# The turnout effects of district competitiveness: evidence from repeated redistricting in North Carolina

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**Abstract**

In this paper, we study whether voters are more likely to turn out when they live in competitive legislative districts. To do this, we employ rich data on the 2006 to 2020 elections in North Carolina. We make use of variation in district competitiveness due to repeated bouts of redistricting, a process in which district boundaries are redrawn. Specifically, we compare individuals who share the same districts in each legislative chamber (U.S. House, NC Senate, NC House) before redistricting but who differ in districts after redistricting. We match these individuals on demographics, party registration, and pre-redistricting turnout. We then compare their turnout behavior in post-redistricting elections. For the U.S. House, switching from an uncompetitive "80-20" district to a competitive "55-45" district increases turnout by an average of 1.12 percentage points per election of exposure. For the NC Senate and NC House, the effects are 0.56 and 0.51 percentage points. We use our causal estimates to predict how outcomes in recent North Carolina elections would have differed if all individuals lived in competitive districts. We find that turnout would have increased across the board, with larger impacts for Democrats and for Unaffiliated registrants. However, the net effect on statewide vote shares would have been negligible.

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# 1 Introduction

A central question in research on voting is whether people turn out more in competitive elections. This question is of both scientific and policy interest. The answer has the potential to illuminate the mechanics of voting behavior. It also can help guide policymakers in designing electoral institutions that promote voter participation.

From a theoretical point of view, the relationship between turnout and competitiveness is uncertain. On the one hand, there are ample reasons why a person may be more likely to turn out when subject to a competitive election. On the other hand, there are also stories in which competitiveness may not affect turnout.[1]

Consistent with the theoretical ambiguity, empirical evidence on the link between competitiveness and turnout is mixed. Papers using cross-sectional techniques often measure a strong association (see Cancela and Geys (2016) for a review). In addition, a few papers have identified non-zero causal effects when studying natural experiments in Europe.[2] However, the few causal papers that study the U.S. have found little effect. For instance, Enos and Fowler (2018) and Gerber et al. (2020) find no impact of randomly providing individuals with information on competitiveness. Similarly, Moskowitz and Schneer (2019) detect only modest impacts when examining the short-run effects of changes in the competitiveness of voters' congressional districts.

In this paper, we expand on the work in Moskowitz and Schneer (2019). Like them, we study the turnout effects of the competitiveness of American legislative districts. Unlike them, we provide results over both the short- and long-run and for districts associated with both federal and state legislatures. In addition, we explore heterogeneity in effects, probe mechanisms, and assess whether results are sensitive to the way that competitiveness is measured. We also quantify how the competitiveness of recently used districts affected election outcomes.

To obtain our results, we employ rich longitudinal data from the state of North Carolina. The data includes each person who was registered to vote (a "registrant") during each of the 2006 to 2020 elections. We exploit variation in competitiveness due to "redistricting", a process in which district boundaries are redrawn. Specifically, we study individuals who lived in the same districts before redistricting but who are assigned to different districts after redistricting. We match these individuals on demographics and pre-redistricting turnout, and we run a number of tests to confirm that the matching eliminates bias. We then examine differences in turnout after redistricting, asking whether individuals who are placed into more competitive districts are induced to turn out more. We find that competitiveness does impact turnout, with effects that sum across legislative chambers and that grow with the number of elections of exposure. Finally, we use our causal estimates to investigate the aggregate electoral consequences of the districts

1. We discuss these stories in detail in Section 2.

2. These relate to runoff elections (Fauvelle-Aymar and Francois 2006; Indridason 2008; Simonovits 2012; Paola and Scoppa 2014), or to access to polling information (Morton et al. 2015; Bursztyn et al. 2021).

that were recently used in North Carolina. We show that the districts used in the 2010s caused relative declines in turnout for Democrats and for Unaffiliated registrants but had little effect on statewide vote shares.

One point about our paper bears emphasizing. Our primary treatment variable is the underlying competitiveness of a legislative district, not of a district's race in a particular election.[3] We focus on *district* competitiveness because it is a lever that can be manipulated by policymakers. Namely, the measures of district competitiveness that we use can be observed at the time of redistricting. As a result, the causal effects that we recover can be employed during redistricting to forecast the impacts of different district configurations. Nonetheless, when exploring mechanisms, we also examine the effects of competitive races.

In the paper, we measure district competitiveness in multiple ways. Two of the measures are based on a simulation procedure developed in Ainsworth (2020). These measures are constructed by simulating a large number of counterfactual elections and then summarizing a district's races across the elections.[4] A third measure is based on the Partisan Voting Index (PVI) from the Cook Political Report. The PVI is relied on in the existing literature; thus, this measure allows us to benchmark our findings. When exploring mechanisms, we also use two measures of race competitiveness. These are the amount of spending in the race and the closeness of the race's actual vote shares.

Our empirical strategy builds on a growing literature that uses redistricting to gain variation in the characteristics of legislative districts.[5] In North Carolina, there were repeated bouts of redistricting in the 2010s. In 2011, districts were redrawn both for the federal legislature (the U.S. House of Representatives) and for the two state legislatures (the NC Senate and NC House). Depending on the legislative chamber, districts were redrawn again—in response to court orders—in 2015, 2017, and 2019. In the analysis, we define a redistricting "episode" as an instance in which the districts for a particular chamber were redrawn. We consider all the episodes in the 2010s by stacking them on top of each other.

Our empirical strategy proceeds in three steps. The first step is to divide North Carolina into regions based on the districts that were used before and after a redistricting episode. In our main analysis, we define regions as areas that have all the same pre-redistricting districts and that differ in districts for only a single chamber after redistricting. These regions allow us to identify the partial effect of changing competitiveness for just one of the three districts that a voter faces.

3. A district's competitiveness is the district's propensity, across elections, to have competitive races. Whether a district has a competitive race in a particular election depends in part on stochastic factors, such as the electoral swings that occur in the election and the quality of the candidates that run in the race.

4. We simulate elections by randomly drawing partisan swings and turnout shocks. The draws come from a distribution that reflects the observed variance of swings and shocks in North Carolina over recent elections.

5. Examples include Ansolabehere, Snyder, and Stewart (2000) and Sekhon and Titiunik (2012) on the strength of a district's incumbency advantage, Fraga (2016) and Henderson, Sekhon, and Titiunik (2016) on a district's racial composition, Moskowitz and Schneer (2019) on district competitiveness, and Fraga, Moskowitz, and Schneer (2021) on a district's partisan makeup.

In an additional analysis, we define regions based only on pre-redistricting districts, and we run comparisons for individuals who may differ in multiple post-redistricting districts. This allows us to explore how the effects of competitiveness aggregate across legislative chambers.

The second step in our empirical strategy is to match registrants—within regions—based on their characteristics. Matching deals with the fact that there may be systematic differences between individuals who are placed into more or less competitive districts, even within regions. In different specifications, we match on a variety of demographic and political variables, including race/ethnicity, gender, age, home value, neighborhood characteristics, registration party, and turnout in up to three elections prior to redistricting. We validate the quality of the matching procedure using a number of tests. For instance, we show that matched registrants exhibit similar turnout in pre-redistricting elections that are not used in matching. Finally, the last step in our empirical strategy is to compare turnout for the matched registrants in post-redistricting elections.

Our empirical strategy suffers from one potential threat. This is that a district's competitiveness may be correlated with other district characteristics that also influence turnout. If so, then the turnout effects that we recover would be the combination of the causal effect of competitiveness and of these other attributes. This concern is relevant in our setting because of the existence of "majority-minority" districts. Majority-minority districts are districts that are intended to give racial minority groups a chance to elect representatives of their choosing. The districts are heavily minority and, in practice, tend to be highly Democratic and uncompetitive. As a result, in North Carolina, district competitiveness is negatively correlated with both share minority and share Democratic. We test for bias due to these correlations by re-estimating effects on a sample that excludes the districts with the largest minority shares. On this sample, the correlations with share minority and share Democratic are zero, which means that our empirical strategy recovers the effect of competitiveness alone. We show that results on the restricted sample are similar to those from the full sample, assuaging concerns about bias.

Our main finding is that district competitiveness influences turnout. Further, effects (i) are linear in competitiveness, (ii) scale in the number of elections of exposure, and (iii) are additive across legislative chambers. In order to explain magnitudes, we consider the impact of switching between two hypothetical districts—an "80-20" district (in which the parties are expected to split the two-party vote 80% to 20%) and a "55-45" district (in which the expected split is 55-45). For the U.S. House, we find that switching into the more competitive district would, on average, cause turnout to increase at a rate of 1.12 percentage points per election. For the NC Senate and NC House, the magnitudes are 0.56 and 0.51 percentage points. If a registrant were to experience a change in districts for multiple chambers, the overall effect would be the sum of the above-described, chamber-specific effects.

Another finding is that the effects on turnout are persistent. To understand this, suppose that in a subsequent round of redistricting, registrants in the 80-20 and 55-45 districts are placed into districts of equal competitiveness. We find that the registrants who had been living in the more competitive district would continue to turn out more. In fact, our results are consistent with a model in which lagged competitiveness has the same effect on turnout as current competitiveness for at least four lagged elections.

In terms of mechanisms, we show that the effect of district competitiveness operates mostly through exposure to competitive races. With respect to different measures of district competitiveness, we find that our main results hold regardless of the measure that we use. That said, effects are slightly larger for the measures based on the Ainsworth (2020) simulation procedure than those based on the Cook PVI. This is likely because the simulation-based measures are better than the PVI-based measure at predicting which districts have competitive races.

To assess external validity, we benchmark our results to those in Moskowitz and Schneer (2019). Moskowitz and Schneer use data from all 50 states; thus, this exercise allows us to compare our North Carolina setting with the country as a whole. When we use the same empirical strategy and analysis period as in Moskowitz and Schneer (2019), we show that we replicate their results. Thus, our findings appear to have broad relevance to other U.S. settings.

With our rich data, we are able to explore considerable heterogeneity in effects. First, we find that effects differ little between midterm and presidential elections. This is despite the fact that presidential years contain more—and higher profile—non-legislative races. Second, we find that effects are similar for males and females, but are slightly larger for young registrants, for registrants in well-educated neighborhoods, and for registrants who did not vote in the election prior to redistricting. Third, we find that effects are larger for whites than for racial minorities. Finally, among whites, effects are largest for Democrats and smallest for Republicans.[6]

The last component of our analysis is to investigate how the competitiveness of North Carolina's recent legislative districts affected election outcomes. To do this, we consider a counterfactual situation in which each district used since 2011 was a 55-45 district. We ask how turnout and vote shares would have differed if individuals had lived in these competitive districts rather than their actual districts. We predict that turnout probabilities would have been higher for most registrants, with relatively larger impacts for Democrats and Unaffiliated registrants. For instance, in the 2020 election, overall turnout would have risen by 2.6 percentage points. For Democrats and Unaffiliated registrants, the increase would have been 2.9 percentage points; for Republicans, it would have been 2.0 percentage points. We translate the turnout impacts into effects on aggregate vote shares by using information on registrants' probabilities of preferring Democratic or Republican candidates.[7] We find that competitive districts would have caused little change in vote shares. This is because the boost in turnout among Democratic registrants is driven by individuals with a significant chance of voting for Republicans.

Our paper relates to a number of literatures. First, the paper contributes to the previously

6. Importantly, the heterogeneity by race and party is not due to the impacts of majority-minority districts.

7. We obtain these probabilities from Ainsworth (2020).

discussed literature on whether electoral competitiveness spurs turnout. Our results indicate that it does, even in a context in which elections feature a variety of races on the ballot. Second, the paper adds to a voluminous literature on the determinants of voting behavior. It provides empirical evidence that is consistent with models in which voters learn from past experiences (Kanazawa 1998; Bendor, Diermeier, and Ting 2003; Esponda and Pouzo 2017) and with research that voting is habit-forming (Gerber, Green, and Shachar 2003; Meredith 2009; Mullainathan and Washington 2009; Fujiwara, Meng, and Vogl 2016). Third, the paper contributes to research on majority-minority districts (e.g., Hayes and McKee (2012), Washington (2012), Fraga (2016), and Davis (2019)). It suggests that the uncompetitive nature of these districts reduces turnout. Fourth, the paper adds to a recent literature that uses causal methods to explore which electoral policies affect turnout (e.g., Braconnier, Dormagen, and Pons (2017), Cantoni and Pons (2019), Bhatt, Dechter, and Holden (2020), Cantoni (2020), and Kaplan and Yuan (2020)). It highlights that the drawing of competitive or uncompetitive legislative districts is one such policy.

The rest of the paper proceeds as follows. In Section 2, we provide a conceptual overview of the relationship between competitiveness and turnout. In Section 3, we describe our institutional setting, the data, and the competitiveness measures. In Section 4, we explain the empirical strategy. In Section 5, we present our main causal results, and in Section 6 we convert these into effects on election outcomes. Finally, in Section 7, we conclude.

# 2 The relationship between competitiveness and turnout

We start with a conceptual overview of the relationship between electoral competitiveness and voter turnout. We first analyze the effect of competitive races. We then consider the impact of competitive districts.

From a theoretical point of view, there are stories both supporting and questioning whether competitive races affect turnout. On the one hand, there are a number of reasons why competitive races may increase turnout. First, people may think they have a better chance of influencing a race's outcome when the race is competitive (Downs 1957). Second, the expressive benefits that individuals get from voting may grow with a race's competitiveness (Riker and Ordeshook 1968; Hamlin and Jennings 2011; Kawai, Toyama, and Watanabe 2021). Third, individuals in competitive races may feel a stronger civic duty to vote (Feddersen and Sandroni 2006; Dellavigna et al. 2016). Fourth, competitive races may generate more media attention or popular buzz (Clarke and Evans 1983). This could help to remind potential voters that an election is occurring or to reduce their uncertainty about the quality of the candidates (Degan and Merlo 2011). Fifth, competitive races may attract better candidates, who are able to motivate additional turnout (Stephanopoulos and Warshaw 2020). Sixth, national and local political parties may devote more resources to competitive races. This could lead to more spending on "get out the vote" efforts, including voter registration drives, advertising campaigns, or door-knocking and canvassing operations (Cox and Munger 1989; Shachar and Nalebuff 1999; Hill and McKee 2005). In sum, there are diverse channels through which competitive races could spur turnout. Some of these channels depend on voters being aware of a race's competitiveness; others do not.

Nonetheless, there are also reasons why a race's competitiveness may not matter for turnout. First, there is evidence that people have little sense of whether a race is competitive (McDonald and Tolbert 2012; Moskowitz and Schneer 2019; Gerber et al. 2020); given this, the channels that depend on voters being aware of competitiveness may not be applicable. Second, elections often feature multiple races, such as races for different political offices, as well as ballot initiatives and referenda. It's possible that the competitiveness of a single race on the ballot is not enough to sway a person's overall turnout decision. Third, the net effect of "get out the vote" efforts is unclear. Namely, parties may work both to increase turnout for their supporters and to decrease turnout for other types of voters (Spenkuch and Toniatti 2018). Aggregated over all parties, these contrasting efforts may cancel. Thus, it may be that competitive races have little effect on turnout.

The turnout effect of competitive districts depends closely on that of competitive races. The main manner by which competitive districts may affect turnout is by increasing the probability that an individual experiences a competitive race. That said, there are two additional ways in which competitive districts may matter, both of which stem from the fact that districts are used for multiple elections. First, living in a competitive district makes a person likely to experience a succession of competitive races over time. This could give individuals an opportunity to learn from past experiences and to realize that future races in their district are likely to be competitive. Second, a district's competitiveness might influence the strength of the local political parties. In competitive districts, the local parties may have more resources and better-developed infrastructure—due, again, to the recurrence of competitive races. This could help the parties to engage residents and be a presence in the community. However, as with "get out the vote" efforts, the impacts of stronger parties may cancel if they are symmetric. Thus, it is unclear *a priori* whether competitive districts affect turnout. Answering this question requires empirical analysis.

# 3 Setting, data, and competitiveness measures

We now discuss three inputs for our paper: the setting, the data, and the competitiveness measures.

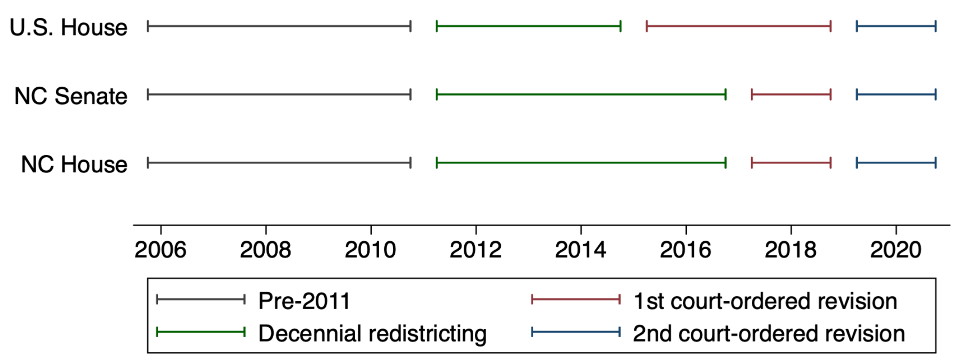
## 3.1 Institutional setting

Our setting is the 2006 to 2020 general elections in North Carolina. There are a few features of this setting that are worth highlighting. First, in North Carolina, general elections involve multiple races. In midterms, there are legislative races, races for judicial offices, ballot initiatives, local races, and, possibly, races for the U.S. Senate. In presidential years, there are additionally races for president and for state offices, such as governor or attorney general.

Second, Democrats and Republicans tend to earn similar shares of the statewide vote in North Carolina. This means that statewide races, such as those for governor or president, can be highly competitive. The existence of competitive statewide races has an uncertain impact on the external validity of the North Carolina setting. On the one hand, these races likely give voters a strong incentive to turn out, regardless of whether they live in competitive legislative districts.[8] If so, then the effects of competitive districts may be smaller in North Carolina than in other settings. On the other hand, competitive statewide races may make North Carolinians relatively sophisticated in terms of their understanding of politics. This could mean that they are more sensitive to district competitiveness than are people elsewhere. In Appendix A5, we provide evidence that the effect of district competitiveness is similar in North Carolina and in other states, suggesting that the two channels may cancel.

Next, in North Carolina, there are three legislative chambers in which races vary by district: the U.S. House of Representatives, the NC Senate, and the NC House. During the sample period, the U.S. House had 13 districts, the NC Senate had 50, and the NC House had 120. In all chambers, representatives face reelection every two years.

Figure 1: The districts used in recent North Carolina elections




The figure summarizes the district configurations that were used in elections since 2006. Districts for elections prior to 2011 were drawn by the state legislature in 2002 for the U.S. House and in 2004 for the NC Senate and NC House. In addition, districts for the NC House were modified to a small degree in two counties between the 2008 and 2010 elections. Due to the minor nature of this change, we ignore it. See Section 3 for details on the districts used after 2011.

Last, legislative districts in North Carolina were redrawn on multiple occasions during the 2010s. The timeline of these changes is summarized in Figure 1. In 2011, districts were redrawn for each of the three legislative chambers, as part of the U.S.'s decennial redistricting process. The new districts were drawn by the Republican-controlled state legislature and were immediately challenged in court. The districts for the U.S. House were used for the 2012 and 2014 elections. However, in advance of the 2016 election, they were deemed a "racial gerrymander" and were overturned.[9] The legislature then revised the districts, and the new configuration was used for the 2016 and 2018 elections. For the NC Senate and the NC House, the original 2011 districts were used for the 2012 through 2016 elections. Prior to the 2018 election, they too were ruled a racial

8. The incentive is particularly strong in presidential elections, as races for president have recently been competitive nationally. In these races, tipping North Carolina was often seen as having the potential to tip the electoral college and determine the overall winner.

9. Specifically, in Harris v. McCrory (2016), a federal court said that the district configuration was excessive in the extent to which it packed racial minorities into the same districts.

gerrymander and were revised. Finally, in 2019, a state court overturned the revised districts for all of the chambers. The legislature then drew new districts, which were used for the 2020 election.

In the paper, we make use of each instance in which districts for a legislative chamber were redrawn. In the sample period, there were three instances for each of the three chambers, making for a total of nine "episodes".

### 3.2 Data

We use three kinds of data: data on registrants, legislative races, and districts.

The data on registrants comes chiefly from the state election authority, the North Carolina State Board of Elections (NC SBE). From this source, we obtain snapshots of North Carolina's voter registration database in each year from 2006 to 2020. The snapshots provide information on all individuals who were registered to vote in the state at a specified point in time. Importantly, they include a unique registrant ID, which allows us to link them longitudinally.

Our first step in compiling the registrant data is to choose a population from which to draw registrants. We focus on individuals who were registered in the election prior to redistricting, which we call the "baseline" election.[10] For individuals in the baseline election, we use the linked registration snapshots to obtain a number of covariates. First, we obtain a registrant's year of birth, gender, self-identified race/ethnicity, date of first registration, and year of death. Second, we obtain the registrant's turnout behavior, party affiliation, legislative districts, and exact address in each of the 2006 to 2020 elections.[11] We then merge covariates from two other data sources. From U.S. Census data, we add the population density of the Census block in which the registrant lived during the baseline election. From this source, we also add the median household income and the share college graduates in the registrant's baseline block-group. From the NC One Map project, we collect information on the value of the property parcel associated with the registrant's baseline address.

Our second type of data concerns the legislative races that occurred in North Carolina during 2006 to 2020. This data is of two varieties. First, from the NC SBE, we gather data on race vote shares. We use these to calculate the closeness of a race, which we define as 1 minus the absolute two-party vote-share margin. Second, we assemble data on race spending. For U.S. House races, this data comes from the FEC; for NC Senate and NC House races, it is from Follow

10. For the decennial redistricting episodes, the baseline election is 2010. For the first court-ordered revision, the baseline election is 2014 for the U.S. House and 2016 for the NC Senate and NC House. Meanwhile, for the second court-ordered revision, it is 2018 for all chambers.

11. Individuals do not appear in a registration snapshot if their registration lapses. This occurs if they die, or if do not vote in four consecutive North Carolina general elections and also do not respond to a mailed information card. When we conduct our analysis, we drop individuals who die before the 2020 election. For other individuals with lapsed registrations, we set their address to the value from when they were last registered. In addition, we assign them a turnout value of zero. A related issue is that we do not have data on registration or voting from states other than North Carolina. This means that we misclassify residential location and, possibly, turnout for individuals who move out of North Carolina. We discuss the significance of this issue in Section 4.3.

the Money. We focus on the total spending in a race, which we compute by summing campaign contributions and independent expenditures for the two highest vote-getters. We convert this value to a per-capita measure by dividing by the district population.

Table 1: Summary statistics

| | **Mean** | **Std. dev.** | **N** |
|---|---|---|---|
| *Panel A: Registrants* | | | |
| Demographics | | | |
| Age | 48.4 | 18.4 | 27,051,274 |
| Female | 0.536 | 0.499 | 27,052,079 |
| Black | 0.224 | 0.417 | 27,052,079 |
| White | 0.703 | 0.457 | 27,052,079 |
| Party affiliation | | | |
| Democrat | 0.407 | 0.491 | 27,052,079 |
| Republican | 0.308 | 0.462 | 27,052,079 |
| Unaffiliated | 0.285 | 0.452 | 27,052,079 |
| Census covariates | | | |
| Population density in Census block | 1,161 | 2,731 | 27,052,079 |
| Median hhld. income in block-group (2010 $) | 51,528 | 25,696 | 27,026,001 |
| Share college graduates in block-group | 0.308 | 0.207 | 27,049,654 |
| Other covariates | | | |
| Parcel value per registrant (2010 $) | 83,522 | 377,553 | 26,067,068 |
| *Panel B: Legislative races* | | | |
| Contested by both parties | 0.673 | 0.469 | 1,464 |
| Closeness | 0.506 | 0.377 | 1,464 |
| Total spending per person (2010 $) | 2.16 | 3.06 | 1,464 |
| *Panel C: Districts* | | | |
| Majority-minority | 0.195 | 0.397 | 732 |
| Competitiveness: main measure, $c_{d,\mathrm{M}}$ | 0.755 | 0.145 | 549 |
| Competitiveness: alternative measure, $c_{d,\mathrm{A}}$ | 0.717 | 0.113 | 549 |
| Competitiveness: Cook measure, $c_{d,\mathrm{PVI}}$ | 0.752 | 0.132 | 549 |

The table presents summary statistics on the data used in the paper. The sample in Panel A is registrants in the 2010, 2014, 2016, and 2018 elections. "Population density" is calculated as people per square km. "Share college graduates" is the fraction of adults age 25 and over who have graduated from college. "Parcel value per registrant" is calculated by dividing the value of a property parcel by the number of individuals registered at its address. The sample in Panel B is races for the U.S. House, NC Senate, and NC House between 2006 and 2020. "Contested by both parties" is an indicator for whether a race included candidates from both the Democratic and Republican parties. "Closeness" is 1 minus the absolute two-party vote-share margin. For uncontested races, it is equal to 0, since the two-party vote-share margin in these races is 1. "Majority-minority" is an indicator equal to 1 if more than 50% of a district's registrants are non-white. This variable is calculated for all districts used between 2006 and 2020. The last three rows in the table are for measures of district competitiveness. Due to missing data, these variables are calculated only for the districts used between 2012 and 2020. For additional details, see Sections 3.2 and 3.3.

Our last category of data is on the districts that were used in North Carolina during 2006 to 2020. We obtain information on district boundaries from the NC SBE. Borrowing from the registrant data, we calculate the share of registrants in a district who are racial minorities and the share who are Democrats. Also, as mentioned, we compute three measures of a district's competitiveness.

Summary statistics for the data are presented in Table 1. The first panel of the table is for the data on registrants. The values in this panel are calculated using the registrant populations from the four baseline elections (2010, 2014, 2016, and 2018).[12] On average, the registrants in these elections are 48 years old. 54% of them are female, with 22% self-identifying as black, 70% as white, and the remaining 8% as other races. 41% register as Democrats, with 31% choosing to be Republicans and 28% staying unaffiliated. The registrants live in Census block-groups where, on average, the median household income is $52,000 and where an average of 31% of adults are college graduates. The registrants' property parcels have a mean value of $84,000 per registered resident.

Panel B of Table 1 summarizes the data on legislative races. It reveals that only two-thirds of races during 2006 to 2020 were contested by both major parties. On average, these races had an absolute two-party vote margin of 49 percentage points. This gives them a mean closeness score of 0.51. In addition, the average spending in the races was $2.16 per district resident.

Finally, Panel C is for the districts that were used in North Carolina during 2006 to 2020. It shows that a fifth of these districts were "majority-minority". It also summarizes the measures of district competitiveness. We defer discussion of these latter variables to the next subsection.

### 3.3 Competitiveness measures

We calculate three measures of district competitiveness. Two of the measures are based on the Ainsworth (2020) simulation procedure. The third is based on the Cook PVI.

To construct the simulation-based measures, we first simulate a large number of district races.[13] We then summarize the races in different ways. Our main measure defines competitiveness as 1 minus a district's absolute expected two-party vote-share margin. Mathematically, let $\mathrm{v}_{ds}^{D}$ ($\mathrm{v}_{ds}^{R}$) be the Democratic (Republican) vote share in district $d$ for simulated race $s$. Also, let $S$ be the total number of simulated races. The district's competitiveness under our main measure is:

$$\mathrm{c}_{d,\mathrm{M}} \equiv 1 - \left| \frac{1}{S} \sum_{s} \frac{\mathrm{v}_{ds}^{D} - \mathrm{v}_{ds}^{R}}{\mathrm{v}_{ds}^{D} + \mathrm{v}_{ds}^{R}} \right| .$$

$\mathrm{c}_{d,\mathrm{M}}$ reveals the degree of similarity in the district's expected vote shares for Democrats and Republicans. It is equal to 1 in a "50-50" district where the two parties have the same expected vote shares. Meanwhile, it is equal to 0 in a "100-0" district where one party is expected to gain all of the two-party vote.[14]

12. The number of registrants in each baseline election is listed in Appendix Table A1. The baseline elections included almost 9 million distinct registrants, with an average of 6.8 million registrants per election.

13. To be precise, we simulate entire elections, which include district races. The simulations that we use differ depending on when a district was created. Specifically, the simulations have two inputs: an electorate (or a set of potential voters) and a mean for the draws of partisan swings and turnout shocks. For each redistricting episode, we set these inputs equal to the values from the episode's baseline election. For instance, for the decennial episodes, we use the 2010 electorate and we calculate the mean of the draws based on votes and turnout in that year. For the first court-ordered revision, inputs are based on either 2014 or 2016. For the second court-ordered revision, they are based on 2018. See Ainsworth (2020) for more details.

14. For an "80-20" district, the measure equals 0.4; for a "55-45" district, it is 0.9.

Our second way of summarizing the simulated races involves calculating the absolute value of vote margins before averaging. This alternative measure defines competitiveness as 1 minus a district's expected absolute two-party vote-share margin. It is:

$$\mathrm{c}_{d,\mathrm{A}} \equiv 1 - \frac{1}{S}\sum_{s}\left|\frac{\mathrm{v}_{ds}^{D} - \mathrm{v}_{ds}^{R}}{\mathrm{v}_{ds}^{D} + \mathrm{v}_{ds}^{R}}\right|.$$

$\mathrm{c}_{d,\mathrm{A}}$ captures the expected closeness of a district's races. It is equal to 1 in a district where the two-party vote-share margin is predicted to always be 0. By contrast, it is 0 in a district where this margin is predicted to always be either 1 or -1.[15]

Our last competitiveness measure is built on the Cook PVI. The PVI is commonly used both in academic literature (e.g., Moskowitz and Schneer (2019)) and by the media. It predicts a district's partisan lean based on how the district's residents voted in past presidential races. Specifically, the PVI is the difference in the two-party presidential vote share between the district and the entire country, averaged over two recent elections. It is:

$$\mathrm{PVI}_d \equiv \frac{1}{2}\sum_{p\in\{p_1,p_2\}}\left(\frac{\mathrm{v}_{dp}^{D}}{\mathrm{v}_{dp}^{D} + \mathrm{v}_{dp}^{R}} - \frac{\mathrm{v}_{p}^{D}}{\mathrm{v}_{p}^{D} + \mathrm{v}_{p}^{R}}\right).$$

Here, $\mathrm{v}_{dp}^{k}$ is the district-$d$ vote share in presidential race $p$ for party $k$, and $\mathrm{v}_{p}^{k}$ is the nationwide vote share for this party. In our implementation, we choose $p_1$ and $p_2$ to be the races from the first and second presidential elections prior to the creation of the district.[16]

The PVI provides information on a district's predicted vote shares but is not a measure of competitiveness. We transform it into a competitiveness measure using the following formula:

$$\mathrm{c}_{d,\mathrm{PVI}} = 1 - 2\cdot|\mathrm{PVI}_d|.$$

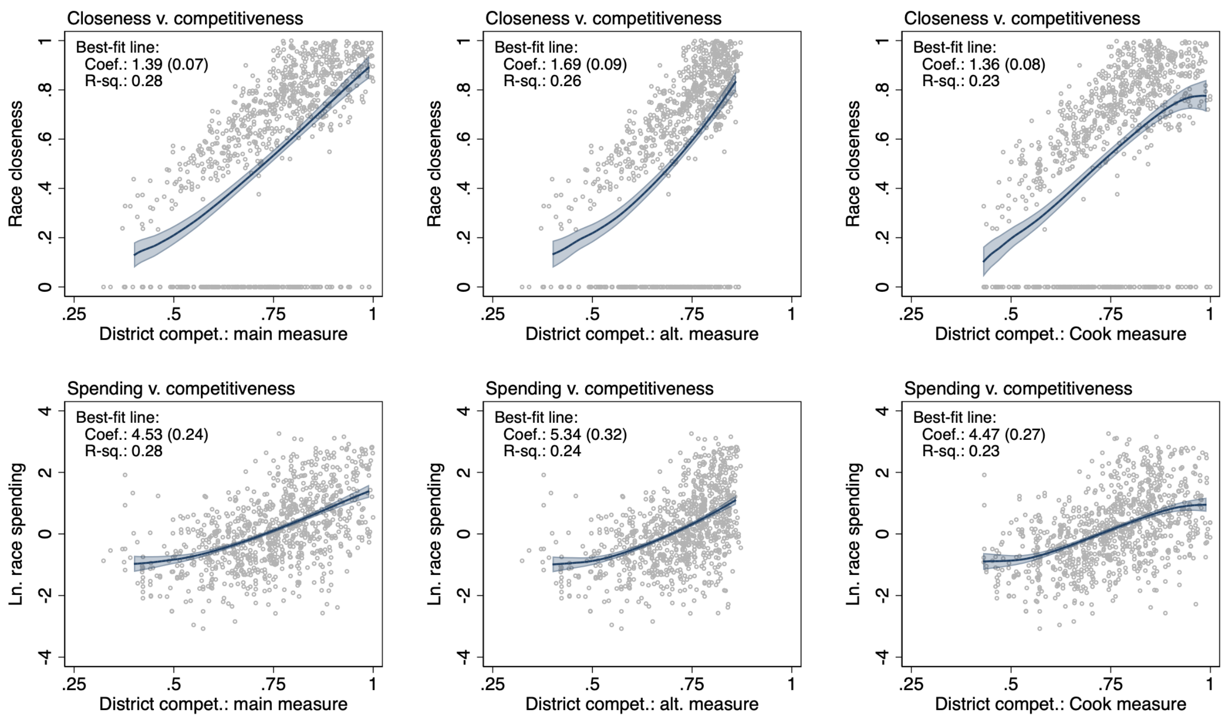
Under this construction, a district that voted 50 percentage points more partisan than the country as a whole would have $\mathrm{c}_{d,\mathrm{PVI}} = 0$; meanwhile, one that voted the same as the entire country would have $\mathrm{c}_{d,\mathrm{PVI}} = 1$.[17]

15. $\mathrm{c}_{d,\mathrm{M}}$ and $\mathrm{c}_{d,\mathrm{A}}$ are equivalent in lopsided districts in which the same party always wins. However, they are different in competitive districts. For instance, a district with $\mathrm{c}_{d,\mathrm{M}} = 1$ will have $\mathrm{c}_{d,\mathrm{A}} = 1$ only if the parties' vote shares are predicted to always be equal, not just to be equal on average. Otherwise, it will have $\mathrm{c}_{d,\mathrm{A}} < 1$. In practice, we find that results are similar using the two measures. We rely more heavily on $\mathrm{c}_{d,\mathrm{M}}$ because we think it is easier to interpret. It can be understood in terms of a district's expected vote shares (e.g., 80-20 or 55-45), which is how districts are often portrayed in the popular press.

16. The Cook Political Report provides the PVI for U.S. House districts, but not for state legislative districts. For these latter districts, we calculate the index ourselves by aggregating the presidential vote according to the districts. Due to data limitations, we cannot do the aggregation for the 2004 election. This impacts our calculation of the PVI for the districts from the decennial redistricting episodes. For these districts, we use just the vote from 2008, the first presidential election prior to redistricting.

17. In one special case, the scaling of $\mathrm{c}_{d,\mathrm{PVI}}$ aligns with that of our main measure, $\mathrm{c}_{d,\mathrm{M}}$. This is when the nationwide presidential vote is even, on average, over the two elections used in calculating the PVI. In practice, this

Figure 2: Predicting race outcomes using district competitiveness measures




The figure plots outcomes in legislative races against the competitiveness of the races' districts. The charts in the first column use our main competitiveness measure, $c_{d,\mathrm{M}}$. Those in the second and third columns use, respectively, the alternative measure, $c_{d,\mathrm{A}}$, and the Cook measure, $c_{d,\mathrm{PVI}}$. "Race closeness" is 1 minus the absolute two-party vote-share margin in the race. This variable is equal to 0 when a race is uncontested. "Ln. race spending" is the natural log of per-person spending in the race, measured in 2010 dollars. The sample is all legislative races that occurred in North Carolina during the 2012 through 2020 elections.

Summary statistics for $c_{d,\mathrm{M}}$, $c_{d,\mathrm{A}}$, and $c_{d,\mathrm{PVI}}$ are presented in Panel C of Table 1. In addition, a correlation matrix for the measures is presented in Appendix Table A2. The summary statistics reveal that the measures have similar means and standard deviations; however, the standard deviation is largest for $c_{d,\mathrm{M}}$ (0.145) and smallest for $c_{d,\mathrm{A}}$ (0.113). The correlation matrix shows that $c_{d,\mathrm{M}}$ and $c_{d,\mathrm{A}}$ are highly correlated (coefficient of 0.98). By contrast, these measures are less correlated with $c_{d,\mathrm{PVI}}$ (coefficients in both cases of 0.85).

We conclude this section by evaluating the quality of the competitiveness measures. To do this, we examine how well the measures predict outcomes in legislative races. Specifically, we plot the closeness and spending in a legislative race against the competitiveness of the race's district. We also provide slope coefficients and R-squared for the corresponding best-fit lines. The results are presented in Figure 2. The figure reveals that all of the competitiveness measures have predictive power for race outcomes. However, our main measure, $c_{d,\mathrm{M}}$, has the most, and the Cook measure, $c_{d,\mathrm{PVI}}$, has the least. Notably, our main measure generates an R-squared of 0.28 for both race closeness and race spending. For the alternative measure, $c_{d,\mathrm{A}}$, R-squared values are 0.26 and

vote is often close to even. Thus, $c_{d,\mathrm{M}}$ and $c_{d,\mathrm{PVI}}$ have similar scaling; the main difference between the measures is the method they use to predict vote shares.

0.24, respectively. Meanwhile, for the Cook measure, values are both 0.23.[18]

# 4 Empirical strategy

We now discuss our empirical strategy. We start by defining our causal parameters of interest. We then show how redistricting—in combination with matching—allows us to identify these parameters. Next, we provide details on how we implement our approach, and we run tests to assess its validity. Finally, we consider the complications posed by majority-minority districts.

## 4.1 Parameters of interest

We are interested in the turnout effects of the competitiveness of legislative districts. We study these effects for three legislative chambers, the U.S. House (USH), the NC Senate (NCS), and the NC House (NCH). To understand our parameters of interest, let $\text{to}_{it}$ be an indicator for whether registrant $i$ turns out to vote in election $t$. Also, let $\text{c}_{it}^{j}$ be a measure of competitiveness for districts in chamber $j$. In particular, let it be competitiveness for the district in this chamber in which $i$ lived during election $t$.

Our hypothesis is that turnout is a function of a registrant's current and past experiences with respect to competitiveness. Based on our empirical findings, we believe that it depends on a weighted sum of these experiences, with weights that vary by chamber. As such, we write $\text{to}_{it}$ as:

$$\text{to}_{it} = \alpha^{\text{USH}} \cdot \sum_{h=0}^{H} \text{c}_{it-h}^{\text{USH}} + \alpha^{\text{NCS}} \cdot \sum_{h=0}^{H} \text{c}_{it-h}^{\text{NCS}} + \alpha^{\text{NCH}} \cdot \sum_{h=0}^{H} \text{c}_{it-h}^{\text{NCH}} + \omega_{it}. \tag{1}$$

Here, $H$ is an arbitrary starting point, and $\sum_{h=0}^{H} \text{c}_{it-h}^{j}$ is the sum of $i$'s competitiveness (in chamber $j$) in elections since $H$. The $\alpha^j$ coefficients are our parameters of interest. They represent the causal effect of increasing current or past competitiveness in a given chamber by one unit. Finally, $\omega_{it}$ is an error term that collects all the other factors that determine whether $i$ turns out.

Equation (1) makes three significant functional form assumptions. First, it assumes that turnout has a linear relationship with competitiveness, as governed by the $\alpha^j$ coefficients. Second, it assumes that the effect of past (or "lagged") competitiveness, $\text{c}_{it-h}^{j}$, is the same as that of current competitiveness, $\text{c}_{it}^{j}$, at least up to a cutoff $h = H$.[19] Third, it assumes that the effects of competitiveness are additively separable across chambers. We provide evidence in support of each of these assumptions in Section 5.

In the main analysis, we identify the $\alpha^j$ coefficients one at a time. That is, we focus on a single

18. In Appendix Table A3, we present the comparison separately by legislative chamber. We also show results for predicting whether a race is contested by both major parties. In all cases, the story is the same: each of the competitiveness measures has predictive power, but our main measure has the most.

19. In a more general model, the effect of lagged competitiveness would decline with $h$. We do not incorporate this because we find no evidence of a decline in our available years of data (up to $h = 4$). However, we acknowledge that effects may begin to decline for $h > 4$.

chamber (called the "chamber of interest"), and we consider the model:

$$\mathrm{to}_{it} = \alpha \cdot \sum_{h=0}^{H} \mathrm{c}_{it-h} + \epsilon_{it}. \tag{2}$$

Here, $\mathrm{c}_{it}$ is competitiveness for districts in the chamber of interest, and $\alpha$ is the causal effect for this chamber. The error term, $\epsilon_{it}$, now incorporates competitiveness in the other chambers.

In a secondary analysis, we deal directly with equation (1). This analysis allows us to confirm the assumption of additive separability. We defer explaining it until later in the paper.

## 4.2 Identification

We identify the $\alpha$ coefficient in equation (2) by making use of redistricting. Our strategy is to use a registrant's district assignment from redistricting as an instrument for the competitiveness that the registrant experiences in post-redistricting elections. In order to explain the strategy, we imagine that there is only a single redistricting episode—i.e., a single instance in which districts for the chamber of interest are redrawn.

We start by re-orienting the time index around the redistricting episode. Let $\tau$ indicate the number of elections since redistricting, with the first post-redistricting election being $\tau = 0$ and the first pre-redistricting election being $\tau = -1$. Also, let $\mathrm{c}_{i\tau}$ and $\mathrm{to}_{i\tau}$ be $i$'s competitiveness and turnout in relative election $\tau$. Finally, define a variable $\mathrm{C}_{i\tau}$ as the sum of $\mathrm{c}_{i\tau}$ in elections between redistricting and $\tau$: $\mathrm{C}_{i\tau} = \sum_{h=0}^{\tau} \mathrm{c}_{ih}$.

Using the new time index, we rewrite (2) in terms of post-redistricting competitiveness, $\mathrm{C}_{i\tau}$:

$$\mathrm{to}_{i\tau} = \alpha \cdot \mathrm{C}_{i\tau} + \varepsilon_{i\tau}, \tag{3}$$

Here, $\alpha$ is the same causal effect as before, but the error term, $\varepsilon_{i\tau}$, now includes competitiveness in pre-redistricting elections.

More generally, $\varepsilon_{i\tau}$ can be thought of as depending on four components. The first, $\xi_i^{\mathrm{pre}}$, is characteristics of the districts that $i$ lived in during pre-redistricting elections (including pre-redistricting competitiveness). The second, $\xi_{i\tau}^{\mathrm{oth}}$, is characteristics of $i$'s post-redistricting districts in legislative chambers other than the chamber of interest. The third, $\xi_{i\tau}^{\mathrm{int}}$, is characteristics other than competitiveness for $i$'s districts in the chamber of interest. The fourth, $\xi_{i\tau}^{\mathrm{reg}}$, is registrant-specific factors that affect $i$'s turnout regardless of the districts in which she lives. These components each may be correlated with $\mathrm{C}_{i\tau}$, which motivates our IV model.

We build the IV model in a few steps. First, we select an analysis sample for the given redistricting episode. We choose this sample to be all registrants from the episode's baseline election who do not die before the 2020 election. Second, we partition North Carolina into regions. A region is defined as an area that has the same pre-redistricting districts, as well as the same post-redistricting districts for chambers other than the chamber of interest. Third, we match registrants within regions based on their covariates, including pre-redistricting turnout. Specifically, we define the "match-group" of registrant $i$, $g_i$, as the set of individuals who were registered in $i$'s region during the baseline election and who share the same covariates as $i$. Fourth, we define $i$'s "assigned district". This district, labeled $a_i$, is the post-redistricting district in the chamber of interest for $i$'s baseline address. It is the district to which $i$ gets "assigned" by the redistricting episode.[20]

Within a match-group, registrants may have different assigned districts.[21] Moreover, if our matching is successful, then the identity of a registrant's assigned district will be unrelated to $\varepsilon_{i\tau}$, after controlling for match-groups. In turn, the competitiveness of this district will also be unrelated to $\varepsilon_{i\tau}$.[22] Building on this logic, we use the competitiveness of the registrant's assigned district as an instrument for $\mathrm{C}_{i\tau}$, the post-redistricting competitiveness that the registrant experiences. The intuition is that assigned competitiveness is correlated with experienced competitiveness; yet it is policy-induced variation that, within a match-group, is likely exogenous.

The specific instrument that we use is labeled $\mathrm{C}_{a_i\tau}$. It is the sum of $i$'s assigned competitiveness over the elections since redistricting. In defining $\mathrm{C}_{a_i\tau}$, we need to account for the fact that the assigned district may not still be in use in election $\tau$, as there may have been a subsequent redistricting episode. We deal with this by summing over only the elections in which the district was in use. Let $\tau_l$ be the last election before the next redistricting episode. Also, let $\mathrm{c}_{a_i}$ be the competitiveness of $i$'s assigned district. Then, $\mathrm{C}_{a_i\tau}$ is:

$$\mathrm{C}_{a_i\tau} = \begin{cases} \sum_{h=0}^{\tau} \mathrm{c}_{a_i} = (\tau+1)\cdot \mathrm{c}_{a_i} & \text{if } \tau \leq \tau_l \\ \sum_{h=0}^{\tau_l} \mathrm{c}_{a_i} = (\tau_l+1)\cdot \mathrm{c}_{a_i} & \text{if } \tau > \tau_l. \end{cases} \tag{4}$$

Finally, our IV model is:

$$\begin{aligned} \mathrm{to}_{i\tau} &= \alpha \cdot \mathrm{C}_{i\tau} + \gamma_{g_i\tau} + e_{i\tau} \\ \mathrm{C}_{i\tau} &= \beta_\tau \cdot \mathrm{C}_{a_i\tau} + \lambda_{g_i\tau} + u_{i\tau}. \end{aligned} \tag{5}$$

In this model, the first equation is the "structural equation", and the second is the "first stage". $\beta_\tau$ is the association between $\mathrm{C}_{a_i\tau}$ and $\mathrm{C}_{i\tau}$, which we allow to vary by relative election, $\tau$. $\gamma_{g_i\tau}$ and $\lambda_{g_i\tau}$ are $\tau$-specific fixed effects for match-groups. $\alpha$ is the causal effect of competitiveness, as in equations (2) and (3).

In order for $\mathrm{C}_{a_i\tau}$ to be a valid instrument for $\mathrm{C}_{i\tau}$, two conditions must be met. First, $\beta_\tau$ must not equal 0, which we can demonstrate empirically. Second, after controlling for the fixed effects, $\mathrm{C}_{a_i\tau}$ cannot be related to $\mathrm{to}_{i\tau}$ through any channel other than its correlation with $\mathrm{C}_{i\tau}$. Since we

20. In post-redistricting elections, the assigned district will be the same as $i$'s actual district if $i$ remains at her baseline address. However, these may differ if $i$ moves.

21. This will occur if the new districts bisect the match-group's region and if the group has members with baseline addresses on either side of the new boundaries.

22. To be precise, it will be unrelated to all components of $\varepsilon_{i\tau}$ except $\xi_{i\tau}^{\mathrm{int}}$. It may be related to $\xi_{i\tau}^{\mathrm{int}}$ if a district's competitiveness is correlated with its other characteristics. We discuss this issue in Section 4.5.

allow $\beta$ to vary by $\tau$, this "exclusion restriction" is:

$$\mathrm{E}[\mathrm{C}_{a_i\tau} \cdot e_{i\tau}|\tau] = 0.$$

In Appendix A2, we show that the exclusion restriction can be equivalently written as:

$$\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]) \cdot (\varepsilon_{i\tau} - \mathrm{E}[\varepsilon_{i\tau}|g_i,\tau])|\tau] = 0. \tag{6}$$

It says: in each relative election $\tau$, $\mathrm{C}_{a_i\tau}$ must not co-vary with non-$\mathrm{C}_{i\tau}$ determinants of turnout, once variables have been de-meaned by combinations of match-group and relative election. If this requirement is satisfied, then $\mathrm{C}_{a_i\tau}$ is a valid instrument and can be used to identify $\alpha$.[23]

There are a few points about our choice of instrument that are worth noting. First, in one special case, $\beta_\tau$ will equal 1. This is if no registrants move out of their assigned districts and if the districts are still in use in $\tau$. By contrast, if there are some registrants who move between the baseline election and $\tau$, then $\beta_\tau$ will likely be less than 1. Also, $\beta_\tau$ may decline over time, as more people move. However, this decay is likely to stop once there is a subsequent redistricting episode. That is, for $\tau > \tau_l$, $\beta_\tau$ should be similar to $\beta_{\tau_l}$. This is because $\mathrm{C}_{a_i\tau}$ remains constant in elections after $\tau_l$. We show each of these points formally in Appendix A3.

## 4.3 Implementation

We now discuss some details related to how we implement our approach.

First, we need to deal with the fact that our data contains multiple redistricting episodes. To account for this, we stack the observations for different episodes on top of each other. That is, we re-interpret the index $i$ as the combination of a registrant and a particular redistricting episode. In some cases, we stack episodes only for a single chamber. This allows us to identify the chamber-specific coefficients, $\alpha^j$. In other cases, we stack the episodes for all chambers. This recovers an average of the chamber-specific coefficients.

Second, we must decide how to define match-groups. In order to explore robustness, we do this in different ways in different specifications. In our main specification, we match on region, gender, three race/ethnicity groups, five age groups, three groups for the education level in the registrant's baseline block-group, three groups for the registrant's party registration in the baseline election, the registrant's history of turnout in the three elections prior to redistricting, and the year in which the registrant first registered in North Carolina. In other specifications, we remove some of these variables and add others. The variables that we add include the value of the registrant's baseline property parcel, the population density of the registrant's baseline Census block, and the

23. We run tests to verify the exclusion restriction in Sections 4.4 and 4.5. Note that it would be inappropriate to simply fit the structural equation in (5) via OLS. This is because part of the variation in $\mathrm{C}_{i\tau}$ within match-groups is due to migration, which may be endogenous. Also, we do not include individual fixed effects in (5) because we are already matching on pre-redistricting turnout; nonetheless, we show that results are robust to adding them.

median household income in the registrant's baseline block-group.

Third, we need to account for the fact that not all match-groups contribute to the estimation. Notably, in some match-groups all registrants are assigned to the same post-redistricting district. These groups have no variation in the instrument and thus do not influence the estimated value of $\alpha$. In order to be transparent about our effective sample size, we drop these groups before obtaining results.[24]

Fourth, we calculate standard errors using two-way clustering. Our two sets of clusters are (i) the combination of pre-redistricting districts (over the three chambers) in a registrant's region and (ii) the registrant's assigned combination of post-redistricting districts. We choose these variables as clusters because we believe they represent the groupings within which registrants are most likely to experience correlated shocks.

Finally, we acknowledge that our analysis faces a limitation due to the fact that we lack data from states other than North Carolina. In particular, our turnout variable is equal to zero for all registrants who leave the state, even if they turn out elsewhere. This means that our causal effects combine a (potentially) non-zero effect for registrants who are still in North Carolina with a zero effect for registrants who are not. As such, our results can be interpreted as representing effects on within-state turnout. This is likely a lower bound for effects on overall turnout.[25]

## 4.4 Tests of the exclusion restriction

We now assess the validity of our approach by testing the exclusion restriction, equation (6).

The first way we test this restriction is by examining whether our instrument, $\mathrm{C}_{a_i\tau}$, predicts a registrant's turnout behavior in pre-redistricting elections. In pre-redistricting elections, the treatment variable, $\mathrm{C}_{i\tau}$, is zero; thus, turnout is equal to the error term in equation (3): $\mathrm{to}_{i\tau} = \varepsilon_{i\tau}$. Consequently, by using $\mathrm{C}_{a_i\tau}$ to predict pre-redistricting turnout, we can test a condition that is similar to the exclusion restriction. We can test:

$$\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]) \cdot (\varepsilon_{i\tilde{\tau}} - \mathrm{E}[\varepsilon_{i\tilde{\tau}}|g_i,\tilde{\tau}])|\tau,\tilde{\tau}] = 0, \text{ for } \tau > 0 \text{ and } \tilde{\tau} < 0. \tag{7}$$

This condition differs from the exclusion restriction only in that it uses a pre-redistricting error, $\varepsilon_{i\tilde{\tau}}$, instead of the desired post-redistricting error, $\varepsilon_{i\tau}$.[26]

24. Appendix Table A4 presents summary statistics on the sample used in estimation and on the included match-groups. The table shows that the estimation sample includes almost 9 million combinations of registrants and episodes and over 500 thousand match-groups. On average, there are 17.3 registrants per match-group, with a standard deviation of 38. Appendix Figure A1 maps which areas in North Carolina contribute to the estimation. Specifically, it shows the baseline Census block-groups of registrants in the included match-groups. The figure reveals that the estimation sample draws broadly from across the state.

25. Fortunately, we do not think that there are issues related to selective attrition. In Appendix A4, we show that being placed in a more competitive district has no effect on a registrant's probability of moving within North Carolina; thus, it probably also has no effect on the probability of leaving the state.

26. To grasp the implications of this difference, note that $\varepsilon_{i\tilde{\tau}}$ contains some, but not all, of the factors that comprise $\varepsilon_{i\tau}$. In particular, it contains $\xi_i^{\mathrm{pre}}$, the component related to the registrant's pre-redistricting districts. In addition,

We test (7) by regressing pre-redistricting turnout, $\text{to}_{i\tilde{\tau}}$, on $\text{C}_{a_i\tau}$ and match-group fixed effects, separately for each combination of $\tau$ and $\tilde{\tau}$. Specifically, we run regressions of the form:

$$\text{to}_{i\tilde{\tau}} = \phi_{\tau\tilde{\tau}} \cdot \text{C}_{a_i\tau} + \phi_{g_i\tau\tilde{\tau}} + \phi_{i\tau\tilde{\tau}}. \tag{8}$$

In these regressions, $\phi_{\tau\tilde{\tau}}$ is proportional to the left-hand side of (7) for the given combination of $\tau$ and $\tilde{\tau}$. Thus, we can see whether the condition holds by examining the magnitude and statistical significance of the coefficient estimates for $\phi_{\tau\tilde{\tau}}$.

The results from the tests are presented in Appendix Tables A5-A7. For robustness, we provide results for three versions of $\text{C}_{a_i\tau}$, which are constructed using our three measures of district competitiveness. In total, we run 72 tests, one for each combination of $\tau$ and $\tilde{\tau}$ and for each version of $\text{C}_{a_i\tau}$.[27] The tests yield strong evidence that (7) holds: the coefficient estimates are all small and statistically insignificant.

We next explore whether $\text{C}_{a_i\tau}$ is associated with the components of $\varepsilon_{i\tau}$. We test conditions of the form:

$$\text{E}[(\text{C}_{a_i\tau} - \text{E}[\text{C}_{a_i\tau}|g_i,\tau]) \cdot (\xi_{i\tau} - \text{E}[\xi_{i\tau}|g_i,\tau])|\tau] = 0,$$

where $\xi_{i\tau}$ is one of $\varepsilon_{i\tau}$'s components. The intuition is that if $\text{C}_{a_i\tau}$ is unrelated to the factors that make up $\varepsilon_{i\tau}$, then it is unlikely to be related to $\varepsilon_{i\tau}$. Analogous to before, we implement the tests by regressing the components on $\text{C}_{a_i\tau}$ and match-group fixed effects, separately for each $\tau$. We then evaluate the coefficients on $\text{C}_{a_i\tau}$, $\phi_\tau$.

The first component that we examine is $\xi_i^{\text{pre}}$, the district characteristics that a registrant experiences in pre-redistricting elections. We run tests for five different characteristics: district competitiveness, district share minority, district share Democratic, race closeness, and race spending. For each characteristic, we sum the value for a registrant's districts over all chambers and all pre-redistricting elections. We then use the sums as the outcome variables in the regressions. The results for the tests are presented in Appendix Tables A8-A10. They reveal that $\text{C}_{a_i\tau}$ is not asso-

it includes the registrant-specific factor, $\xi_{i\tau}^{\text{reg}}$, but for the wrong election ($\tilde{\tau}$ rather than $\tau$). By contrast, it does not include the components related to the registrant's post-redistricting districts, $\xi_{i\tau}^{\text{oth}}$ and $\xi_{i\tau}^{\text{int}}$. Thus, condition (7) is partially informative about the exclusion restriction. It reveals whether the instrument is associated with the combined effect on turnout of $\xi_i^{\text{pre}}$ and $\xi_{i\tilde{\tau}}^{\text{reg}}$, after controlling for match-groups.

27. We use all combinations of $\tau$ and $\tilde{\tau}$ that exist in our data. Depending on the redistricting episode, we can observe up to five elections after redistricting ($\tau = 4$) and up to seven elections before ($\tilde{\tau} = -7$). All episodes have data in $\tau = 0$. Thus, for $\tau = 0$, we can run tests for all pre-redistricting elections, $\tilde{\tau} = -1, \ldots, -7$. Results for these tests are presented in Table A5. Next, in $\tau = 1$, we have data for all episodes except the second court-ordered revision. For these episodes, we observe turnout up to six elections prior to redistricting. Thus, we can run six tests, which are shown in Table A6. Third, for $\tau = 2$, we can run five tests, as seen in Table A7. Finally, for $\tau = 3$ and $\tau = 4$, we have data only for the decennial redistricting episodes. For these episodes, we observe turnout only for three pre-redistricting elections, $\tilde{\tau} = -1, \ldots, -3$. Importantly, $\text{C}_{a_i\tau}$ never has predictive power for turnout in these elections, since we match on this turnout in creating the match-groups. As a result, we know $\phi_{\tau\tilde{\tau}} = 0$ for all the tests for $\tau = 3$ and $\tau = 4$, and we don't present results for these tests in a table.

In sum, for each version of $\text{C}_{a_i\tau}$, we have seven tests for $\tau = 0$, six for $\tau = 1$, five for $\tau = 2$, and three each for $\tau = 3$ and $\tau = 4$. Thus, in total, we have 72 tests.

ciated with a registrant's pre-redistricting district experiences, once we control for match-groups. In none of the 76 tests can we reject that $\phi_\tau$ is 0.

The second component that we consider is the registrant-specific factor, $\xi_{i\tau}^{\text{reg}}$. We cannot observe this component; thus, we cannot directly test whether it is associated with $\mathrm{C}_{a_i\tau}$. However, from prior results, we can deduce that the association is likely to be small. In particular, we've already shown that $\mathrm{C}_{a_i\tau}$ has no predictive power for turnout in any pre-redistricting election. In addition, we know that $\mathrm{C}_{a_i\tau}$ is not associated with pre-redistricting district experiences. Together, these facts mean that it does not predict pre-redistricting versions of $\xi_{i\tau}^{\text{reg}}$. Thus, it likely also does not predict this factor in the desired post-redistricting election.

The third component that we study is $\xi_{i\tau}^{\text{oth}}$, the characteristics of a registrant's post-redistricting districts in chambers other than the chamber of interest. For this component, we run tests for the same five characteristics as in the tests for $\xi_i^{\text{pre}}$. For each characteristic, we create an outcome variable by summing the value in a registrant's districts over all chambers other than the chamber of interest and over all post-redistricting elections from zero to $\tau$. As with $\xi_i^{\text{pre}}$, we then regress these variables on $\mathrm{C}_{a_i\tau}$ and match-group fixed effects, separately for each $\tau$. The results for the tests are presented in Appendix Tables A11-A15. They show that $\mathrm{C}_{a_i\tau}$ has little association with district characteristics in other chambers, after controlling for match-groups. The coefficient estimates are often statistically significant; however, this is likely due to our large sample size, as they are negligible in magnitude.[28]

The last potential threat to our empirical strategy is $\xi_{i\tau}^{\text{int}}$. This component captures district characteristics other than competitiveness for a registrant's districts in the chamber of interest. We discuss this threat in the next subsection.

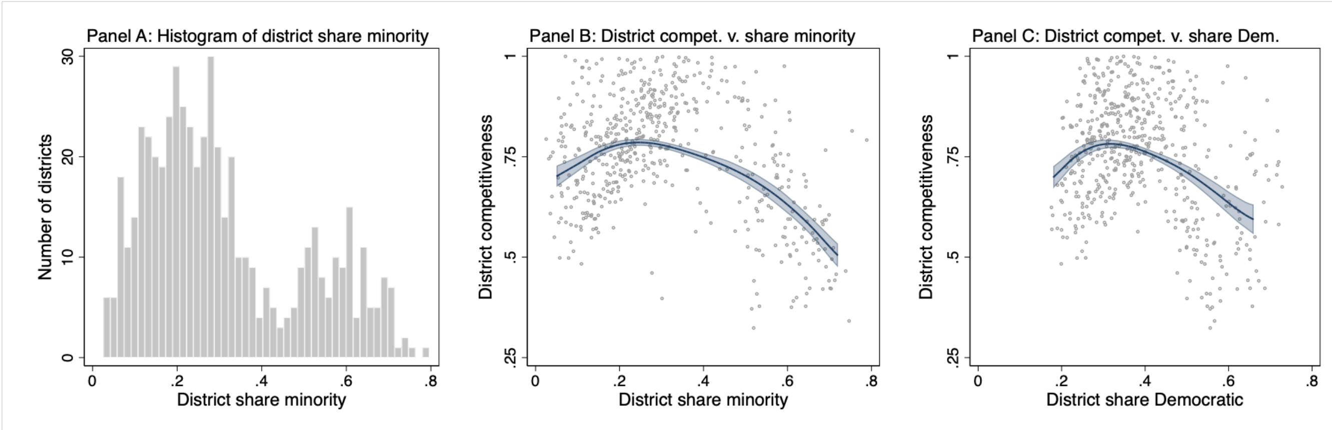
## 4.5 Majority-minority districts

A potential issue with our empirical strategy is that a district's competitiveness may be correlated with its other attributes. If so, then individuals who live in more competitive districts will be subject to a bundle of treatments. Our IV model will not isolate the causal effect of district competitiveness alone. Instead, it will recover the combined effect of competitiveness and of the other district characteristics that are associated with competitiveness.

This concern is relevant in North Carolina because of the existence of "majority-minority" districts. As seen in Figure 3, a substantial fraction of the state's districts have a large share of registrants who are racial minorities (Panel A). These districts tend to be highly Democratic

28. For instance, using our main competitiveness measure, a one unit increase in $\mathrm{C}_{a_i\tau}$ is associated with a less than 0.03 unit increase in a registrant's post-redistricting sum of competitiveness in other chambers. We can illustrate how small this value is by comparing it with the size of the first stage from the IV model. The first stage captures the association between $\mathrm{C}_{a_i\tau}$ and the registrant's post-redistricting sum of competitiveness in the chamber of interest. As we show in Appendix A3, the first stage ranges from 0.8 to 0.9, depending on the relative election. In other words, it is almost 30 times as large as the coefficient estimates in the tests for $\xi_{i\tau}^{\text{oth}}$. Nonetheless, in Section 5, we run a version of the IV model that explicitly accounts for potential confounding due to $\xi_{i\tau}^{\text{oth}}$. We find that this has no impact on our results.

Figure 3: The relationship between competitiveness, share minority, and share Democratic




The figure plots information on the relationship between district competitiveness, district share minority, and district share Democratic. The sample is all districts that were used in North Carolina during the 2012 through 2020 elections.

and highly uncompetitive. As a result, there is an overall negative correlation between a district's competitiveness and both its share minority (Panel B) and its share Democratic (Panel C).[29] Prior research finds that a district's racial and partisan makeup each affect voter turnout. In particular, the research finds that registrants are more likely to turn out when they live in districts with a larger share of people who are of either their same race (Fraga 2016; Henderson, Sekhon, and Titiunik 2016) or party (Fraga, Moskowitz, and Schneer 2021). As such, in our setting, the effect of living in a competitive district may partially reflect these other treatments.

The issue of correlated treatments is mitigated by the fact that the race and party channels are "match effects". That is, they have different signs for different types of registrants. As an example, the minority share is expected to increase turnout for minority registrants but decrease turnout for white registrants. Similarly, the share Democratic should raise turnout for Democrats but reduce turnout for other registrants. Over all registrants, the match effects should mostly cancel; thus, the influence of the race and party channels should be small. On the other hand, the channels may have an appreciable impact when we explore effects for groups of registrants that are homogenous in terms of race or party.

We deal with the issue of correlated treatments by presenting two sets of results. In our main results, we do not attempt to adjust for the race or party channels. These results reveal the total effect of living in a more or less competitive district in North Carolina, inclusive of the fact that uncompetitive districts in the state are often heavily minority and Democratic. These "total effects" are relevant in North Carolina and in other states with majority-minority districts. In a second set of results, we isolate the partial effect of competitiveness, accounting for the effects of race and party. These "partial effects" quantify the causal channel that we are most

29. In Appendix Tables A16 and A17, we show that these negative correlations remain even when we control for match-groups. Specifically, the tables show that the instrument, $C_{a_i\tau}$, predicts both the share minority and the share Democratic in a registrant's districts, conditional on match-group-by-$\tau$ fixed effects.

interested in and that was discussed in Section 2. They are relevant in settings in which a district's competitiveness is not associated with its racial or partisan composition.

We obtain the second set of results by fitting the IV model on a trimmed sample. Namely, we restrict the sample to exclude registrants who are assigned to districts with an extremely high minority share. After controlling for match-groups, this kills the associations between the instrument, $\mathrm{C}_{a_i\tau}$, and the values of district share minority or share Democratic that registrants experience.[30] Thus, on the trimmed sample, the instrument is related to turnout only via its association with district competitiveness. In turn, the IV model estimated on the trimmed sample identifies the partial effect of competitiveness.

In practice, we find that the partial and total effects of competitiveness are similar. Results are nearly identical for effects measured using all registrants. In addition, they differ only slightly for effects that subset by a registrant's race or party. Motivated by this finding, we rely primarily on the total effects, which are more precise.

# 5 Causal effects

We now examine the turnout effects of district competitiveness. We first present our main results, and we show that they are robust. We then discuss external validity, probe mechanisms, and test alternative competitiveness measures. Finally, we explore heterogeneity in effects, and we provide evidence that effects are additive across legislative chambers.

## 5.1 Main results

Our main results are displayed in Table 2. The table provides coefficient estimates and standard errors for the $\alpha$ coefficient from the IV model, equation (5).[31] The first column is for a sample that uses all redistricting episodes. This column reveals an average of the effects of district competitiveness in the three legislative chambers. The other columns are for samples that use only the episodes for the listed chamber. They reveal chamber-specific effects.

The results indicate that competitiveness matters for turnout. Averaged across chambers, a one unit increase in $\mathrm{C}_{i\tau}$ causes a 1.16 percentage point increase in turnout. For the U.S. House, the effect is 2.24 percentage points. For the NC Senate and the NC House, effects are 1.12 and 1.02 percentage points. All values are statistically significant.

The magnitudes just mentioned can be understood as the effect of one election worth of exposure to a highly competitive 50-50 district versus a highly uncompetitive 100-0 district. This difference in competitiveness is larger than the range of competitiveness that is typically observed

30. We trim the sample separately by a registrant's race and party. This way, we kill the associations both overall and conditional on a registrant's type. For white-Democrats, we drop registrants who are assigned to districts that are more than 62.5% minority. For white-Unaffiliated registrants and white-Republicans, the cutoffs are 63.5% and 64.5%. For minorities, the cutoff is 61.5%. Appendix Tables A18 and A19 present the associations for the trimmed sample. They show that the associations are all zero, both overall and by registrant type.

31. Results for the first stage are discussed in detail in Appendix A3. They reveal that $\mathrm{C}_{a_i\tau}$ is a strong instrument.

Table 2: The turnout effects of district competitiveness

| | All | Chamber | | |
|---|---|---|---|---|
| | | U.S. House | NC Senate | NC House |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** (0.230) | 2.24*** (0.796) | 1.12** (0.473) | 1.02*** (0.310) |
| Turnout percentage | 58.1 | 58.4 | 58.3 | 57.8 |
| Clusters | 406 | 176 | 221 | 376 |
| Registrants | 5,204,602 | 1,588,992 | 1,679,413 | 4,024,634 |
| Registrant-episode-elections | 31,393,692 | 6,516,888 | 7,012,454 | 17,864,350 |

The table presents results from the IV model, equation (5). Specifically, it shows coefficient estimates, $\hat{\alpha}$, and standard errors from 2SLS regressions of $\text{to}_{i\tau}$ on $C_{i\tau}$ and match-group-by-$\tau$ fixed effects. The results in the column titled "All" are calculated using all redistricting episodes. The results in the other columns are calculated using just the episodes for the listed legislative chamber. The treatment variable, $C_{i\tau}$, and the instrument, $C_{a_i\tau}$, are constructed using our main measure of district competitiveness, $c_{d,\mathrm{M}}$. Coefficient estimates and standard errors are denominated in percentage points. "Turnout percentage" is the percent of observations that turned out to vote. "Registrants" is the number of distinct registrants in the sample. "Registrant-episode-elections" is the number of observations. Standard errors are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum number of clusters across the two dimensions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

among real-world districts. To give a more realistic sense of effect sizes, we consider the impact of spending an election in a 55-45 district versus an 80-20 district. Under this new scaling, magnitudes are half as big as those shown in Table 2. They are 1.12 percentage points for the U.S. House, 0.56 percentage points for the NC Senate, and 0.51 percentage points for the NC House. Alternatively, the values in Table 2 can be interpreted as the impact of spending two elections in a 55-45 district versus an 80-20 district.

The IV model, equation (5), relies on two functional form assumptions. First, it assumes that turnout has a linear relationship with district competitiveness. Second, it assumes that competitiveness in a prior election has the same impact as competitiveness in the current election—at least for the number of prior elections that are observable in our data.[32] If these assumptions are invalid, then the results in Table 2 may be misleading.

We test the linearity assumption by fitting the IV model on samples of the data that have varying degrees of competitiveness. In the second column of Table 3, we include only registrants who were assigned to competitive districts, defined as those that are more competitive than a 65-35 district. In the third column, we include only registrants who were assigned to uncompetitive districts, defined as those that are less competitive than a 60-40 district. If turnout is linear in competitiveness, then effects calculated on the two samples should be the same; if it is not, then they may differ.[33] The results provide strong evidence for linearity: the coefficient estimates are similar in magnitude to each other and to the effect for the full sample.

We assess the second assumption by fitting a version of the IV model where we allow the coefficient on $C_{i\tau}$ to vary by relative election. This version replaces $\alpha$ in equation (5) with $\alpha_\tau$ (just as the first-stage coefficients, $\beta_\tau$, vary with $\tau$). The $\alpha_\tau$ coefficients reveal whether the effect

32. The full causal model, equation (1), also assumes additive separability. This assumption is not made in the IV model, which compares individuals who differ in assigned districts for only a single chamber.

33. For instance, if the relationship is quadratic or exponential, then effects among competitive districts should be larger than those among uncompetitive ones.

of $C_{i\tau}$ changes as $C_{i\tau}$ sums over more elections. If, as assumed, current and past competitiveness have equal impacts on turnout, then all the $\alpha_\tau$ coefficients should be the same. By contrast, if experiences in certain elections matter more than in others, then they may differ. For instance, if lagged competitiveness has a smaller impact than current competitiveness, then the $\alpha_\tau$ coefficients should decline with $\tau$. This is because lagged competitiveness makes up an increasingly large share of $C_{i\tau}$ as $\tau$ increases.

Table 3: Effects by degree of competitiveness and by relative election

| | **All** | **Competitiveness** | | **Election relative to redistricting, $\tau$** | | | | |
|---|---|---|---|---|---|---|---|---|
| | | High | Low | Zero | One | Two | Three | Four |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** (0.230) | 1.17* (0.658) | 1.14* (0.603) | 1.37*** (0.433) | 1.03*** (0.251) | 1.11*** (0.278) | 1.13*** (0.214) | 1.24*** (0.303) |
| Turnout percentage | 58.1 | 59.4 | 55.8 | 64.7 | 49.9 | 61.8 | 49.8 | 61.9 |
| Clusters | 406 | 289 | 263 | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 3,335,715 | 2,624,806 | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episode-elections | 31,393,692 | 16,602,844 | 13,241,713 | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results related to the functional form assumptions that underlie the IV model. The values in the column titled "All" are for our main version of the IV model; they match those in the "All" column of Table 2. The results in the columns titled "Competitiveness" are for samples of the data that differ in competitiveness. "High" ("Low") includes only registrants who were assigned to districts that are more (less) competitive than 65-35 (60-40). The results in the remaining columns are for a version of the IV model where $\alpha$ is allowed to vary by $\tau$. These results are calculated using separate IV regressions for each $\tau$. All other details in the table are the same as in Table 2.

The results by relative election are presented starting in the fourth column of Table 3. They again support the functional form assumption. Namely, the coefficient estimates for $\alpha_\tau$ are all close in magnitude; in addition, they are similar to the estimate from the main version of the IV model, where $\alpha$ does not vary by $\tau$.[34] These results indicate that the impact of district competitiveness is highly persistent. In fact, they imply that lagged competitiveness has the same impact on turnout as current competitiveness for at least four prior elections.[35]

The high degree of persistence exhibited in Table 3 is in line with prior research on voting behavior. First, research suggests that people heavily weigh their own past experiences when they make voting decisions (Kanazawa 1998; Bendor, Diermeier, and Ting 2003; Esponda and Pouzo 2017). Second, there is substantial evidence that voting is habit-forming; i.e., the very act

34. One may worry that the stability of the coefficient estimates in Table 3 is due to changes in the sample for different $\tau$. We find that this is not the case. In Appendix Table A20, we replicate Table 3 using only the decennial redistricting episodes, which have data in all relative elections. The table reveals that the estimates remain stable.

35. To see this, note that $C_{i4}$ is the sum of current competitiveness and four lags of competitiveness. Thus, Table 3 shows that $\alpha_\tau$ remains stable for a version of $C_{i\tau}$ that depends on four prior elections. This finding is particularly interesting because most of the instrument-induced variation in $C_{i4}$ is due to variation in lagged competitiveness, not current competitiveness. Notably, the only observations with data in $\tau = 4$ are from the decennial redistricting episodes. However, the districts from the decennial episodes are no longer in use in $\tau = 4$. For the U.S. House, the decennial districts were replaced after $\tau = 1$ (2014); for the state legislative chambers, they were replaced after $\tau = 2$ (2016). In Appendix A3, we show that being assigned to a more competitive district as part of the decennial episodes has little predictive power for the competitiveness that a registrant experiences once the districts are replaced. Thus, most of the identifying variation in $C_{i4}$ is due to variation in $c_{i0}$, $c_{i1}$, and $c_{i2}$, not in $c_{i3}$ or $c_{i4}$. As such, the results for $\alpha_4$ present direct evidence that the effects of competitiveness endure after registrants stop being exposed to competitive districts.

of voting makes a person more likely to vote in the future (Gerber, Green, and Shachar 2003; Meredith 2009; Fujiwara, Meng, and Vogl 2016). In our setting, learning from past experiences may mean that registrants gradually update their expectations about competitiveness as they spend more time in a competitive district. This would cause turnout to grow with additional elections of exposure. Also, habit formation may cause a person's turnout to remain elevated after she leaves a competitive district. Importantly, this could occur even if the person comes to realize that her new district is not as competitive. In sum, prior research suggests that exposure to a competitive district may have a long-lasting impact on turnout—just as we find.

## 5.2 Robustness

Our results are highly robust.

First, they are robust to the fact that a district's competitiveness is correlated with both its share minority and share Democratic. As we explained in Section 4.5, we test for bias due to these other treatments by re-estimating the IV model on a trimmed sample on which the correlations are zero. The coefficient estimate from the trimmed sample is presented in the second column of Table 4. It is 1.24, quite similar to our main value of 1.16.

Table 4: Robustness to alternative specifications

| | **Main specification** | **Restricting to the trimmed sample** | **Summing over competitiveness in all chambers** | **Including individual fixed effects** |
|---|---|---|---|---|
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** | 1.24*** | 1.13*** | 1.12*** |
| | (0.230) | (0.403) | (0.225) | (0.215) |
| Turnout percentage | 58.1 | 59.7 | 58.1 | 52.6 |
| Clusters | 406 | 371 | 406 | 406 |
| Registrants | 5,204,602 | 4,488,353 | 5,204,602 | 5,204,602 |
| Registrant-episode-elections | 31,393,692 | 24,546,557 | 31,393,692 | 70,185,520 |

The table presents results from three robustness exercises. The column titled "Main specification" is the main version of the IV model. It repeats the values from the "All" column of Table 2. "Restricting to the trimmed sample" is calculated on a sample that excludes the districts with the highest minority shares. See Section 4.5 for more details on this sample. "Summing over competitiveness in all chambers" uses a different version of the treatment variable, $C_{i\tau}$. It defines $C_{i\tau}$ as the sum of competitiveness in a registrant's districts in each of the three chambers, not just the chamber of interest. "Including individual fixed effects" uses data from all relative elections and adds registrant-by-episode fixed effects. All other details in the table are the same as in Table 2.

Second, the results are robust to a concern related to the tests of the exclusion restriction. In Section 4.4, we found that the instrument, $C_{a_i\tau}$, is slightly correlated with the competitiveness that registrants experience in chambers other than the chamber of interest. To gauge the impact of this correlation, we run a version of the IV model where we redefine the treatment variable to be the sum of competitiveness across all chambers. This specification endogenizes differences in competitiveness for the other chambers, rather than leaving them in the error term. The coefficient estimate is 1.13 (third column of Table 4), again close to that from our main specification.

Third, the results are robust to including individual fixed effects. In the last column of Table 4, we run a version of the IV model that uses data from all relative elections—including pre-redistricting elections—and that adds registrant-by-episode fixed effects. The coefficient estimate is 1.12, again hardly changed.

Finally, the results are also not sensitive to the way in which we construct match-groups. In Appendix Table A21, we present coefficient estimates for seven alternative sets of match-groups. The values are all similar to our main effect.

## 5.3 External validity

A key question about our results is whether they are unique to North Carolina or whether they have external validity for other states. We evaluate external validity by benchmarking our results with those of Moskowitz and Schneer (2019). Moskowitz and Schneer study only the decennial redistricting episode for the U.S. House. In addition, they use a slightly different empirical strategy than we do, and they examine outcomes in only the 2012 and 2014 elections. Nonetheless, they have data from all fifty states. Thus, by appropriately comparing our results with theirs, we can isolate the role of the North Carolina setting.

In Appendix A5, we re-estimate our results using the same empirical strategy as in Moskowitz and Schneer (2019). We also restrict our sample to reflect the redistricting episode and analysis period considered in their paper. We show that, when we do this, we closely match their effects. Thus, there seems to be nothing peculiar about the North Carolina setting. Instead, our results appear to have broad relevance to other states.

## 5.4 The effect of competitive races

We next probe the mechanisms that underlie the effect of district competitiveness. We show that the effect operates mostly, but not entirely, through exposure to competitive races.

To do this, we run a version of the IV model where we construct the treatment variable using measures related to race competitiveness, not district competitiveness. The measures that we use are the closeness of a race and the natural log of spending in the race. We then fit equation (5) as before. The $\alpha$ coefficients from these models reveal whether individuals turn out more when they are assigned to districts that *end up* having competitive races. We are interested to know if this ex post way of defining the treatment has a larger effect than the ex ante concept of district competitiveness.

The results from the exercise are presented in Table 5. In the table, the first column repeats the effect of district competitiveness from Table 2. The second and third columns display the effects of race closeness and race spending. Finally, the last column provides results from a "horse race" in which district competitiveness and race closeness are included in the same equation. One complication in comparing the effects for the different measures is that the measures have different standard deviations. To deal with this, the middle rows of the table list the standard deviations and also report the effects in terms of standard deviations.

Table 5 offers two pieces of evidence supporting the claim that competitive races drive the

Table 5: The turnout effects of race competitiveness

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Sum of district competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** | | | 0.546* |
| | (0.230) | | | (0.280) |
| Sum of race closeness in a registrant's districts, $C_{i\tau}$ | | 0.631*** | | 0.520*** |
| | | (0.103) | | (0.122) |
| Sum of ln. race spending in a registrant's districts, $C_{i\tau}$ | | | 0.144*** | |
| | | | (0.034) | |
| Standard deviation of $c_{i\tau}$ | 0.143 | 0.342 | 1.23 | - |
| Effect of a 1 s.d. increase in $c_{i\tau}$ | 0.166 | 0.216 | 0.177 | - |
| Turnout percentage | 58.1 | 58.1 | 58.1 | 58.1 |
| Clusters | 406 | 406 | 406 | 406 |
| Registrants | 5,204,602 | 5,204,602 | 5,204,602 | 5,204,602 |
| Registrant-episode-elections | 31,393,692 | 31,393,692 | 31,393,692 | 31,393,692 |

The table reveals the turnout effects of race competitiveness. Specifically, Columns (2) and (3) present results for versions of the IV model where both the instrument, $C_{a_i\tau}$, and the treatment, $C_{i\tau}$, are constructed using measures of race competitiveness. In Column (2), these variables are constructed using the closeness of the race. In Column (3), they are built using the natural log of spending in the race. As a comparison, Column (1) provides results for the main version of the IV model, where $C_{a_i\tau}$ and $C_{i\tau}$ are based on district competitiveness. Column (4) is for an IV model with two treatments, one built on district competitiveness and the other built on race closeness. In this model, there are two instruments, based on the two aforementioned measures. "Standard deviation of $c_{i\tau}$" is the standard deviation of the listed measure in registrants' districts. "Effect of a 1 s.d. increase in $c_{i\tau}$" is the product of the coefficient estimate and the standard deviation of $c_{i\tau}$. All other details in the table are the same as in Table 2.

effect of district competitiveness. First, the coefficient estimate is largest (in standard deviation units) for race closeness and smallest for district competitiveness.[36] Second, in the horse race, most (but not all) of the weight is placed on race closeness. The horse race suggests that a one standard deviation increase in race closeness causes a 0.178 percentage point increase in turnout, holding constant district competitiveness.[37] Meanwhile, a one standard deviation increase in district competitiveness causes a 0.078 percentage point increase in turnout, holding constant race closeness.

These results are consistent with the discussion in Section 2. As we explained there, theory predicts that most of the effect of district competitiveness is due to experiencing competitive races; however, there are also ways in which district competitiveness may matter on its own.

## 5.5 Effects for different measures of district competitiveness

We next show that our results do not depend on how we measure district competitiveness.

In Table 6, we present effects for models in which we construct the treatment variable using three different measures of district competitiveness: our main measure, $c_{d,\mathrm{M}}$, the alternative measure, $c_{d,\mathrm{A}}$, and Cook measure, $c_{d,\mathrm{PVI}}$. In addition, we provide a horse race between the main measure and the Cook measure.

Table 6 yields two conclusions. First, coefficient estimates are statistically significant regardless

36. The effect for race spending (0.177 percentage points) is only slightly larger than that for district competitiveness (0.166). In addition, it is considerably smaller than the effect for race closeness (0.216). This possibly suggests that spending is worse than closeness at capturing a race's competitiveness.

37. This value is obtained by multiplying the coefficient estimate for race closeness, 0.520, by the standard deviation of race closeness, 0.342.

Table 6: Robustness to alternative competitiveness measures

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$: main measure | 1.16*** | | | 0.980 |
| | (0.230) | | | (0.737) |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$: alternative measure | | 1.40*** | | |
| | | (0.288) | | |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$: Cook measure | | | 1.15*** | 0.188 |
| | | | (0.239) | (0.754) |
| Standard deviation of $c_{i\tau}$ | 0.143 | 0.111 | 0.126 | - |
| Effect of a 1 s.d. increase in $c_{i\tau}$ | 0.166 | 0.155 | 0.145 | - |
| Turnout percentage | 58.1 | 58.1 | 58.1 | 58.1 |
| Clusters | 406 | 406 | 406 | 406 |
| Registrants | 5,204,602 | 5,204,602 | 5,204,602 | 5,204,602 |
| Registrant-episode-elections | 31,393,692 | 31,393,692 | 31,393,692 | 31,393,692 |

The table presents results for different measures of district competitiveness. In Column (1), $C_{a_i\tau}$ and $C_{i\tau}$ are constructed using our main competitiveness measure, $c_{d,\mathrm{M}}$. Results in this column correspond with those in the "All" column of Table 2. In Column (2), $C_{a_i\tau}$ and $C_{i\tau}$ are built using the alternative measure $c_{d,\mathrm{A}}$. In Column (3), they are based on the Cook measure, $c_{d,\mathrm{PVI}}$. Column (4) is for an IV model with two treatments, one constructed using $c_{d,\mathrm{M}}$ and the other constructed using $c_{d,\mathrm{PVI}}$. In this model, there are two instruments, based on the aforementioned measures. "Standard deviation of $c_{i\tau}$" is the standard deviation of the listed competitiveness measure in registrants' districts. "Effect of a 1 s.d. increase in $c_{i\tau}$" is the product of the coefficient estimate and the standard deviation of $c_{i\tau}$. All other details in the table are the same as in Table 2.

of the measure that we use. Second, the simulation-based measures are better than the Cook measure at channeling the effect of district competitiveness. The latter point can be seen in two ways. First, effect sizes are slightly larger for the simulation-based measures. For $c_{d,\mathrm{M}}$ ($c_{d,\mathrm{A}}$), a one standard deviation increase in competitiveness causes a 0.166 (0.155) percentage point increase in turnout. For the Cook measure, the magnitude is 0.145 percentage points. Second, the horse race places most of the weight on the main measure, $c_{d,\mathrm{M}}$. According to the horse race, the effect of a one standard deviation increase in competitiveness under $c_{d,\mathrm{M}}$, holding constant competitiveness under $c_{d,\mathrm{PVI}}$, is 0.140 percentage points. By contrast, the effect of a similarly sized increase under $c_{d,\mathrm{PVI}}$, holding constant competitiveness under $c_{d,\mathrm{M}}$, is only 0.024 percentage points.[38]

The fact that the Cook measure performs worse is consistent with our prior findings. Notably, we've already shown that the effect of district competitiveness is driven by exposure to competitive races; we've also shown that the simulation-based measures are better at predicting which districts have competitive races. Thus, it's unsurprising that they generate more robust causal effects. At the same time, the fact that the Cook measure also produces a significant effect indicates that it is a high-quality substitute in settings in which the simulation-based measures are not available.

## 5.6 Heterogeneity in effects

We next explore heterogeneity in the effect of district competitiveness.

In Table 7, we present heterogeneity by whether an observation is from a midterm or presidential election. The table shows that effects are similar for the two election types. This result

38. These values are again obtained by multiplying the coefficient estimates for the different measures by the standard deviations of the measures.

Table 7: Heterogeneity in effects by election type

| | All | Election type | |
|---|---|---|---|
| | | Midterm | Presidential |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** (0.230) | 1.11*** (0.198) | 1.18*** (0.276) |
| Turnout percentage | 58.1 | 48.1 | 64.2 |
| Clusters | 406 | 401 | 406 |
| Registrants | 5,204,602 | 4,486,355 | 5,204,602 |
| Registrant-episode-elections | 31,393,692 | 12,029,051 | 19,364,641 |

The table shows how effects vary based on whether an observation is from a midterm or presidential election. Specifically, the values in the columns titled "Election type" are for versions of the IV model that are calculated using only observations from the listed election type. As a comparison, the values in the "All" column are for the main version of the IV model, which uses observations from both election types. All other details in the table are the same as in Table 2.

is somewhat surprising, given that there are fewer races on the ballot in midterms. One might expect that legislative races matter more in these elections, due to increased salience. However, this is not what we find.[39]

In Table 8, we present heterogeneity by a registrant's race and party. We divide registrants into four groups (white-Democrats, white-Republicans, white-Unaffiliated registrants, and racial minorities), and we calculate effects for each group.[40] Panel A of the table displays "total effects", which are based on the full sample. Panel B provides "partial effects", which are from the trimmed sample. We distinguish between the two types of effects in presenting this heterogeneity because the effects may differ conditional on a registrant's group. As discussed in Section 4.5, the race and party channels may not net to zero when registrants are homogenous in terms of race or party.

Panel A reveals that there is substantial heterogeneity across groups in the effect of living in a competitive legislative district. Among whites, the effect is largest for Democrats (2.02 percentage points) and smallest for Republicans (0.98 percentage points). For minorities, the effect is small and statistically insignificant (0.42 percentage points).

Panel B reveals that the partial effects are similar to the total effects. For each group, the difference between the effects is modest and statistically insignificant. Nonetheless, the signs of the differences align with what would be predicted based on prior research. For instance, for minorities, we would expect the partial effect to be larger than the total effect. This is because minorities tend to gain a positive racial and partisan match when they live in uncompetitive districts. In line with this, Table 8 shows that the partial effect is larger for minorities, but only to a minimal degree. Next, for white-Republicans and white-Unaffiliated registrants, the partial effect should be smaller than the total effect, since these groups tend to receive negative match effects from uncompetitive districts. We again find the expected result; however, the differences are

39. Moreover, we continue to find little difference in effects when we restrict the sample to just the first relative election after redistricting ($\tau = 0$). Thus, the result is not simply due to the fact that turnout depends on both current and past competitiveness.

40. We do not divide racial minorities by party because over 73% of minority registrants are registered as Democrats, with less than 6% being registered as Republicans.

Table 8: Heterogeneity in effects by a registrant's race and party

| | All | White | | | Minority |
|---|---|---|---|---|---|
| | | Dem. | Rep. | Unaffil. | |
| *Panel A: Full sample ("total effects")* | | | | | |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** | 2.02*** | 0.981*** | 1.53*** | 0.417 |
| | (0.230) | (0.246) | (0.320) | (0.294) | (0.340) |
| Turnout percentage | 58.1 | 62.0 | 63.6 | 51.9 | 53.4 |
| Clusters | 406 | 405 | 405 | 406 | 402 |
| Registrants | 5,204,602 | 1,059,113 | 1,578,979 | 1,205,238 | 1,498,613 |
| Registrant-episode-elections | 31,393,692 | 6,741,649 | 9,571,531 | 6,464,521 | 8,615,991 |
| *Panel B: Trimmed sample ("partial effects")* | | | | | |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.24*** | 2.20*** | 0.737 | 1.36*** | 0.493 |
| | (0.403) | (0.457) | (0.494) | (0.448) | (0.731) |
| Turnout percentage | 59.7 | 63.0 | 64.4 | 53.1 | 54.6 |
| Clusters | 371 | 366 | 371 | 366 | 359 |
| Registrants | 4,488,353 | 957,262 | 1,495,876 | 1,117,259 | 1,040,424 |
| Registrant-episode-elections | 24,546,557 | 5,670,656 | 8,692,447 | 5,641,132 | 4,542,322 |

The table shows how effects vary based on a registrant's race and party. Results in the columns titled "White" and "Minority" are for versions of the IV model that are calculated using only registrants with the given combination of race and party. Results in the column titled "All" are calculated using all registrants. Panel A provides "total effects", which are obtained from a sample that includes all districts. Panel B provides "partial effects", which are obtained from a trimmed sample that excludes districts with high minority shares. See Section 4.5 for more details. "Dem", "Rep." and "Unaffil." refer to registrants who are registered as Democrats, Republicans, or Unaffiliated. All other details are the same as in Table 2.

again limited. Finally, for white-Democrats, the total and partial effects should be similar. This is because white-Democrats tend to gain conflicting match effects from uncompetitive districts: they receive a positive partisan match and a negative racial match. As expected, the effects are similar for this group; however, the partial effect is slightly larger. In sum, the results are consistent with the existence of race and party match effects. Yet, the match effects appear to be too small to cause significant distortion.

Table 9: Heterogeneity in effects by additional registrant characteristics

| | All | Gender | | Age | | Education | | Voted in baseline | |
|---|---|---|---|---|---|---|---|---|---|
| | | Male | Female | $\leq 35$ | $> 35$ | Low | High | No | Yes |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** | 1.26*** | 1.07*** | 1.36*** | 1.06*** | 0.990*** | 1.47*** | 1.33*** | 0.927*** |
| | (0.230) | (0.248) | (0.230) | (0.397) | (0.196) | (0.286) | (0.348) | (0.303) | (0.189) |
| Turnout percentage | 58.1 | 57.0 | 58.9 | 40.6 | 65.8 | 56.1 | 62.1 | 34.8 | 85.3 |
| Clusters | 406 | 406 | 405 | 405 | 406 | 387 | 254 | 406 | 404 |
| Registrants | 5,204,602 | 2,317,207 | 2,889,324 | 1,835,063 | 3,569,413 | 3,649,666 | 1,798,853 | 3,006,963 | 2,711,076 |
| Registrant-episode-elections | 31,393,692 | 13,855,249 | 17,538,443 | 9,594,081 | 21,799,611 | 21,052,935 | 10,340,757 | 16,930,896 | 14,462,796 |

The table presents heterogeneity in effects by a variety of registrant characteristics. The column titled "All" provides results from the main version of the IV model. It corresponds with the "All" column of Table 2. Results in the remaining columns are for versions of the IV model that are calculated using only registrants with the specified characteristics. "Age" is measured in the baseline election. "Low education" ("High education") is an indicator for whether the share of college graduates in a registrant's baseline block-group is less than (at least) 0.4. "Voted in baseline" is an indicator for whether the registrant turned out to vote in the baseline election. All other details are the same as in Table 2.

In Table 9, we present heterogeneity by additional registrant characteristics. The table shows that effects are relatively similar for males (1.26 percentage points) and females (1.07). However, they are larger for young registrants (1.36) than for older registrants (1.06). Effects are also larger for registrants who live in more- versus less-educated block-groups (1.47 v. 0.99). Finally, they are larger for people who did not vote in the baseline election (1.33) than for those who did (0.93).

Table 10: Heterogeneity in effects by age and education

| | All | Low education | | High education | |
|---|---|---|---|---|---|
| | | Age $\leq 35$ | Age $> 35$ | Age $\leq 35$ | Age $> 35$ |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16*** (0.230) | 0.924** (0.442) | 1.02*** (0.262) | 2.11*** (0.661) | 1.16*** (0.283) |
| Turnout percentage | 58.1 | 38.7 | 63.7 | 44.4 | 69.9 |
| Clusters | 406 | 385 | 387 | 250 | 253 |
| Registrants | 5,204,602 | 1,257,474 | 2,505,562 | 629,161 | 1,224,802 |
| Registrant-episode-elections | 31,393,692 | 6,425,343 | 14,627,592 | 3,168,738 | 7,172,019 |

The table shows how effects vary for subsets of registrants defined by the interaction of age and education. See Table 9 for details on the definitions of the age and education variables. See Table 2 for all other details.

In Table 10, we investigate the age and education heterogeneity in more detail. The table reveals that effects are particularly large for young registrants in high-education block-groups. For these registrants, the coefficient estimate is almost twice as big as that for any other combination of age and education. We believe this result is further evidence that learning plays a role in mediating the effect of district competitiveness. Namely, young registrants have less political experience; as such, conditions in recent elections may have a larger impact on both their expectations about competitiveness and their voting behavior.[41] In addition, among young registrants, those who live in areas with more education may be better able to perceive whether their district is competitive.

Finally, in Appendix Tables A22 and A23, we present further heterogeneity based on a registrant's race. The tables show effects for subsets of registrants defined by the interaction of various electoral and demographic characteristics with whether a registrant is white or minority. For both racial groups, the patterns of effects resemble those already discussed; however, effects are always smaller for minorities.

In sum, the results in this subsection indicate that there is substantial heterogeneity across registrants in the effect of district competitiveness.

## 5.7 Additivity across chambers

We conclude the causal analysis by providing evidence that effects are additive across legislative chambers—just as we assumed in our original causal model, equation (1).

To do this, we make use of variation in competitiveness from multiple chambers, rather than from a single chamber at a time. Exploiting this variation requires us to introduce two changes relative to our previous empirical approach. First, we alter the definition of a redistricting episode: we now define an episode as an instance in which districts are redrawn for any chamber, not for a particular chamber.[42] Second, we redefine the regions that we rely on in constructing match-

41. Somewhat in the same vein, Ghitza, Gelman, and Auerbach (2022) find that people settle on their party preferences in early adulthood and then hardly change them through the rest of their lives.

42. Under this definition, there are four episodes, which occurred in 2011, 2015, 2017, and 2019.

groups: we have them depend only on pre-redistricting districts, not also on post-redistricting districts. As a result of these changes, there can be variation (within match-groups) in assigned districts for multiple chambers.

Table 11: Effects calculated using variation in multiple chambers

| | (1) | (2) |
|---|---|---|
| Weighted sum of competitiveness in a registrant's districts: all chambers | 1.03*** | |
| | (0.179) | |
| Sum of competitiveness in a registrant's districts: U.S. House | | 2.81*** |
| | | (0.878) |
| Sum of competitiveness in a registrant's districts: NC Senate | | 0.954* |
| | | (0.512) |
| Sum of competitiveness in a registrant's districts: NC House | | 1.01*** |
| | | (0.338) |
| Turnout percentage | 58.0 | 58.0 |
| Clusters | 408 | 408 |
| Registrants | 5,608,264 | 5,608,264 |
| Registrant-episode-elections | 27,948,710 | 27,948,710 |

The table presents results from versions of the IV model that are calculated using variation in competitiveness from multiple legislative chambers. In these versions, individuals within match-groups may differ in assigned districts for up to three chambers. The treatment variable in Column (1) is a weighted sum of competitiveness in a registrant's districts across all chambers. In the sum, competitiveness for each chamber is weighted by the coefficient estimate for the chamber in Table 2 (2.24 for the U.S. House, 1.12 for the NC Senate, and 1.02 for the NC House). The instrument is a weighted sum of assigned competitiveness. The model in Column (2) includes three treatment variables: the sum of competitiveness in each of the three chambers. These variables are instrumented using the sum of assigned competitiveness for the corresponding chamber. All other details in the table are the same as in Table 2.

We assess the additivity assumption in two ways. First, we test whether the combined effect of changing competitiveness for multiple chambers is equal to the sum of the effects for each constituent chamber. To implement this test, we fit a version of the IV model, equation (5), that uses a special form of the treatment variable, $\mathrm{C}_{i\tau}$. In this version, $\mathrm{C}_{i\tau}$ is a weighted sum of competitiveness in a registrant's districts across all chambers:

$$\mathrm{C}_{i\tau} = \hat{\alpha}^{\mathrm{USH}} \cdot \sum_{h=0}^{\tau} \mathrm{c}_{ih}^{\mathrm{USH}} + \hat{\alpha}^{\mathrm{NCS}} \cdot \sum_{h=0}^{\tau} \mathrm{c}_{ih}^{\mathrm{NCS}} + \hat{\alpha}^{\mathrm{NCH}} \cdot \sum_{h=0}^{\tau} \mathrm{c}_{ih}^{\mathrm{NCH}}.$$

The weight for each chamber is equal to the predicted effect of competitiveness in the chamber, which is captured by the chamber's coefficient estimate, $\hat{\alpha}^j$, from Table 2. If, as assumed, effects are additive across chambers, then the coefficient on $\mathrm{C}_{i\tau}$ in this model should equal 1. That is, the impact of increasing competitiveness for multiple chambers should be the sum of the chamber-specific impacts implied by the values in Table 2. By contrast, if effects aggregate according to a different functional form, then the coefficient on $\mathrm{C}_{i\tau}$ may not equal 1.

The test results back the additivity assumption. As seen in Column (1) of Table 11, the estimate for the coefficient on $\mathrm{C}_{i\tau}$ is 1.03, insignificantly different from 1.

Our second strategy is to re-estimate the chamber-specific effects, $\alpha^j$, using a version of the IV model that explicitly imposes additivity. Analogous to equation (1), this version has three treatment variables (the sum of competitiveness since redistricting in each chamber) and includes them in an additively separable manner. Identification is based on comparing turnout for individuals who differ in assigned competitiveness for potentially multiple chambers. If the assumption of additivity is correct, then the coefficient estimates for the model should match the values in Table 2. That is, we should recover the same estimates for $\alpha^j$ regardless of whether we use variation from multiple chambers or from just one chamber at a time.[43]

The results from the model are listed in Column (2) of Table 11. They again support the additivity assumption. The coefficient estimates are broadly similar to those in Table 2, and none of the differences are statistically significant.

Thus, this subsection provides substantial evidence that effects are additive across chambers. It shows that changing competitiveness for multiple chambers generates an impact that is the sum of the chamber-specific impacts. In addition, it indicates that we recover similar estimates, $\hat{\alpha}^j$, whether we compare individuals who differ in multiple chambers or in only a single chamber.

# 6 Impacts of North Carolina's legislative districts

As a last exercise, we study the impacts of North Carolina's recent legislative districts. Specifically, we simulate how turnout and votes would have differed in the 2012-2020 elections if all registrants had lived in competitive districts during this period. The analysis involves combining the causal effects from Section 5 with information on registrants' experiences with respect to competitiveness. The results are necessarily specific to North Carolina. Nonetheless, they illustrate the types of magnitudes we might expect if states were to make districts more competitive. They are also interesting from a historical perspective.

We first detail the methodology behind our simulation. We then provide summary statistics on the competitiveness of registrants' districts. Finally, we present the simulation results.

## 6.1 Methodology

We conduct the simulation in four steps.

First, we restrict the data to the analysis sample for the decennial redistricting episodes. This is individuals who were registered in 2010 and who did not die before the 2020 election. We make this restriction so that we can observe each person's districts and turnout in each election and so that everyone is subject to the treatment of competitive districts for the same length of time. As in Section 5, we do not attempt to adjust for the fact that individuals may move to other states. In this way, the exercise can be interpreted as revealing the influence of competitive districts on the within-state voting behavior of still-alive 2010 registrants.

Second, we compute impacts on turnout. In particular, we calculate how turnout probabili-

43. Unfortunately, it's possible for the two sets of coefficient estimates to differ even if the additivity assumption is valid. This is because there are complications associated with using variation from multiple chambers. For instance, there may be distortions due to multicollinearity: if competitiveness is correlated across chambers, then the model may struggle to parse the distinct effects of each chamber. As such, the test is only partially informative. It cannot disprove the additivity assumption; yet it can serve as evidence in favor of the assumption.

ties would have differed if registrants lived in competitive 55-45 districts, instead of their actual districts. To understand the calculation, let $\hat{\alpha}^j_{m_i}$ be an estimate of the causal effect of competitiveness in chamber $j$. Given the results in Section 5, let it vary based on $i$'s characteristics, as represented by membership in a group $m$. Also, let $0.9 - \mathrm{c}^j_{ih}$ be the difference in competitiveness between that of a 55-45 district and that of $i$'s actual chamber-$j$ district in election $h$. Finally, let $\sum_{h=2012}^{t}(0.9 - \mathrm{c}^j_{ih})$ be the sum of these differences for elections between 2012 and $t$. Then, for each election $t = 2012, \ldots, 2020$, we compute:

$$\Delta^{\text{to}}_{it} = \sum_{j \in \{\text{USH, NCS, NCH}\}} \hat{\alpha}^j_{m_i} \cdot \sum_{h=2012}^{t} (0.9 - \mathrm{c}^j_{ih}). \tag{9}$$

$\Delta^{\text{to}}_{it}$ is the predicted change in $i$'s turnout probability in election $t$ due to living in competitive districts. Equation (9) says that this quantity is a weighted sum of the change in $i$'s competitiveness in each chamber, with weights that reflect the chambers' causal effects of competitiveness.

Third, we quantify how competitive districts would have affected registrants' vote probabilities. This calculation requires two inputs. First, we suppose that district competitiveness has no effect on registrants' preferences over political parties; as a result, we can assume that impacts on votes are due only to changes in turnout. Second, we borrow information on registrants' party preferences from Ainsworth (2020). For each registrant in each election, we obtain the registrant's probability of preferring the candidate for major-party $k$ to all the other options. Let this probability be written $p^k_{it}$.[44] Then, the change in $i$'s vote probability for party $k$ in election $t$ is:

$$\Delta^k_{it} = p^k_{it} \cdot \Delta^{\text{to}}_{it}.$$

It is the change in $i$'s turnout probability scaled by the probability that $i$ prefers the given party.

In a last step, we summarize $\Delta^{\text{to}}_{it}$ and $\Delta^k_{it}$ over all the registrants in our sample. This allows us to observe the aggregate impacts of competitive districts.

## 6.2 The competitiveness of registrants' districts

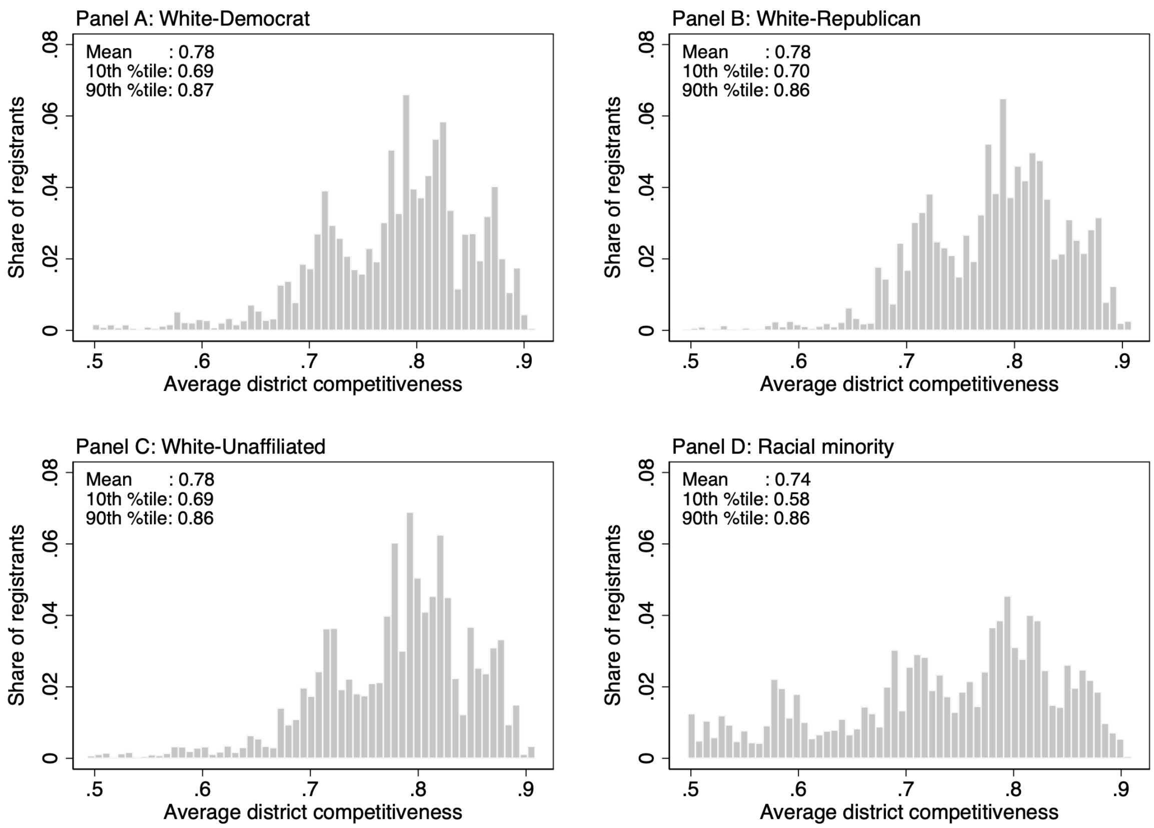
Before turning to the simulation results, we examine registrants' experiences with respect to competitiveness. For each registrant, we calculate the average competitiveness of the registrant's districts during the 2012 to 2020 elections. This quantity is

$$\bar{\mathrm{c}}_i = \frac{1}{15} \sum_{j \in \{\text{USH, NCS, NCH}\}} \sum_{t=2012}^{2020} \mathrm{c}^j_{it}.$$

44. The other options include the candidate for the other major party, third-party or write-in candidates, and leaving the question blank. We calculate $p^k_{it}$ in a "generic" race in which candidates are given the average quality for their party in the election. $p^k_{it}$ differs across registrants according to a rich set of covariates. These include a registrant's demographics, characteristics of the registrant's neighborhood, the registrant's history of turnout and party registration, as well as geographical fixed effects. See Ainsworth (2020) for more details.

We then summarize the distribution of $\bar{c}_i$ for four groups of registrants: white-Democrats, white-Republicans, white-Unaffiliated registrants, and racial minorities. The results are shown in Figure 4. For each group, the figure displays a histogram of $\bar{c}_i$ and also lists the mean and the 10th and 90th percentiles.

Figure 4: Histograms of $\bar{c}_i$ by a registrant's race and party




The figure plots histograms of $\bar{c}_i$ for the specified groups of registrants. "Mean", "10th %tile", and "90th %tile" are the mean, the 10th percentile, and the 90th percentile of $\bar{c}_i$ across registrants in the given group. $c_{it}^{j}$ is calculated using our main measure of district competitiveness, $c_{d,\mathrm{M}}$. The sample includes 5,667,577 registrants. Of these, 1,264,456 are white-Democrats, 1,713,142 are white-Republicans, 1,130,949 are white-Unaffiliated registrants, and 1,559,030 are racial minorities.

The figure yields four takeaways. First, among whites, the distribution of $\bar{c}_i$ does not depend on a registrant's party affiliation. Second, racial minorities experienced less competitive districts during 2012-2020 than did whites. Third, most registrants lived in moderately uncompetitive districts during this period. For whites, the mean of $\bar{c}_i$ is 0.78, which corresponds to a 61-39 district; for minorities, it is 0.74, or a 63-37 district. Fourth, there is substantial variation in exposure to competitiveness across registrants. For whites, the 10th percentile of $\bar{c}_i$ is 0.69 (a 65.5-35.5 district); for minorities, it is 0.58 (a 71-29 district). For both whites and minorities, the 90th percentile of $\bar{c}_i$ is 0.86 (a 57-43 district).[45]

45. Appendix Tables A24 and A25 provide additional information on the competitiveness of registrants' districts.

## 6.3 Simulation results

We now present the simulation results.

We calculate results for two versions of the simulation. These differ in how we define the groups, $m$, along which we allow $\hat{\alpha}^j_{m_i}$ to vary. Our main version is meant to capture the heterogeneity in causal effects that we uncovered in Section 5. In this version, we let $m$ consist of the same four groups as in Figure 4: white-Democrats, white-Republicans, white-Unaffiliated registrants, and minorities. In an alternative version, we restrict $\hat{\alpha}^j_{m_i}$ to be constant across registrants, while still allowing it to vary by chamber; that is, we set $\hat{\alpha}^j_{m_i} = \hat{\alpha}^j$. This version serves as a benchmark: it is correct if the heterogeneity by registrant type is merely noise. The values of $\hat{\alpha}^j_{m_i}$ that we use are displayed in Table 12. Those for our main version are shown in the "White" and "Minority" columns. Those for the alternative version are in the "All" column.

Table 12: The coefficient estimates used in the simulation

| | **All** | **White** | | | **Minority** |
|---|---|---|---|---|---|
| | | Dem. | Rep. | Unaffil. | |
| *Panel A: U.S. House* | | | | | |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 2.24*** | 3.76*** | 2.01*** | 2.90** | 1.14 |
| | (0.796) | (1.02) | (0.717) | (1.20) | (1.43) |
| Turnout percentage | 58.4 | 62.6 | 64.2 | 52.1 | 53.8 |
| Clusters | 176 | 171 | 171 | 171 | 171 |
| Registrants | 1,588,992 | 326,742 | 478,001 | 340,930 | 450,251 |
| Registrant-episode-elections | 6,516,888 | 1,424,986 | 1,862,704 | 1,307,915 | 1,921,283 |
| *Panel B: NC Senate* | | | | | |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.12** | 2.06*** | 0.972** | 1.56*** | 0.467 |
| | (0.473) | (0.719) | (0.453) | (0.573) | (0.660) |
| Turnout percentage | 58.3 | 62.5 | 63.6 | 52.0 | 54.0 |
| Clusters | 221 | 216 | 218 | 215 | 217 |
| Registrants | 1,679,413 | 340,799 | 500,087 | 366,590 | 486,734 |
| Registrant-episode-elections | 7,012,454 | 1,447,644 | 2,167,082 | 1,416,563 | 1,981,165 |
| *Panel C: NC House* | | | | | |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.02*** | 1.80*** | 0.816* | 1.34*** | 0.268 |
| | (0.310) | (0.377) | (0.468) | (0.411) | (0.401) |
| Turnout percentage | 57.8 | 61.6 | 63.5 | 51.7 | 53.0 |
| Clusters | 376 | 372 | 374 | 375 | 368 |
| Registrants | 4,024,634 | 829,286 | 1,234,109 | 923,259 | 1,118,373 |
| Registrant-episode-elections | 17,864,350 | 3,869,019 | 5,541,745 | 3,740,043 | 4,713,543 |

The table presents the coefficient estimates, $\hat{\alpha}^j_{m_i}$, that are used in the simulation. Values are obtained by running the IV model, equation (5), for the specified set of registrants and for the specified legislative chamber. The main version of the simulation uses the coefficient estimates in the 2nd through 5th columns of the table. The alternative version uses the coefficient estimates in the "All" column; these match the values in the "Chamber" columns of Table 2. All other details in the table are the same as in Table 2.

In presenting results, we focus first on our main version of the simulation. We later provide a comparison with the alternative version.

Table A24 shows how competitiveness varied across legislative chambers. Table A25 reveals how it differed by election. The tables show that state legislative districts tended to be less competitive than U.S. House districts. In addition, the districts that were used in 2018 and 2020 were more competitive than those from earlier elections.

We start by showing how competitive districts would have affected registrants' turnout probabilities. Specifically, we present summary statistics for $\Delta_{it}^{\text{to}}$. Results (for our main version of the simulation) are in Table 13. They reveal a few points. First, registrants would have been more likely to turn out during 2012-2020 if they had lived in competitive districts during this period. For 2012, the average change in the probability of turning out is an increase of 0.64 percentage points. By 2020, this value grows to 2.60 percentage points. Second, there is substantial variation

Table 13: The change in registrants' turnout probabilities under competitive districts

| **Election** | **All registrants** | | | **Means by group** | | | | **Means by party** | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | 10th percentile | 90th percentile | White-Dem. | White-Rep. | White-Unaffil. | Minority | Dem. | Rep. | Unaffil. |
| 2012 | 0.64 | 0.20 | 1.20 | 1.02 | 0.52 | 0.79 | 0.35 | 0.70 | 0.51 | 0.70 |
| 2014 | 1.27 | 0.40 | 2.39 | 2.03 | 1.03 | 1.57 | 0.70 | 1.40 | 1.01 | 1.39 |
| 2016 | 1.72 | 0.50 | 3.26 | 2.77 | 1.38 | 2.13 | 0.94 | 1.90 | 1.35 | 1.89 |
| 2018 | 2.14 | 0.53 | 4.12 | 3.51 | 1.69 | 2.67 | 1.15 | 2.38 | 1.66 | 2.36 |
| 2020 | 2.60 | 0.68 | 5.16 | 4.27 | 2.04 | 3.24 | 1.39 | 2.89 | 2.00 | 2.87 |

The table provides summary statistics for $\Delta_{it}^{\text{to}}$. That is, it shows how turnout would have differed in the 2012-2020 elections if registrants had lived in 55-45 districts during this period. The values in the "All registrants" columns represent the election-specific mean, 10th percentile, and 90th percentile of $\Delta_{it}^{\text{to}}$ over all individuals in our sample. The values in the "Means by group" and "Means by party" columns are election-specific means of $\Delta_{it}^{\text{to}}$ for individuals in the specified subsets. All values are denominated in percentage points. Results are from the main version of the simulation.

in $\Delta_{it}^{\text{to}}$ across registrants. For instance, in 2020, the 10th percentile of $\Delta_{it}^{\text{to}}$ is an increase of 0.68 percentage points, while the 90th percentile is an increase of 5.16 percentage points. Third, much of the variation in impacts can be explained by a registrant's group, $m$. Across groups, values are largest for white-Democrats and smallest for racial minorities.[46] Fourth, there are also differences in impacts by party. Impacts tend to be similar for Democrats and Unaffiliated registrants but smaller for Republicans. As an example, in 2020, the mean of $\Delta_{it}^{\text{to}}$ for Republicans is an increase of only 2 percentage points. For Democrats and Unaffiliated registrants, it is an increase of almost 2.9 percentage points.

We next present impacts on aggregate turnout. That is, we show how the number of registrants who turn out would have changed if everyone had lived in competitive districts. For context, we also show the number who actually did turn out in each of the 2012-2020 elections. The results are in Table 14. They are similar to the impacts on mean turnout probabilities in Table 13; however, they incorporate the fact that groups and parties differ in size.

The results in Table 14 indicate that competitive districts would have generated a considerable increase in aggregate turnout, especially in later elections. For instance, in 2020, overall turnout would have been higher by 147,359 registrants. This is a 4.25% increase over the election's actual turnout among our sample. The results by group are similar to those for turnout probabilities: namely, the increase in aggregate turnout would have been largest for white-Democrats and small-

46. The relatively small impact for racial minorities suggests that their small values of $\hat{\alpha}_{m_i}^{j}$ counteract the fact that they experienced the least amount of competitiveness.

Table 14: The change in aggregate turnout under competitive districts

| Election | Actual turnout | Change in turnout | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | All | By group | | | | By party | | |
| | | | White-Dem. | White-Rep. | White-Unaffil. | Minority | Dem. | Rep. | Unaffil. |
| 2012 | 3,649,120 | 36,041 | 12,854 | 8,852 | 8,880 | 5,455 | 17,124 | 9,126 | 9,790 |
| 2014 | 2,445,870 | 72,065 | 25,705 | 17,700 | 17,756 | 10,903 | 34,239 | 18,250 | 19,576 |
| 2016 | 3,449,519 | 97,501 | 35,074 | 23,674 | 24,144 | 14,609 | 46,509 | 24,402 | 26,591 |
| 2018 | 2,765,452 | 121,564 | 44,343 | 29,022 | 30,244 | 17,955 | 58,369 | 29,910 | 33,285 |
| 2020 | 3,467,293 | 147,359 | 53,969 | 35,007 | 36,661 | 21,723 | 70,909 | 36,075 | 40,376 |

The table summarizes the impact of competitive districts on aggregate turnout. "Actual turnout" is the number of registrants in our sample who turned out in the specified election. "Change in turnout" is how turnout would have differed if all districts used since 2012 were 55-45 districts. The values in the "Change in turnout" columns are calculated by summing $\Delta_{it}^{\text{to}}$ over the specified subset of registrants for the given election. Results are from the main version of the simulation.

est for minorities. By contrast, the results by party differ from before. For aggregate turnout, Democrats would have seen a substantially larger increase than the other parties. For example, in 2020, Democratic turnout would have been higher by 70,909 registrants, or almost half of the total increase in that election.

We next present impacts on votes. We show how the aggregate number of votes received by each party would have differed under competitive districts. The results are in Table 16. In the table, the columns titled "Change in votes" display the sum of $\Delta_{it}^{k}$ over all registrants in our sample. The column titled "Net change for Dem." lists the difference between the change in votes for Democrats and that for Republicans—it reveals impacts on vote margins. Finally, for context, the columns titled "Predicted votes" provide predictions for how the registrants in our sample voted in each election.[47]

Table 15: The change in aggregate votes under competitive districts

| Election | Predicted votes | | Change in votes | | Net change for Dem. |
|---|---|---|---|---|---|
| | Democrats | Republicans | Democrats | Republicans | |
| 2012 | 1,685,578 | 1,819,760 | 15,274 | 18,953 | -3,679 |
| 2014 | 1,137,003 | 1,165,079 | 32,633 | 32,173 | 460 |
| 2016 | 1,572,427 | 1,711,289 | 42,097 | 49,792 | -7,695 |
| 2018 | 1,347,548 | 1,315,227 | 64,873 | 52,905 | 11,968 |
| 2020 | 1,642,948 | 1,731,547 | 67,697 | 73,640 | -5,942 |

The table summarizes the impact of competitive districts on aggregate votes. "Predicted votes" is the predicted number of votes for each major party among registrants in our sample. For party $k$ and election $t$, it is the sum of $p_{it}^{k}$ over the individuals in our sample who turned out in the election. "Change in votes" is how votes would have differed if all districts used since 2012 were 55-45 districts. For party $k$ and election $t$, it is the sum of $\Delta_{it}^{k}$ over our entire sample. "Net change for Dem." is the difference between the change in votes for Democrats and that for Republicans. Results are from the main version of the simulation.

Table 15 reveals that competitive districts would have led to a higher number of votes; however, they would have had a negligible impact on vote margins. For instance, in 2020, vote totals would have increased by 67,697 for Democrats and 73,640 for Republicans. As a result, the change in

47. These values are calculated by summing $p_{it}^{k}$ over the registrants who turned out.

the vote margin would have been a relative loss in votes for Democrats of 5,942. This value is only about 7% of the election's predicted margin among our sample.

Table 16: The partisan composition of the increase in votes by party and group

| Election | By party | | | | | | By group | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Dem. | | Rep. | | Unaffil. | | White-Dem. | | White-Rep. | | White-Una. | | Minority | |
| | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. |
| 2012 | 60 | 35 | 13 | 83 | 39 | 54 | 49 | 46 | 12 | 85 | 35 | 59 | 89 | 7 |
| 2014 | 63 | 28 | 11 | 79 | 46 | 42 | 54 | 35 | 10 | 80 | 43 | 46 | 87 | 8 |
| 2016 | 61 | 35 | 13 | 81 | 40 | 52 | 50 | 45 | 12 | 82 | 36 | 56 | 87 | 8 |
| 2018 | 72 | 25 | 14 | 82 | 55 | 42 | 65 | 32 | 13 | 83 | 52 | 45 | 91 | 6 |
| 2020 | 65 | 33 | 12 | 84 | 43 | 50 | 55 | 42 | 11 | 85 | 40 | 54 | 89 | 7 |

The table displays $f_{st}^k$ for different sets of registrants. That is, for each set, it shows the percent of the set's increase in votes, due to competitive districts, that accrues to each major party. In the table, the registrant set $s$ is specified in the second row; the party $k$ is specified in the third row. Values of $f_{st}^k$ do not sum to 100 across the major parties. This because voters may choose other options, such as third parties or abstaining. Results are from the main version of the simulation.

The fact that competitive districts would have hardly affected vote margins is somewhat surprising. In particular, in Table 14, we found that aggregate turnout would have increased more for Democrats than for other parties. Thus, one might expect that Democrats would have also obtained a larger increase in votes.

To understand this puzzle, we examine the party preferences of the individuals who would have been induced to turn out by competitive districts. For different sets of registrants, we calculate the percent of the set's increase in votes that accrues to each major party. Specifically, for set $s$ in election $t$ and for party $k$, we calculate:

$$f_{st}^k = 100 \cdot \frac{\sum_i \Delta_{it}^k \cdot \mathbb{1}\{s_i = s\}}{\sum_i \Delta_{it}^{\text{to}} \cdot \mathbb{1}\{s_i = s\}}.$$

$f_{st}^k$ captures the partisan composition of set $s$'s increase in votes.

Values of $f_{st}^k$ are exhibited in Table 16. They show that in our simulation there was an asymmetry in how changes in a party's turnout translated into changes in votes for the party. Notably, for Republicans, the increase in turnout due to competitive districts generated votes that accrued almost entirely to Republicans (Columns 3 and 4). By contrast, for Democrats, the increase in turnout yielded votes for both parties (Columns 1 and 2). The reason for the latter finding is two-fold. First, most of the increase in turnout for Democrats was due to increases among white-Democrats (Table 14). Second, the white-Democrats who were induced to turn out were moderates who had a decent chance of voting for Republicans (Cols. 7 and 8 of Table 16).

Results for the alternative version of the simulation are provided in Appendix Tables A26-A29. They reveal that findings remain similar when we do not allow causal effects to vary by group. As in our main simulation, we find that competitive districts would have spurred higher turnout, but would have caused only small changes in vote margins. One difference is that, in the alternative simulation, the margins always shift in favor of Democrats. This is because, in this simulation, more of the increase in Democratic turnout is due to increases among racial minorities.

In sum, our results offer two conclusions about North Carolina's recent legislative districts. They suggest that the lack of competitiveness of these districts reduced turnout. However, it likely had little effect on parties' statewide vote shares or on who won statewide races. The only way it could have is if it had impacts via channels not captured in our simulation, such as by causing more people to become registered.

# 7 Conclusion

In this paper, we asked a central question in research on voting—whether competitive electoral environments induce additional turnout.

We studied this question in the context of American legislative districts, and we found a number of results. First, competitiveness does indeed spur turnout. For the U.S. House, switching from an 80-20 district to a 55-45 district increases turnout by an average of 1.12 percentage points per election of exposure. For the NC Senate and the NC House, magnitudes are 0.56 and 0.51 percentage points. Second, effects are long-lasting: lagged competitiveness has the same impact on turnout as current competitiveness for at least four lagged elections. Third, effects are additive across legislative chambers. Fourth, the effect of district competitiveness operates mainly, but not entirely, via exposure to competitive races. Fifth, effects can be observed for a variety of competitiveness measures, including one based on the readily available Cook PVI. Sixth, effects are heterogeneous. They are non-zero for all types of registrants; however, they are smaller for racial minorities.

Our results lend insight into the determinants of electoral turnout. In addition, they help to illuminate the mechanics of voting behavior: they provide evidence in support of theories that voters learn from past experiences and that voting is habit-forming.

Our results also have practical implications for the drawing of legislative districts. First, they imply that competitiveness is one of the criteria along which districts should be evaluated. Historically, districts have often been drawn to be uncompetitive, so as to protect incumbents. Our results suggest that these "bipartisan" gerrymanders suppress turnout, one of the core means of participation in democracy. Second, our results imply that majority-minority districts should be configured carefully. These districts should have a large enough minority share for minorities to hold sway over electoral outcomes; however, they also should be competitive enough to ensure that outcomes aren't a foregone conclusion. Third, our results highlight that policymakers may be able to influence elections by manipulating district competitiveness. In particular, policymakers can mobilize or demobilize voters by placing them in more or less competitive districts. This can be done strategically to sway statewide vote shares—although, in our empirical analysis, we found that the districts that were used in North Carolina during the 2010s did not affect vote shares.

Importantly, the results in our paper can be employed during redistricting to evaluate different district configurations. Specifically, stakeholders can forecast the turnout impacts of the competitiveness of the districts by measuring competitiveness and multiplying by our causal effects.

Despite the advances in this paper, many questions remain. One task for future work is to disentangle the channels through which competitive races drive turnout. A second task is to understand the effects of district competitiveness on outcomes other than turnout. How does district competitiveness affect legislator behavior, pork barrel spending, the incumbency advantage, voter preferences, etc.? Some research already exists in this area (Lindgren and Southwell 2014; McCarty et al. 2019; Finnegan 2021), but there is much still to be done. Finally, competitiveness is only one of the features of districts that we might care about. Others include the demographic and partisan composition of the districts, whether the districts are geographically compact, whether they preserve or divide communities of interest, and whether they generate a legislative seat share that proportionally reflects the statewide vote share. More work is needed to understand the relative importance of these features so that we can gain a sense of the optimal district configuration.

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# A1 Appendix figures and tables

Table A1: The number of registrants in each baseline election

| Baseline election | Registrants |
|---|---|
| 2010 | 6,255,853 |
| 2014 | 6,664,171 |
| 2016 | 6,979,559 |
| 2018 | 7,152,496 |
| Average | 6,763,020 |
| Distinct | 8,963,975 |
| Total | 27,052,079 |

The table summarizes the number of registrants in the baseline elections. 2010 is the baseline for the decennial redistricting episodes. 2014 is the baseline for the first court-ordered revision for the U.S. House. 2016 is the baseline for the same revision for the NC Senate and NC House. Finally, 2018 is the baseline for episodes associated with the second court-ordered revision.

Table A2: Correlations among measures of district competitiveness

| | Main measure | Alt. measure | Cook measure |
|---|---|---|---|
| Main measure, $c_{d,\mathrm{M}}$ | 1 | - | - |
| Alternative measure, $c_{d,\mathrm{A}}$ | 0.980*** | 1 | - |
| Cook measure, $c_{d,\mathrm{PVI}}$ | 0.846*** | 0.845*** | 1 |

The table presents a correlation matrix for three measures of district competitiveness. The sample is the 549 districts that were used in North Carolina during the 2012-2020 elections. See Section 3.3 for details on the competitiveness measures.

Table A3: Predicting race outcomes using district competitiveness measures: results by legislative chamber

| | **Main measure** | | **Alt. measure** | | **Cook measure** | |
|---|---|---|---|---|---|---|
| | Coef. (s.e.) | R-sq. | Coef. (s.e.) | R-sq. | Coef. (s.e.) | R-sq. |
| *Panel A: All chambers* (N=915) | | | | | | |
| Closeness | 1.39 (0.07) | 0.28 | 1.69 (0.09) | 0.26 | 1.36 (0.08) | 0.23 |
| Contested by both parties | 0.97 (0.10) | 0.09 | 1.17 (0.13) | 0.08 | 0.93 (0.11) | 0.07 |
| Ln. spending per person | 4.53 (0.24) | 0.28 | 5.34 (0.32) | 0.24 | 4.47 (0.27) | 0.23 |
| *Panel B: U.S. House* (N=65) | | | | | | |
| Closeness | 1.17 (0.21) | 0.33 | 1.64 (0.29) | 0.33 | 1.36 (0.30) | 0.25 |
| Contested by both parties | 0.22 (0.26) | 0.01 | 0.30 (0.36) | 0.01 | 0.14 (0.34) | 0.00 |
| Ln. spending per person | 3.56 (1.02) | 0.16 | 4.57 (1.43) | 0.14 | 2.88 (1.44) | 0.06 |
| *Panel C: NC Senate* (N=250) | | | | | | |
| Closeness | 1.36 (0.14) | 0.26 | 1.66 (0.19) | 0.24 | 1.29 (0.16) | 0.22 |
| Contested by both parties | 0.83 (0.20) | 0.06 | 0.96 (0.26) | 0.05 | 0.76 (0.21) | 0.05 |
| Ln. spending per person | 4.82 (0.49) | 0.28 | 5.72 (0.64) | 0.25 | 4.87 (0.51) | 0.27 |
| *Panel D: NC House* (N=600) | | | | | | |
| Closeness | 1.37 (0.09) | 0.28 | 1.64 (0.12) | 0.25 | 1.34 (0.10) | 0.23 |
| Contested by both parties | 0.99 (0.13) | 0.09 | 1.18 (0.16) | 0.08 | 0.94 (0.14) | 0.07 |
| Ln. spending per person | 4.47 (0.30) | 0.28 | 5.24 (0.38) | 0.24 | 4.38 (0.33) | 0.23 |

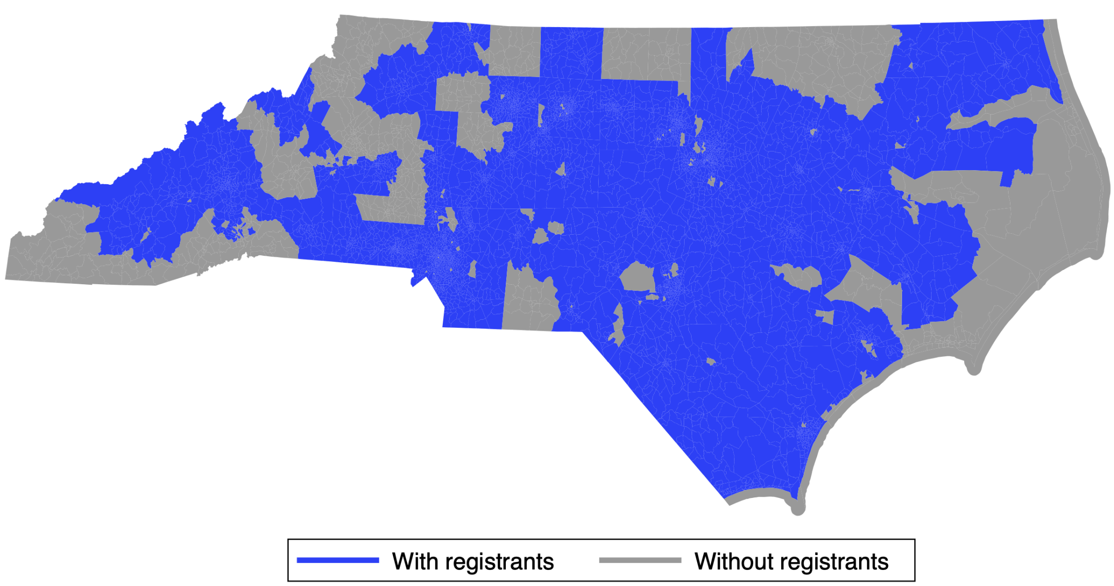
The table presents results from regressions of race outcomes on the competitiveness of the races' districts. The outcomes are listed in the rows of the table. The results in the columns titled "Main measure" are for regressions of the indicated outcomes on our main competitiveness measure, $c_{d,\mathrm{M}}$. The results under "Alt. measure" are for regressions that instead use the alternative simulation-based measure, $c_{d,\mathrm{A}}$. Finally, the results under "Cook measure" use the Cook PVI-based measure, $c_{d,\mathrm{PVI}}$. The sample in Panel A is races in all legislative chambers. The samples in the other panels are races in the specified chamber. Sample sizes are in parenthesis in the panel headings. "Contested by both parties" is an indicator for whether a race features both a Democratic and Republican candidate. See Figure 2 for definitions of the other outcomes.

Table A4: Summary statistics on registrants and match-groups in the estimation sample

| Episode | Registrants | Match-groups | Registrants per match-group | | | |
|---|---|---|---|---|---|---|
| | | | Mean | Std. dev. | Min | Max |
| Decennial redistricting | | | | | | |
| U.S. House | 1,049,735 | 56,120 | 18.7 | 33 | 2 | 660 |
| NC Senate | 1,194,384 | 63,776 | 18.7 | 36 | 2 | 1,038 |
| NC House | 2,897,904 | 125,243 | 23.1 | 49 | 2 | 1,559 |
| 1st court-ordered revision | | | | | | |
| U.S. House | 307,405 | 19,017 | 16.2 | 36 | 2 | 1,016 |
| NC Senate | 326,601 | 24,617 | 13.3 | 32 | 2 | 852 |
| NC House | 1,110,999 | 80,932 | 13.7 | 33 | 2 | 1,043 |
| 2nd court-ordered revision | | | | | | |
| U.S. House | 345,998 | 24,128 | 14.3 | 37 | 2 | 1,584 |
| NC Senate | 387,332 | 31,080 | 12.5 | 28 | 2 | 988 |
| NC House | 1,152,832 | 82,580 | 14.0 | 33 | 2 | 1,153 |
| All episodes | 8,773,190 | 507,493 | 17.3 | 38 | 2 | 1,584 |

The table provides summary statistics on the estimation sample. The estimation sample draws from individuals registered in a baseline election. It excludes registrants who die before the 2020 election and registrants who are in match-groups with no variation in the assigned post-redistricting district. For the decennial redistricting episodes, the baseline election is 2010. For episodes from the 1st court-ordered revision, the baseline election is 2014 for the U.S. House and 2016 for the NC Senate and NC House. For episodes from the 2nd court-ordered revision, the baseline election is 2018. In the row labeled "All episodes", the value under "Registrants" is the number of combinations of registrants and redistricting episodes. The estimation sample includes 5,204,602 distinct registrants. Values are calculated using our main definition of match-groups. See Section 4.3 for more details.

Figure A1: Census block-groups with registrants in the estimation sample




The figure reveals the geographic distribution of the registrants that form the estimation sample. Specifically, it shows the Census block-groups where these registrants lived during the baseline election. The map is created using our main definition of match-groups. See Section 4.3 for more details.

## Table A5: Predicting pre-redistricting turnout, $\text{to}_{i\tilde{\tau}}$, using $\text{C}_{a_i 0}$

| | Election prior to redistricting | | | | | | |
|---|---|---|---|---|---|---|---|
| | Seven | Six | Five | Four | Three | Two | One |
| *Panel A* | | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: main measure | 0.290 | -0.309 | -0.203 | -0.326 | 0 | 0 | 0 |
| | (0.691) | (0.488) | (0.348) | (0.473) | - | - | - |
| *Panel B* | | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: alternative measure | -0.128 | -0.343 | -0.311 | -0.423 | 0 | 0 | 0 |
| | (0.847) | (0.676) | (0.462) | (0.663) | - | - | - |
| *Panel C* | | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: Cook measure | 0.723 | 0.051 | -0.077 | -0.411 | 0 | 0 | 0 |
| | (0.819) | (0.598) | (0.492) | (0.577) | - | - | - |
| Turnout percentage | 24.9 | 41.9 | 44.3 | 51.2 | 37.8 | 64.3 | 50.2 |
| Clusters | 100 | 215 | 239 | 239 | 406 | 406 | 406 |
| Registrants | 1,800,263 | 2,783,147 | 2,946,696 | 2,946,696 | 5,204,602 | 5,204,602 | 5,204,602 |
| Registrant-episodes | 1,886,162 | 3,323,762 | 3,631,167 | 3,631,167 | 8,773,190 | 8,773,190 | 8,773,190 |

The table presents regression results from equation (8). Specifically, it shows coefficient estimates, $\hat{\phi}_{\tau\tilde{\tau}}$, and standard errors from separate regressions of $\text{to}_{i\tilde{\tau}}$ on $\text{C}_{a_i\tau}$ and match-group fixed effects. Each cell in the table represents a different regression. Results in different columns are for regressions that use different values of $\tilde{\tau}$. Results in different rows are for regressions that use different competitiveness measures to construct $\text{C}_{a_i\tau}$. All regressions are for $\tau = 0$. Coefficient estimates and standard errors are denominated in percentage points. "Turnout percentage" is the percent of observations that turned out in the given election. "Registrants" is the number of distinct registrants in the sample. "Registrant-episodes" is the number of observations. We cluster standard errors in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum number of clusters across the two dimensions. All regressions for a given value of $\tilde{\tau}$ have the same values for "Turnout percentage", "Clusters", "Registrants", and "Registrant-episodes". As a result, we provide this information in a single footer for each column. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table A6: Predicting pre-redistricting turnout, $\text{to}_{i\tilde{\tau}}$, using $\text{C}_{a_i 1}$

| | Election prior to redistricting | | | | | |
|---|---|---|---|---|---|---|
| | Six | Five | Four | Three | Two | One |
| *Panel A* | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: main measure | -0.359 | -0.362 | -0.645 | 0 | 0 | 0 |
| | (0.878) | (0.360) | (0.638) | - | - | - |
| *Panel B* | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: alternative measure | -0.260 | -0.449 | -0.738 | 0 | 0 | 0 |
| | (1.23) | (0.472) | (0.936) | - | - | - |
| *Panel C* | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: Cook measure | 0.227 | -0.581 | -0.765 | 0 | 0 | 0 |
| | (1.16) | (0.503) | (0.993) | - | - | - |
| Turnout percentage | 26.9 | 54.3 | 43.3 | 37.3 | 63.8 | 49.0 |
| Clusters | 127 | 167 | 167 | 401 | 401 | 401 |
| Registrants | 1,346,719 | 1,599,610 | 1,599,610 | 4,486,355 | 4,486,355 | 4,486,355 |
| Registrant-episodes | 1,437,600 | 1,745,005 | 1,745,005 | 6,887,028 | 6,887,028 | 6,887,028 |

The table presents results from equation (8). Values are analogous to those in Table A5; however, they are from regressions for $\tau = 1$.

## Table A7: Predicting pre-redistricting turnout, $\text{to}_{i\tilde{\tau}}$, using $\text{C}_{a_i 2}$

| | Election prior to redistricting | | | | |
|---|---|---|---|---|---|
| | Five | Four | Three | Two | One |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: main measure | 0.247 | -0.635 | 0 | 0 | 0 |
| | (0.764) | (0.741) | - | - | - |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: alternative measure | 0.251 | -0.937 | 0 | 0 | 0 |
| | (1.04) | (1.06) | - | - | - |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: Cook measure | 0.478 | 0.380 | 0 | 0 | 0 |
| | (1.48) | (1.78) | - | - | - |
| Turnout percentage | 30.9 | 63.7 | 29.8 | 68.9 | 42.7 |
| Clusters | 40 | 40 | 287 | 287 | 287 |
| Registrants | 307,405 | 307,405 | 3,932,662 | 3,932,662 | 3,932,662 |
| Registrant-episodes | 307,405 | 307,405 | 5,449,428 | 5,449,428 | 5,449,428 |

The table presents results from equation (8). Values are analogous to those in Table A5; however, they are from regressions for $\tau = 2$.

## Table A8: Predicting pre-redistricting district characteristics using $\text{C}_{a_i 0}$

| | Sum in pre-redistricting elections | | | | |
|---|---|---|---|---|---|
| | Competitiveness | Share minority | Share Democratic | Closeness | Ln. spending |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: main measure | 0.010 | -0.023 | -0.024 | -0.035 | 0.089 |
| | (0.006) | (0.035) | (0.024) | (0.044) | (0.147) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: alternative measure | 0.013 | -0.029 | -0.028 | -0.063 | 0.076 |
| | (0.009) | (0.044) | (0.029) | (0.055) | (0.179) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $\text{C}_{a_i\tau}$: Cook measure | 0.007 | -0.003 | 0.005 | -0.036 | 0.185 |
| | (0.007) | (0.033) | (0.024) | (0.050) | (0.169) |
| Mean of outcome variable | 7.93 | 3.67 | 5.77 | 7.33 | 2.63 |
| Clusters | 239 | 406 | 406 | 406 | 406 |
| Registrants | 2,946,696 | 5,204,602 | 5,204,602 | 5,204,602 | 5,204,602 |
| Registrant-episodes | 3,631,167 | 8,773,190 | 8,773,190 | 8,773,190 | 8,773,190 |

The table presents results related to the association between $\xi_i^{\text{pre}}$ and $\text{C}_{a_i\tau}$. Specifically, it presents coefficient estimates, $\hat{\phi}_\tau$, and standard errors for $\tau$-specific regressions of pre-redistricting district characteristics on $\text{C}_{a_i\tau}$ and match-group fixed effects. See Section 4.4 for details on these regressions. Each cell in the table represents a different regression. Results in different columns are for regressions that use the listed district or race characteristic in calculating the outcome variable. Results in different rows are for regressions that use different competitiveness measures to construct $\text{C}_{a_i\tau}$. All regressions are for $\tau = 0$. Outcome variables are calculated by summing the value of the listed characteristic in the registrant's districts over all chambers and all pre-redistricting elections. "Competitiveness" is calculated using our main competitiveness measure, $\text{c}_{d,\text{M}}$. We lack data on district competitiveness for districts used in 2010 and earlier. Consequently, we do not sum over these elections in calculating the "Competitiveness" outcome variable. Standard errors are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum number of clusters across these two dimensions. All regressions for a given outcome variable have the same values for "Mean of outcome variable", "Clusters", "Registrants", and "Registrant-episodes". As a result, we provide this information in a single footer for each column.

## Table A9: Predicting pre-redistricting district characteristics using $C_{a_i 1}$

| | Sum in pre-redistricting elections | | | | |
|---|---|---|---|---|---|
| | Competitiveness | Share minority | Share Democratic | Closeness | Ln. spending |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i \tau}$: main measure | 0.001<br>(0.005) | -0.001<br>(0.003) | -0.000<br>(0.002) | -0.002<br>(0.005) | -0.008<br>(0.015) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i \tau}$: alternative measure | 0.000<br>(0.008) | -0.001<br>(0.004) | -0.000<br>(0.002) | -0.004<br>(0.006) | -0.012<br>(0.018) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i \tau}$: Cook measure | 0.001<br>(0.008) | -0.000<br>(0.003) | 0.001<br>(0.002) | -0.002<br>(0.004) | -0.001<br>(0.015) |
| Mean of outcome variable | 6.42 | 3.10 | 4.96 | 6.05 | 1.58 |
| Clusters | 167 | 401 | 401 | 401 | 401 |
| Registrants | 1,599,610 | 4,486,355 | 4,486,355 | 4,486,355 | 4,486,355 |
| Registrant-episodes | 1,745,005 | 6,887,028 | 6,887,028 | 6,887,028 | 6,887,028 |

The table presents results related to the association between $\xi_i^{\text{pre}}$ and $C_{a_i \tau}$. Values are analogous to those in Table A8. However, they are from regressions for $\tau = 1$.

## Table A10: Predicting pre-redistricting district characteristics using $C_{a_i 2}$ or $C_{a_i 3}$

| | Sum in pre-redistricting elections | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | *Regression on* $C_{a_i 2}$ | | | | *Regression on* $C_{a_i 3}$ | | | |
| | Share minority | Share Democratic | Closeness | Ln. spending | Share minority | Share Democratic | Closeness | Ln. spending |
| *Panel A* | | | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i \tau}$: main measure | -0.001<br>(0.001) | -0.000<br>(0.001) | -0.001<br>(0.003) | 0.003<br>(0.009) | -0.001<br>(0.001) | -0.001<br>(0.001) | -0.001<br>(0.003) | 0.003<br>(0.009) |
| *Panel B* | | | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i \tau}$: alternative measure | -0.001<br>(0.002) | -0.001<br>(0.001) | -0.001<br>(0.003) | 0.002<br>(0.011) | -0.001<br>(0.002) | -0.001<br>(0.001) | -0.001<br>(0.003) | 0.002<br>(0.012) |
| *Panel C* | | | | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i \tau}$: Cook measure | -0.001<br>(0.001) | -0.001<br>(0.001) | 0.000<br>(0.003) | 0.007<br>(0.010) | -0.001<br>(0.001) | -0.001<br>(0.001) | 0.000<br>(0.003) | 0.006<br>(0.010) |
| Mean of outcome variable | 2.52 | 4.22 | 5.02 | 1.61 | 2.44 | 4.09 | 4.84 | 1.43 |
| Clusters | 287 | 287 | 287 | 287 | 247 | 247 | 247 | 247 |
| Registrants | 3,932,662 | 3,932,662 | 3,932,662 | 3,932,662 | 3,843,970 | 3,843,970 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 5,449,428 | 5,449,428 | 5,449,428 | 5,449,428 | 5,142,023 | 5,142,023 | 5,142,023 | 5,142,023 |

The table presents results related to the association between $\xi_i^{\text{pre}}$ and $C_{a_i \tau}$. Values are analogous to those in Table A8. However, they are from regressions for either $\tau = 2$ or $\tau = 3$. Results are not provided for the "Competitiveness" outcome variable due to lack of data. Regressions are not shown for $\tau = 4$ because they are the same as those for $\tau = 3$.

Table A11: Predicting the sum of post-redistricting district competitiveness for a registrant's districts in other chambers

| | **Election relative to redistricting, $\tau$** | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | 0.005*** | 0.009*** | 0.008*** | 0.019*** | 0.028*** |
| | (0.001) | (0.003) | (0.003) | (0.007) | (0.008) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | 0.007*** | 0.011*** | 0.010*** | 0.025*** | 0.036*** |
| | (0.001) | (0.003) | (0.003) | (0.009) | (0.011) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | 0.004*** | 0.007*** | 0.008** | 0.014 | 0.022** |
| | (0.001) | (0.002) | (0.003) | (0.009) | (0.010) |
| Mean of outcome variable | 1.55 | 3.05 | 4.60 | 6.18 | 7.76 |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results for the association between $\xi^{\text{oth}}_{i\tau}$ and $C_{a_i\tau}$. Specifically, it presents coefficient estimates, $\hat{\phi}_\tau$, and standard errors for $\tau$-specific regressions of post-redistricting district characteristics on $C_{a_i\tau}$ and match-group fixed effects. Each cell in the table represents a different regression. Results in different columns are for regressions that use different relative elections, $\tau$. Results in different rows are for regressions that use different competitiveness measures to construct $C_{a_i\tau}$. In all regressions, the outcome variable is related to district competitiveness. It is the sum of our main competitiveness measure, $c_{d,\text{M}}$, in a registrant's districts for chambers other than the chamber of interest over the elections 0 to $\tau$. Standard errors are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum number of clusters across these two dimensions. All regressions for a given relative election, $\tau$, have the same values for "Mean of outcome variable", "Clusters", "Registrants", and "Registrant-episodes". As a result, we provide this information in a single footer for each column.

Table A12: Predicting the sum of post-redistricting district share minority for a registrant's districts in other chambers

| | **Election relative to redistricting, $\tau$** | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | -0.005*** | -0.009*** | -0.009*** | -0.019*** | -0.026*** |
| | (0.001) | (0.002) | (0.003) | (0.006) | (0.007) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | -0.006*** | -0.011*** | -0.012*** | -0.025*** | -0.034*** |
| | (0.001) | (0.003) | (0.003) | (0.008) | (0.009) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | -0.004*** | -0.007*** | -0.011*** | -0.019** | -0.026*** |
| | (0.001) | (0.002) | (0.003) | (0.008) | (0.009) |
| Mean of outcome variable | 0.61 | 1.23 | 1.82 | 2.46 | 3.15 |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results for the association between $\xi^{\text{oth}}_{i\tau}$ and $C_{a_i\tau}$. Values are analogous to those in Table A11. However, the outcome variable is related to district share minority. It is the sum of share minority in a registrant's districts for chambers other than the chamber of interest over the elections 0 to $\tau$.

Table A13: Predicting the sum of post-redistricting district share Democratic for a registrant's districts in other chambers

| | Election relative to redistricting, $\tau$ | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | -0.003*** | -0.006*** | -0.006*** | -0.014*** | -0.019*** |
| | (0.001) | (0.002) | (0.002) | (0.004) | (0.005) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | -0.004*** | -0.008*** | -0.009*** | -0.018*** | -0.024*** |
| | (0.001) | (0.002) | (0.002) | (0.005) | (0.006) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | -0.003*** | -0.005*** | -0.008*** | -0.015*** | -0.020*** |
| | (0.001) | (0.001) | (0.002) | (0.005) | (0.007) |
| Mean of outcome variable | 0.81 | 1.65 | 2.48 | 3.26 | 4.00 |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results for the association between $\xi_{i\tau}^{\mathrm{oth}}$ and $C_{a_i\tau}$. Values are analogous to those in Table A11. However, the outcome variable is related to district share Democratic. It is the sum of share Democratic in a registrant's districts for chambers other than the chamber of interest over the elections 0 to $\tau$.

Table A14: Predicting the sum of post-redistricting race closeness for a registrant's districts in other chambers

| | Election relative to redistricting, $\tau$ | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | 0.008*** | 0.011** | 0.009** | 0.022** | 0.027** |
| | (0.002) | (0.005) | (0.004) | (0.010) | (0.012) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | 0.011*** | 0.014*** | 0.013*** | 0.030** | 0.037** |
| | (0.002) | (0.005) | (0.005) | (0.013) | (0.016) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | 0.007*** | 0.010*** | 0.009** | 0.010 | 0.011 |
| | (0.002) | (0.004) | (0.004) | (0.013) | (0.015) |
| Mean of outcome variable | 1.26 | 2.32 | 3.35 | 4.79 | 6.19 |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results for the association between $\xi_{i\tau}^{\mathrm{oth}}$ and $C_{a_i\tau}$. Values are analogous to those in Table A11. However, the outcome variable is related to race closeness. It is the sum of race closeness in a registrant's districts for chambers other than the chamber of interest over the elections 0 to $\tau$.

Table A15: Predicting the sum of post-redistricting race ln. spending for a registrant's districts in other chambers

| | Election relative to redistricting, $\tau$ | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | 0.016** | 0.033* | -0.011 | 0.062 | 0.116* |
| | (0.007) | (0.018) | (0.021) | (0.050) | (0.065) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | 0.019* | 0.045** | -0.015 | 0.081 | 0.135 |
| | (0.010) | (0.020) | (0.025) | (0.067) | (0.084) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | 0.006 | 0.023 | -0.015 | 0.003 | 0.016 |
| | (0.008) | (0.016) | (0.022) | (0.063) | (0.074) |
| Mean of outcome variable | 0.76 | 1.19 | 0.90 | 1.94 | 2.99 |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results for the association between $\xi^{\text{oth}}_{i\tau}$ and $C_{a_i\tau}$. Values are analogous to those in Table A11. However, the outcome variable is related to race spending. It is the sum of the natural log of race spending in a registrant's districts for chambers other than the chamber of interest over the elections 0 to $\tau$.

Table A16: Predicting the sum of post-redistricting district share minority in a registrant's districts for the chamber of interest

| | All | White | | | Minority |
|---|---|---|---|---|---|
| | | Dem. | Rep. | Unaffil. | |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | -0.578*** | -0.561*** | -0.498*** | -0.470*** | -0.703*** |
| | (0.080) | (0.094) | (0.097) | (0.111) | (0.049) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | -0.754*** | -0.765*** | -0.675*** | -0.633*** | -0.858*** |
| | (0.092) | (0.108) | (0.116) | (0.138) | (0.059) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | -0.685*** | -0.692*** | -0.597*** | -0.578*** | -0.788*** |
| | (0.061) | (0.075) | (0.076) | (0.091) | (0.043) |
| Mean of outcome variable | 0.85 | 0.78 | 0.73 | 0.71 | 1.17 |
| Clusters | 406 | 405 | 406 | 405 | 402 |
| Registrants | 5,204,602 | 1,059,113 | 1,205,238 | 1,578,979 | 1,498,613 |
| Registrant-episode-elections | 31,393,692 | 6,741,649 | 6,464,521 | 9,571,531 | 8,615,991 |

The table presents results for the association between $\xi^{\text{int}}_{i\tau}$ and $C_{a_i\tau}$. Specifically, it presents coefficient estimates and standard errors for regressions of post-redistricting district characteristics on $C_{a_i\tau}$ and match-group-by-$\tau$ fixed effects. The regressions pool observations for each post-redistricting election and fit a single coefficient on $C_{a_i\tau}$. The outcome variable in the regressions is related to district share minority. It is the sum of share minority in a registrant's districts for the chamber of interest over the elections 0 to $\tau$. Each cell in the table represents a different regression. Results in different columns are for regressions that use the listed subset of registrants. Results in different rows are for regressions that use different competitiveness measures to construct $C_{a_i\tau}$. Standard errors are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum number of clusters across these two dimensions. All regressions for a given subset of registrants have the same values for "Mean of outcome variable", "Clusters", "Registrants", and "Registrant-episodes". As a result, we provide this information in a single footer for each column.

Table A17: Predicting the sum of post-redistricting district share Democratic in a registrant's districts for the chamber of interest

| | All | White | | | Minority |
|---|---|---|---|---|---|
| | | Dem. | Rep. | Unaffil. | |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | -0.314*** | -0.304*** | -0.262*** | -0.230*** | -0.404*** |
| | (0.050) | (0.058) | (0.060) | (0.072) | (0.031) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | -0.419*** | -0.426*** | -0.365*** | -0.325*** | -0.498*** |
| | (0.059) | (0.068) | (0.073) | (0.090) | (0.038) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | -0.371*** | -0.372*** | -0.309*** | -0.284*** | -0.454*** |
| | (0.044) | (0.052) | (0.054) | (0.067) | (0.030) |
| Mean of outcome variable | 1.11 | 1.11 | 1.02 | 1.00 | 1.30 |
| Clusters | 406 | 405 | 406 | 405 | 402 |
| Registrants | 5,204,602 | 1,059,113 | 1,205,238 | 1,578,979 | 1,498,613 |
| Registrant-episode-elections | 31,393,692 | 6,741,649 | 6,464,521 | 9,571,531 | 8,615,991 |

The table presents results for the association between $\xi_{i\tau}^{\text{int}}$ and $C_{a_i\tau}$. Values are analogous to those in Table A16. However, the outcome variable is related to district share Democratic. It is the sum of share Democratic in a registrant's districts for the chamber of interest over the elections 0 to $\tau$.

Table A18: Predicting the sum of post-redistricting district share minority in the chamber of interest: trimmed sample

| | All | White | | | Minority |
|---|---|---|---|---|---|
| | | Dem. | Rep. | Unaffil. | |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | -0.042 | -0.060 | -0.035 | -0.052 | 0.000 |
| | (0.131) | (0.136) | (0.136) | (0.141) | (0.135) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | -0.055 | -0.099 | -0.044 | -0.070 | 0.031 |
| | (0.192) | (0.213) | (0.201) | (0.196) | (0.202) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | -0.203 | -0.238 | -0.136 | -0.195 | -0.264 |
| | (0.134) | (0.149) | (0.135) | (0.136) | (0.161) |
| Mean of outcome variable | 0.72 | 0.71 | 0.67 | 0.67 | 0.88 |
| Clusters | 395 | 392 | 391 | 394 | 387 |
| Registrants | 4,775,382 | 1,014,093 | 1,161,571 | 1,539,592 | 1,189,749 |
| Registrant-episode-years | 27,136,162 | 6,265,820 | 6,064,357 | 9,185,302 | 5,620,683 |

The table presents results for the association between $\xi_{i\tau}^{\text{int}}$ and $C_{a_i\tau}$. Values are analogous to those in Table A16. However, the sample excludes registrants who are assigned to the districts with the largest minority shares. For white-Democrats, white-Unaffiliated registrants, and white-Republicans, it drops registrants who are assigned to districts with minority shares greater than 0.625, 0.635, and 0.645, respectively. For minorities, it drops registrants who are assigned to districts with minority shares greater than 0.615.

Table A19: Predicting the sum of post-redistricting district share Democratic in the chamber of interest: trimmed sample

| | All | White | | | Minority |
|---|---|---|---|---|---|
| | | Dem. | Rep. | Unaffil. | |
| *Panel A* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: main measure | 0.010<br>(0.089) | -0.029<br>(0.097) | 0.019<br>(0.090) | 0.036<br>(0.092) | 0.002<br>(0.094) |
| *Panel B* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: alternative measure | -0.000<br>(0.130) | -0.070<br>(0.149) | 0.015<br>(0.132) | 0.033<br>(0.130) | 0.006<br>(0.139) |
| *Panel C* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$: Cook measure | -0.075<br>(0.102) | -0.128<br>(0.116) | -0.019<br>(0.101) | -0.035<br>(0.102) | -0.168<br>(0.121) |
| Mean of outcome variable | 1.03 | 1.07 | 0.98 | 0.98 | 1.11 |
| Clusters | 395 | 392 | 391 | 394 | 387 |
| Registrants | 4,775,382 | 1,014,093 | 1,161,571 | 1,539,592 | 1,189,749 |
| Registrant-episode-years | 27,136,162 | 6,265,820 | 6,064,357 | 9,185,302 | 5,620,683 |

The table presents results for the association between $\xi^{\text{int}}_{i\tau}$ and $C_{a_i\tau}$. Values are analogous to those in Table A17. However, the sample is the same as that in Table A18.

Table A20: The turnout effects of district competitiveness: decennial redistricting episodes

| | All | Election relative to redistricting, $\tau$ | | | | |
|---|---|---|---|---|---|---|
| | | Zero | One | Two | Three | Four |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.17***<br>(0.245) | 1.82***<br>(0.564) | 1.07***<br>(0.272) | 1.10***<br>(0.282) | 1.13***<br>(0.214) | 1.24***<br>(0.303) |
| Turnout percentage | 56.5 | 65.1 | 43.9 | 61.5 | 49.8 | 61.9 |
| Clusters | 247 | 247 | 247 | 247 | 247 | 247 |
| Registrants | 3,843,970 | 3,843,970 | 3,843,970 | 3,843,970 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 25,710,115 | 5,142,023 | 5,142,023 | 5,142,023 | 5,142,023 | 5,142,023 |

The table is analogous to Table 3. However, the sample is restricted to the decennial redistricting episodes.

Table A21: The turnout effects of district competitiveness: robustness to alternative match-groups

| | Main | Alternative match groups | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.16***<br>(0.230) | 1.16***<br>(0.238) | 1.13***<br>(0.232) | 1.15***<br>(0.227) | 1.18***<br>(0.231) | 1.01***<br>(0.220) | 0.975***<br>(0.232) | 1.06***<br>(0.243) |
| Turnout percentage | 58.1 | 57.4 | 58.2 | 57.5 | 57.4 | 58.9 | 58.8 | 58.3 |
| Clusters | 406 | 406 | 406 | 406 | 406 | 406 | 406 | 405 |
| Registrants | 5,204,602 | 5,824,085 | 5,320,601 | 5,454,724 | 5,551,175 | 4,848,670 | 4,879,974 | 4,768,116 |
| Registrant-episode-elections | 31,393,692 | 38,993,025 | 32,570,723 | 33,603,934 | 34,406,199 | 28,280,987 | 28,792,239 | 27,097,928 |

The table presents results for alternative ways of constructing match-groups. Specifically, it shows coefficient estimates, $\hat{\alpha}$, and standard errors for versions of equation (5) that define match-groups using different sets of covariates. "Main" is for our main definition. It corresponds with the "All" column of Table 2. The remaining columns add or remove a single covariate. Column (1) removes the education level of a registrant's baseline block-group. Column (2) removes a registrant's turnout behavior in $\tau = -3$. Column (3) removes the registrant's gender. Column (4) removes the registrant's baseline party registration. Column (5) adds the population density in the registrant's baseline Census block. Column (6) adds the value of the registrant's baseline property parcel. Column (7) adds the median household income in the registrant's baseline block-group. All other details are the same as in Table 2.

Table A22: Heterogeneity in effects for white registrants

| | All | Chamber | | Gender | | Age | | Education | | Voted in baseline | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | U.S. House | NC legisl. | Male | Female | $\leq 35$ | $> 35$ | Low | High | No | Yes |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 1.51*** | 2.83*** | 1.38*** | 1.53*** | 1.49*** | 2.06*** | 1.29*** | 1.42*** | 1.65*** | 1.78*** | 1.19*** |
| | (0.234) | (0.765) | (0.246) | (0.267) | (0.224) | (0.441) | (0.205) | (0.330) | (0.326) | (0.321) | (0.201) |
| Turnout percentage | 59.8 | 60.3 | 59.7 | 60.2 | 59.5 | 42.4 | 66.5 | 57.8 | 63.1 | 35.6 | 85.7 |
| Clusters | 406 | 173 | 400 | 406 | 405 | 405 | 406 | 387 | 254 | 406 | 404 |
| Registrants | 3,709,065 | 1,138,871 | 3,322,360 | 1,707,378 | 2,002,919 | 1,188,424 | 2,664,704 | 2,435,523 | 1,465,687 | 2,053,767 | 2,037,635 |
| Registrant-episode-elections | 22,777,701 | 4,595,605 | 18,182,096 | 10,396,663 | 12,381,038 | 6,343,941 | 16,433,760 | 14,085,897 | 8,691,804 | 11,777,170 | 11,000,531 |

The table presents heterogeneity in effects for white registrants. Specifically, it presents results from versions of the IV model, equation (5), that are calculated using only observations for white registrants with the specified characteristics. The results in the column titled "All" use all observations for white registrants. The results in the columns titled "Chamber" use only the redistricting episodes for the listed chamber. "NC legisl." refers to the state legislative chambers in North Carolina: the NC Senate and the NC House. Definitions for the variables for "Education" and "Voted in baseline" are provided in Table 9. All other details are the same as in Table 2.

Table A23: Heterogeneity in effects for racial minorities

| | All | Chamber | | Gender | | Age | | Education | | Voted in baseline | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | U.S. House | NC legisl. | Male | Female | $\leq 35$ | $> 35$ | Low | High | No | Yes |
| Sum of competitiveness in a registrant's districts, $C_{i\tau}$ | 0.417 | 1.14 | 0.342 | 0.650* | 0.248 | 0.228 | 0.527 | 0.247 | 0.909* | 0.488 | 0.306 |
| | (0.340) | (1.428) | (0.358) | (0.359) | (0.373) | (0.531) | (0.322) | (0.386) | (0.527) | (0.406) | (0.313) |
| Turnout percentage | 53.4 | 53.8 | 53.3 | 47.5 | 57.4 | 37.0 | 63.3 | 52.7 | 56.3 | 32.7 | 84.2 |
| Clusters | 402 | 171 | 395 | 402 | 401 | 402 | 401 | 381 | 243 | 401 | 402 |
| Registrants | 1,498,613 | 450,251 | 1,333,134 | 611,008 | 888,264 | 647,628 | 906,253 | 1,215,559 | 334,067 | 954,255 | 674,271 |
| Registrant-episode-elections | 8,615,991 | 1,921,283 | 6,694,708 | 3,458,586 | 5,157,405 | 3,250,140 | 5,365,851 | 6,967,038 | 1,648,953 | 5,153,726 | 3,462,265 |

The table is analogous to Table A22. However, it is for registrants who are racial minorities.

Table A24: Summarizing $\bar{c}_i^j$, the average competitiveness of a registrant's districts in chamber $j$

| | **Mean** | **Std. dev.** | **Percentile** | |
|---|---|---|---|---|
| | | | 10th | 90th |
| *Panel A: U.S. House* | | | | |
| All registrants | 0.80 | 0.07 | 0.71 | 0.88 |
| White-Democrats | 0.81 | 0.06 | 0.71 | 0.87 |
| White-Republicans | 0.82 | 0.05 | 0.73 | 0.88 |
| White-Unaffiliated | 0.81 | 0.06 | 0.71 | 0.88 |
| Racial minorities | 0.77 | 0.09 | 0.64 | 0.88 |
| *Panel B: NC Senate* | | | | |
| All registrants | 0.76 | 0.10 | 0.63 | 0.88 |
| White-Democrats | 0.78 | 0.09 | 0.66 | 0.88 |
| White-Republicans | 0.77 | 0.09 | 0.66 | 0.88 |
| White-Unaffiliated | 0.77 | 0.09 | 0.66 | 0.88 |
| Racial minorities | 0.73 | 0.12 | 0.53 | 0.87 |
| *Panel C: NC House* | | | | |
| All registrants | 0.74 | 0.12 | 0.59 | 0.89 |
| White-Democrats | 0.76 | 0.11 | 0.60 | 0.90 |
| White-Republicans | 0.75 | 0.11 | 0.60 | 0.88 |
| White-Unaffiliated | 0.76 | 0.11 | 0.60 | 0.89 |
| Racial minorities | 0.71 | 0.14 | 0.50 | 0.88 |

The table shows how registrants' exposure to competitiveness varies by legislative chamber. Specifically, it presents summary statistics for chamber-specific versions of $\bar{c}_i$. These are:

$$\bar{c}_i^j = \frac{1}{5}\sum_{t=2012}^{2020} c_{it}^j.$$

The values in Panel A are for $\bar{c}_i^{\mathrm{USH}}$, while those in Panels B and C are for $\bar{c}_i^{\mathrm{NCS}}$ and $\bar{c}_i^{\mathrm{NCH}}$. See Section 6.2 and Figure 4 for more details.

Table A25: Summarizing $\bar{c}_{it}$, the average competitiveness of a registrant's districts in election $t$

| **Election** | **Mean** | **Std. dev.** | **Percentile** | |
|---|---|---|---|---|
| | | | 10th | 90th |
| 2012 | 0.75 | 0.09 | 0.64 | 0.86 |
| 2014 | 0.75 | 0.09 | 0.64 | 0.86 |
| 2016 | 0.77 | 0.08 | 0.66 | 0.88 |
| 2018 | 0.79 | 0.10 | 0.66 | 0.92 |
| 2020 | 0.79 | 0.11 | 0.62 | 0.92 |

The table shows how registrants' exposure to competitiveness varies by election. Specifically, it presents summary statistics for election-specific versions of $\bar{c}_i$. These are:

$$\bar{c}_{it} = \frac{1}{3}\sum_{j} c_{it}^j.$$

The values in a given row are for $\bar{c}_{it}$ from the specified election. See Section 6.2 and Figure 4 for more details.

Table A26: The change in registrants' turnout probabilities under competitive districts: results calculated under common causal effects

| **Election** | **All registrants** | | | **Means by group** | | | | **Means by party** | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | 10th percentile | 90th percentile | White-Dem. | White-Rep. | White-Unaffil. | Minority | Dem. | Rep. | Unaffil. |
| 2012 | 0.66 | 0.25 | 1.09 | 0.58 | 0.60 | 0.59 | 0.82 | 0.71 | 0.60 | 0.63 |
| 2014 | 1.31 | 0.49 | 2.16 | 1.17 | 1.20 | 1.19 | 1.65 | 1.42 | 1.21 | 1.26 |
| 2016 | 1.78 | 0.61 | 3.06 | 1.59 | 1.61 | 1.61 | 2.25 | 1.94 | 1.62 | 1.71 |
| 2018 | 2.21 | 0.63 | 3.90 | 2.00 | 1.98 | 2.01 | 2.79 | 2.42 | 1.99 | 2.14 |
| 2020 | 2.68 | 0.86 | 4.83 | 2.44 | 2.38 | 2.44 | 3.38 | 2.93 | 2.40 | 2.60 |

The table is analogous to Table 13. However, results are for the alternative version of the simulation. This version uses a single causal effect per legislative chamber; i.e., $\hat{\alpha}^j_{m_i} = \hat{\alpha}^j$. The estimates for these effects are presented in the "All" column of Table 12.

Table A27: The change in aggregate turnout under competitive districts: results calculated under common causal effects

| **Election** | **Actual turnout** | **Change in turnout** | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | **All** | **By group** | | | | **By party** | | |
| | | | White-Dem. | White-Rep. | White-Unaffil. | Minority | Dem. | Rep. | Unaffil. |
| 2012 | 3,649,120 | 37,199 | 7,386 | 10,250 | 6,703 | 12,860 | 17,452 | 10,897 | 8,850 |
| 2014 | 2,445,870 | 74,372 | 14,770 | 20,496 | 13,404 | 25,702 | 34,888 | 21,790 | 17,694 |
| 2016 | 3,449,519 | 100,913 | 20,081 | 27,538 | 18,182 | 35,112 | 47,564 | 29,290 | 24,060 |
| 2018 | 2,765,452 | 125,458 | 25,335 | 33,839 | 22,750 | 43,534 | 59,332 | 36,001 | 30,125 |
| 2020 | 3,467,293 | 151,935 | 30,840 | 40,778 | 27,584 | 52,733 | 71,947 | 43,381 | 36,608 |

The table is analogous to Table 14. However, results are for the alternative version of the simulation.

Table A28: The change in aggregate votes under competitive districts: results calculated under common causal effects

| **Election** | **Predicted votes** | | **Change in votes** | | **Net change for Dem.** |
|---|---|---|---|---|---|
| | Democrats | Republicans | Democrats | Republicans | |
| 2012 | 1,685,578 | 1,819,760 | 18,610 | 16,877 | 1,733 |
| 2014 | 1,137,003 | 1,165,079 | 38,016 | 29,712 | 8,304 |
| 2016 | 1,572,427 | 1,711,289 | 50,669 | 44,555 | 6,114 |
| 2018 | 1,347,548 | 1,315,227 | 72,681 | 49,138 | 23,543 |
| 2020 | 1,642,948 | 1,731,547 | 79,574 | 66,088 | 13,485 |

The table is analogous to Table 15. However, results are for the alternative version of the simulation.

Table A29: The partisan composition of the increase in votes: results calculated under common causal effects

| **Election** | **By party** | | | | | | **By group** | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Dem. | | Rep. | | Unaffil. | | White-Dem. | | White-Rep. | | White-Una. | | Minority | |
| | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. | Dem. | Rep. |
| 2012 | 74 | 21 | 14 | 82 | 46 | 47 | 49 | 46 | 12 | 85 | 35 | 59 | 89 | 7 |
| 2014 | 76 | 17 | 11 | 78 | 52 | 37 | 54 | 35 | 10 | 80 | 43 | 45 | 87 | 8 |
| 2016 | 74 | 22 | 14 | 80 | 46 | 45 | 50 | 45 | 12 | 82 | 36 | 56 | 87 | 8 |
| 2018 | 82 | 15 | 15 | 81 | 61 | 36 | 65 | 32 | 13 | 83 | 52 | 45 | 91 | 6 |
| 2020 | 78 | 20 | 13 | 82 | 49 | 44 | 55 | 42 | 11 | 85 | 40 | 54 | 89 | 7 |

The table is analogous to Table 16. However, results are for the alternative version of the simulation.

## A2 Re-writing the exclusion restriction

In this appendix, we prove the claim from Section 4.2 about the exclusion restriction in the IV model, equation (5). Specifically, we show that the restriction is equivalent to equation (6).

To do this, we first remove the fixed effects from the IV model by de-meaning the variables. We get:

$$\begin{aligned} \mathrm{to}_{i\tau} - \mathrm{E}[\mathrm{to}_{i\tau}|g_i,\tau] &= \alpha \cdot (\mathrm{C}_{i\tau} - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau]) + e_{i\tau} \\ \mathrm{C}_{i\tau} - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau] &= \beta_\tau \cdot (\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]) + u_{i\tau}. \end{aligned}$$

From this formulation, we see that the exclusion restriction can be written as:

$$\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]) \cdot e_{i\tau}|\tau] = 0.$$

Finally, we notice that there is a simple relationship between $e_{i\tau}$ and $\varepsilon_{i\tau}$. Namely,

$$e_{i\tau} = \varepsilon_{i\tau} - \mathrm{E}[\varepsilon_{i\tau}|g_i,\tau].$$

Thus, after substituting for $e_{i\tau}$, we obtain our result:

$$\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]) \cdot (\varepsilon_{i\tau} - \mathrm{E}[\varepsilon_{i\tau}|g_i,\tau])|\tau] = 0.$$

## A3 The first stage in the IV model

In this appendix, we discuss the first stage in the IV model, equation (5). We start by characterizing the structure of the first-stage coefficient, $\beta_\tau$. We then derive the claims about the first stage that are stated in Section 4.2. Finally, we investigate the first stage empirically.

### A3.1 Characterizing the first-stage coefficient, $\beta_\tau$

$\beta_\tau$ is the coefficient from the first-stage regression:

$$\mathrm{C}_{i\tau} = \beta_\tau \cdot \mathrm{C}_{a_i\tau} + \lambda_{g_i\tau} + u_{i\tau}.$$

In order to better understand $\beta_\tau$, we manipulate it as follows. First, we eliminate the fixed effects in the first-stage regression by de-meaning the variables. We get:

$$\mathrm{C}_{i\tau} - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau] = \beta_\tau \cdot (\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]) + u_{i\tau}.$$

Next, we use the formula for an OLS coefficient to obtain a formula for $\beta_\tau$. We get:

$$\beta_\tau = \frac{\mathrm{E}[(\mathrm{C}_{i\tau} - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau]) \cdot (\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau])|\tau]}{\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau])^2|\tau]}.$$

Finally, using the law of iterated expectations, we adjust the formula for $\beta_\tau$ as:

$$\begin{aligned} \beta_\tau &= \frac{\mathrm{E}[\mathrm{E}\{(\mathrm{C}_{i\tau} - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau]) \cdot (\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau])|g_i,\tau,\mathrm{C}_{a_i\tau}\}|\tau]}{\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau])^2|\tau]} \\ &= \frac{\mathrm{E}[(\mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau,\mathrm{C}_{a_i\tau}] - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau]) \cdot (\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau])|\tau]}{\mathrm{E}[(\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau])^2|\tau]}. \end{aligned} \tag{10}$$

This expression reveals that $\beta_\tau$ is a ratio with a denominator that depends only on the instrument, $\mathrm{C}_{a_i\tau}$, and numerator that depends partially on the treatment variable, $\mathrm{C}_{i\tau}$. It also indicates that we can assess the magnitude of $\beta_\tau$ by comparing $\mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau,\mathrm{C}_{a_i\tau}] - \mathrm{E}[\mathrm{C}_{i\tau}|g_i,\tau]$ with $\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i,\tau]$.

### A3.2 Deriving the claims from Section 4.2

We now derive the claims from Section 4.2.

To do this, we make two simplifying assumptions. First, we assume that, within a match-group, a registrant's value of the instrument does not affect the registrant's probability of being registered in her assigned district, $a_i$. Formally, let $s_{i\tau}$ be an indicator for whether registrant $i$ is still registered in $a_i$ in election $\tau$. We assume that this variable is mean-independent of $\mathrm{C}_{a_i\tau}$, conditional on match-groups and $\tau$. I.e.,

$$\Pr[s_{i\tau} = 1 | g_i, \tau, \mathrm{C}_{a_i\tau}] = \Pr[s_{i\tau} = 1 | g_i, \tau]. \tag{11}$$

Second, we assume that when registrants register in a new district, the competitiveness of their new district is also mean-independent of $\mathrm{C}_{a_i\tau}$, conditional on $g_i$ and $\tau$. I.e.,

$$\mathrm{E}[\mathrm{c}_{i\tau} | g_i, \tau, \mathrm{C}_{a_i\tau}, s_{i\tau} = 0] = \mathrm{E}[\mathrm{c}_{i\tau} | g_i, \tau, s_{i\tau} = 0]. \tag{12}$$

These assumptions are not necessary for the IV model to be valid. However, they are likely close to true, and they permit an easier analysis of the magnitude of $\beta_\tau$.[48]

The first claim in Section 4.2 concerns the case where no registrants have moved since the baseline election and where the assigned districts are still in use in $\tau$. In this case, $\mathrm{C}_{i\tau} = \mathrm{C}_{a_i\tau}$. Thus, $\beta_\tau = 1$, as claimed.

The second and third claims deal with the case where the assigned districts are still in use but where some registrants have moved. To show the claims, we derive a formula for $\beta_\tau$ in this case. We start by deriving the formulas for $\tau = 0$ and $\tau = 1$. We then provide the general formula.

When $\tau = 0$, we have: $\mathrm{E}[\mathrm{C}_{i0} | g_i, \mathrm{C}_{a_i0}] - \mathrm{E}[\mathrm{C}_{i0} | g_i]$

$$\begin{aligned}
&= \mathrm{E}[\mathrm{c}_{i0} | g_i, \mathrm{C}_{a_i0}] - \mathrm{E}[\mathrm{c}_{i0} | g_i] \\
&= \mathrm{E}[\mathrm{c}_{i0} | g_i, \mathrm{C}_{a_i0}, s_{i0} = 1] \cdot \Pr[s_{i0} = 1 | g_i, \mathrm{C}_{a_i0}] - \mathrm{E}[\mathrm{c}_{i0} | g_i, s_{i0} = 1] \cdot \Pr[s_{i0} = 1 | g_i] \\
&+ \mathrm{E}[\mathrm{c}_{i0} | g_i, \mathrm{C}_{a_i0}, s_{i0} = 0] \cdot \Pr[s_{i0} = 0 | g_i, \mathrm{C}_{a_i0}] - \mathrm{E}[\mathrm{c}_{i0} | g_i, s_{i0} = 0] \cdot \Pr[s_{i0} = 0 | g_i] \\
&= (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i]) \cdot \Pr[s_{i0} = 1 | g_i].
\end{aligned}$$

Here, the first equality is due to the definition of $\mathrm{C}_{i0}$, the second equality uses the law of total expectation, and the last equality is due to assumptions (11) and (12). Also, we have:

$$\mathrm{C}_{a_i0} - \mathrm{E}[\mathrm{C}_{a_i0} | g_i] = \mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i].$$

Substituting the resulting quantities into equation (10), we get:

$$\beta_0 = \frac{\mathrm{E}[(\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i])^2 \cdot \Pr[s_{i0} = 1 | g_i]]}{\mathrm{E}[(\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i])^2]}.$$

When $\tau = 1$, we have: $\mathrm{E}[\mathrm{C}_{i1} | g_i, \mathrm{C}_{a_i1}] - \mathrm{E}[\mathrm{C}_{i1} | g_i]$

$$\begin{aligned}
&= \mathrm{E}[\mathrm{c}_{i0} | g_i, \mathrm{C}_{a_i1}] - \mathrm{E}[\mathrm{c}_{i0} | g_i] + \mathrm{E}[\mathrm{c}_{i1} | g_i, \mathrm{C}_{a_i1}] - \mathrm{E}[\mathrm{c}_{i1} | g_i] \\
&= (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i]) \cdot \Pr[s_{i0} = 1 | g_i] + (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i]) \cdot \Pr[s_{i1} = 1 | g_i] \\
&= (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i} | g_i]) \cdot \sum_{h=0}^{1} \Pr[s_{ih} = 1 | g_i].
\end{aligned}$$

48. For instance, in Table A33, we show that a registrant's value of the instrument does not affect the registrant's probability of being registered in her assigned district, after controlling for match-group-by-$\tau$ fixed effects. This reveals that $\Pr[s_{i\tau} = 1 | g_i, \tau, \mathrm{C}_{a_i\tau}] - \Pr[s_{i\tau} = 1 | g_i, \tau]$ is uncorrelated with $\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau} | g_i, \tau]$. However, it doesn't fully prove assumption (11), as this assumption requires that $\Pr[s_{i\tau} = 1 | g_i, \tau, \mathrm{C}_{a_i\tau}] - \Pr[s_{i\tau} = 1 | g_i, \tau]$ is always zero.

Also, we know $\mathrm{C}_{a_i 1} - \mathrm{E}[\mathrm{C}_{a_i 1}|g_i] = (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i]) \cdot 2$. Again substituting into equation (10), we get:

$$\beta_1 = \frac{\mathrm{E}[(\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i])^2 \cdot \frac{1}{2} \cdot \sum_{h=0}^{1} \Pr[s_{ih} = 1|g_i]]}{\mathrm{E}[(\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i])^2]}.$$

In general, for $\tau \leq \tau_l$,

$$\begin{aligned} \mathrm{E}[\mathrm{C}_{i\tau}|g_i, \tau, \mathrm{C}_{a_i\tau}] - \mathrm{E}[\mathrm{C}_{i\tau}|g_i, \tau] &= (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i]) \cdot \sum_{h=0}^{\tau} \Pr[s_{ih} = 1|g_i], \\ \mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i, \tau] &= (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i]) \cdot (\tau + 1), \\ \text{and} \qquad \beta_\tau &= \frac{\mathrm{E}[(\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i])^2 \cdot \frac{1}{\tau+1} \cdot \sum_{h=0}^{\tau} \Pr[s_{ih} = 1|g_i]|\tau]}{\mathrm{E}[(\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i])^2|\tau]}. \end{aligned}$$

By assumption, $\Pr[s_{i\tau} = 1|g_i, \tau]$ is less than 1. Thus, we have now shown that $\beta_\tau$ is less than 1, as claimed. Further, if $\Pr[s_{i\tau} = 1|g_i, \tau]$ is decreasing in $\tau$, then $\beta_\tau$ will decline in $\tau$, as claimed.

The last claim is about an election that occurs after a subsequent redistricting episode (i.e., $\tau > \tau_l$). In this case, $\Pr[s_{ih} = 1|g_i] = 0$ for all $h \in \{\tau_l + 1, \ldots, \tau\}$.[49] Thus, we have:

$$\mathrm{E}[\mathrm{C}_{i\tau}|g_i, \tau, \mathrm{C}_{a_i\tau}] - \mathrm{E}[\mathrm{C}_{i\tau}|g_i, \tau] = (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i]) \cdot \sum_{h=0}^{\tau_l} \Pr[s_{ih} = 1|g_i].$$

Also,

$$\mathrm{C}_{a_i\tau} - \mathrm{E}[\mathrm{C}_{a_i\tau}|g_i, \tau] = (\mathrm{c}_{a_i} - \mathrm{E}[\mathrm{c}_{a_i}|g_i]) \cdot (\tau_l + 1).$$

Thus, $\beta_\tau = \beta_{\tau_l}$, as claimed.

In practice, the mean-independence assumptions, (11) and (12), may not hold exactly. In addition, when we calculate the first stage, we average over multiple redistricting episodes. Some of the episodes do not have data in all relative elections. Moreover, the episodes differ in the relative election in which a subsequent redistricting episode occurs. As a result of these complications, the first-stage coefficients may diverge from the patterns we just discussed. We explore these issues in the next subsection by examining $\beta_\tau$ empirically.

## A3.3 Results for the first stage

Results for the first stage are presented in Table A30. The table presents coefficient estimates, $\hat{\beta}_\tau$, and standard errors for $\tau$-specific regressions of the treatment variable, $\mathrm{C}_{i\tau}$, on the instrument, $\mathrm{C}_{a_i\tau}$, and match-group fixed effects.

The results in the table align with some, but not all, of our predictions. Namely, the coefficient estimates are all less than 1. However, they do not decline over time and instead are relatively constant. As mentioned, there are a variety of potential explanations for why the coefficient estimates may diverge from our predictions. These include failures of the mean-independence assumptions, changes in the sample, or issues related to the timing of subsequent redistricting episodes.

To understand the role of each explanation, we estimate the first stage separately by redistricting episode. This allows us to hold the sample constant and to eliminate differences in the

49. This is because the district ceased existing in $\tau_l + 1$.

Table A30: The first stage from the IV model

| | Election relative to redistricting, $\tau$ | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$ | 0.872*** | 0.889*** | 0.811*** | 0.866*** | 0.888*** |
| | (0.005) | (0.016) | (0.009) | (0.019) | (0.024) |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results from the first stage regression in the IV model, equation (5). Specifically, it presents coefficient estimates, $\hat{\beta}_\tau$, and standard errors from $\tau$-specific regressions of $C_{i\tau}$ on $C_{a_i\tau}$ and match-group fixed effects. The columns list the results for the specified election. All other details are the same as in Table 2.

timing of subsequent episodes. The results are presented in Table A31. For convenience, the table provides results only for the decennial redistricting episodes; we find that results for the other episodes would tell a similar story.

The results in Table A31 mostly align with our predictions. Coefficient estimates are all less than 1; in addition, they now decline in elections in which an episode's districts are still in use. The only conflict with our predictions is that values are slightly higher in elections that occur after a subsequent redistricting episode than in the last election with the original districts.[50] That is, individuals who are assigned to more competitive districts in an initial redistricting episode experience slightly more competitive districts after a subsequent one. An explanation is that the

Table A31: The first stage for the decennial redistricting episodes

| | Election relative to redistricting, $\tau$ | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| *Panel A: U.S. House* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$ | 0.874*** | 0.853*** | 0.863*** | 0.871*** | 0.872*** |
| | (0.008) | (0.009) | (0.011) | (0.016) | (0.016) |
| Clusters | 113 | 113 | 113 | 113 | 113 |
| Registrants | 1,049,735 | 1,049,735 | 1,049,735 | 1,049,735 | 1,049,735 |
| *Panel B: NC Senate* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$ | 0.848*** | 0.824*** | 0.793*** | 0.804*** | 0.810*** |
| | (0.010) | (0.010) | (0.010) | (0.018) | (0.029) |
| Clusters | 138 | 138 | 138 | 138 | 138 |
| Registrants | 1,194,384 | 1,194,384 | 1,194,384 | 1,194,384 | 1,194,384 |
| *Panel C: NC House* | | | | | |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$ | 0.869*** | 0.845*** | 0.813*** | 0.897*** | 0.930*** |
| | (0.010) | (0.011) | (0.012) | (0.026) | (0.032) |
| Clusters | 214 | 214 | 214 | 214 | 214 |
| Registrants | 2,897,904 | 2,897,904 | 2,897,904 | 2,897,904 | 2,897,904 |

The table presents results analogous to those in Table A30, but for only the decennial redistricting episodes. Recall that the decennial districts were used for the U.S. House in $\tau = 0$ and $\tau = 1$. For the NC Senate and NC House, they were used in $\tau = 0, \dots, 2$.

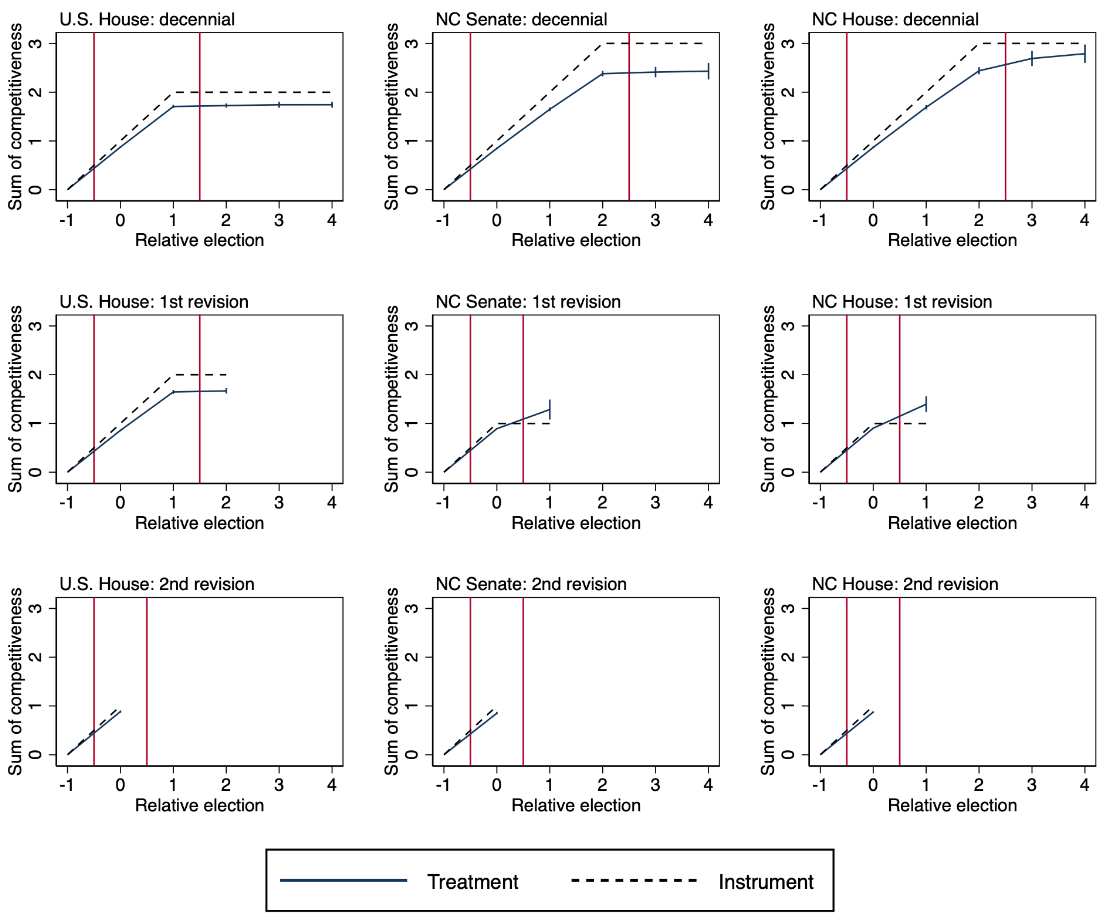
districts drawn in subsequent episodes may maintain some features of the original districts. This is not an issue for our empirical strategy; however, it is a violation of the second mean-independence

50. For example, for the NC House, the coefficient estimates in $\tau = 3$ and $\tau = 4$ are larger than that in $\tau = 2$. The same holds for the NC Senate, though to a modest degree. Finally, for the U.S. House, the values in $\tau = 2, \dots, 4$ are larger than that in $\tau = 1$, though again to only a modest degree.

assumption, equation (12). Thus, the results in Table A31 indicate that there are a few reasons why the values in Table A30 diverge from our predictions. One set of explanations relates to the fact that, in Table A30, we are averaging over multiple redistricting episodes. Another explanation is that policymakers pay attention to existing districts when they draw new ones.

We conclude this subsection by providing a graphical illustration of the first stage. In particular, we break up the first stage into its components, we plot these components, and we explain how they aggregate into our main first-stage coefficients. We believe that doing this provides intuition into both the structure of the first stage and the workings of our natural experiment.

Figure A2: A graphical illustration of the first stage




The figure presents a graphical illustration of the first stage. In the figure, each plot is a different redistricting episode. In the plots, the left red line designates the specified episode, and the right red line designates the next episode. Thus, the districts for the specified episode are in use during the elections between the two red lines. The solid blue line ("Treatment") displays coefficient estimates for the coefficients on $c_{a_i}$ in $\tau$-specific regressions of $C_{i\tau}$ on $c_{a_i}$ and match-group fixed effects. The dashed black line ("Instrument") reveals corresponding coefficient estimates from regressions of $C_{a_i\tau}$ on $c_{a_i}$ and match-group fixed effects. The vertical bars for the Treatment line represent 95% confidence intervals for the coefficient estimates. There are no vertical bars for the Instrument line because $C_{a_i\tau}$ and $c_{a_i}$ are perfectly collinear. Episode-specific first-stage coefficients are equal to the ratio of the Treatment line to the Instrument line. Our main first-stage coefficients, $\beta_\tau$, are a weighted-average of these ratios across the episodes.

The results are presented in Figure A2. In the figure, each plot depicts a different redistricting episode. The plots show how—for the specified episode—being assigned to a more competitive district affects the competitiveness that a registrant experiences.

In each plot, the vertical red lines demarcate the elections during which the districts from the specified episode are in use. The solid blue line ("Treatment") is the quantity of interest. It displays coefficient estimates from $\tau$-specific regressions of $C_{i\tau}$ on a registrant's assigned competitiveness, $c_{a_i}$, controlling for match-group fixed effects. This line reveals the increase in $C_{i\tau}$ that a registrant would obtain in $\tau$, relative to others in his match-group, if he were assigned to a 50-50 district and they were assigned to a 100-0 district. As a comparison, the dashed black line ("Instrument") provides corresponding coefficient estimates from regressions that use $C_{a_i\tau}$ as the outcome variable. This line shows what the difference in $C_{i\tau}$ would be if none of the registrants moved out of their assigned districts and if all of them were placed in the same district in the subsequent redistricting episode. Finally, the ratio of the Treatment line to the Instrument line is equal to an episode-specific first-stage coefficient.

As mentioned, Figure A2 is interesting for two reasons. First, it unveils the structure of our main first-stage coefficients. For each $\tau$, $\beta_\tau$ is a weighted average, across redistricting episodes, of the ratio of the Treatment line to the Instrument line.

Second, the figure also exposes the mechanics behind the redistricting episodes. In particular, it reveals when the gaps in $C_{i\tau}$ develop and how they change after the districts are revised. For instance, for the decennial episode for the U.S. House, the gap in $C_{i\tau}$ grows for the two elections in which the districts are in use and then is stable thereafter. In other words, being assigned to a more competitive district causes a registrant to experience a higher degree of competitiveness for two elections and then has no effect on the competitiveness that the registrant experiences in the remaining elections. The mechanics of the decennial episode for the NC Senate are similar; however, for this episode, the gap in $C_{i\tau}$ grows for three elections. Meanwhile, the story for the decennial episode for the NC House is somewhat different. For this episode, the gap continues to grow (by a small amount) after the districts are revised. That is, individuals experience higher competitiveness for three elections and then also experience slightly higher competitiveness in the remaining elections, after the subsequent redistricting episode. The mechanics of the other redistricting episodes can be interpreted in a similar manner. Overall, our natural experiment is the combination of the variation in competitiveness due to each of the episodes.

## A4 Effects on the probability of moving

In this appendix, we examine whether being assigned to a more competitive legislative district affects the probability that a registrant moves and re-registers in a different location in North Carolina.

Table A32: The effect of $C_{a_i\tau}$ on whether a registrant is registered in his/her baseline county

| | **Election relative to redistricting, $\tau$** | | | | |
|---|---|---|---|---|---|
| | Zero | One | Two | Three | Four |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$ | 0.049<br>(0.223) | 0.001<br>(0.206) | -0.049<br>(0.211) | -0.131<br>(0.246) | -0.151<br>(0.307) |
| Percent still in the baseline county | 96.4 | 94.6 | 92.0 | 90.3 | 88.1 |
| Clusters | 406 | 401 | 287 | 247 | 247 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 | 3,843,970 | 3,843,970 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 | 5,142,023 | 5,142,023 |

The table presents results from equation (13). Specifically, it presents coefficient estimates, $\hat{\theta}_\tau$, and standard errors from $\tau$-specific regressions of $s_{i\tau}$ on $C_{a_i\tau}$ and match-group fixed effects. The outcome variable, $s_{i\tau}$, is an indicator for whether the registrant is still registered in his or her baseline county in election $\tau$. Coefficient estimates and standard errors are denominated in percentage points. "Percent still in the baseline county" is the percent of baseline registrants who are still registered in their baseline county in the listed election. It is the mean of the outcome variable in the election. Standard errors are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum of these two sets of clusters.

This question is interesting for two reasons. First, it lends insight into the mechanisms driving the first-stage coefficients in the IV model, equation (5). Second, it provides evidence on whether being assigned to a more competitive district affects the probability of moving and re-registering in a different state. The latter question is important because we do not observe registration and turnout in states other than North Carolina. Instead, our outcome variables capture only whether individuals registered in the baseline election turn out in North Carolina. If competitiveness influences the probability of moving to a different state, then our causal effects may be biased by differential attrition.

Table A33: The effect of $C_{a_i\tau}$ on whether a registrant is registered in his/her assigned district

| | **Election rel. to redistricting, $\tau$** | | |
|---|---|---|---|
| | Zero | One | Two |
| Sum of competitiveness in a registrant's assigned district, $C_{a_i\tau}$ | 0.304<br>(0.638) | 0.427<br>(0.547) | 0.273<br>(0.467) |
| Percent still in the assigned district | 92.3 | 70.6 | 62.4 |
| Clusters | 406 | 401 | 287 |
| Registrants | 5,204,602 | 4,486,355 | 3,932,662 |
| Registrant-episodes | 8,773,190 | 6,887,028 | 5,449,428 |

The table presents results similar to those in Table A32, but for a different outcome variable. The outcome is an indicator for whether the registrant is still registered in his or her assigned district in election $\tau$. This variable is set to 0 if the assigned district no longer exists in $\tau$. Results are omitted for relative elections $\tau = 3$ and $\tau = 4$. This is because we know $\theta_\tau = 0$ in these elections. Namely, all districts are modified by subsequent redistricting episodes prior to $\tau = 3$; thus, $s_{i\tau} = 0$ for all registrants in elections $\tau \geq 3$. Coefficient estimates and standard errors are denominated in percentage points. "Percent still in the assigned district" is the percent of baseline registrants who are still registered in their assigned district in the listed election. It is the mean of the outcome variable in the election. See the notes to Table A32 for details on clustering.

In the analysis, we consider two variables related to moving. The first variable is an indicator for whether, in election $\tau$, a registrant is still registered in his or her baseline county. The second variable is an indicator for whether the registrant is still registered in his or her assigned district,

$a_i$. To be consistent with Appendix A3, the second variable is set to 0 if the assigned district is no longer in use in election $\tau$.

We recover effects by running $\tau$-specific regressions of each variable on the instrument, $\mathrm{C}_{a_i\tau}$, and on match-group fixed effects. Specifically, for each $\tau$, we fit the model:

$$s_{i\tau} = \theta_\tau \cdot \mathrm{C}_{a_i\tau} + \theta_{g_i\tau} + \theta_{i\tau}. \tag{13}$$

In this equation, $s_{i\tau}$ is one of the variables related to moving, and $\theta_\tau$ is the coefficient of interest.

The results from the regressions are presented in Tables A32 and A33. Table A32 provides results for whether a registrant is still registered in his or her baseline county; Table A33 is for whether the registrant is still registered in his or her assigned district.

The results reveal that being assigned to a more competitive district has no effect on the probability of moving. In all cases, the coefficient estimate $\hat{\theta}_\tau$ is small and statistically insignificant. This implies that the first stage is not impacted by differences in the probability of moving out of the assigned district. In addition, it is suggestive evidence that there are no differences in the probability of moving to a different state.

# A5 Comparison with Moskowitz and Schneer (2019)

In this appendix, we compare our paper with Moskowitz and Schneer (2019). We first summarize Moskowitz and Schneer (2019). We then compare the settings and empirical strategies of the two papers. Finally, we assess the degree of similarity in the results.

## A5.1 Summarizing Moskowitz and Schneer (2019)

Moskowitz and Schneer study the turnout effects of being assigned to a more competitive U.S. House district as part of the decennial redistricting episode. They use data on registration and turnout during the 2008 to 2014 elections from the data vendor Catalist. Their data includes over 2 million individuals from all 50 states.

Moskowitz and Schneer use a variety of empirical strategies to assess the effect of competitiveness. One of these is a matching strategy that is similar to the one that we employ in this paper. When Moskowitz and Schneer use this strategy, they find that being assigned to a more competitive district has a statistically significant effect on the probability of turning out. However, they interpret the magnitude of the effect as "substantively near zero".

## A5.2 Comparing the settings and empirical strategies

The setting of our paper differs from that of Moskowitz and Schneer (2019) in important ways. First, Moskowitz and Schneer study all 50 states, while we focus on North Carolina. Second, Moskowitz and Schneer observe outcomes only in the 2012 and 2014 elections; by contrast, we see them through 2020. Finally, Moskowitz and Schneer study the effects of competitiveness only with respect to U.S. House districts. In comparison, we consider the effects of both U.S. House districts and state legislative districts.

The empirical strategies used in the two papers are broadly similar. Like us, Moskowitz and Schneer match registrants based on demographics, pre-redistricting turnout, and pre-redistricting residential location. Also like us, they then examine whether registrants who are assigned to more competitive districts turn out more in post-redistricting elections.

Nonetheless, there are a few technical differences between our approach and theirs. First, in creating match-groups, we match on pre-redistricting districts for both the U.S. House and the state legislative chambers. In addition, we match on assigned post-redistricting districts for chambers other than the chamber of interest. Meanwhile, Moskowitz and Schneer match only on pre-redistricting U.S. House districts. Second, we use more competitiveness measures than Moskowitz and Schneer. They use only two measures, one based on the Cook PVI and one based on the ex-post closeness of district races. Third, Moskowitz and Schneer scale their measures differently from ours. They define the Cook competitiveness measure as $-100 \cdot |\mathrm{PVI}_d|$. Similarly, they define the race closeness measure as $-100$ times the absolute two-party win margin.[51] Fourth, Moskowitz and Schneer do not instrument for actual competitiveness using assigned competitiveness. Instead, they restrict the sample to individuals who stay in their assigned districts during the entire analysis period. This way, their first-stage coefficients are forced to equal to 1. Finally, Moskowitz and Schneer make a different functional form assumption with respect to the relationship between competitiveness and turnout. They do not model turnout as depending on the sum of competitiveness since redistricting, $\mathrm{C}_{a_i\tau}$. Rather, they calculate the average effect of the level of assigned competitiveness, $\mathrm{c}_{a_i}$, across both the 2012 and 2014 elections.[52]

51. The absolute two-party *win* margin is half of the absolute two-party *vote* margin.

52. When Moskowitz and Schneer rely on race closeness as their measure of competitiveness, they relate turnout to closeness in the same election.

### A5.3 Comparing the results

We now compare our results with those of Moskowitz and Schneer (2019). In order to understand the source of any differences in results, we conduct the comparison in three steps.

In the first step, we implement the Moskowitz and Schneer approach in our North Carolina setting, using the same redistricting episode and analysis period as in their paper. Specifically, we use their approach to calculate the effect of competitiveness for U.S. House districts in North Carolina in the 2012 and 2014 elections. We then compare these results with the effects reported in their paper. This first step reveals the impact of the fact that we study just North Carolina, rather than all states.

In the second step, we implement *our* approach on the same sample as in the first step. Specifically, we use our approach to calculate the effect of competitiveness for U.S. House districts in North Carolina in the 2012 and 2014 elections. We then compare these results with the effects based on the approach of Moskowitz and Schneer. This comparison illuminates the impact of differences in the empirical strategy.

In the third step, we implement our approach on our full U.S. House sample. Specifically, we use our approach to calculate the effect of competitiveness for U.S. House districts in North Carolina in all available elections and redistricting episodes. We then compare these results with those computed using just the decennial episode and the 2012 and 2014 elections. This last step shows the influence of using a longer analysis period.

Finally, in all steps, we use the same competitiveness measures as Moskowitz and Schneer. (In some cases, we scale them according to Moskowitz and Schneer's convention; in other cases, we use our scaling.) We do not believe that differences in competitiveness measures are a source of differences in results. This is because, in the main text, we found that effects vary only slightly across measures.

Table A34: Comparing effects in all states and in North Carolina, using the approach of Moskowitz and Schneer (2019)

| | **All states** | | **North Carolina** | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| Cook competitiveness | 0.051*** | | 0.071*** | |
| | (0.012) | | (0.027) | |
| Race closeness | | 0.015 | | 0.025** |
| | | (0.018) | | (0.012) |
| Turnout percentage | - | - | 53.7 | 53.7 |
| Clusters | - | - | 113 | 113 |
| Registrants | - | - | 953,135 | 953,135 |
| Registrant-elections | 2,707,914 | 2,676,278 | 1,906,270 | 1,906,270 |

The table displays results from the first step in the analysis. Specifically, it shows coefficient estimates and standard errors for the coefficient on assigned competitiveness, $c_{a_i}$, in regressions of turnout on $c_{a_i}$ and match-group-by-$\tau$ fixed effects. The results in the "All states" columns are taken from Table 3 in Moskowitz and Schneer (2019). The results in the "North Carolina" columns are obtained by implementing the Moskowitz and Schneer empirical strategy on our data. The sample uses only the decennial redistricting episode for the U.S. House. For outcome data, it uses only the 2012 and 2014 elections. In addition, the sample is limited to registrants who remain registered in their baseline U.S. House district during 2012 and 2014. The model pools observations in the 2012 and 2014 elections and estimates the average effect of $c_{a_i}$ on turnout in these elections. Competitiveness measures are scaled according to the Moskowitz and Schneer convention. "Cook competitiveness" is the Cook PVI-based measure of the competitiveness of the registrant's assigned district. "Race closeness" is the closeness of the district's race in the election in which turnout is measured. Coefficient estimates and standard errors are denominated in percentage points. Standard errors in the "North Carolina" columns are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum of these two sets of clusters.

The results from the first step are presented in Table A34. They reveal that effects in the North

Carolina setting are similar to those calculated using all 50 states. In particular, using a 50-state sample, Moskowitz and Schneer report that a one point increase in their Cook competitiveness measure causes an average increase in turnout in 2012 and 2014 of 0.051 percentage points. In North Carolina, we find that the effect is 0.071 percentage points. For race closeness, Moskowitz and Schneer report a coefficient estimate of 0.015; we find an impact of 0.025.

Table A35: The impact of differences in the empirical strategy

| | **M&S approach** | | **Our approach** | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| Cook competitiveness | 2.35*** | | 2.21*** | |
| | (0.887) | | (0.775) | |
| Race closeness | | 0.831** | | 1.19*** |
| | | (0.405) | | (0.408) |
| Turnout percentage | 53.7 | 53.7 | 53.7 | 53.7 |
| Clusters | 113 | 113 | 113 | 113 |
| Registrants | 953,135 | 953,135 | 953,135 | 953,135 |
| Registrant-elections | 1,906,270 | 1,906,270 | 1,906,270 | 1,906,270 |

The table displays results from the second step in the analysis. The values in the "M&S approach" columns are isomorphic to those in the "North Carolina" columns of Table A34. However, the coefficient estimates and standard errors are transformed so as to correspond with our convention for scaling the competitiveness measures. As part of the transformation, we multiply the coefficient estimates and standard errors by 2/3. This way, they represent the effect of one election worth of exposure to competitiveness, rather than the average of the effects of one and two elections worth of exposure, as in Table A34. The values in the "Our approach" columns are obtained by implementing our empirical strategy on the same sample as in the "M&S approach" columns. Specifically, they are coefficient estimates and standard errors for the coefficient on $C_{a_i\tau}$ in a regression of turnout on $C_{a_i\tau}$ and match-group-by-$\tau$ fixed effects. Since the sample is restricted to registrants who do not move, this coefficient is equivalent to $\alpha$ in the IV model, equation (5). See the notes to Table A34 for details on the sample and on clustering of standard errors.

Table A35 provides the results for the second step of the analysis. It suggests that differences in the empirical strategy also have little impact on estimates of causal effects. The first two columns in the table list effects calculated using the Moskowitz and Schneer approach. (The values in these columns are the same as those in the "North Carolina" columns of Table A34. However, they are transformed in accordance with our scaling of competitiveness measures.) The last two columns present effects calculated using our approach on the same 2012-2014 sample. The table shows that the coefficient estimates are similar across the two sets of columns.

Table A36: The impact of differences in the analysis period

| | **2012 & 2014** | | **All elections & episodes** | |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| Sum of competitiveness in | 2.21*** | | 2.47*** | |
| a registrant's districts: Cook measure | (0.775) | | (0.811) | |
| Sum of race closeness in | | 1.19*** | | 1.25*** |
| a registrant's districts | | (0.408) | | (0.393) |
| Turnout percentage | 53.7 | 53.7 | 58.4 | 58.4 |
| Clusters | 113 | 113 | 176 | 176 |
| Registrants | 953,135 | 953,135 | 1,588,992 | 1,588,992 |
| Registrant-episode-elections | 1,906,270 | 1,906,270 | 6,516,888 | 6,516,888 |

The table displays results from the third step in the analysis. The values in the "2012 and 2014" columns are the same as those in the "Our approach" columns of Table A35. The values in the "All elections and episodes" columns use all the data that we have available for estimating the effect of the competitiveness of U.S. House districts. The sample for these columns is not restricted to registrants who do not move. Thus, the values in the columns are obtained by implementing the IV model, equation (5). Standard errors are clustered in two ways based on a registrant's pre-redistricting districts and assigned post-redistricting districts. "Clusters" is the minimum of these two sets of clusters.

Next, Table A36 presents results for the third step of the investigation. It indicates that differences in the analysis period also do not affect estimates. In the table, all effects are calculated using our empirical strategy. The values in the first two columns are obtained using just the 2012 and 2014 elections. (They are the same as in the "Our approach" columns in Table A35.) The values in the other columns are computed using all the available elections and redistricting episodes for the U.S. House. It can be seen that the coefficient estimates for the full period are similar to—though slightly larger—than those for 2012 and 2014.

Thus, the results in this appendix imply that differences between our findings and those of Moskowitz and Schneer are not due to features of our empirical strategy or our setting. Instead, they seem to be due mainly to differences in interpretation. With only two post-redistricting elections, Moskowitz and Schneer did not realize that the turnout effects of competitiveness grow with additional elections of exposure. In addition, by studying a single legislative chamber, they did not realize that effects sum across chambers. Consequently, they concluded that competitiveness has little overall impact on turnout. By contrast, we find that effects add up and can become sizable.

Another implication of the comparison with Moskowitz and Schneer (2019) is that our findings may have considerable external validity. We have shown that when we use the Moskowitz and Schneer strategy, we replicate their results. This is despite the fact that we have data only on North Carolina, while they study all states. As such, it may be the case that effects in North Carolina are representative of those in the country as a whole. In turn, our findings may have broad relevance.