boundaries.

Outside of the United States, voter registration and turnout records are rarely available beyond individual municipalities. In addition, in most countries, administrative data on party affiliation simply do not exist, which would make it impossible to study partisan preferences using our design. In contrast, 30 American states record people's party affiliation when they register to vote—in many cases, to determine eligibility to participate in the primary elections of the Democratic and Republican parties.

Still, building a panel spanning the entire US territory is challenging, because files commercialized by political data vendors typically contain voters' residential

information as of the day the records are purchased, but lack any information on movers' previous addresses. Fortunately, Catalist's data allow us to overcome this limitation.

Catalist is a political data vendor that maintains a national database of over 256 million unique voting-age individuals. Information on registered voters comes from voter registration and turnout records collected from all 50 states and the District of Columbia. These administrative data are supplemented by commercial records on unregistered individuals provided by data aggregation firms and based on customer files from retailers and direct marketing companies.

Catalist continually updates its database by incorporating new state voter files as well as commercial data refreshes, and it identifies deceased voters based on the Social Security Death Master File (SSDMF) datasets. Crucially for our ability to follow movers across states, Catalist also identifies people changing addresses based on records in the United States Postal Service National Change of Address (NCOALink) and by systematically comparing the voter lists and commercial records of different states. Catalist gives each person a unique ID, invariant across years and files, and uses data matching procedures to identify potential matches across files. For example, if a voter registered with the first name "Bob," but commercial records include an individual called "Robert" with the same last name, address, and sociodemographic characteristics, Catalist will recognize that it is the same individual and reconcile the two entries (Ansolabehere and Hersh 2014).

The files we received from Catalist contain longitudinal information on each individual's state and county of residence and on their voting behavior. To the best of our knowledge, we are the first researchers to use voter-level panel data on geographic residence, registration, turnout, and party affiliation covering the vast majority of the US voting-eligible population. According to these data, about 4.3 million voters (resp. 5.5, 4.9, 6.0, and 6.6 million) moved across state borders between 2008 and 2010 (resp. 2010–2012, 2012–2014, 2014–2016, and 2016–2018). The corresponding numbers of within-state cross-county movers are, respectively, 5.5, 8.3, 6.9, 12.5, and 9.5 million voters.

Catalist's data also contain age, race, and gender. This information is available for nearly all voters and has been shown to be very reliable (Fraga 2016). Other variables in the Catalist's full database are only available for a subset of individuals or at a more aggregate level (such as the census block). We did not request them out of budgetary considerations.

While an in-depth assessment of the Catalist's database is beyond the scope of this project,⁴ it is important to note two limitations of the data. First, Catalist's coverage of the unregistered population is imperfect. In fact, Catalist acknowledges that the commercial data used for unregistered citizens cover the voting-age (VAP), rather than the voting-eligible population (VEP). Moreover, Jackman and Spahn (2021) estimate that at least 11 percent of the adult citizenry does not appear in commercial voter lists like Catalist's. Second, Ansolabehere and Hersh (2014) argue that Catalist's deceased flag misses some dead voters, making the total number of

⁴See Ansolabehere and Hersh (2014) for a thorough discussion of Catalist's database and the underlying data collection and maintenance practices. Other papers using *cross-sectional extractions* of Catalist's data (not the panel data, like we do) include Nickerson and Rogers (2014); Fraga (2016); and Hersh and Nall (2016).

deceased voters in the voter file lower than it really is. The mis-categorization of some deceased voters and commercial data covering the VAP instead of the VEP likely explain why Catalist's state turnout rates are lower than those compiled by McDonald (2021).⁵

Despite this discrepancy in levels, two-way and Spearman's rank correlations between Catalist's and McDonald's state-by-year turnout rates are very high (respectively 0.90 and 0.89), assuaging concerns that cross-state heterogeneity in the quality of Catalist's state registration records may bias our estimates. Moreover, our event-study results are very similar when we compute mean state turnout using data from McDonald (2018) instead of Catalist (see Section IIIB).

Further details on the Catalist panel data are given in online Appendix A.1.

B. Summary Statistics

Our estimation strategy, described in detail in Section II, relies on tracking the voting behavior of cross-state movers. Our sample includes a total of 14.3 million one-time movers, who crossed state boundaries exactly once. Online Appendix Table A.1 shows the fraction of movers who moved between any two of the nine census divisions: East North Central, East South Central, Middle Atlantic, Mountain, New England, Pacific, South Atlantic, West North Central, and West South Central. Overall, 72.3 percent of cross-state movers moved across census divisions. The number of one-time within-state cross county movers is larger: 22 million. For simplicity, all analyses exclude voters who change states more than once.⁶ Table 1 reports summary statistics separately for one-time movers and nonmovers, who never cross state borders in the study period. On average, movers are more likely to be White and women than nonmovers, and they are slightly younger. They have higher registration and turnout rates and are more likely to be affiliated with the Republican Party or registered but affiliated neither with the Republicans nor with the Democrats. Online Appendix Figure A.1 plots the distributions of destination-minus-origin differences in mean state turnout, registration, and party affiliation (being registered and affiliated with either of the two major parties, with the Republican Party, and with the Democratic Party) for one-time movers. All distributions are roughly symmetric and the average differences in outcomes are approximately zero, which implies that moves from low- to high-participation states or moves from red to blue states are as frequent as moves in the opposite directions.

⁵McDonald's turnout figures are widely considered the most reliable estimates of the share of the state voting-eligible population turning out in a particular election. See McDonald and Popkin (2001) for a discussion on how these rates are computed.

⁶As shown in online Appendix Table A.3, our results are virtually unchanged when we include movers who change states multiple times, using a specification in the form of equation (1), in which $\rho_{i,t}$ is redefined as a set of fixed effects for election relative to the *first* move, along with sets of fixed effects for election relative to the second, third, fourth, and fifth move. Since our sample includes six elections, a voter can move five times at most.

TABLE 1—SUMMARY STATISTICS ON MOVERS AND NONMOVERS

	Nonmovers (1)	Movers (2)
Female	0.527	0.545
Non-Hispanic White	0.734	0.834
Non-Hispanic Black	0.118	0.083
Other race	0.051	0.034
Hispanic	0.096	0.049
<i>Age</i>		
Missing values	0.102	0.026
Mean	49.01	47.78
Standard deviations	18.21	18.19
Voted	0.422	0.518
Registered	0.681	0.770
<i>Party registration</i>		
Living in a state recording party registration	0.556	0.592
...and registered as Democrat	0.160	0.165
...and registered as Republican	0.112	0.147
...and registered for other party or unaffiliated	0.106	0.145
...and unregistered	0.178	0.135
Average N elections observed	4.29	5.44
Number of voters	348,147,968	14,337,595
Number of voter-years	1,494,237,077	77,988,312

Notes: Columns 1 and 2 report summary statistics, respectively, in the samples of nonmovers (i.e., voters who never cross state borders) and movers (i.e., voters who cross state borders exactly once).

II. Empirical Specifications

A. Decomposition of Voter Behavior Differences across States

The first part of our analysis aims to estimate the share of differences in voter behavior across states (or counties) that results from differences in contextual factors instead of differences in the individual characteristics of the people living in each state. This decomposition is based on the following equation:

$$(1) \quad y_{ijt} = \alpha_i + \gamma_j + \tau_t + \rho_{r(i,t)} + \varepsilon_{ijt},$$

where y_{ijt} is a binary outcome for voter i living in state j at election t . (For the decomposition of differences across counties, j indicates the county.)⁷ The variables α_i , γ_j and τ_t denote voter, state, and election fixed effects, respectively. Election fixed effects are normalized to be equal to zero on average. For movers, $r(i,t) = t - t_i^*$ is the election relative to the first post-move election t_i^* (so $r(i,t) = 0$ if t is the first election after the move, $r(i,t) = -1$ if t is the last election before the move, etc.) and $\rho_{r(i,t)}$ indicates fixed effects for election relative to move. We assume additive separability in i, j , and t and $E(\varepsilon_{ijt}|i,j,t) = 0$.

We estimate the parameters in this equation using all movers and nonmovers in the Catalist database. The equation is only identified because the data include

⁷Henceforth, with “voter” we mean any registered or unregistered individual appearing in the Catalist data.

movers. Otherwise, the state fixed effects γ_j would be absorbed by the individual fixed effects α_i .

Estimating equation (1), we pursue two objectives. First, we want to estimate the total contribution of state-specific characteristics (such as voting rules and the media) and voter-specific factors (such as race and education) to cross-state variation in voter behavior. Second, we aim to decompose the *share* of variation in voter behavior due to these two sets of factors and, thus, determine the relative influence of state and voter characteristics on registration, turnout, and party affiliation.

Our decomposition between these two types of factors follows Finkelstein, Gentzkow, and Williams (2016). Let \bar{y}_{jt} be the expectation of y_{ijt} across voters living in state j in election t , and \bar{y}_j be the average of \bar{y}_{jt} across t ; \bar{y}_{jt}^{vot} and \bar{y}_j^{vot} denote the analogous expectations for the part of voter behavior imputable to voter characteristics, $y_{it}^{vot} = \alpha_i + \rho_{r(i,t)}$. Using this notation, equation (1) implies that $\bar{y}_j = \bar{y}_j^{vot} + \gamma_j$ and, for any two states j and j' ,

$$(2) \quad \bar{y}_j - \bar{y}_{j'} = (\gamma_j - \gamma_{j'}) + (\bar{y}_j^{vot} - \bar{y}_{j'}^{vot}).$$

Equation (2) shows that the difference in average voter behavior across states j and j' , $\bar{y}_j - \bar{y}_{j'}$, is the sum of two components. The first component, imputable to state-specific factors, is given by the difference between the corresponding state fixed effects: $\gamma_j - \gamma_{j'}$. The second component, due to voter characteristics, is given by the difference between the voter-specific components: $\bar{y}_j^{vot} - \bar{y}_{j'}^{vot}$.

The shares of the difference in voter behavior between states j and j' attributable to states and voters are then given by, respectively,

$$(3) \quad S^{state}(j, j') = \frac{\gamma_j - \gamma_{j'}}{\bar{y}_j - \bar{y}_{j'}},$$

$$(4) \quad S^{voter}(j, j') = \frac{\bar{y}_j^{vot} - \bar{y}_{j'}^{vot}}{\bar{y}_j - \bar{y}_{j'}} = 1 - S^{state}(j, j').$$

Although $S^{state}(j, j')$ and $S^{voter}(j, j')$ sum to one, neither needs to be within the unit simplex, since $\gamma_j - \gamma_{j'}$ and $\bar{y}_j^{vot} - \bar{y}_{j'}^{vot}$ can have opposite signs. When we apply our decomposition to the difference in behavior between groups of states, \bar{y}_R , \bar{y}_R^{vot} , and $\bar{\gamma}_R$ denote the simple averages of \bar{y}_j , \bar{y}_j^{vot} , and γ_j across the states in group R .⁸ Similarly, we define $S^{state}(R, R')$ and $S^{voter}(R, R')$ as the shares of differences in voter behavior between states in groups R and R' attributable to states and voters, respectively. We compute the sample analogues of \bar{y}_j directly from the Catalist data and denote them \hat{y}_j . We obtain consistent estimates $\hat{\gamma}_j$ of γ_j from estimating equation (1) and derive consistent estimates of \bar{y}_j^{vot} by subtracting $\hat{\gamma}_j$ from \hat{y}_j : $\hat{y}_j^{vot} = \hat{y}_j - \hat{\gamma}_j$. The shares $S^{state}(R, R')$ and $S^{voter}(R, R')$ reflect the influence of all the place and individual factors varying across states. By contrast, they are unaffected by, and silent about, the influence of contextual factors present across all states.

⁸Online Appendix Tables A.4 and A.5 show the robustness of our results to replacing \bar{y}_R , \bar{y}_R^{vot} , and $\bar{\gamma}_R$ with averages weighted by McDonald's estimates of the voting-eligible population of states (averaged across the six elections in our sample).

Equation (1) allows for arbitrary differences in outcome levels across voters. In particular, via the α_i s, the mean behavior of movers can be arbitrarily different from that of nonmovers without biasing our estimates. Moreover, fixed effects for election relative to move $\rho_{r(i,t)}$ permit differential *trends* in voter behavior across movers and nonmovers. Such differential trends may arise for example for turnout and registration, if movers face a cost of reregistering to the voter rolls of the state of destination (Squire, Wolfinger, and Glass 1987) or if the loss of pre-existing social ties associated with moving decreases civic engagement (Gay 2012).

Despite the flexibility given by the voter and relative election fixed effects, our model is restrictive in three important ways.

First, like in other studies using movers to estimate value-added models, the crucial identifying assumption required to uncover unbiased estimates from equation (1) is that *changes* in individual drivers of voter behavior for movers do not correlate systematically with differences in average outcomes between their states of origin and destination. Importantly, the influence of individual factors that do not change over time is captured by the individual fixed effects, so we do not need to assume that the *level* of individual factors is uncorrelated with state differences. For instance, the possibility that voters who have long felt close to the Democratic Party sort to blue states does not threaten our identification. What does is if voters whose preferences converge to the Democratic Party platform over time disproportionately follow this trajectory or if voters who become more politically engaged respond by moving to relatively high-turnout states.

We can empirically test for changes of this type that develop *gradually*. Gradual changes in individual drivers of movers' behavior that correlate with outcomes in the origin and destination would appear as pre-trends in the event-study analysis described in Section IIB. We find little evidence of this, which indicates that our event-study estimates do not mistakenly capture underlying changes in movers' individual characteristics and reinforces our confidence in the decomposition of cross-state differences in voter behavior based on equation (1).

In contrast, we do not have any direct way to test for the presence of shocks to movers' behavior that coincide *exactly* with the year of the move or take place in the following years, and that also correlate with outcome differences between origin and destination. Importantly, sudden shocks that are uncorrelated with origin-minus-destination outcome differences are orthogonal to the state fixed effects γ_j . Thus, they simply enter the error term ε_{ijt} and do not threaten the validity of our estimates. Furthermore, we check the robustness of our results to excluding voters below 25 or above 60, who may be affected by particularly impactful shocks such as entering or exiting the labor market. Reassuringly, the results of our decompositions remain very similar in this subsample (see online Appendix Table A.3).

Second, equation (1) assumes that voter behavior is additively separable in its voter- ($\alpha_i + \rho_{r(i,t)}$) and state-specific components (γ_j). Since relative election effects $\rho_{r(i,t)}$ do not depend on the specific states of origin and destination, additive separability of voter and state effects implies that the absolute change in voter behavior for voters moving from j to j' (experiencing a change in state factors equal to $\gamma_{j'} - \gamma_j$) should, net of the effects of the $\rho_{r(i,t)}$, be the same as for voters moving from j' to j (experiencing a change in state factors equal to $\gamma_j - \gamma_{j'}$). We present a test of this implication in Section IIIA for voter turnout and Section IVA for the

other outcomes. A second implication is that state fixed effects estimated based on movers of different races, genders, and ages should be of similar magnitude. We test this implication and show the robustness of our decompositions to including race-, gender-, or age-specific state fixed effects in Section V.

Finally, we assume that movers and nonmovers face identical state effects γ_s . If movers differ from nonmovers in ways that alter the relevant state effects or if state effects also capture state-specific deviations from the average fixed effects for election relative to move $\rho_{r(i,t)}$ (e.g., due to cross-state variations in the cost of re-registering after moving), then our decomposition between state- and individual-level determinants of voter behavior only applies to movers, and not to the rest of the population.

B. Event-Study Specification

To trace out changes in voter behavior around moves, we also estimate an event-study equivalent of equation (1). For voter i who moves from origin state $o(i)$ to destination state $d(i)$, equation (1) can be rearranged as

$$(5) \quad y_{ijt} = \alpha_i + \gamma_{o(i)} + I_{r(i,t) \geq 0} \times S^{state}(d(i), o(i)) \times \delta_i + \tau_t + \rho_{r(i,t)} + \varepsilon_{ijt},$$

where δ_i is the difference in average outcomes between i 's states of destination and origin, $\bar{y}_{d(i)} - \bar{y}_{o(i)}$, and $I_{r(i,t) \geq 0}$ is an indicator for post-move elections.⁹

Combining $\alpha_i + \gamma_{o(i)}$ into a single voter fixed effect $\tilde{\alpha}_i$, replacing $I_{r(i,t) \geq 0}$ with indicators for election relative to move, and replacing δ_i with its sample analogue $\hat{\delta}_i = \hat{y}_{d(i)} - \hat{y}_{o(i)}$ (computed using both movers and nonmovers), we obtain the following event-study specification:

$$(6) \quad y_{it} = \tilde{\alpha}_i + \theta_{r(i,t)} \hat{\delta}_i + \tau_t + \rho_{r(i,t)} + \varepsilon_{it}.$$

The parameters of interest are the $\theta_{r(i,t)}$ s. In relative election $r(i,t)$, $\theta_{r(i,t)}$ measures movers' response to differences in average outcomes between states of destination and origin. Assuming heterogeneity in S^{state} is orthogonal to the other terms in the model (and in particular to δ_i), $\theta_{r(i,t)}$ is a weighted average of $S^{state}(d(i), o(i))$, with weights given by the relative frequency of all pairs of origin and destination states.

The pattern of estimated effects offers indirect tests of our identification assumption: if move-induced changes in state characteristics cause changes in movers' behavior, then $\theta_{r(i,t)}$ should be approximately flat in all pre-move elections. For $r(i,t) \geq 0$, $\theta_{r(i,t)}$ s describe the extent to which post-move voter behavior adjusts to the difference in average outcomes between states of destination and origin. Namely, a discontinuity in the level of $\theta_{r(i,t)}$ after the move indicates how much state-level factors influence individual-level voter behavior. Moreover, the pattern of post-move coefficients can illuminate the underlying mechanisms: effects that appear suddenly on move and then remain stable suggest that discrete factors that are easy to get accustomed to (e.g., election laws) are important drivers of voter behavior, while effects that increase over time underscore the importance of "slow-moving" factors

⁹To recover equation (1), observe that $S^{state}(d(i), o(i)) \times \delta_i = \frac{\gamma_{d(i)} - \gamma_{o(i)}}{\bar{y}_{d(i)} - \bar{y}_{o(i)}} \times (\bar{y}_{d(i)} - \bar{y}_{o(i)}) = \gamma_{d(i)} - \gamma_{o(i)}$.

such as the influence of other voters or learning about the candidates in the destination state.¹⁰ Because we include voter fixed effects, the $\theta_{r(i,t)}$ coefficients are only identified up to a constant term; we therefore normalize θ_{-1} to zero.

In all event-study specifications, we compute two-way clustered standard errors by states and voters, thus accounting for the possibility that regression residuals are serially correlated at the individual level and spatially correlated at the state level.

We use the method outlined in this section to assess the influence of contextual and individual factors on voter turnout, in Section III, and on registration and party affiliation, in Section IV.

III. Voter Turnout

A. Relative Influence of Individual and Contextual Factors

Descriptive Analysis.—Figure 1, panel A shows average turnout rates across the 50 states and the District of Columbia. State averages are computed by first calculating the percentage of individuals in the state who turn out in each election, and then taking a simple average across elections. The map reveals a North-versus-South turnout divide, with states in the northern half of the country characterized by higher voter participation than their southern counterparts. As shown on the histogram in online Appendix Figure A.2a, the mean state has an average turnout rate of 43.8 percent. Minnesota has the highest turnout rate (57.8 percent), while Mississippi trails all other states with an average turnout of only 33.8 percent.

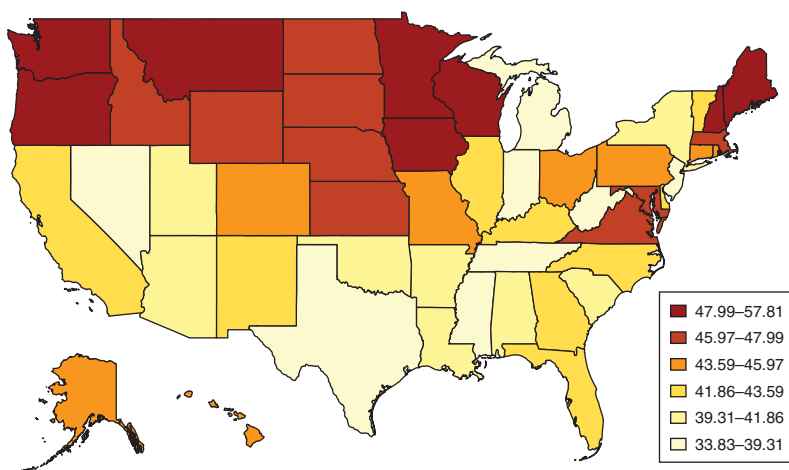
The first step of our analysis tracks changes in the participation of movers after they cross state borders to estimate the share of differences in Figure 1, panel A, that results from differences in contextual factors rather than differences in the individual characteristics of the people living in each state.

As a preliminary look at how voter turnout changes after move, Figure 2, panel A, plots the change in movers' turnout against the destination-origin difference in voter participation δ_i . For each mover, we compute the change in voter turnout as the difference between average turnout in all post-move elections minus average turnout in all pre-move elections. If states explained individual-level turnout entirely, we would expect the slope of the graph to be one. Conversely, if voter turnout were independent of state characteristics, we would expect the slope to be zero.

Figure 2, panel A shows that the slope is 0.37, suggesting that state characteristics explain around 37 percent of the observed variation in voter participation. The relationship is symmetric around zero and linear, thus lending support to our model, which implies identical absolute changes in voter turnout for voters moving from state j to j' and for voters moving in the opposite direction.

¹⁰One possible concern is that the patterns of pre- and post-move coefficients may be driven by compositional effects, since these coefficients are estimated out of different samples. For instance, the coefficient corresponding to relative year -5 is only estimated out of people whose first post-move election was 2018, the last election in the sample. Event-study graphs using samples of movers whose first post-move election was 2010, 2012, 2014, 2016, and 2018, respectively, and who are observed in all six elections covered by our data, are shown in online Appendix Figures A.12 through A.16. Reassuringly, the patterns in these graphs are consistent with those visible on Figures 3 and 6.

Panel A. Voter turnout



Panel B. Democratic two-party affiliation share

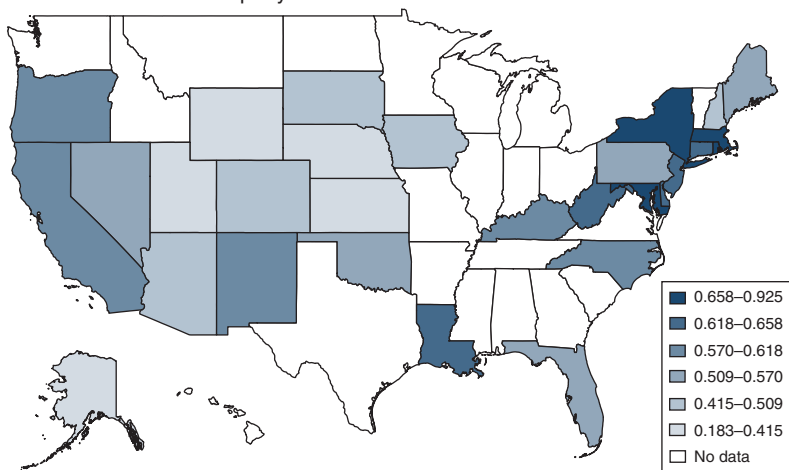


FIGURE 1. VOTER TURNOUT AND DEMOCRATIC TWO-PARTY AFFILIATION SHARE BY STATE, 2008–2018

Notes: The maps plot average state voter turnout and Democratic two-party affiliation share (the number of voters affiliated with the Democratic Party as a fraction of voters affiliated either with the Republican or the Democratic Party) in the Catalist data in six bins. The lower and upper limits of the outcome in each bin are displayed in the legend. For each state, we take the simple outcome average across the six elections (2008–2018) in the Catalist data. The sample consists of all movers and nonmovers.

With an \times , we also plot average changes in voter turnout for a sample of matched nonmovers. The matched sample is constructed by randomly drawing, for each mover, a nonmover who shares the mover's state of origin, sex, race, and age ventile bin, and who is observed over the same elections.¹¹ To construct Figure 2, panel A, the matched sample is assigned $\delta = 0$. The matched sample and all points for movers lie vertically below zero, which reflects an overall decline of voter participation

¹¹ Age ventile bins are computed using all movers and nonmovers with nonmissing values of age. Along with these 20 bins, we have another category for voters with missing age.

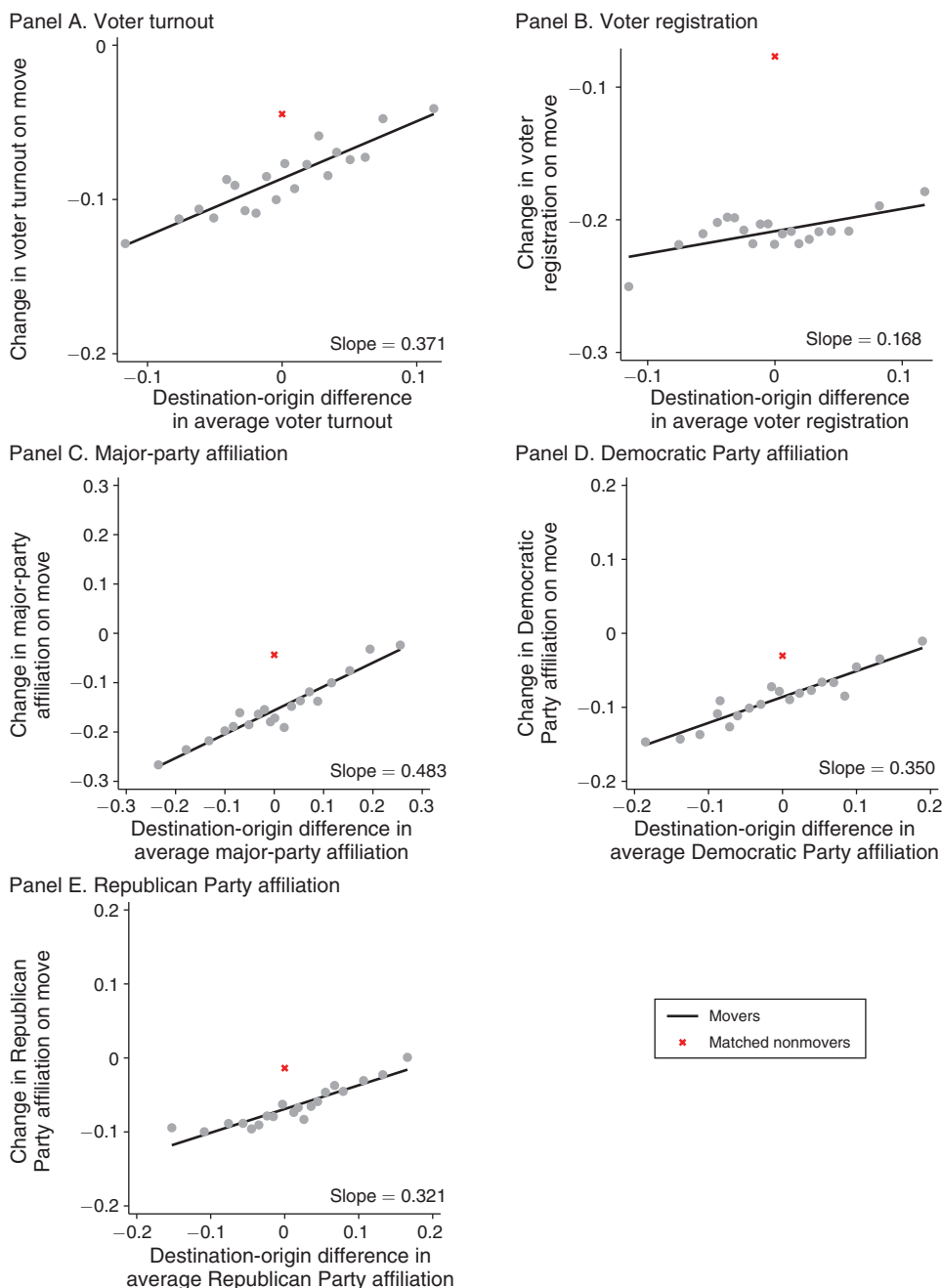


FIGURE 2. CHANGE IN MOVERS' VOTER TURNOUT, REGISTRATION, AND PARTY AFFILIATION AGAINST DESTINATION-ORIGIN DIFFERENCES

Notes: The figure shows how voter turnout, voter registration, major-party affiliation, affiliation with the Democratic Party, and affiliation with the Republican Party change before and after move in relation to differences in average outcomes across states of destination and origin. The x -axis displays the average $\hat{\delta}_i$ for movers in each ventile. For each ventile, the y -axis shows average turnout in all post-move elections minus average turnout in all pre-move elections. The line represents the best linear fit from a simple ordinary least squares (OLS) regression using the 20 data points, and its slope is reported on the graph. For comparison, we also compute the change in turnout for a sample of matched non-movers and denote it with an \times in the graph. Details on the matching procedure are provided in the text.

TABLE 2—LINEARLY ADDITIVE DECOMPOSITION OF VOTER TURNOUT DIFFERENCES

Outcome: 1(voted)	Top 25/ bottom 26 states (1)	Top 15/ bottom 15 states (2)	Top 10/ bottom 10 states (3)	Top 5/ bottom 5 states (4)
Difference in average voter turnout				
Overall	0.072	0.106	0.126	0.158
Due to voters	0.045	0.068	0.077	0.098
Due to states	0.027	0.038	0.049	0.061
Share of difference due to				
Voters	0.629	0.643	0.613	0.617
States	0.371 (0.004)	0.357 (0.003)	0.387 (0.003)	0.383 (0.003)

Notes: Each column reports the results of our main decomposition of voter turnout using a different set of states R and R' . Standard errors (in parentheses) are calculated using a voter-level bootstrap with 50 replications. The sample used to run the underlying regression (1) consists of all movers and nonmovers (observations = 1,572,225,389 voter-years).

occurring in our sample period. Moreover, the matched sample lies vertically above all points for movers, indicating that cross-state moves are associated with a decline in voter participation. In our model, this negative effect of moving is captured by the relative election dummies $\rho_{r(i,t)}$.

Main Decomposition of Cross-State Variation in Voter Turnout.—We implement two decompositions of voter turnout in its state- and voter-driven components. We start with the linearly additive decomposition discussed in Section IIA. Using both movers and nonmovers, we run a specification in the form of equation (1) to estimate place and voter effects for all 50 states and the District of Columbia. For different sets of high- and low-turnout states, we then estimate the overall and relative contributions of state and voter characteristics; that is, for different groups of states R and R' (with high and low turnout, respectively), Table 2 reports estimates of the following quantities: the total difference in average voter turnout ($\bar{y}_R - \bar{y}_{R'}$), the difference due to voters ($\bar{y}_R^{vot} - \bar{y}_{R'}^{vot}$), the difference due to states ($\gamma_R - \gamma_{R'}$), the share of difference due to voters ($S^{voter}(R, R') = (\bar{y}_R^{vot} - \bar{y}_{R'}^{vot})/(\bar{y}_R - \bar{y}_{R'})$), and the share of difference due to states ($S^{state}(R, R') = (\gamma_R - \gamma_{R'})/(\bar{y}_R - \bar{y}_{R'})$).

Column 1 reports the comparison between states with above- and below-median turnout. The difference in average turnout across the two groups is 7.2 percentage points, of which 4.5 and 2.7 percentage points are due to voter and place characteristics, respectively. This translates to voter factors accounting for approximately 63 percent of the overall difference and state factors for the residual 37 percent. Standard errors are computed using a voter-level bootstrap with 50 replications. Thanks to the large number of cross-state movers (approximately 14 million) and the large total number of observations (more than 1.5 billion), the estimated shares are extremely precise: their standard error is equal to 0.4 percentage points, which is two orders of magnitude smaller than the corresponding point estimates.

In columns 2–4, we report comparisons for other groups of high- and low-turnout states. Column 2 compares the 15 highest- and 15 lowest-turnout states. The overall difference in turnout is 10.6 percentage points, of which 6.8 and 3.8 percentage points are due to voter and state characteristics, respectively. The overall difference

grows to 12.6 percentage points in the top-10-versus-bottom-10 comparison (column 3), and to 15.8 percentage points in the top-5-versus-bottom-5 comparison (column 4), with voter and state characteristics accounting for 7.7 and 4.9 percentage points and 9.8 and 6.1 percentage points respectively.

The corresponding relative contributions of voter and state factors are very similar across comparisons, with states accounting for 36 to 39 percent of the overall variation in voter turnout. These figures are in line with the slope of 0.37 on Figure 2, panel A. Along with the linear relationship shown in Figure 2, panel A, the stability of the state shares estimated using different groups of high- and low-turnout states suggests that $S^{state}(j, j')$ is not strongly correlated with $\bar{y}_j - \bar{y}_{j'}$.

As discussed in Section IIA, a possible concern is that our results only apply to movers. To control for differences between movers and nonmovers in the sociodemographic characteristics observed in the Catalist data, we implement a new decomposition weighting movers based on the fraction of people with the same age, gender, and race in their state of origin. Using these weights, we find that voter effects explain 73 percent of the difference in turnout between above- and below-median states, which is broadly in line with (albeit slightly larger than) our baseline estimate of 63 percent (online Appendix Table A.3). While we cannot account for unobserved differences between movers and nonmovers, the closeness between these two estimates suggests that the validity of our decomposition goes beyond movers.¹²

Another possible concern is that the effects of states on electoral outcomes vary over time. Indeed, building on recent critiques of two-way (i.e., unit and time) fixed effects regressions,¹³ Hull (2018) shows that causal interpretation of mover regressions with multiple treatments requires assuming homogeneous effects across treatments and time, along with parallel trends and impersistent outcomes.¹⁴ Motivated by this observation, Hull (2018) proposes a novel mover average treatment effect (MATE) estimator which, to be interpreted causally, only requires the parallel trends, effect homogeneity, and outcome impersistence assumptions to hold *conditionally* on a set of covariates. Under these assumptions, MATEs can be consistently estimated as a weighted average of individual-level outcome growth, ΔY_i , with weights given by functions of the treatments and the propensity scores (i.e., the probabilities that an individual follows a certain treatment trajectory).

Online Appendix Table A.6 reports the results from MATE decompositions of outcome differences across states with above- and below-median outcomes

¹²Online Appendix Table A.2 reports average outcomes by gender, race, age, and gender-by-race-by-age combinations, separately for movers and nonmovers. The goal is to explore whether outcome differences across movers and nonmovers depend on the distinct demographic makeups of the two groups—e.g., movers' higher likelihood to be White and women (Table 1) may be associated with different propensities to register, vote, and affiliate with the Republican or Democratic Party—or whether, even within observable demographic cells, movers and nonmovers behave differently. Residual outcome differences between movers and nonmovers within gender-by-race-by-age cells are very small for registration, major-party affiliation, Democratic Party affiliation, and Republican Party affiliation. Movers-minus-nonmovers turnout differences also shrink within gender-by-race-by-age cells, although not as much. These results bring additional support for the view that our decomposition may hold validity for nonmovers, particularly after weighting movers to resemble the demographic composition of nonmovers.

¹³In particular, de Chaisemartin and D'Haultfoeuille (2020) show that the two-way fixed effects estimator represents a weighted sum of unit-by-period cells' average treatment effects. Since some weights can be negative, the two-way fixed effects estimate may even have the opposite sign of *all* cell-level average treatment effects.

¹⁴Outcome impersistence means that potential outcomes only depend on the current treatment and not on previous treatment values. Analogously to difference-in-difference design, parallel trends mean that, in the absence of moves, potential outcomes for movers and nonmovers would have followed identical trajectories.

(panel A), as well as across states in the top and bottom terciles (panel B) and quartiles (panel C) of the outcome distribution. To allay concerns related to assuming time-invariant state effects, treatment groups are redefined over time; that is, we compute average state outcomes by pairs of consecutive elections and then, in each pair, classify states based on the resulting distribution of average state outcomes. Thanks to this reclassification, if a state experiences an increase in turnout after implementing convenience voting policies, for example, that state may, after the policies' implementation, switch from being a below- to an above-median turnout state.¹⁵

Results from MATE decompositions (ranging from 0.315 to 0.441) are substantively similar to the main estimates reported in Table 2 (ranging from 0.357 to 0.387), thus reinforcing confidence in our main decompositions.

Variance Decomposition.—Online Appendix Table A.7 presents a second, alternative decomposition to assess the relative importance of state and voter factors. We first compute the cross-state variance of average voter turnout and estimate the cross-state variances of voter and state effects, along with their correlation: $Var(\bar{y}_j)$,

$Var(\bar{y}_j^{vot})$, $Var(\gamma_j)$, and $Corr(\bar{y}_j^{vot}, \gamma_j) = \frac{Cov(\bar{y}_j^{vot}, \gamma_j)}{\sqrt{Var(\bar{y}_j^{vot})Var(\gamma_j)}}$. To estimate the variance

of γ_j and \bar{y}_j^{vot} and the covariance between these variables we use a split-sample approach to take into account the fact that the underlying parameters are themselves estimated.¹⁶ We then estimate the share of cross-state variance in voter turnout due to voter characteristics, defined as the share of the total variance that would be eliminated by erasing differences in voter characteristics. Since $\bar{y}_j = \bar{y}_j^{vot} + \gamma_j$, the variance in \bar{y}_j remaining after erasing differences in voter characteristics is $Var(\gamma_j)$ and the share of the total variance due to voter characteristics is given by

$$(7) \quad S_{var}^{voter} = 1 - \frac{Var(\gamma_j)}{Var(\bar{y}_j)}.$$

Similarly, the share of the total variance due to place characteristics is given by

$$(8) \quad S_{var}^{state} = 1 - \frac{Var(\bar{y}_j^{vot})}{Var(\bar{y}_j)}.$$

¹⁵ Online Appendix Table A.6 also reports place shares from mover regressions whose underlying specification mirrors the structure of the MATE estimator (e.g., they are run in first differences and rely on groupings of states into broader treatment groups like above versus below the median, terciles, and quartiles), for comparison. For computational reasons, all results in the table are based on a random 1 percent subsample of voters from the Catalyst data. MATE-based decompositions can be estimated with or without nonmovers. Without nonmovers, the parallel trends assumption is relaxed to hold (conditionally) only for movers, at the cost of obtaining a slightly less informative estimand (Hull 2018). In online Appendix Table A.6, we focus on MATE decompositions (and decompositions from mover regressions) without nonmovers, since MATE decompositions with nonmovers feature significant rejections of the omnibus specification test.

¹⁶ We randomly assign movers within each origin-destination pair and nonmovers within each state to either of two subsamples of approximately identical size. We then estimate equation (1) separately on each subsample. We estimate $Var(\gamma_j)$ (resp. $Var(\bar{y}_j^{vot})$) as the covariance between the estimated γ_j (resp. \bar{y}_j^{vot}) from the two subsamples. To estimate $Cov(\gamma_j, \bar{y}_j^{vot})$, we take the simple average of the covariances between the estimated γ_j from one subsample and the estimated \bar{y}_j^{vot} from the other subsample.

The advantage of this decomposition is that, unlike S^{voter} and S^{state} , S_{var}^{voter} and S_{var}^{state} do not require choosing specific sets of states to be compared. However, differently from S^{voter} and S^{state} , the variance decomposition is not additive: S_{var}^{voter} and S_{var}^{state} will not sum to 1 as long as $Cov(\gamma_j, \bar{y}_j^{voter}) \neq 0$.

The variance decomposition delivers two key results. First, equalizing voter effects would eliminate 64 percent of the cross-state variance in voter turnout; equalizing state effects would reduce the variance slightly less, by 42 percent. Second, the correlation between voter and state effects is positive, which explains why S_{var}^{voter} and S_{var}^{state} sum to more than one. This positive correlation could signal, for instance, that states with politically engaged voters face more requests to approve forms of convenience voting and that they tend to respond to these demands.

Overall, the main decomposition and the variance decomposition yield the consistent conclusion that contextual factors explain a lower share of cross-state variation in voter turnout than individual factors. Possible reasons include the low malleability of voting behavior, particularly for older voters (an explanation we return to in Section V, when we examine heterogeneity across age groups); limited cross-state variation in contextual factors exerting a large influence on participation (because these factors are present either in very few states or, instead, in nearly all states); and the bundling of contextual factors offsetting each other. While our data do not allow us to fully adjudicate how much each of these reasons accounts for the lower share of cross-state turnout variation explained by contextual factors, we shed light on this question by examining which specific factors contribute the most to overall place and individual effects. This investigation begins by studying the change in movers' participation over time, using our event-study design.

B. Event Studies

Our main event-study results are shown in Figure 3, which plots the estimated $\theta_{r(i,t)}$ coefficients on indicators for election relative to move from equation (6). The 95 percent confidence intervals are constructed from two-way clustered standard errors at the voter and state levels. Event-study regressions use $\hat{\delta}_i$ s estimated using all movers and nonmovers in each state, but, for computational ease, they are run using the movers sample only.¹⁷

The plot reveals no (partial) correlation between pre-move turnout and destination-minus-origin differences in average state turnout: estimates of θ_{-5} to θ_{-2} are close to zero and statistically insignificant. The pattern of $\theta_{r(i,t)}$ then jumps discretely at the first post-move election and slightly decreases afterwards. The point estimates on the $\theta_{r(i,t)}$ s and their standard errors are reported in online Appendix Table A.8, column 1.

The pre-move effects help identify differences in turnout trends as a function of where voters move (as described by δ_i). The lack of pre-trends supports the key identifying assumption that changes in individual drivers of voter turnout are not systematically correlated with differences in average participation between origin

¹⁷ Estimating two-way clustered standard errors (by voters and states) using both movers and nonmovers is in fact computationally very costly. However, we find virtually identical point estimates when estimating equation (6) on the full sample (results available upon request).

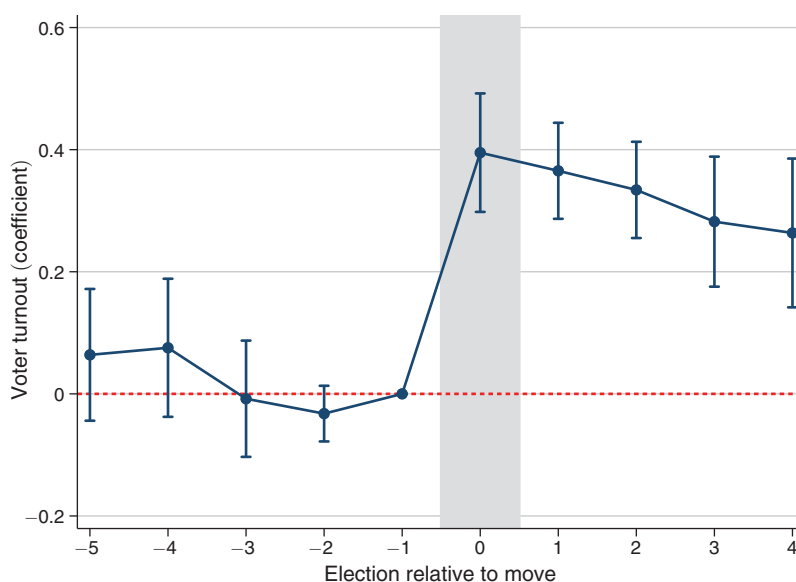


FIGURE 3. EVENT-STUDY PLOT, VOTER TURNOUT

Notes: The figure plots estimates of $\theta_{r(i,t)}$ and 95 percent confidence intervals (robust to two-way clustering by states and individuals) from event-study specification (6). The dependent variable y_{it} is a dummy equal to one if voter i voted in election t , and zero otherwise. For each mover, $\hat{\delta}_i$ is constructed using the difference in average turnout in the state of destination across all elections in our sample minus average turnout in the state of origin. The sample consists of all mover-years ($N = 77,988,314$).

and destination. In other words, moves are not systematically preceded by gradual changes in individual determinants of voter turnout (e.g., increases in political activism before moving to high-turnout states) which would complicate the causal interpretation of post-move estimates.

The sharp positive change in $\theta_{r(i,t)}$ in the first post-move election (i.e., at $r(i,t) = 0$) indicates a significant and immediate effect of state factors on voter turnout. The influence of state factors on participation may be direct (e.g., if voters are driven to the polls by more competitive elections in the destination state) or indirect (e.g., if the supply of local newspapers in that state is richer, leading to increases in movers' media consumption and, in turn, in their political interest and participation). Consistent with the slope of Figure 2, panel A and with the linearly additive decomposition of turnout differences in Table 2, the magnitude of the discontinuity is 0.40, suggesting that state characteristics explain approximately 40 percent of the observed cross-state variation in voter turnout. Moreover, the lack of increase in post-move adjustments is consistent with the state characteristics driving turnout being "discrete" (e.g., voting rules) and easy for voters to adapt to.¹⁸ In contrast, peer effects could in principle be captured as place-specific

¹⁸There are two possible interpretations for the fact that the $\theta_{r(i,t)}$ s are actually decreasing after the move. The first is that the influence of the contextual drivers of voter turnout in the destination state decreases over time. For instance, registration costs may vary across states and be particularly impactful shortly after the move, with

factors, since the composition of peers usually changes when a voter moves, but they would likely take some time to fully materialize.¹⁹ The fact that coefficients do not increase over time after movers arrive in their destination state suggests that peer effects or other factors involving slow changes are not the main mechanism responsible for post-move adjustments of participation.

State average turnout rates computed from the Catalist data enter in the $\hat{\delta}_i$ s used as regressors in the event study and they directly affect the estimated $\theta_{r(i,t)}$ as a result. For this reason, limitations of the Catalist data discussed earlier may affect the $\hat{\delta}_i$ s and thus the event-study results. To assess whether this is the case, we estimate a specification in the form of equation (6) replacing the $\hat{\delta}_i$ s computed using the Catalist data with $\hat{\delta}_i$ s constructed using McDonald's turnout figures (McDonald 2018).²⁰ Online Appendix Figure A.6, which relies on $\hat{\delta}_i$ s computed using McDonald's data, is very similar to the event-study plot based on the Catalist data. Like in Figure 3, there is no evidence of significant pre-trends. The variable θ jumps upwards by 0.36 on move, decreases to 0.23 in the fourth election since moving across states and goes back up a little in the fifth election (see column 2 of online Appendix Table A.8). The overall similarity of Figures 3 and 6 allays the concern that the data limitations discussed in Section IA dramatically affect our results and increases our confidence in the event-study estimates and in the related decomposition of turnout between voter and state factors.

effects that dissipate over time, as progressively more movers register in their destination state. However, this interpretation is inconsistent with the stable pattern of post-move $\theta_{r(i,t)}$ s in the event study using voter registration as outcome (Figure 6, panel A). A second possible interpretation is that the influence of contextual factors remains equally strong two to five elections after the move but that differences in these factors across states decreased between 2008 and 2018. Since the $\hat{\delta}_i$ s (the differences in average turnout between states of destination and origin) with which the $\theta_{r(i,t)}$ s are interacted in equation (6) are time invariant, we should then mechanically expect the observed decreasing pattern. Online Appendix Figure A.5 brings support for this interpretation. We report the results from a specification in the form of equation (6), in which we interact the $\theta_{r(i,t)}$ s with year-specific $\hat{\delta}_i$ s: $y_{it} = \alpha_i + \theta_{r(i,t)}\hat{\delta}_{it} + \tau_t + \rho_{r(i,t)} + \varepsilon_{it}$. In this equation, we can estimate all $\theta_{r(i,t)}$ s, which are no longer collinear, and the magnitude of the post-move $\theta_{r(i,t)}$ s can no longer be directly interpreted as weighted averages of $S^{state}(d(i), o(i))$ but should be compared to the pre-move $\theta_{r(i,t)}$ s. We observe a jump between θ_{-1} and θ_0 of similar magnitude as the on-move jump visible in Figure 3. In addition, the $\theta_{r(i,t)}$ s remain flat afterwards, which is in line with this second interpretation.

¹⁹On the other hand, mover selection into *within-state* place characteristics could be captured as a voter-specific factor if this selection is identical in origin and destination states (e.g., if poorer voters tend to live in areas with a low density of polling places in their state of origin and to move to similar areas in their state of destination), even if the effect of these characteristics on turnout is immediate. Indeed, state effects only capture the effect of contextual factors varying across states. This concern is arguably less salient in cross-county decompositions, since there is less variation in (and thus less scope for selection into) place characteristics within these smaller geographies. Thus, if mover selection into place characteristics substantially affected our decomposition, we would expect to observe relatively large place effects in cross-county decompositions. Instead, Section IIIC shows that the share of turnout due to place effects is much smaller in decompositions of voting behavior across counties of the same state than across states, thus alleviating the concern that voter selection into within-state location characteristics substantively affects our results.

²⁰We use McDonald's rates defined as the count of votes cast for the highest office in a given state-election divided by the estimated voting-eligible population in the same state-year. McDonald also reports two other turnout rates: the total number of ballots counted divided by the voting-eligible population, which is not available for all states-years, and the count of votes cast for the highest office divided by the voting-age population. Results (available upon request) based on the count of votes cast for the highest office divided by the voting-age population are very similar to those we report in the paper.

C. Main Components of Voter and Place Effects

The patterns shown in Figure 3 give a first indication on the type of factors responsible for place effects. We now explore the main components of place and individual effects in more detail.

Cross-County Moves.—While contextual factors such as the identity of the governor or voter registration requirements vary at the state level, others such as local economic conditions, distance to the polling station and waiting time on election day, and the composition of peers vary at a more local level. We provide evidence on the relative importance of both types of factors by comparing the decomposition of cross-state variation in turnout, shown in Table 2, with the decomposition of within-state cross-county variation between county- and voter-driven components.

The results of this second decomposition and the corresponding event study are shown in online Appendix Table A.9, panel A and in Figure 4. They are based on versions of equations (1) and (6), in which the state fixed effects γ_j are replaced by county fixed effects, and cross-state movers are excluded. The restriction of the sample to nonmovers and within-state movers ensures that the new decomposition only accounts for turnout differences across counties of the same state and that it isolates the effect of contextual factors varying at the county (not state) level.

Post-move adjustments in the participation of cross-county movers shown in Figure 4 are much smaller than the jump visible in the event-study graph for cross-state movers (Figure 3). In addition, on Figure 4, we observe that the participation of cross-county movers converges to the average in the county destination before they move, implying that the (small) post-move adjustment may reflect the continuation of a pre-existing trend as much as the impact of place factors. Overall, the share of within-state cross-county differences in participation explained by county effects oscillates between 6 and 12 percent for different sets of high- and low-turnout counties, which is much less than the share of cross-state turnout differences explained by state effects. This suggests that county-level factors have a more modest influence on voter turnout than state-level factors.

Correlates of Place Effects.—To go one additional step and assess the contribution of specific factors to place effects, we use the fixed effects $\hat{\gamma}_{js}$ estimated from equation (1) as an independent variable in cross-sectional regressions that control for observable state and voter characteristics. We then repeat the same exercise but using the $\hat{\gamma}_j^{vot}$ s as an independent variable, to shed light on the determinants of voter effects. While the results do not necessarily represent causal evidence (causation may actually run in the opposite direction for some characteristics), we improve upon multivariate regressions of voter turnout or partisanship by identifying observable correlates of their state and voter components.

We explore three sets of state characteristics: voting and registration rules, characteristics of the electoral landscape, and socioeconomic and political environment. Among voting rules, we include the availability of same-day registration, automatic registration, early voting, no-excuse absentee voting, strict voter ID laws, open

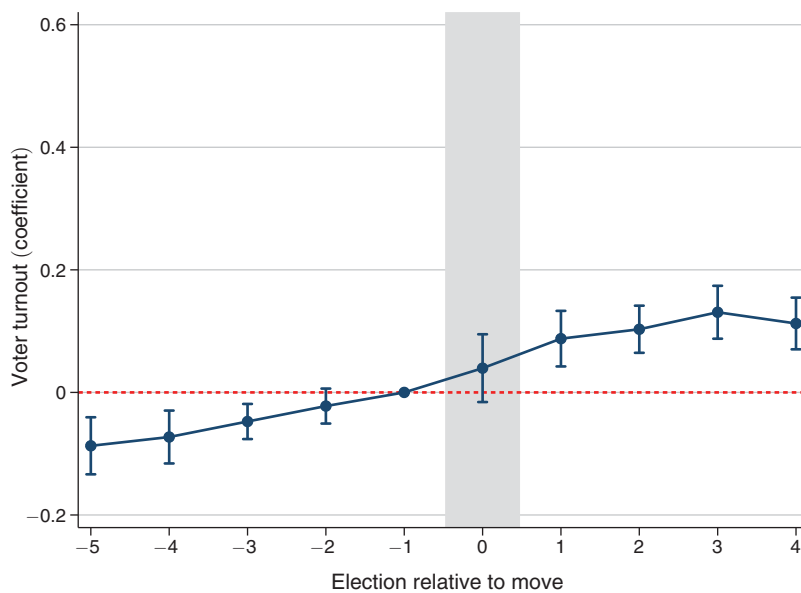


FIGURE 4. EVENT-STUDY PLOT, VOTER TURNOUT, CROSS-COUNTY MOVES WITHIN STATES

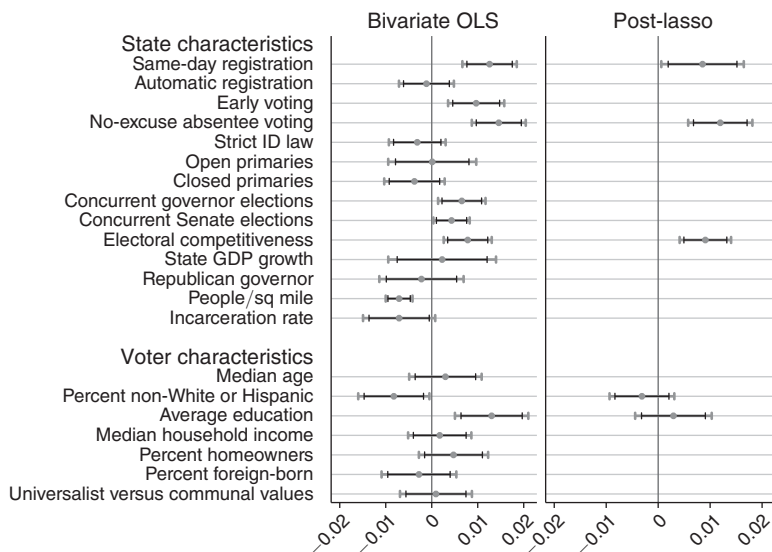
Notes: The figure plots estimates of $\theta_{r(t,t)}$ and 95 percent confidence intervals (robust to two-way clustering by states and individuals) from event-study specification (6) in which the state fixed effects are replaced by county fixed effects. The sample consists of all within-state mover-years for movers whose county is known ($N = 115,466,589$). Other notes as in Figure 3.

primaries, and closed primaries.²¹ While our regressions are cross-sectional, some states changed these voting rules during our sample period. We therefore construct time-invariant regressors by measuring the share of elections in our sample covered by each rule. The second group of state factors includes the share of 2008–2018 elections concurring with gubernatorial and US Senate elections and average electoral competitiveness in presidential elections.²² We expect voting rules and the electoral landscape to primarily affect registration and turnout state fixed effects. Instead, our third group of state factors may affect both participation and party affiliation, which we turn to in the next section. We characterize the socioeconomic and political environment through five variables. State GDP growth may affect the likelihood of reelecting the incumbent, and having a Republican governor may affect partisanship as well as the stringency with which voting rules are applied. Population density might matter for at least two reasons: low density might limit interpersonal discussions about politics and hence voters' interest in elections, and it also correlates with larger average distance to the polling station, thus making voting more costly. The incarceration rate may also be an important obstacle to participation.

²¹We describe the data sources and construction of the correlates of place and voter effects in online Appendix A.2.

²²We use electoral competitiveness in presidential elections because Washington DC elects no voting member of Congress (though it holds mayoral elections concurrently with midterm federal elections). Results are very similar when we use average electoral competitiveness in congressional elections and drop Washington, DC.

Panel A. Correlates of voter turnout state effects



Panel B. Correlates of voter turnout average voter effects

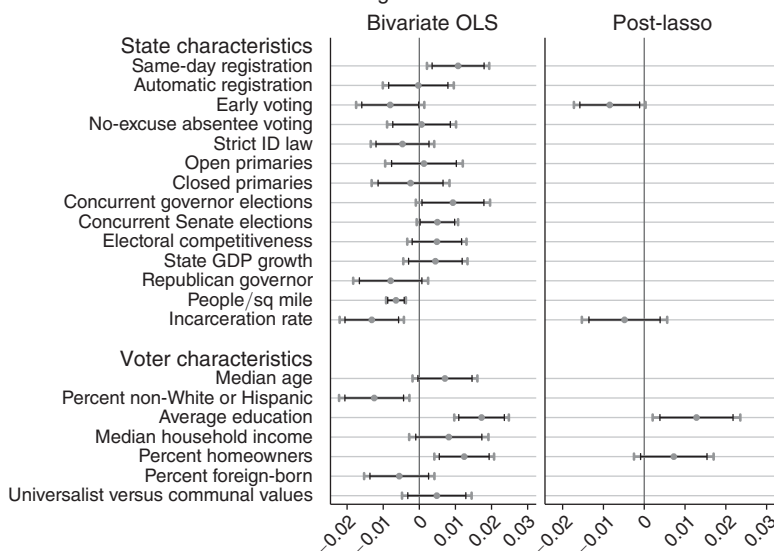


FIGURE 5. CORRELATES OF VOTER TURNOUT STATE AND VOTER EFFECTS

(continued)

For voters, we include state averages of standard sociodemographic predictors of voter turnout and ideology: age, minority status, education, income, the fraction of homeowners, and the fraction of immigrants. We also include voters' relative emphasis on universalist relative to communal values, which may influence their vote choice when candidates make different appeals to both sets of values.

Figure 5, panel A summarizes the correlates of the estimated state effects on voter turnout. Each row represents a different correlate. The left panel reports estimates and heteroskedasticity-robust confidence intervals (both at the 90 and 95 percent

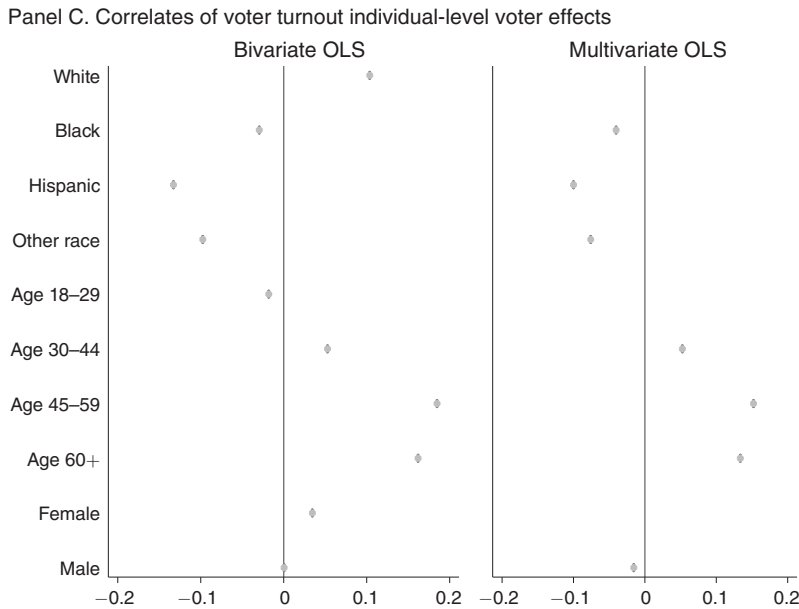


FIGURE 5. CORRELATES OF VOTER TURNOUT STATE AND VOTER EFFECTS (CONTINUED)

Notes: The left panel of Figure 5, panel A reports estimates and heteroskedasticity-robust confidence intervals (at the 90 and 95 percent levels) from bivariate OLS regressions of state fixed effects $\hat{\gamma}_j$ s on state and average voter characteristics. The right panel shows results of a post-Lasso multivariate regression. All covariates are standardized to have mean zero and unitary standard deviation. To obtain post-Lasso estimates, we first run a Lasso regression using all covariates, choosing the penalty with a 10-fold cross-validation to minimize the mean squared error. We then run a single multivariate OLS regression on the covariates selected by the Lasso regression. The sample in both panels consists of the 50 states plus the District of Columbia. See online Appendix A.2 for details on the data sources and construction of the correlates. Figure 5, panel B shows results from bivariate OLS regressions (left panel) and from a post-Lasso multivariate regression (right panel) of state-level averages of voter effects ($\hat{\gamma}_j^{vor}$) on state and voter characteristics. Figure 5, panel C shows results from bivariate OLS regressions (left panel) and from a multivariate OLS regression (right panel) of individual voter effects ($\hat{\gamma}_j^{vor}$) on voter characteristics available in the Catalyst data. The reference categories (i.e., the voter characteristics omitted from the right panel) for age and gender consist of voters with missing age and gender information, respectively. The reference category for race includes voters with unknown race, along with non-White, non-Hispanic, non-Black voters.

levels) from bivariate OLS regressions of the estimated state effects ($\hat{\gamma}_j$) on state and voter correlates. All covariates are standardized by subtracting the mean and dividing by the standard deviation. There are 51 observations, corresponding to the 50 states and the District of Columbia. In the right panel, we present estimates and confidence intervals from a multivariate OLS regression on regressors chosen with a first-stage Lasso²³ regression (Belloni and Chernozhukov 2013).²⁴ Our discussion primarily focuses on the covariates that are significantly correlated with state or voter effects in the multivariate post-Lasso regression.

Among state characteristics, the strongest predictors of turnout state effects are the availability of election day voter registration and no-excuse absentee voting, which

²³ Least absolute shrinkage and selection operator (Lasso).

²⁴ In the first stage, we select regressors using a Lasso regression with a penalty chosen by a 10-fold cross-validation to minimize the mean squared error. In the second stage, we estimate coefficients and standard errors through a multivariate OLS on the selected covariates. One caveat is that the Lasso regression will generally select only one of two highly collinear factors even if both are important contributors to state effects.

lower the cost of voting, as well as electoral competitiveness, which increases its benefits and may, in addition, proxy for the intensity of the campaign. All three variables are positively correlated with state effects, consistent with intuition and the existing literature, and they are all significant at the 5 percent level, in the multivariate regression. Strikingly, in this regression, none of the state averages of sociodemographic characteristics is significantly correlated with state fixed effects, suggesting that the characteristics of voters' neighbors contribute only a limited amount to place effects.

Three factors—this apparently weak effect of neighbors' characteristics, along with the rapidity of the post-move adjustment to the average participation in the destination state observed in Figure 3 and the fact that the share of cross-county variation in turnout due to county effects is lower than the share of cross-state variation due to state effects—point to the same conclusion: place effects on turnout reflect the influence of institutions and of the economic and political environment—most prominently, competitiveness, same-day voter registration, and no-excuse absentee voting—more than the impact of more local factors, including the influence of peers.

Correlates of Voter Effects.—Figure 5, panel B reports the corresponding results for the estimated average voter effects. We find a negative (but not significant) correlation of voter effects with incarceration rate, likely explained by the cost faced by convicted felons to vote (and, in some states, their outright ineligibility). We also find a surprising negative, marginally significant correlation with early voting, which could result from an institutional effort to facilitate participation in places in which the propensity to vote is low. Bivariate correlations with voter characteristics are broadly consistent with previous research: average voter effects are higher in states with more US-born, non-Hispanic White, homeowners, older, richer, and more educated voters. This is particularly true for education, which is the only voter characteristic selected in the Lasso regression and statistically significant.

Finally, we regress *individual-level* (not average) voter fixed effects ($\hat{\alpha}_i$ s) on individual-level covariates available from Catalist: age, gender, and race. We show the results in Figure 5, panel C.²⁵ Again, we find a positive correlation with age and a negative one with minority status.

IV. Voter Registration and Party Affiliation

We now disentangle the role played by contextual and individual factors in explaining variation in voter registration and party affiliation. The decomposition of cross-state differences in voter registration can shed light on the decomposition we obtained for turnout. Instead, party affiliation relates to a different dimension: not whether people vote, but which party they vote for. There is substantial evidence that the party voters identify with, if any, is strongly correlated with the party they

²⁵ When we explore correlates of average voter fixed effects, we have only 51 observations (i.e., one per state) but a large set of covariates, thus justifying the Lasso selection procedure. At the opposite, here we have a large number of observations (i.e., one per voter) but few controls. Therefore, in each graph exploring the correlates of the individual-level fixed effects, the right panel reports estimates from a multivariate OLS regression that includes all voter characteristics that are observed at the individual level. These regressions also include separate dummies for voters with missing age or gender.

are registered with, and that it is one of the strongest predictors of their actual vote choice (e.g., Bartels 2000; Gerber, Huber, and Washington 2010). We study party affiliation together with voter registration because we only observe it conditional on people being registered.²⁶

We first consider voters' *unconditional* likelihood to be affiliated with certain parties: outcomes defined whether voters are registered or not, and equal to one if they are affiliated with these parties, and zero if they are either not registered or registered but not affiliated. We restrict the sample to the 30 states for which party affiliation information is available and study three distinct outcomes: affiliation with the Democratic Party; affiliation with the Republican Party; and affiliation with either (as opposed to not being affiliated with any party or being affiliated with a third party), henceforth defined as "major-party affiliation." The last outcome measures voters' decision to engage in politics beyond simply registering to vote. For instance, voters may choose to affiliate with a party in order to participate in primary elections in states where they are restricted to affiliated voters.

A. *Relative Influence of Individual and Contextual Factors*

Descriptive Analysis.—Figure 1, panel B is the counterpoint of Figure 1, panel A. It shows an equally striking but different geographic clustering of partisanship. We plot state averages of the Democratic two-party affiliation share, defined as the fraction of voters affiliated with the Democratic Party among voters affiliated with either major party, and observe a familiar divide between blue coastal states and red interior states.²⁷

To estimate the share of registration and party affiliation differences across states that result from contextual factors, our method relies again on tracking the behavior of movers as they cross state borders. Figure 2, panels B, C, D, and E first plot the change in movers' behavior against the destination-origin difference in voter registration, major-party affiliation, Democratic Party affiliation, and Republican Party affiliation. The relationship is symmetric around zero and linear, similarly as for voter turnout, and consistent with the additive separability assumption. In addition, the matched sample of nonmovers lies vertically above all points for movers, indicating that cross-state moves are associated with a decline in voter registration (since people need to register again after the move) and in unconditional party affiliation.

Decompositions of Cross-State Variation in Registration and Party Affiliation.—We exploit the variation underlying Figure 2, panels B through E and specifications in the form of equation (1) to implement linearly additive decompositions of voter registration and of our three party affiliation outcomes in their state- and voter-driven components.

²⁶For other recent studies using party affiliation as a proxy for partisanship, see for instance Hall and Yoder (2022) and Brown and Enos (2021).

²⁷The histogram in online Appendix Figure A.2b reports the Democratic Two-Party affiliation share in each state. Online Appendix Figures A.3 and A.4 show the same maps and histograms for the four following additional outcomes: registration, major-party affiliation, affiliation with the Democratic Party, and affiliation with the Republican Party.

TABLE 3—LINEARLY ADDITIVE DECOMPOSITION OF VOTER REGISTRATION AND PARTY AFFILIATION DIFFERENCES

	Top 25/ bottom 26 states (1)	Top 15/ bottom 15 states (2)	Top 10/ bottom 10 states (3)	Top 5/ bottom 5 states (4)
<i>Panel A. Outcome: 1(registered)</i>				
Overall difference	0.069	0.107	0.132	0.163
Due to voters	0.047	0.085	0.102	0.111
Due to states	0.022	0.022	0.030	0.051
Share due to voters	0.684	0.797	0.775	0.684
Share due to states	0.316 (0.004)	0.203 (0.004)	0.225 (0.003)	0.316 (0.004)
<i>Panel B. Outcome: 1(affiliated with a major party)</i>				
Overall difference	—	0.157	0.215	0.281
Due to voters	—	0.087	0.117	0.151
Due to states	—	0.070	0.098	0.130
Share due to voters	—	0.555	0.546	0.537
Share due to states	—	0.445 (0.003)	0.454 (0.003)	0.463 (0.003)
<i>Panel C. Outcome: 1(affiliated with the Democratic Party)</i>				
Overall difference	—	0.142	0.195	0.276
Due to voters	—	0.102	0.136	0.197
Due to states	—	0.041	0.058	0.079
Share due to voters	—	0.713	0.700	0.713
Share due to states	—	0.287 (0.003)	0.300 (0.003)	0.287 (0.003)
<i>Panel D. Outcome: 1(affiliated with the Republican Party)</i>				
Overall difference	—	0.111	0.160	0.244
Due to voters	—	0.087	0.120	0.199
Due to states	—	0.024	0.040	0.045
Share due to voters	—	0.783	0.749	0.816
Share due to states	—	0.217 (0.005)	0.251 (0.004)	0.184 (0.004)

Notes: Each panel in this table replicates the decompositions of Table 2 using a different outcome. In panels B–D, the sample of the underlying regressions is restricted to the 30 states for which Catalist records party affiliation. N in panel A = 1,572,225,389 voter-years; N in panels B–D = 877,053,808 voter-years.

As shown in Table 3, contextual factors account for 32 percent of the difference in average voter registration between states with above- and below-median registration, and for 44 percent (resp. 29 and 22 percent) of the difference in party affiliation between the 15 states with highest and lowest major-party affiliation (resp. Democratic Party and Republican Party affiliation). We obtain similar estimates of the share due to states when we consider other groups of states; weight movers based on the fraction of people with the same age, gender, and race in their state of origin, to increase the representativeness of the results (online Appendix Table A.3); and when we use Hull's (2018) MATE-based decomposition (online Appendix Table A.6).

Overall, these results indicate that the relative influence of contextual factors, in comparison to individual factors, is lower for registration than it is for voter turnout, and that these factors also exert a substantial influence on partisanship. The results of the second decomposition, which estimates the shares of cross-state variance in registration and party affiliation due to voter characteristics and place characteristics, bring further support for this conclusion (online Appendix Table A.10).

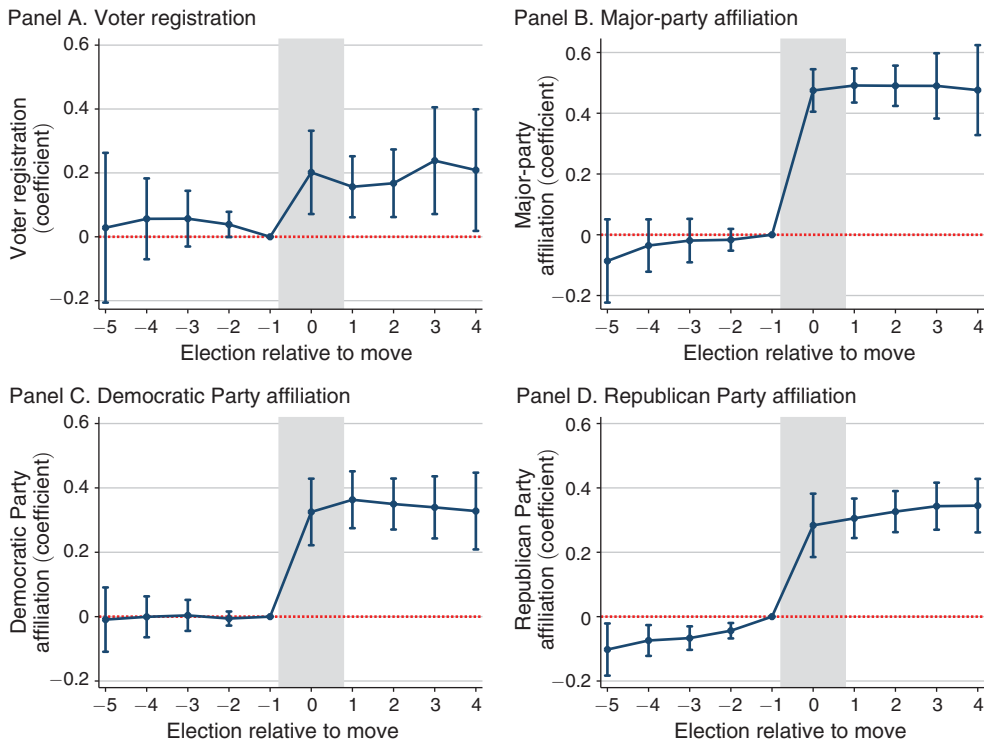


FIGURE 6. EVENT-STUDY PLOTS, VOTER REGISTRATION AND PARTY AFFILIATION

Notes: The figure plots estimates of $\theta_{r(i,t)}$ and 95 percent confidence intervals (robust to two-way clustering by states and individuals) from event-study specification (6). In the first graph, the dependent variable y_{it} is a dummy equal to one if voter i was registered for election t , and zero otherwise. In the second, third, and fourth graphs, the dependent variable is a dummy equal to one if voter i was affiliated with either of the two major parties, with the Democratic Party, and with the Republican Party for election t and zero if they were registered but not affiliated with this party or if they were not registered. For each mover, $\hat{\delta}_i$ is constructed using the difference in average outcome in the state of destination across all elections in our sample minus average outcome in the state of origin. The sample consists of all mover-years ($N = 77,988,314$) in the first graph and all mover-years for moves between states in which party affiliation is available ($N = 28,009,915$) in the second, third, and fourth graphs.

Event Studies.—We use the event study in equation (6) to analyze election-to-election changes in movers' registration and party affiliation. The pre-move effects for voter registration show no systematic correlation with destination-minus-origin differences in this outcome (Figure 6, panel A). Instead, we see a sharp positive change in the first post-move election, and no significant change afterwards. The size of the jump after move is 0.20 (see online Appendix Table A.11 column 1 for the detailed point estimates).

People's likelihood to be affiliated with either major party as a function of destination-minus-origin differences in average major-party affiliation is also flat before the move, jumps by 0.47 on the first post move election, and remains flat afterward (Figure 6, panel B). We observe a similar pattern for Democratic Party affiliation, with a slightly smaller jump (0.33) (Figure 6, panel C). Republican Party affiliation shows a small but significant convergence to the destination state averages before the move (Figure 6, panel D), suggesting that voters who become

closer to the Republican Party are a bit more likely to move to more Republican states. However, the post-move change is of a different order of magnitude, 0.28, and the adjustment continues in the following elections. Overall, the size of the jumps broadly corresponds to the share of cross-state differences in party affiliation explained by contextual factors, in the decompositions shown in Table 3.²⁸

B. Place and Voter-Driven Effects on Partisanship

While party affiliation is the best available proxy for voters' partisanship in individual-level administrative data, this outcome has two important limitations. First, voters may update their party affiliation even absent any actual ideological change. For instance, Democrats may turn into independents if they move from a state with closed primaries to a state with primaries open to unaffiliated voters, even if their likelihood to vote for Democratic candidates remains as high as before. Second, we only observe the party affiliation of registered voters. As mentioned earlier, our unconditional party affiliation outcomes are set to zero for unregistered voters. While this choice eliminates any risk of endogenous sample composition that would come from missing values, it implies that changes in voters' registration status may change our party affiliation outcomes independently of changes in political preferences.

We use two distinct approaches to circumvent these limitations and get as close as possible to isolating the impact of contextual factors on actual partisanship.

Moves between States with Identical Primary Rules.—First, we control for differences in primary rules by running the event studies on the subsample of moves between pairs of states with identical rules.²⁹ As shown in online Appendix Figure A.8 and online Appendix Table A.12, the post-move changes are only slightly lower than the changes in the full sample of states with party affiliation. This suggests that, in the full sample, differences in states' primary rules only explain a small part of the post-move change in party affiliation. However, we now observe an increase in point estimates over multiple elections after the move both for major-party affiliation and for Republican Party affiliation, suggesting that some contextual factors have an impact on party affiliation which is less immediate than for participation. We return to exploring the factors responsible for place and individual effects on party affiliation in Section IVC.

²⁸ A specific concern related to party affiliation is that pre-trends may fail to reveal pre-move outcome convergence to destination averages if partisan leanings evolve over time but voters are sluggish at updating their voter registration. For example, a mover who is a registered Democrat in her state of origin and registers as a Republican after moving to a more Republican state may have already become conservative in pre-move elections, even if she only switches party after moving. Assuaging this concern, online Appendix Figure A.7 shows that party affiliation event studies remain substantively unaffected by restricting the sample to voters who updated their voter registration (and, thus, could easily change their party affiliation) between the second-to-last and the last election before moving.

²⁹ We follow the National Conference of State Legislatures, which distinguishes between closed primaries, partially closed primaries, partially open primaries, primaries open to unaffiliated voters, and top-two primaries. Importantly, note that differences within each of these groups can remain substantial, and that this classification only considers primary rules used for nonpresidential elections (but states may have different rules for presidential elections). See <https://www.ncsl.org/research/elections-and-campaigns/primary-types.aspx> (accessed October 7, 2020).

Beyond differences in primary rules, registered voters may change their party affiliation for reasons orthogonal to changes in ideology—e.g., because they want to get more involved politically. In addition, restricting the sample to moves between pairs of states with identical primary rules does not eliminate the possibility that changes in voter registration status mechanically affect unconditional party affiliation. Therefore, we now consider an outcome that is as close as possible to partisanship: being affiliated with the Democratic Party (or with the Republican Party), *conditional on being registered and on being affiliated with either of the two major parties*. Changes in this outcome can be driven neither by changes in registration nor by changes in the decision to be affiliated with a party.

Conditional Party Affiliation.—Due to selection bias, we cannot estimate the influence of the context on conditional party affiliation simply by focusing on the subset of voters who are registered and affiliated with a major party. Indeed, some voters may register and affiliate with a party only when they move to states with high rates of registration and party affiliation, so that their conditional party affiliation is observed only in a subset of states.

We address this selection issue with the following two-step procedure.

We first use a difference-in-difference design to measure the impact of moving to a state with higher *unconditional* major-party affiliation and higher *conditional* Democratic Party affiliation than the state of origin (henceforth, trajectory one) relative to moving to a state with lower unconditional major-party affiliation and lower conditional Democratic Party affiliation (trajectory zero):

$$(9) \quad y_{it} = \alpha_i + \beta I_{r(i,t) \geq 0} \times T_i + \tau_t + \rho_{r(i,t)} + \varepsilon_{it},$$

where $T_i = 1$ if mover i followed trajectory one and 0 if she followed trajectory zero. We restrict the sample to movers who followed trajectory one or zero and measure effects on unconditional major-party affiliation and on unconditional Democratic Party affiliation. We compare moves to states with higher versus lower conditional Democratic Party affiliation to measure the effect of state differences in partisanship; and we require trajectory one (resp. trajectory zero) destination states to have higher (resp. lower) average unconditional major-party affiliation than the state of origin to satisfy a “no-defiers” assumption described below.

Second, we construct bounds on the effects on conditional Democratic Party affiliation by adapting Lee’s (2009) method to our setting and using the estimates derived in step one.

Using the potential outcomes framework, we define A_0 and A_1 as variables indicating if the mover registers and gets affiliated with either major party after the move when $T = 0$ and $T = 1$, respectively. In the data, we only observe $A = TA_1 + (1 - T)A_0$: we know whether movers following trajectory one affiliate after the move but not whether they would have done so had they followed trajectory zero, and vice versa. Similarly, we define D_0 and D_1 as variables indicating if the mover affiliates with the Democratic Party conditional on getting affiliated with either major party when $T = 0$ and $T = 1$. Again, we only observe $D = A[TD_1 + (1 - T)D_0]$.

We further define four types of movers: “always takers,” who always affiliate with either of the two major parties after the move; “never takers,” who never affiliate after the move; “compliers,” who affiliate after the move if they follow trajectory one, not if they follow trajectory zero; and “defiers,” who affiliate after the move if they follow trajectory zero, not trajectory one. The key assumption we use to derive bounds is that there are no defiers. Our choice to compare moves to states with higher unconditional major-party affiliation versus lower affiliation makes this assumption likely to be satisfied. Under this assumption, online Appendix A.3 shows we can write

$$\begin{aligned}
 (10) \quad & \overbrace{E[D_1 - D_0 | A_1 = 1]}^{\text{Effect on Dem affiliation conditional on being always-taker or complier}} \\
 &= \frac{1}{E(A_1)} \left[\underbrace{E(D_1 A_1 - D_0 A_0)}_{\text{Effect on } D} - \underbrace{\text{Prob}(A_1 > A_0)}_{\text{Effect on } A} \cdot \underbrace{E(D_0 | A_1 > A_0)}_{\text{Unobservable}} \right].
 \end{aligned}$$

$E(D_0 | A_1 > A_0)$ is the likelihood that, after moving, compliers would affiliate with the Democratic Party if they got affiliated with either of the two major parties, absent treatment (i.e., when they follow trajectory zero). By definition, compliers do not affiliate when they follow trajectory zero (but only when they follow trajectory one). This term is thus unobservable. Since all the other terms on the right-hand side of equation (10) are observed, we can derive bounds on the effect on getting affiliated with the Democratic Party conditional on being registered and affiliated with either of the two major parties by making assumptions about this term.

To obtain an upper bound, we set $E(D_0 | A_1 > A_0) = 0$. To obtain a lower bound, we need to make an assumption about how high $E(D_0 | A_1 > A_0)$ could possibly be. Conservatively, we replace this term by the fraction of affiliated Democrats among trajectory one movers affiliated with either of the major parties in their state of destination: 57 percent. (See online Appendix A.3 for a more detailed discussion of these assumptions.)

We use this method to construct bounds for the impact of trajectory one relatively to trajectory zero on average conditional Democratic Party affiliation after the move, using estimates of unconditional major-party affiliation and unconditional Democratic Party affiliation effects based on equation (9). We use a bootstrapping procedure to estimate the standard errors of the bounds: we draw a sample from our data with replacement, compute the lower and upper bounds as indicated above, repeat these two steps 50 times, and estimate the bounds’ empirical standard deviation.

Finally, we divide the results by the difference in average conditional Democratic Party affiliation between trajectory-one destination and origin states minus the difference in this outcome between trajectory-zero destination and origin states, so that the magnitude of the change after the move can be compared to estimates for unconditional outcomes provided in the rest of the paper.

We replicate this process to construct bounds on the effects on conditional Republican Party affiliation of moving to a state with higher unconditional major-party affiliation and higher conditional Republican Party affiliation than the state of origin (trajectory three), relative to moving to a state with lower

TABLE 4—BOUNDS ON THE DECOMPOSITION OF CONDITIONAL PARTY AFFILIATION DIFFERENCES

	1 (affiliated with the Democratic Party) (1)	1 (affiliated with the Republican Party) (2)
<i>Quantities on the right-hand side of equation (10)</i>		
Unconditional effect on major-party affiliation: $\text{Prob}(A_1 > A_0)$	0.061 (0.013)	0.114 (0.015)
Unconditional effect on party affiliation: $E(D_1 A_1 - D_0 A_0)$	0.058 (0.009)	0.067 (0.008)
Share major-party affiliated after trajectory 1: $E(A_1)$	0.443	0.471
Assumption on minimum value of $E(D_0 A_1 > A_0)$	0.000	0.000
Assumption on maximum value of $E(D_0 A_1 > A_0)$	0.570	0.455
<i>Bounds on conditional party affiliation effects of trajectory one relative to trajectory zero</i>		
Lower bound for effects on conditional party affiliation: $E(D_1 - D_0 A_1 = 1)$	0.051 (0.001)	0.031 (0.001)
Upper bound for effects on conditional party affiliation: $E(D_1 - D_0 A_1 = 1)$	0.130 (0.001)	0.142 (0.001)
<i>Bounds on conditional party affiliation effects rescaled by outcome differences</i>		
Δ conditional party affiliation between trajectory-one destination and origin states	0.109	0.106
Δ conditional party affiliation between trajectory-zero destination and origin states	-0.109	-0.103
Lower bound/ $(\Delta$ trajectory one - Δ trajectory zero)	0.236 (0.004)	0.150 (0.005)
Upper bound/ $(\Delta$ trajectory one - Δ trajectory zero)	0.598 (0.005)	0.677 (0.006)
Observations	15,138,474	12,871,530

Notes: The table reports bounds on conditional party affiliation effects following the procedure described in Section IVB. The sample in column 1 (resp. column 2) consists of two subsets of movers: movers going to a state with higher unconditional major-party affiliation and higher conditional Democratic Party (resp. Republican Party) affiliation than the state of origin (i.e., trajectory-one movers in column 1, trajectory-three movers in column 2), and voters moving to a state with lower major-party affiliation and lower conditional Democratic Party (resp. Republican Party) affiliation (i.e., trajectory-zero movers in column 1, trajectory-two movers in column 2). Impact estimates of unconditional effects come from regressions in the form of equation (9) and the corresponding standard errors are two-way clustered by voters and states. To make an assumption on the maximum value of $E(D_0 | A_1 > A_0)$ in column 1 (resp. column 2), we take the fraction of affiliated Democrats (resp. Republicans) among trajectory-one (resp. trajectory-three) movers affiliated with either major party in their states of destination. Standard errors for bounds on conditional party affiliation effects are computed using a voter-level bootstrap with 50 replications.

unconditional major-party affiliation and lower conditional Republican Party affiliation (trajectory two). Since the difference in conditional Republican Party affiliation is negative the difference in conditional Democratic Party affiliation, each mover falls in exactly one trajectory (zero, one, two, or three), and our estimates exploit the entire set of movers.

The results are shown in Table 4. Following trajectory one instead of trajectory zero increases the likelihood of movers to affiliate with the Democratic Party by 5.1 to 13.0 percentage points, or 23.6 to 59.8 percent of the relative difference in conditional Democratic Party affiliation between destination and origin states (column 1). Following trajectory three instead of trajectory two increases movers' likelihood to affiliate with the Republican Party by 15.0 to 67.7 percent of the relative difference in conditional Republican Party affiliation (column 2). These point

estimates can also be read as a decrease in movers' likelihood to affiliate with the Democratic Party by 15.0 to 67.7 percent of the relative difference in this outcome. These bounds are consistent with those shown in column 1 but a bit wider. Applied to two independent sets of movers, our method delivers similar results on the relative influence of contextual factors.

We conclude that contextual factors exert a large influence not only on unconditional party affiliation but also on conditional party affiliation and partisanship. Overall, place effects explain at least one fourth of the variation in partisanship across states.

C. Main Components of Voter and Place Effects

Cross-County Moves.—In Figure 6, panel D and online Appendix Figures A.8a and A.8c, the increase in post-move coefficients over time suggests that peer effects or other slow-moving factors are important drivers of partisanship.³⁰ Unlike state-level factors (such as the party exerting power, or state-wide party platforms), peers' influence and other local factors may generate differences in party affiliation across counties of the same state. If these factors contribute extensively to place effects, one should expect the fraction of cross-county differences in party affiliation explained by county fixed effects to be high.

Online Appendix Table A.9 shows the decomposition of within-state cross-county variation for registration and for the three unconditional party affiliation outcomes between county- and voter-driven components, and provides support for this prediction. The corresponding event studies are shown in Figure 7. The share of cross-county differences in registration explained by county effects is low: it oscillates between 7 and 11 percent for different sets of high- and low-registration counties (online Appendix Table A.9, panel B). This result is similar to the result obtained for turnout and calls for the same explanation. The share of cross-county differences in party affiliation due to county fixed effects is generally higher for unconditional major-party affiliation, affiliation with the Democratic Party, and affiliation with the Republican Party: 23 to 37 percent, 20 to 22 percent, and 17 to 18 percent, respectively (online Appendix Table A.9, panels C, D, and E). These fractions are only slightly lower than the shares of cross-state differences in party affiliation explained by state effects (see Table 3), contrasting with the large difference we observed between the cross-county and cross-state turnout decomposition.

The pattern of post-move coefficients in party affiliation event studies, and the large share of cross-county differences explained by county effects both point to effects of the composition of peers and possibly other local factors on partisanship. We contribute one additional piece of evidence to the exploration of the main components of place effects on registration and party affiliation by identifying the factors that most strongly correlate with the corresponding state fixed effects.

³⁰Online Appendix Table A.13 reports the results of a specification replacing the post-move coefficients in equation (6) with a post-move dummy and a linear trend. The trend is not statistically significant in the regressions corresponding to Figure 6, panel D, and online Appendix Figures A.8a and A.8c, but it is positive and significant at the 1 or 5 percent levels for all three party affiliation outcomes in the regressions for within-state cross-county moves corresponding to Figure 7, panels B, C, and D.

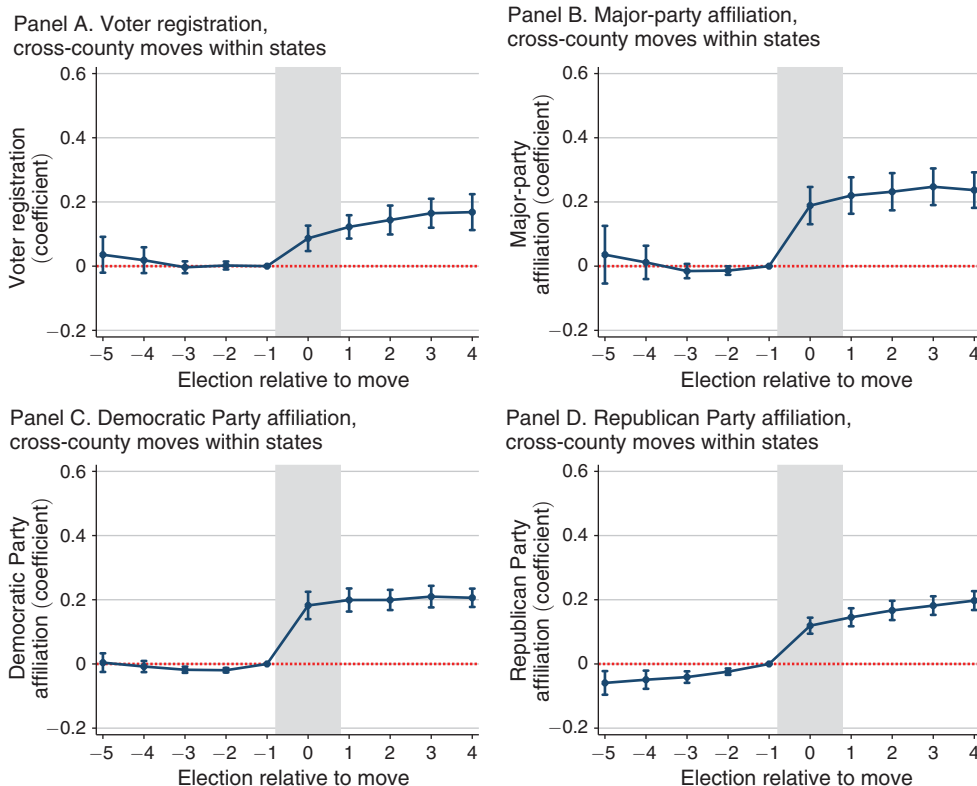


FIGURE 7. EVENT-STUDY PLOTS, VOTER REGISTRATION AND PARTY AFFILIATION, CROSS-COUNTY MOVES WITHIN STATES

Notes: The figure plots estimates of $\theta_{r(i,t)}$ and 95 percent confidence intervals (robust to two-way clustering by states and individuals) from event-study specification (6) in which the state fixed effects are replaced by county fixed effects. The sample consists of all within-state mover-years for movers whose county is known ($N = 115,466,589$) in the first graph and all within-state mover-years for moves within states in which party affiliation is available and for movers whose county is known ($N = 60,204,902$) in the second, third, and fourth graphs. Other notes as in Figure 6.

Correlates of Place Effects.—We report the results from the regressions of voter registration and unconditional party affiliation state fixed effects estimated from equation (1) on observable state and voter characteristics in Figure 8, panel A (for Democratic Party affiliation) and in online Appendix Figures A.9a, A.10a, and A.11a (for the other outcomes). Again, we focus on the covariates that are significantly correlated with state effects in the multivariate post-Lasso regression (right panels).

Interestingly, election day voter registration is negatively correlated with registration state effects (online Appendix Figure A.9a). This may result from the fact that many people only register conditional on voting, in states in which same-day registration is available, while in other states the number of registrants exceeds the number of those who actually turn out on the day of the election. In addition, electoral competitiveness is positively correlated with registration state fixed effects, similarly to the positive correlation we observed with turnout state fixed effects.

We interpret the results on observable correlates of place and, below, average voter effects on unconditional party affiliation with more caution since they rely

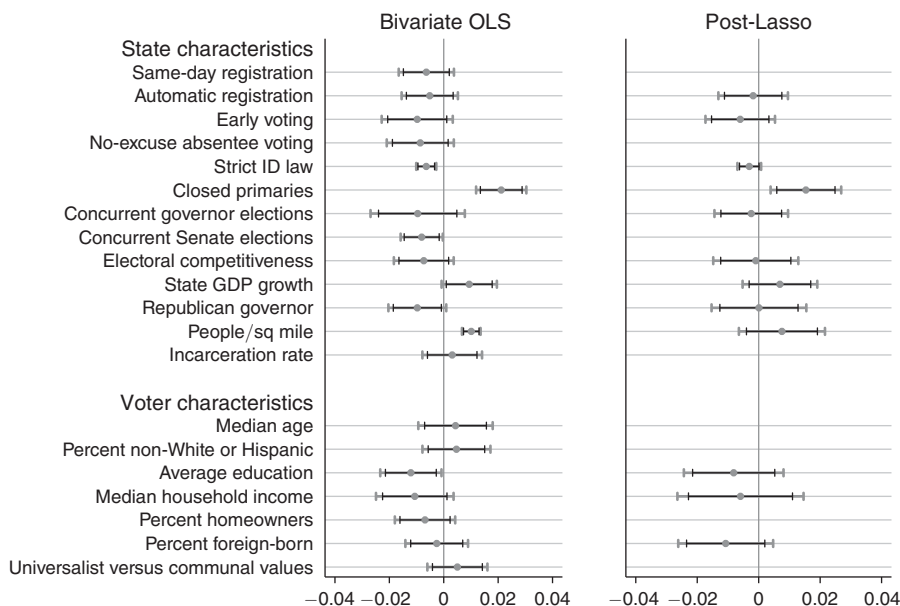
on fewer observations (i.e., the 30 states for which Catalist has party affiliation records) and any factor's impact on these outcomes may reflect its influence on registration, on getting affiliated with a party, or on affiliating specifically with the Republican or Democratic Party, conditional on being affiliated. As shown on online Appendix Figure A.10a, state effects for affiliating with either of the two major parties are positively correlated with closed primaries. In closed-primaries states, participating in the primaries of the Democratic or Republican Party is conditional on being affiliated with that party, providing a strong incentive for voters to take this step. Interestingly, state effects for major-party affiliation are also negatively correlated with household income, contrasting with the lack of significant correlation of turnout and registration state effects with any state average of sociodemographic characteristics. This correlation may reveal the influence of peers: as shown below, household income is also negatively correlated with voter effects. The positive and negative correlations with closed primaries and median household income are also observed for Democratic Party affiliation (Figure 8, panel A), and the negative correlation (albeit here insignificant) with automatic registration for Republican Party affiliation (online Appendix Figure A.11a).

Correlates of Voter Effects.—Average voter effects on registration are again negatively correlated with incarceration rate (online Appendix Figure A.9b). At the individual level, being Hispanic, being older than 30, and missing age information correlate negatively with the probability to be registered (online Appendix Figure A.9c). The latter correlation (which can be inferred from the positive correlation of all age-group dummies in the left panel) is unsurprising, as it is harder for Catalist to obtain age information for never-registered voters. The negative correlation with being above 30 years old probably comes from the fact that young voters are more likely to interact with a Department of Motor Vehicles and thus benefit more from motor voter and automatic registration laws.

Average voter effects on unconditional major-party affiliation are negatively correlated with median household income, perhaps reflecting an opportunity cost that richer voters face when getting involved in politics beyond registering and voting (online Appendix Figure A.10b). Consistent with intuition, average voter effects on Democratic Party affiliation are negatively correlated with average education (Figure 8, panel B). More surprising is the positive correlation found with median age. We also find a significant positive correlation with population density, which may reflect the fact that voters living in more urban areas are more likely to hold issue positions aligned with the Democrats. The only (negative) covariate of average voter effects on Republican Party affiliation selected by a Lasso regression is universalist versus communal values (online Appendix Figure A.11b), which corroborates Enke (2020)'s finding that Republican voters are more likely than Democrats to hold communal values.

Correlates of individual-level voter fixed effects (Figure 8, panel C and online Appendix Figures A.10c and A.11c for Democratic Party, major-party, and Republican Party affiliation, respectively) mostly follow intuition: minority status and being a woman strongly and positively correlate with Democratic affiliation, and negatively so with Republican affiliation. Finally, being aged 45 and above correlates with larger voter fixed effects for any type of affiliation than being a young voter.

Panel A. Correlates of Democratic Party affiliation state effects



Panel B. Correlates of Democratic Party affiliation average voter effects

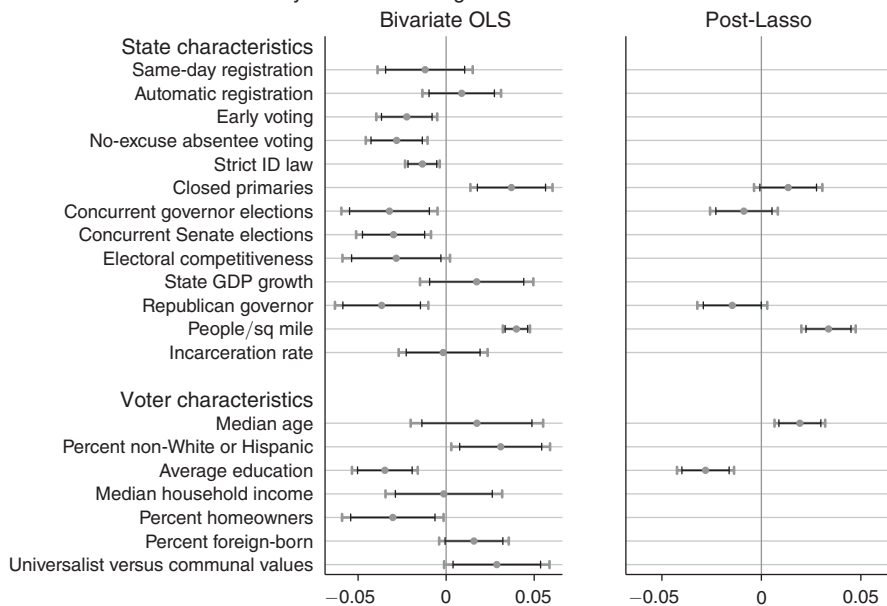


FIGURE 8. CORRELATES OF DEMOCRATIC PARTY AFFILIATION STATE AND VOTER EFFECTS

(continued)

V. Heterogeneity by Voter Characteristics

Thus far, the object of our investigation was the variation in *overall* participation and party affiliation. We now shift focus to specific groups of voters, defined by

Panel C. Correlates of Democratic Party affiliation individual-level voter effects

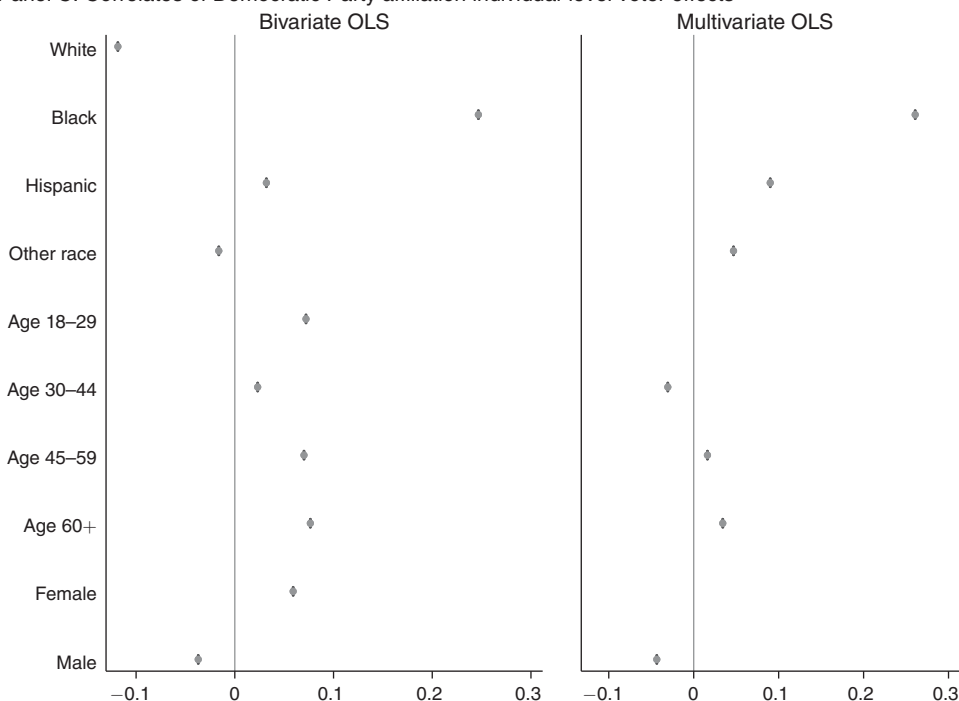


FIGURE 8. CORRELATES OF DEMOCRATIC PARTY AFFILIATION STATE AND VOTER EFFECTS (CONTINUED)

Notes: See notes to Figure 5.

age, gender, or race, and we test how the relative importance of contextual factors varies across them.

Formally, we run linearly additive decompositions in the form of equation (1) separately for men and women, Whites and non-Whites, and for voters aged 18 through 29, 30 through 44, 45 through 59, and 60 and above. In each regression, the sample is restricted to movers and nonmovers of the corresponding group. The results are shown in Table 5. Before presenting them, let us make an important technical aside on additive separability.

A. Robustness of the Main Results to Using Group-Specific State Fixed Effects

Heterogeneity across age, gender, and race in the share of outcome differences attributable to state effects $S^{state}(R, R')$ may result from heterogeneity in any of the following dimensions: the split between the states with highest and lowest average outcome (R, R'), the difference between voter-specific components ($\bar{y}_R^{vot} - \bar{y}_{R'}^{vot}$), and the difference between state fixed effects ($\gamma_R - \gamma_{R'}$).

The difference between state fixed effects can only vary across voter types if state fixed effects are themselves different across voters, which would violate the hypothesis of additive separability. Our estimates of equation (1) separately by groups of voters allow to explore this possibility. The correlations between state fixed effects

TABLE 5—DECOMPOSITION OF VOTER TURNOUT, REGISTRATION,
AND PARTY AFFILIATION DIFFERENCES BY SUBGROUP

Sample	Observations (1)	Mean outcome (2)	Difference in outcome above/ below median (3)	Difference due to voters (4)	Difference due to states (5)	Share due to voters (6)
<i>Panel A. Outcome: 1(voted)</i>						
1. Aged 18 through 29	244,305,968	0.342	0.077	0.041	0.036	0.529
2. Aged 30 through 44	368,066,693	0.392	0.085	0.056	0.029	0.662
3. Aged 45 through 59	398,033,570	0.514	0.092	0.062	0.030	0.671
4. Aged 60 or older	405,816,813	0.574	0.088	0.073	0.015	0.825
5. White	1,161,493,774	0.458	0.072	0.042	0.031	0.573
6. Non-White	410,731,615	0.339	0.082	0.070	0.012	0.849
7. Female	829,378,379	0.437	0.071	0.046	0.025	0.646
8. Male	713,789,295	0.430	0.075	0.046	0.029	0.615
<i>Panel B. Outcome: 1(registered)</i>						
1. Aged 18 through 29	244,305,968	0.809	0.096	0.041	0.054	0.433
2. Aged 30 through 44	368,066,693	0.709	0.084	0.060	0.023	0.722
3. Aged 45 through 59	398,033,570	0.749	0.066	0.053	0.013	0.806
4. Aged 60 or older	405,816,813	0.756	0.055	0.076	-0.020	1.365
5. White	1,161,493,774	0.697	0.073	0.055	0.018	0.749
6. Non-White	410,731,615	0.653	0.123	0.072	0.051	0.587
7. Female	829,378,379	0.692	0.072	0.058	0.014	0.803
8. Male	713,789,295	0.697	0.071	0.048	0.023	0.673
<i>Panel C. Outcome: 1(affiliated with a major party)</i>						
1. Aged 18 through 29	135,305,803	0.502	0.200	0.103	0.098	0.513
2. Aged 30 through 44	203,190,514	0.470	0.176	0.109	0.066	0.622
3. Aged 45 through 59	222,805,392	0.552	0.172	0.115	0.057	0.668
4. Aged 60 or older	231,677,307	0.608	0.162	0.111	0.051	0.683
5. White	631,302,653	0.500	0.152	0.087	0.065	0.575
6. Non-White	245,751,155	0.467	0.199	0.112	0.087	0.564
7. Female	461,490,881	0.509	0.161	0.092	0.069	0.571
8. Male	398,460,629	0.487	0.159	0.094	0.065	0.592
<i>Panel D. Outcome: 1(affiliated with the Democratic Party)</i>						
1. Aged 18 through 29	135,305,803	0.319	0.167	0.120	0.047	0.718
2. Aged 30 through 44	203,190,514	0.283	0.155	0.113	0.042	0.731
3. Aged 45 through 59	222,805,392	0.310	0.161	0.121	0.040	0.752
4. Aged 60 or older	231,677,307	0.344	0.166	0.119	0.048	0.714
5. White	631,302,653	0.247	0.121	0.079	0.041	0.657
6. Non-White	245,751,155	0.389	0.200	0.130	0.070	0.652
7. Female	461,490,881	0.315	0.149	0.106	0.044	0.707
8. Male	398,460,629	0.263	0.141	0.103	0.038	0.733
<i>Panel E. Outcome: 1(affiliated with the Republican Party)</i>						
1. Aged 18 through 29	135,305,803	0.183	0.122	0.101	0.021	0.829
2. Aged 30 through 44	203,190,514	0.187	0.116	0.097	0.020	0.830
3. Aged 45 through 59	222,805,392	0.242	0.127	0.104	0.024	0.813
4. Aged 60 or older	231,677,307	0.264	0.138	0.095	0.043	0.689
5. White	631,302,653	0.253	0.121	0.092	0.029	0.761
6. Non-White	245,751,155	0.079	0.056	0.045	0.011	0.801
7. Female	461,490,881	0.193	0.113	0.091	0.023	0.801
8. Male	398,460,629	0.224	0.113	0.085	0.028	0.753

Notes: The table reports state-level decompositions for states above versus below the median outcome, estimated separately by age categories, gender, and race. Each panel corresponds to a different outcome. Each row corresponds to a distinct sample/decomposition. In panels C–E, the sample of the underlying regressions is restricted to the 30 states for which Catalist records party affiliation.

estimated separately on men and women, voters younger and older than 45, and Whites and non-Whites are not perfect but strong: 0.99, 0.67, and 0.76, respectively.

In addition, we check the robustness of the main decompositions shown in Sections III and IV to including group-specific state, time, and election relative to move fixed effects in an augmented version of equation (1):

$$(11) \quad y_{ijt} = \alpha_i + \sum_k (\gamma_j^k + \tau_t^k + \rho_{r(i,t)}^k) \times 1(i \in k) + \varepsilon_{ijt},$$

where k denotes a group of voters defined by age, gender, or race. We use this equation to compute alternative decompositions of outcome differences between state and voter shares. We first define \tilde{y}_{jt} as the weighted average of y_{ijt} across groups of voters living in state j in election t , and \tilde{y}_j as the average of \tilde{y}_{jt} across t . For a reason that will become clear below, these averages use weights δ^k that correspond to the share of voters of each group in the entire US population, not just in state j . As a result, \tilde{y}_{jt} and \tilde{y}_j differ from the simple state averages \bar{y}_{jt} and \bar{y}_j defined in Section IIA. Using the estimates from equation (11), we can write \tilde{y}_j as the sum of a place effect, equal to the weighted average of group-specific state fixed effects, and a voter effect, equal to the weighted average of group-specific voter effects: $\sum_k \delta^k \gamma_j^k$ and $\sum_k \delta^k \bar{y}_j^{\text{vot},k}$. Then, for any two states j and j' ,

$$(12) \quad \tilde{y}_j - \tilde{y}_{j'} = \left(\sum_k \delta^k \gamma_j^k - \sum_k \delta^k \gamma_{j'}^k \right) + \left(\sum_k \delta^k \bar{y}_j^{\text{vot},k} - \sum_k \delta^k \bar{y}_{j'}^{\text{vot},k} \right),$$

and the share of the difference in voter behavior between these states attributable to state effects is given by

$$(13) \quad S^{\text{state,weight,group}}(j,j') = \frac{\sum_k \delta^k \gamma_j^k - \sum_k \delta^k \gamma_{j'}^k}{\tilde{y}_j - \tilde{y}_{j'}}.$$

We assess the extent to which allowing state fixed effects to differ across groups affects our results by comparing $S^{\text{state,weight,group}}(j,j')$ (or $S^{\text{state,weight,group}}(R,R')$, for groups of states R and R') to $S^{\text{state,weight}}(j,j') = (\gamma_j - \gamma_{j'})/(\tilde{y}_j - \tilde{y}_{j'})$ (or $S^{\text{state,weight}}(R,R')$), the share obtained by using the average state fixed effects γ_j estimated with equation (1) instead of group-specific state fixed effects γ_j^k , in equation (13); $S^{\text{state,weight}}(j,j')$ differs from $S^{\text{state}}(j,j')$, defined in Section IIA, since \tilde{y}_j differs from \bar{y}_j .

The reason why $S^{\text{state,weight,group}}(j,j')$ requires identical group-specific weights δ^k across all states is that, for each group, state fixed effects are only identified up to a constant. Thus, if we used state-specific weights (e.g., group k 's share of state j 's population), $S^{\text{state,weight,group}}(j,j')$ would depend on the arbitrary choice of the baseline state. To see why, imagine that group k 's state fixed effects are all scaled by a constant μ . This constant cancels out when multiplied by the same δ^k on both sides of the difference in the numerator of equation (13), but it would not cancel out using state-specific weights δ_j^k and $\delta_{j'}^k$.³¹

³¹ Using identical group-specific weights δ^k across all states has a second advantage. Suppose voters of some race, gender, or age always select into locations with specific characteristics (e.g., areas with high polling place wait time), whichever state they go to. The effects on voting behavior of such characteristics would then be mistakenly captured by these voters' fixed effects, as already discussed in footnote 19. In turn, this would affect our main decomposition, since the fraction of voters of a specific group varies across states, but not our decomposition using group-specific fixed effects. Indeed, with group-specific weights identical across all states, any effect shifting the voting behavior of a certain group in all states cancels out from $\tilde{y}_j - \tilde{y}_{j'}$, which is at the denominator of $S^{\text{state,weight,group}}(j,j')$.

Using group-specific state fixed effects does not substantively alter our results. As shown in online Appendix Table A.14, the estimated shares of differences between states with above- and below-median outcomes that are due to state effects are similar whether we use group-specific state fixed effects ($S^{state,weight,group}(R, R')$) or not ($S^{state,weight}(R, R')$).

B. *Heterogeneity across Ages*

A growing body of evidence shows that the first years of adult life can be critical because young adults undergo a learning process that profoundly and durably shapes attitudes and behaviors, including voting behavior (Neundorff and Smets 2017). On the other hand, a different strand of the literature finds that the behavior of young voters is partly determined by traits developed in early childhood, such as psychosocial skills (e.g., Holbein 2017). Table 5 first compares the relative influence of contextual and individual factors for voters of different ages. If voters in their twenties are truly more malleable, one could expect them to be relatively more influenced by place effects.

We observe that the difference between state fixed effects of the top and bottom half of states ranked by average turnout is larger for young voters (3.6 percentage points), who also participate less on average, than for any other age. The younger the voters, the higher the share of state differences in their average turnout that are explained by place factors: from only 17 percent, for voters older than 60, to 47 percent, for voters less than 30 years old. We find qualitatively similar patterns for registration and two of our party affiliation outcomes: being affiliated with either major party or with the Democratic Party. The picture only looks different for Republican Party affiliation: place effects on this outcome are of similar magnitude for voters aged 18 through 29, 30 through 44, and 45 through 59, and they are higher for voters above 60, who are also the most likely to be affiliated with the Republicans.

Overall, these decompositions by age group reveal that the context matters relatively more for younger voters, and particularly so when it comes to the decision of voting or staying at home. While these voters have been found to be more susceptible to the influence of specific factors such as voting rules (e.g., Holbein and Hillygus 2016) or electoral campaigns (e.g., Le Pennec and Pons 2020), our results provide a more general test of the impressionable years hypothesis.

C. *Heterogeneity across Races*

Table 5 further reports the results of linearly additive decompositions for Whites and non-Whites (Blacks, Hispanics, or voters of other race).

Average participation is lower by nearly 12 percentage points among non-Whites. A large literature investigates the causes of this difference. Our research setting does not allow us to address this question directly: our identification based on movers enables to decompose outcome variation across places or within groups, not across groups.³² Yet, we can shed light on one particular hypothesis: the possibility that the

³² State fixed effects obtained by estimating equation (1) separately for different types of voters are only identified up to a constant, in each group. Therefore, their magnitude cannot be directly compared across groups.

low turnout of ethnic minorities comes from efforts by a subset of states to selectively disenfranchise them. A first prediction of this theory, supported in our data, is that the electoral participation of ethnic minorities varies a lot across states. Indeed, the difference in registration and turnout rates of non-Whites between states above versus below the median outcome is 12.3 and 8.2 percentage points, respectively, which is larger than the difference for Whites (7.3 and 7.2 percentage points). A second prediction is that the share of the difference in these outcomes due to place effects is itself very large. We find some support for this prediction for voter registration, with a share of place effects for non-Whites of 41 percent, compared to 25 percent for Whites, suggesting that registration rules and other registration-related place factors exert more influence on the former. However, the comparison is completely reversed for voter turnout, with shares of place effects of 43 and 15 percent, for Whites and non-Whites, and differences between average turnout in the 25 top and bottom states due to states of 3.1 and 1.2 percentage points, respectively. The low share of cross-state variation in the participation of non-Whites that is explained by the state-specific component suggests that, overall, their low average turnout is driven by individual factors (which are responsible for a difference between top and bottom states of 7.0 percentage points, as compared to 4.2 percentage points for Whites) and by contextual factors present across all states (such as racial disparities in voting wait times (e.g., Chen et al. forthcoming)), more than by exclusionary voting laws and other contextual factors prevailing in some specific states only.

In contrast to the differences observed for registration and voter turnout, the decomposition of cross-state variation in party affiliations between individual and place factors is very similar across Whites and non-Whites.

D. *Heterogeneity across Genders*

Finally we compare the role of place- and voter-driven effects across genders. The decomposition of cross-state variation in voter registration and turnout between individual and place factors is much closer for men and women than it is across age groups and races. Similarly, the share of differences in party affiliation between states above versus below the median outcome which is due to place effects is very similar for both genders.

VI. Conclusion

This paper gives precise new evidence on the overall influence of the context versus individual factors on voter behavior, using a total of over one and a half billion observations. We complement a large number of studies that have provided piecemeal responses to this question, each focusing on a single factor. In contrast, our method relies on identifying voters who move across states and tracking their behavior over time. We find that movers' turnout, registration, and party affiliation are mostly stable before move, but they jump immediately thereafter, closing part of the gap in average outcomes between their states of origin and destination. Exploiting the variation underlying these event-study results, we estimate that place factors explain about 37 percent of the observed difference in participation between states with above- and below-median voter turnout, as well as 32, 22, 29, and 44 percent

of the cross-state variation in voter registration, Republican affiliation, Democratic affiliation, and affiliation with either major party, respectively.

While the overall influence of the context on participation and partisanship is of similar magnitude, the underlying mechanisms differ. Peer effects appear to contribute to the influence of context on party affiliation, whereas place effects on turnout mostly reflect the impact of state-specific rules, as well as economic and political environments. The strongest correlates of the state effects also differ across the two outcomes: electoral competitiveness, along with the availability of same-day registration and no-excuse absentee voting, for voter turnout, and closed primaries and median income, for party affiliation.

Overall, our findings demonstrate that context exerts a considerable influence on participation and on partisanship, but that it does not outweigh individual drivers of voting behavior. The fact that place effects are nearly as large for partisanship as for turnout is particularly striking, given the widely held prior that partisan views are highly persistent. It remains that more than half of the variation in registration, turnout, and any type of party affiliation observed across states, overall and for most groups of voters, is driven by voter effects. The relative influence of context versus the individual on voting behavior is substantially lower than for other types of behavior such as health care utilization (Finkelstein, Gentzkow, and Williams 2016) or purchases of consumer goods (Bronnenberg, Dubé, and Gentzkow 2012).

Only among one group—young voters—does context match individual factors in determining voter behavior. The influence of place on the participation and, to some extent, party affiliation of voters aged 18 to 29 is much larger than for older voters. These differences across age are greater than across gender and race. Furthermore, education is one of the strongest correlates of average voter effects for both turnout and party affiliation, suggesting that formative experiences shape people's attitudes and behavior for a long time. Current debates rightly focus on the effects of voting rules across race, and the consequences of the underrepresentation of women and ethnic minorities among politicians for the political engagement of these groups. Our results suggest a broadening of this discussion to another minority, young voters. The contextual forces responsible for the long-term decline in turnout and for the recent rise in polarization in the United States—whatever those may be—are likely to affect youths disproportionately, with lasting consequences. This is another important reason to work to understand and counteract such trends.

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