



# Context and Accountability: How the Informational and Partisan Contexts Shape Voter Behavior and Representation

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**Context and Accountability:  
How the Informational and Partisan Contexts Shape  
Voter Behavior and Representation**

A dissertation presented

by

Daniel J. Moskowitz

to

The Department of Government

in partial fulfillment of the requirements  
for the degree of  
Doctor of Philosophy  
in the subject of  
Political Science

Harvard University  
Cambridge, Massachusetts

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# Context and Accountability: How the Informational and Partisan Contexts Shape Voter Behavior and Representation

## Abstract

This dissertation examines how the informational and partisan contexts affect the behavior of voters. I study these contextual effects by leveraging variation induced by the geography of media markets and redistricting. The first essay investigates the extent to which the nationalization of the news explains the nationalization of U.S. elections. I examine local television news coverage of governors and U.S. senators and show that increased news coverage translates into greater knowledge of governors and senators and higher rates of split-ticket voting. These results imply that local news coverage attenuates the nationalization of elections even in the present polarized context. The second essay explores the role of the informational context in retrospective voting. I assess whether greater exposure to relevant local news coverage enables voters to reward or punish Senate incumbents based on the extremity of their roll-call voting. The results are consistent with the hypothesis that voters infer incumbent ideology from news coverage and utilize this information in their vote choice: in-state television provides moderate incumbent senators with an electoral boost, while extreme senators experience an electoral penalty. The third essay, coauthored with Benjamin Schneer (Harvard Kennedy School) and Bernard L. Fraga (Indiana University) considers whether individuals are more likely to vote when their party dominates election outcomes. Leveraging nationwide voter file data and the redistricting process, we present

causal evidence on this question via a longitudinal analysis of individual-level political participation. We find a measurable increase in turnout for individuals assigned to districts aligned with their partisan identities as compared to individuals in misaligned districts.

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## Introduction

In this dissertation, I explore the role that the informational and partisan contexts play in the behavior of voters. I study these contextual effects by leveraging variation in contextual features induced by the geography of media markets and redistricting. The first essay investigates the extent to which the nationalization of the news explains the nationalization of U.S. elections. The upheaval in the broader local news environment may have contributed substantially to the nationalization of U.S. elections in recent years. With nationalized elections, voters are more likely to apply their national, partisan judgment for the presidential race to down-ballot races. I hypothesize that local news coverage mitigates nationalizing electoral forces by providing voters with relevant information about down-ballot races. This information allows voters to assess candidates for these offices separately from their national, partisan assessment. I examine local television news coverage of governors and U.S. senators and show that the geography of media markets structures coverage: stations in markets that span multiple states provide significantly more coverage of governors and senators for the state in which most of their viewers reside (in-state markets). This increased news coverage translates into greater knowledge of governors and senators. Finally, I demonstrate that access to local news coverage substantially increases rates of split-ticket voting. These results imply that local news coverage attenuates the nationalization of elections even in the present polarized context.

The second essay in the dissertation explores the role of the informational context in retrospective voting. While previous research has found that voters tend to punish incumbents who exhibit extremism, the mechanism through which voters learn incumbent ideology is

mostly speculative and understudied. Again, I leverage the geography of media markets to assess whether greater exposure to relevant local news coverage enables voters to reward or punish Senate incumbents based on the extremity of their roll-call voting. The results are consistent with the hypothesis that voters infer incumbent ideology from news coverage and utilize this information in their vote choice: in-state television provides moderate incumbent senators with an electoral boost, while extreme senators experience an electoral penalty.

The third essay, coauthored with Benjamin Schneer (Harvard Kennedy School) and Bernard L. Fraga (Indiana University) considers whether individuals are more likely to vote when their party dominates election outcomes. Leveraging nationwide voter file data and the redistricting process, we present causal evidence on this question via a longitudinal analysis of individual-level political participation. Tracking turnout before and after a redistricting cycle, where the boundaries of congressional districts change, we observe what happens when registrants experience a shock to the partisan composition of their congressional district. We find a measurable increase in turnout for individuals assigned to districts aligned with their partisan identities as compared to individuals in misaligned districts. An analysis of survey data spanning the districting cycle, as well as evidence from past experimental research, point to the expressive benefits of voting for the winning party as a key mechanism.

# 1 | Local News, Information, and the Nationalization of U.S. Elections

## 1.1 Introduction

The nationalization of U.S. elections is a striking trend and represents one of the most important and fundamental changes to U.S. electoral politics in the post-World War II era. U.S. House, U.S. Senate, gubernatorial, and other state and local election outcomes have grown increasingly tied to presidential election outcomes, resulting in a decline in incumbency advantage and fewer legislators representing districts misaligned with their partisanship (Abramowitz and Webster 2016; Hopkins 2018; Jacobson 2015). This trend in the nationalization of elections has serious implications for the functioning of representative democracy in the U.S. First, electoral nationalization likely exacerbates elite polarization. Highly nationalized elections reduce the likelihood that Republican (Democratic) legislators represent relatively liberal (conservative) districts. Importantly, legislators representing ideologically mismatched districts tend to exhibit more moderate roll-call voting behavior (McCarty, Poole, and Rosenthal 2009). Second, electoral nationalization presents a troubling situation for democratic accountability. With nationalized elections, *national* forces, such as the popularity of the president and the state of the national economy, increasingly determine the electoral fate of congressional, state, and local candidates rather than *local* circumstances, such as the candidates' policy positions and the performance of officeholders. In a federal system in which the actions and responsibilities of state and local elected officials are distinct from those of federal officials, binding their electoral fate likely impedes state and local accountability.

Coinciding with this period of electoral nationalization is a period of dramatic change in the media environment in the United States. Newspapers have experienced substantial declines in circulation and advertising revenues and, in turn, have reduced their staff by nearly 40 percent from 1994 to 2014 (Pew Research Center 2016). Similarly, audiences for local late night television newscasts, typically the most watched time slot, have dropped by 31 percent over the past decade (Pew Research Center 2017). The often suspected culprit for the decline of traditional local news sources is the entrance of national cable news channels, increased access to broadband internet, the proliferation of digital news sources, and the rise of social media (Pew Research Center 2016). Does this nationalization of news help explain the nationalization of U.S. elections?

Greater exposure to local news coverage may provide voters with relevant information about candidates and officeholders, such as their performance in office and policy priorities, that allows voters to make assessments of down-ballot races separately from their judgment in the national race at the top of the ticket. On the other hand, voters with less exposure to local news coverage may be more likely to apply their partisan, national judgment to down-ballot races. In other words, exposure to local news coverage may reduce a voter's propensity to cast a straight-ticket ballot—i.e., selecting candidates from the same party for president and down-ballot offices.<sup>1</sup> Thus, I seek to investigate the degree to which local news coverage mitigates the nationalization of elections. And, further, does local news coverage *matter* in the present hyper-polarized political context?

In this paper, I examine local television news coverage to help answer the broader question of whether the nationalization of the news explains the nationalization of electoral politics. In particular, I analyze how, and to what extent, local news coverage allows voters to make judgments for down-ballot races separately from their national, partisan judgment in the presidential race. The factors that drive individuals to consume, or not consume, par-

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<sup>1</sup>I focus on split-ticket voting, as it is the individual-level phenomenon driving the trend in electoral nationalization: as voters become increasingly likely to vote for the same party across multiple offices, the aggregate consequence is a closer relationship between a party's presidential vote share and that same party's vote share for other offices.

ticular media sources are often also related to outcomes of interest. This selection problem makes studying the effect of media exposure problematic in the absence of (quasi-)random variation. I exploit the geography of television media markets as a source of quasi-random variation in exposure to local television news coverage (e.g., Stewart and Reynolds 1990). Due to exclusivity contracts between networks and television stations as well as Federal Communications Commission rules, media market boundaries generally determine the television stations that viewers can access. Importantly, the boundaries of media markets often span multiple states, which places some voters in media markets that are predominantly composed of residents from their own state (*in-state markets*) and other voters in media markets that are predominantly composed of residents from another state (*out-of-state markets*).

Television stations focus their coverage on topics relevant to their audience, so stations tend to provide more local news coverage of a state’s elected officials if a larger proportion of the media market (i.e., the station’s potential audience) resides in that state. Because there is no central repository for local newscast videos or transcripts as there are for newspapers and national newscasts, it is difficult to examine patterns in local television news coverage (Arnold 2004). However, I leverage publicly available closed captioning transcript data from the Internet Archive’s Television News Archive, and I provide strong evidence of this first-stage relationship between a state’s population share of a media market and the amount of news coverage that stations in the market devote to that state’s governor and senators. In other words, the level of coverage governors and senators receive increases as their state’s population share of a media market increases. This pattern in news coverage across media markets is the key source of variation: voters living in the same state but in separate media markets are exposed to different local news broadcasts with more or less coverage of their governor and senators, depending on the state’s population share of the market.

Exposure to relevant local news coverage has important consequences both in terms of voter knowledge and voter behavior. Using survey data from the 2012 and 2016 elections, I compare residents within the same state and election year—holding constant all of the features of the statewide electoral context for both senators and governors—and find that

residents of in-state media markets, who consequently receive greater exposure to local news coverage of statewide elected officials, demonstrate greater knowledge of their governor and senators than residents of out-of-state markets, who receive less exposure to relevant local news coverage. Importantly, however, residents of in-state markets do not demonstrate greater general or national political knowledge, which is strongly suggestive that local news coverage is the source of the state-specific knowledge advantages rather than unobserved differences between residents of in-state and out-of-state markets. Additionally, exposure to local news coverage increases the probability of casting a split president-governor ticket by about 4-5 percentage points and a split president-senator ticket by about 2-3 percentage points. Given that the overall rate of split-ticket voting during the time period under study is 8-9 percent for both senators and governors, the estimated effects are substantively large. Finally, I consider a plausible alternative explanation—television campaign advertisements—and provide strong evidence that exposure to local news coverage, not ads, primarily accounts for the observed knowledge gaps between residents of in-state markets and out-of-state markets.

Overall, the results point to the important role of local television news in providing information to voters about governors and senators, which then allows voters to make judgments in these races that are separate from their national, partisan judgment. Given the recent time period under examination, it is difficult to assess the extent to which the decline in local news audiences explains the longer trend of electoral nationalization. Nevertheless, the results are strongly suggestive of the important role that local news coverage plays in influencing the degree to which elections are nationalized, even in the present polarized context.

## 1.2 Literature & Theory

### 1.2.1 The Shifting Media Environment

The major shifts in the media environment—in particular, the rise of cable news and broadband internet—over the past 20-30 years have unsurprisingly attracted the attention of social scientists. One line of research investigates whether the increased diversity of news sources results in voters opting to consume news sources that primarily reinforce preexisting beliefs—i.e., echo chambers or the selective exposure hypothesis (e.g., Iyengar and Hahn 2009; Gentzkow and Shapiro 2011; Stroud 2011; Flaxman, Goel, and Rao 2016). These studies examine the consumption patterns of media sources, and the results vary by news medium (internet vs. television) and research design (experimental vs. observational). While the results are somewhat mixed, fears of extreme echo-chambers seem largely overblown.

Another line of research investigates whether exposure to partisan or slanted media sources, in particular Fox News, has an effect on vote choice and aggregate election outcomes (e.g., DellaVigna and Kaplan 2007; Hopkins and Ladd 2014; Martin and Yurukoglu 2017). These studies consistently find that greater exposure to Fox News increases Republican vote share.<sup>2</sup> A related strand of research considers whether partisan media (Arceneaux and Johnson 2013; Levendusky 2013a,b,c), the internet (Lelkes, Sood, and Iyengar 2017; Boxell, Gentzkow, and Shapiro 2017), and, more broadly, media fragmentation (Davis and Dunaway 2016) increase polarization. Overall the results are mixed. Arceneaux and Johnson (2013) and Levendusky (2013a,b,c) utilize experimental research designs but arrive at largely opposite conclusions as to whether partisan news is a major contributor to polarization. Similarly, while Lelkes, Sood, and Iyengar (2017) find that access to broadband internet increases affective polarization, Boxell, Gentzkow, and Shapiro (2017) conclude that the internet only has played a limited role in rising polarization. And, Davis and Dunaway (2016) find that media fragmentation is associated with greater partisan-ideological sorting,

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<sup>2</sup>Hopkins and Ladd (2014) analyze individual-level survey data and find that Fox News availability increases Republican vote intention among Republicans and Independents.

but their results are conditional on interest in news and politics.<sup>3,4</sup>

The natural resultant to the rise of these new forms of more nationalized media is the decline of traditional local sources of media. An abundance of research points to the importance of local news in informing voters and holding elected officials accountable (e.g., Arnold 2004; Oberholzer-Gee and Waldfogel 2009; Snyder and Strömberg 2010; Hayes and Lawless 2015, 2018; Song 2016). For example, Snyder and Strömberg (2010) demonstrate that members of Congress representing districts with boundaries that are more congruent with newspaper markets receive more news coverage. This coverage results in better informed citizens who are more likely to vote in House races and officeholders who work harder for their constituents. Similarly, Hayes and Lawless (2015, 2018) find a link between newspaper coverage of House races and voter engagement. In his broader study of the nationalization of politics, Hopkins (2018) finds only minimal changes over time in the degree to which newspapers and local newscasts focus their coverage on local/state politics vs. national politics. Instead, audiences have shifted away from local news sources to more nationalized sources. Hopkins also leverages the structure of TV media markets and finds that more exposure to local TV news coverage of state politics boosts turnout in gubernatorial elections; the size of the effect varies over time consistently with trends in the nationalization of politics.

### 1.2.2 Local Television News & Split-Ticket Voting

Local television news has three especially important features for the purposes of this study. First, the audience of a television station is “spatially bounded,” which results in more local coverage relative to non-geographically constrained news sources (Hopkins 2018).

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<sup>3</sup>For a more comprehensive review on the media and polarization literature, see Prior (2013).

<sup>4</sup>In addition to these studies on the relatively recent changes in media, scholars have studied the political consequences of other shifts to the media environment, usually in the early- and mid-20th century. For instance, researchers have examined the effect of newspaper entry and exit in local markets on turnout (Gentzkow, Shapiro, and Sinkinson 2011); the effect of radio on public spending (Strömberg 2004); the effect of the introduction and expansion of television on turnout (Gentzkow 2006), the incumbency advantage (Ansolabehere, Snowberg, and Snyder 2006; Prior 2006), and social capital (Putnam 2000); and the effect of cable television, with its array of entertainment channels, on the composition of the electorate (Prior 2007). These studies are a mere sampling from vast bodies of research.

As discussed in greater detail below, stations focus their coverage on topics most relevant to their audience. In this context, news coverage of a state's elected officials increases with the state's population share of the media market. Second, the habits of television viewers result in incidental exposure to and learning from local newscasts (Krugman and Hartley 1970; Boemer 1987).<sup>5</sup> In particular, rates of audience retention are high from one program to the next (Webster 2006; McDowell and Dick 2003). These inheritance effects between programs on the same channel can result in incidental exposure to local news content. Despite declines in audiences over recent years, local television news continues to have broad reach: "in an average week in the first quarter of 2017, local news reached 40% of persons 25-54," and these viewers watched on average nearly two and a half hours of local news in that week (Nielsen Company 2017).<sup>6</sup> Third, despite low trust in the media in general, when evaluating specific sources of news, voters express the most trust in and hold the most positive views about local television news. Importantly, unlike most sources of news, there are only modest differences in trust and sentiment toward local television news across partisan and ideological identities (Media Insight Project 2018; Reuters Institute 2018).

To recapitulate: (1) local television stations provide news coverage relevant to their geographically constrained audiences, (2) viewers experience incidental exposure to local TV news coverage, and (3) viewers have relatively high trust in local television news. All of these unique features suggest that local television news may play an especially important role in helping voters evaluate candidates and officeholders. With greater exposure to local television news coverage, voters have more information about the candidates in races for offices such as governor and senator. This information allows voters to assess the individual candidates and the local circumstances in these races separately from their national, partisan judgment for president. Voters with less exposure to relevant local news coverage are more likely to apply their national, partisan judgment for president to down-ballot races. In fact,

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<sup>5</sup>Downs (1957) refers to this type of information as "accidental free information."

<sup>6</sup>On the other hand, Prior (2005) finds that the advent of cable and the internet increased viewers' choices, resulting in uninterested viewers opting to watch entertainment programming rather than news programming.

partisan cues are less powerful in high-information environments (Bullock 2011; Peterson 2017). In the context of this study, greater exposure to relevant local news coverage should increase rates of split-ticket voting. Voters with less information, due to lower exposure to relevant local news coverage, should be more likely to engage in straight-party voting.

### 1.3 The Geography of Television Media Markets

One of the major impediments to making inferences about the media’s effects on electoral politics and voting behavior is that different types of voters choose to consume different sources of media. As a result, any observed differences in voting behavior could be the result of underlying differences in voter characteristics rather than the media sources that voters opt to consume.<sup>7</sup> To remedy this selection problem, scholars have a long history of utilizing clever natural experiments that exploit quasi-random variation in exposure or access to the medium of interest.<sup>8</sup> For instance, in an article published over 60 years ago, Simon and Stern (1955) exploit a Federal Communications Commission (FCC) freeze on the issuance of television licenses to examine the effect of television on turnout and vote share in the 1952 presidential election in Iowa, where television was expanding at the time. Since this seminal work, scholars have continued to utilize natural experiments to study media effects, including an 8-month driver strike that disrupted newspaper delivery in Pittsburgh prior to the 1992 election (Mondak 1995), the roll-out of cable news channels and local Spanish-language news (DellaVigna and Kaplan 2007; Oberholzer-Gee and Waldfogel 2009; Hopkins and Ladd 2014), the ordering of channels within a channel lineup (Martin and Yurukoglu 2017), and the geographic structure of media markets for radio, newspapers, and television

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<sup>7</sup>For instance, the audience of Fox News is predominantly conservative, but it is difficult to infer if Fox News causes these individuals to adopt conservative views or if people who are conservative simply choose to watch Fox News. Levendusky (2011) clearly describes the selection problem in the context of political information. Another major difficulty is the accurate measurement of media exposure/consumption. Self-reported measures of news consumption are subject to considerable measurement error (e.g., Prior 2009; Guess 2015).

<sup>8</sup>Alternative approaches to overcome the selection problem include lab or survey experiments (e.g. Arce-neaux and Johnson 2013; Levendusky 2013a), field experiments (e.g., Gerber, Karlan, and Bergan 2009; King, Schneer, and White 2017), and within-subject observational studies (e.g., Barabas and Jerit 2009).

(Stewart and Reynolds 1990; Levy and Squire 2000; Ansolabehere, Snowberg, and Snyder 2006; Schaffner 2006; Althaus and Trautman 2008; Snyder and Strömberg 2010; Song 2016; Hopkins 2018).

In this paper, I examine whether greater exposure to local television news allows voters to make judgments about governor and senator races separately from their national, partisan judgment for president. This paper is unique in its examination of the relationship between exposure to local television news coverage and ticket splitting. Given the broader focus on the nationalization of elections, split-ticket voting is the most appropriate outcome. Elections are nationalized when voters apply their national, partisan assessment for president to all other offices (i.e., straight-ticket voting). In fact, the degree to which voters engage in ticket splitting determines (mechanically) the degree to which electoral politics are nationalized. Said differently, in the complete absence of split-ticket voting (and differential roll off), election outcomes across offices are identical and perfectly nationalized.<sup>9</sup> The geography of television media markets conveniently allows for the comparison of voters participating in the same state-level electoral context with access to entirely different television stations and, thus, different local news content. The Nielsen Company sets the boundaries of media markets (designated market areas—DMAs) based on the viewing patterns of residents within each county, and these boundaries do not simply conform to state boundaries. Consequently, residents of a given state are usually divided into multiple media markets, and these markets often span state boundaries.<sup>10</sup>

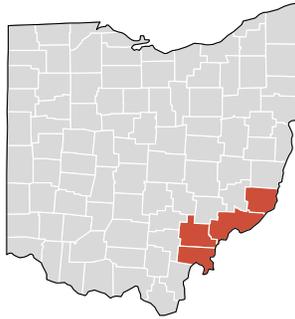
Media market boundaries largely determine the television stations to which a viewer has access. Networks grant stations exclusive content rights within media markets, and FCC non-duplication and syndicated exclusivity rules provide stations with a quick enforcement mechanism should a cable or satellite provider (or other multichannel video programming

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<sup>9</sup>Hirano and Snyder (2018) use split-ticket voting as a measure of candidate-centered elections/voting, which is essentially the opposite of nationalized elections.

<sup>10</sup>The Nielsen Company assigns counties into mutually exclusive DMAs based on residents viewing patterns. In very rare instances, Nielsen splits a county into multiple DMAs if within-county viewing patterns differ substantially in terms of geography. While market boundaries do change over time, such changes are relatively rare.

Figure 1.1: **Four Ohio Counties**



The four adjacent Ohio counties along the West Virginia border discussed are shaded in red. All four counties are located in different TV markets. From southwest to northeast: Meigs, Athens, Washington, and Monroe Counties.

distributor—MVPD) violate the exclusivity within the media market (Government Accountability Office 2015; Federal Communications Commission 2016). As a result, with very few exceptions, MVPDs are restricted from including out-of-market television stations in their channel lineups. While residents may be able to receive over-the-air signals from out-of-market stations, the vast majority of television households subscribe to a cable or satellite provider.<sup>11</sup> Prior to the rise of MVPD subscription rates (see Figure A.1), media market boundaries were likely far more porous due to over-the-air signals penetrating across media market boundaries.<sup>12</sup> In sum, in the present context, viewers residing within a given media market generally only have access to the television stations within that market.

Because media markets can span multiple states, a person may reside in a market that is predominantly comprised of residents from another state. To give a sense of how idiosyncratic these boundaries can be, in Ohio, Meigs, Athens, Washington, and Monroe Counties are adjacent to one another and all sit along the Ohio-West Virginia border (see Figure 1.1). Despite their relatively close proximity to one another, Meigs is assigned to the Charleston,

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<sup>11</sup>Based on recent data, approximately 96 percent of households are television households, and about 90 percent of television households utilize an MVPD (predominantly cable and satellite companies) for broadcast service (Federal Communications Commission 2017b).

<sup>12</sup>Nielsen has also improved the technology used to record audience data relying far less on diaries; see <http://en-us.nielsen.com/sitelets/cls/documents/nielsen/Local-Measurement-Methodology-Ex.pdf>. More accurate measurement of viewing patterns should also result in more appropriate DMA boundaries.

WV market (21% of this market resides in Ohio), Athens to the Columbus, OH market (100% of this market resides in Ohio), Washington to the Parkersburg, WV market (39% of this market resides in Ohio), and Monroe to the Bluefield, WV market (53% of this market resides in Ohio).

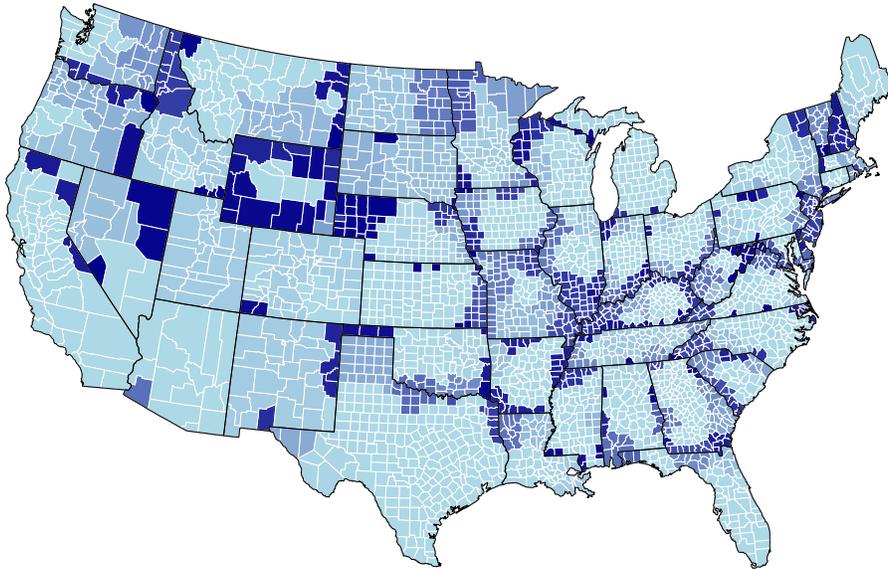
The degree to which a person's media market is comprised of residents from that person's state is the key geographic feature of media markets that induces variation in exposure to local news content. A person lives in an out-of-state market if most of the residents of that media market are from another state, while a person lives in an in-state market if most of the residents of that market are from the same state. Among the four Ohio counties discussed above, the *in-state share of DMA* measure that I employ in analyses below is coded as 0.21, 1, 0.39, and 0.53 for Meigs, Athens, Washington, and Monroe counties, respectively. As I demonstrate below, a station's news coverage of a given state's governor (senators) increases with the proportion of residents in the media market from the governor's (senators') state. Thus, residents of Athens county (in the Columbus, OH media market with an in-state share of DMA of 1) are expected to receive the most coverage of Ohio Governor Kasich and Senators Brown and Portman, the two senators from Ohio, and residents of Meigs county (in the Charleston, WV market with an in-state share of DMA of 0.21) are expected to receive the least coverage of these Ohio officeholders.

There is broad variation across the country in the degree to which counties are located in relatively in-state vs. out-of-state markets. Overall, about 8 percent of counties are in predominantly out-of-state markets in which less than 20 percent of the market's residents are in-state, and 38 states have at least one county that is located in an out-of-state market based on this 20-percent threshold.<sup>13</sup> See Figure 1.2 for a map of the lower 48 states and variation within each state in terms of counties in relatively in-state vs. relatively out-of-state markets.

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<sup>13</sup>Another 8 percent of counties are in markets in which 20-40 percent of the market's residents are in-state; 7 percent of counties are in markets in which 40-60 percent of the market's residents are in-state; 8 percent of counties are in markets in which 60-80 percent of the market's residents are in-state; and 68 percent of counties are in markets in which at least 80 percent of the market's residents are in-state.

Figure 1.2: Share of In-State Residents in a County’s Media Market



Darker shades indicate that counties are located in predominantly out-of-state media markets, while lighter shades indicate in-state media markets.

## 1.4 How Media Markets Structure Local News Coverage

The biggest difficulty in studying local television news coverage is data availability. As Arnold (2004) points out, there is no archive or repository for local television newscast videos or transcripts. In addition, seemingly few stations make transcripts of their newscasts publicly available.<sup>14</sup> To overcome this obstacle, I rely on publicly available closed captioning data from the Internet Archive’s TV News Archive.<sup>15</sup> In total, the Internet Archive’s Television News Archive has collected two million hours of news coverage, equating to 5.7 billion words, from over 150 stations (Leetaru 2016). Many of the stations in the archive are national or international networks such as CNN, MSNBC, Fox News, and BBC News London.

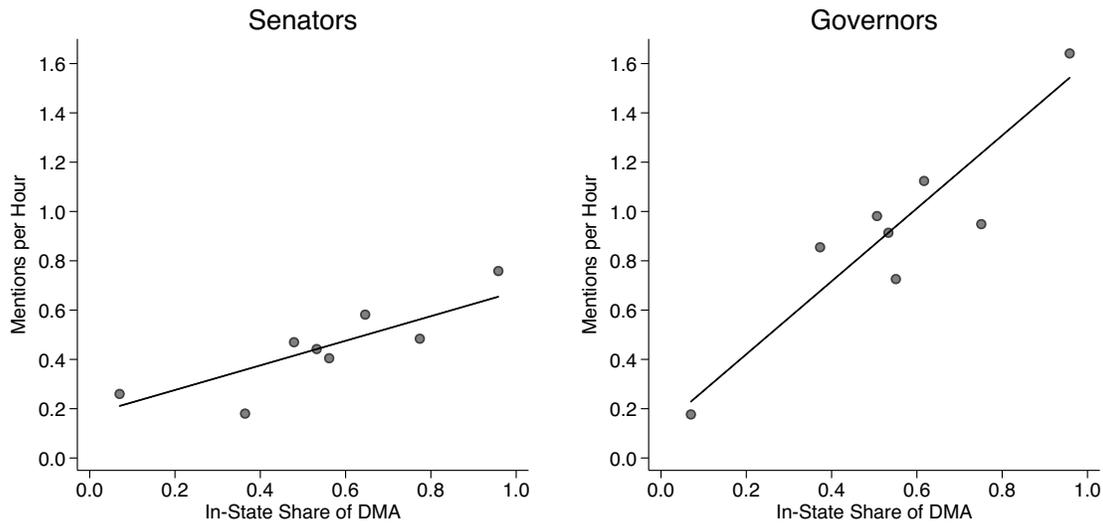
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<sup>14</sup>Some data vendors (e.g., ShadowTV, TVEyes) collect video and closed captioning information from television stations. Their clients are usually companies and public relations firms interested in monitoring and tracking media attention. Accessing the proprietary data from these vendors can be cost-prohibitive for academic researchers.

<sup>15</sup>See <https://archive.org/details/tv>.

I restrict the sample to all local stations affiliated with one of the four major networks (ABC, CBS, NBC, or Fox) and analyze coverage during 2016.<sup>16</sup> The sample includes 99 stations from 25 media markets and, in total, about 24,000 hours of coverage. I retrieve data through an API to count the number of 15-second clips that mention a specific governor or senator from all states that overlap with the station’s media market (see Section A.3 for more technical details). For example, the Boston DMA overlaps with Massachusetts, New Hampshire, and Vermont, so I count clips that mention senators and governors from those three states for all stations in the Boston DMA.

Figure 1.3: Local TV News Coverage of Senators & Governors by State’s Share of DMA



A station devotes more coverage to senators (left) and governors (right) from states within their media market as the share of residents from that state comprise a larger proportion of the market. The plot is residualized to account for senator/governor fixed effects. Each point corresponds to local mean, and the line is estimated using OLS.

Figure 1.3 demonstrates this first-stage relationship between a state’s population share of a DMA and the level of news coverage that the governor and senators from that state receive from stations in the DMA. As is clear from the figure, television stations provide greater coverage of a governor (senator) as the governor’s (senator’s) state makes up a larger population share of the DMA. A governor is expected to receive an additional 1.5 mentions

<sup>16</sup>Closed captioning text is only available during 2016 for most of the local stations in the archive.

per hour of non-entertainment coverage from a television station as the share of population overlap between a station’s market and the governor’s state increases from 0 to 1. The same estimated quantity for a senator is an additional 0.5 mentions per hour hour of non-entertainment coverage.<sup>17</sup> While the magnitude is substantially larger for governors, both quantities represent substantively meaningful increases in coverage. In sum, the relationship between a state’s population share of the media market and local news coverage for that state’s governor and senators is positive and strong. The full results from the local news coverage analysis along with additional technical information are available in Section A.3.

These patterns provide strong evidence that residents of in-state media markets have access to far more news coverage of their senators and governor than residents of out-of-state markets. The relationship is approximately linear and provides an empirical basis of support for using a continuous measure of the in-state share of DMA.

## 1.5 Local News Coverage Informs Voters

Remarkably, Representative Andrew Maguire of New Jersey’s 7th district pointed to the idea that residents who lack access to in-state television stations are unable to stay informed: “The citizens in the northern part of the State are better able to recognize Mayor Koch of New York City and the political issues facing local legislators in Albany then they are able to identify their own local political leaders and State officials. They are deprived, for the most part, of local advertising and public affairs programming that is targeted to their own needs”(*Congressional Record* 1980).<sup>18</sup> Does exposure to relevant local news coverage about governors and senators inform voters? I exploit the idiosyncratic geography of media markets to help examine this question. For this analysis, I use data from the 2012 and 2016 Cooperative Congressional Election Study (CCES). The large sample size of the CCES provides substantial geographic coverage of respondents in counties located in both relatively

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<sup>17</sup>See specification (5) in Tables A.1-A.2.

<sup>18</sup>New Jersey did not have any full power television stations located in the state at that time.

in-state markets and counties located in relatively out-of-state markets. Conveniently, the CCES asks respondents several questions that gauge knowledge of their elected officials. The empirical setup is as follows:

$$\mathbb{E}[\textit{knowledge}_{ist}] = \lambda_{st} + \rho \cdot P_{ist} + X_{ist} \cdot \beta$$

where  $\textit{knowledge}_{ist}$  is the knowledge of respondent  $i$  in state  $s$  in year  $t$  (measures of knowledge are discussed below),  $\lambda_{st}$  is a state-year fixed effect,  $P_{ist}$  is the in-state population share of the DMA in which respondent  $i$  lives, and  $X_{ist}$  is a vector of individual-level control variables. The coefficient of interest is  $\rho$ . The inclusion of state-year fixed effects ensures that comparisons are of individuals within the same statewide electoral context. Given that this is a study of statewide offices (i.e., governor, senator), the state-year fixed effects hold constant the officeholders and candidates. All models are estimated using ordinary least squares.

Table 1.1 displays the relationship between the in-state share of DMA (i.e., the proportion of the DMA’s population from the respondent’s state) and three measures of knowledge. The party recall measure indicates whether the respondent can correctly recall their senators’ or governor’s party (see specifications 1 and 4, labeled: “Recall”); the ability to evaluate measure indicates whether the respondent is able to evaluate (i.e., approve or disapprove) their senators or governor (see specifications 2 and 5, labeled: “Eval.”); and the ability to place on an ideological scale measure indicates whether the respondent is able to place their senators or governor on an ideological scale (see specifications 3 and 6, labeled: “Ideo.”).<sup>19</sup> All specifications reported in Table 1.1 include demographic control variables: family income, gender, race, education, age, and marital status.<sup>20</sup> Alternative specifications without control

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<sup>19</sup>For senators, a knowledge variable is coded = 0 if the respondent has knowledge of neither senator (e.g., lacks party recall of both senators), = 0.5 if the respondent has knowledge of only one senator (e.g., correctly recalls the party of only one senator), = 1 if the respondent has knowledge of both senators (e.g., recalls the parties of both senators). Results are nearly identical if the knowledge measures are based on knowledge of each senator separately rather than the combined knowledge. For governors, a knowledge variable is coded = 0 if the respondent does not have knowledge of the governor (e.g., is unable to recall the governor’s party) and = 1 if the respondent has knowledge of the governor (e.g., is able to recall the governor’s party correctly).

<sup>20</sup>See Section A.5 for descriptions and definitions of these and other variables.

variables as well as with additional control variables are reported in Tables A.4-A.6 of the Appendix. Estimates are remarkably stable across all specifications.

Table 1.1: Voter Knowledge of Senators & Governors

	Senator			Governor		
	(1) Recall	(2) Eval.	(3) Ideo.	(4) Recall	(5) Eval.	(6) Ideo.
In-State Share of DMA	0.088* (0.012)	0.085* (0.012)	0.076* (0.012)	0.106* (0.012)	0.035* (0.007)	0.069* (0.012)
Overall Knowledge Rate	0.698	0.791	0.746	0.765	0.900	0.803
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	111,517	111,765	110,594	111,894	112,324	111,435
Clusters	333	333	333	333	333	333

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable is whether the individual can recall the party of the senator/governor in specifications (1) and (4), whether the individual is able to evaluate the senator/governor in specifications (2) and (5), and whether the individual is able to place the senator/governor on an ideological scale in specifications (3) and (6). Basic control variables include family income, gender, race, education, age, and marital status.

Across the three knowledge measures for both senators and governors, there is a substantively large relationship with the share of the DMA population from the respondent's state (in-state share of DMA). Importantly, the in-state share of DMA variable ranges from  $< 0.01$  to 1, so it is approximately correct to interpret the estimated coefficient as the expected increase in voter knowledge moving from an entirely out-of-state market to an entirely in-state market.<sup>21</sup> Thus, a person living in an entirely in-state market is about 9 percentage points more likely to recall their senator's party (specification 1) and 11 percentage points more likely to recall their governor's party (specification 4) than a person living in the same state in an entirely out-of-state market. Similarly, relative to residents of entirely out-of-state markets, residents of entirely in-state markets are about 9 percentage points more likely to be able to evaluate their senator (specification 2), 4 percentage points more likely to be able to evaluate their governor (specification 5), 8 percentage points more likely to be able to

<sup>21</sup>There are several "orphan" counties in out-of-state markets whose states comprise an extremely small share of that media market.

place their senator on an ideological scale (specification 3), and 7 percentage points more likely to be able to place their governor on an ideological scale (specification 6).<sup>22</sup>

Table 1.2: Voter Knowledge of Non-Incumbent Senate Candidates

	Incumb. Race		Open-Seat Race	
	(1) Challenger	(2) D Cand.	(3) R Cand.	
In-State Share of DMA	0.096* (0.018)	0.108* (0.032)	0.131* (0.050)	
Overall Knowledge Rate	0.535	0.632	0.690	
State-Year Fixed Effects	Yes	Yes	Yes	
Basic Controls	Yes	Yes	Yes	
Observations	62,705	14,749	14,771	
Clusters	270	81	81	

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable is coded =1 if the individual is able to place the candidate on an ideological scale and =0 if the individual is unable to make an ideological placement. Basic control variables include family income, gender, race, education, age, and marital status.

Exposure to local television news not only increases voter knowledge about officeholders but also knowledge about non-incumbent candidates. The CCES asks respondents to place Senate candidates, including those candidates challenging an incumbent and those running in an open-seat race (unfortunately, the CCES does not ask respondents about non-incumbent gubernatorial candidates). The results are displayed in Table 1.2. For all three types of candidates—challenger, Democratic open-seat, and Republican open-seat—the relationship between in-state share of DMA and ability to place the candidate on an ideological scale is substantively large. Residents of entirely in-state markets are 10-13 percentage points more likely to be able place a candidate on an ideological scale relative to residents of entirely out-of-state markets.

One especially important concern—and serious threat to inference—is that the residents of in-state markets are more politically knowledgeable than residents of out-of-state markets

<sup>22</sup>The smallest reported knowledge effect is on ability to evaluate the governor (specification 5 in Table 1.1). This is likely due to a ceiling effect, as the overall, baseline rate for this knowledge measure is very high at 90%.

Table 1.3: Voter Knowledge Placebo Tests

	Chamber Majorities		Parties			S. Court
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.010 (0.010)	0.014 (0.011)	-0.010 (0.008)	0.001 (0.008)	0.012 (0.010)	-0.005 (0.010)
Overall Knowledge Rate	0.626	0.580	0.889	0.881	0.817	0.831
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	112,487	112,385	111,637	111,499	104,587	111,536
Clusters	334	334	334	334	333	334

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable for models (1) and (2) is whether the individual knows which party has a majority of seats in the House (1) and Senate (2); the dependent variable for models (3), (4), and (5) is whether the individual can place the Democratic Party (3) and Republican Party (4) on an ideological scale and whether the individual correctly places the Democratic Party to the left of the Republican Party (5); and the dependent variable for model (6) is whether the individual is able to place the Supreme Court on an ideological scale. Basic control variables include family income, gender, race, education, age, and marital status.

due to unobserved differences between the two groups of residents. If this were the case, then residents of in-state markets should demonstrate greater general political knowledge as well. Table 1.3 displays results from six placebo tests which all capture general knowledge of national politics: knowledge of which party controls the House (1) and Senate (2), ability to place the Democratic Party (3) and Republican Party (4) on an ideological scale, correctly placing the Democratic Party to the left of the Republican Party on an ideological scale (5), and ability to place the Supreme Court on an ideological scale.<sup>23</sup> Across all six placebo tests, the estimated coefficient for the in-state share of the market is very near to zero and not statistically significant. These results provide strong evidence that residents of in-state markets are more knowledgeable about their governors and senators due to relevant local news coverage rather than bias from confounding variables.

<sup>23</sup>These placebo tests also address the concern that, due to unobserved differences, residents of in-state markets have a higher propensity to respond to these types of questions. The null results suggest this is not the case.

## 1.6 Local News Coverage and Split-Ticket Voting

As discussed above, greater exposure to local news coverage may allow voters to assess down-ballot candidates separately from their national, partisan judgment for president. Voters with less exposure to local news coverage may be more likely to apply their national, partisan judgment to down-ballot races. Using a state’s population share of the media market as the key source of variation in the level of local news coverage, I examine the degree to which exposure to local news coverage increases the probability of a voter engaging in split-ticket voting for governors and senators. The empirical strategy remains the same from the previous section:

$$\mathbb{E}[split_{ist}] = \lambda_{st} + \rho \cdot P_{ist} + X_{ist} \cdot \beta$$

where  $split_{ist}$  is coded = 1 if respondent  $i$  in state  $s$  in year  $t$  casts a split-ticket ballot (i.e., votes for candidates from different major parties for president and senator/governor) and is coded = 0 if respondent  $i$  casts a straight-ticket ballot (i.e., votes for candidates from the same major party for president and senator/governor),  $\lambda_{st}$  is a state-year fixed effect,  $P_{ist}$  is the in-state population share of the DMA in which respondent  $i$  lives, and  $X_{ist}$  is a vector of individual-level control variables.<sup>24</sup> The coefficient of interest is  $\rho$ . Again, the state-year fixed effects allow for the comparison of voters within the same statewide electoral context across different media markets. This strategy exploits quasi-random variation in the level of local news coverage but ensures that these voters experience identical statewide electoral conditions (e.g., candidates, incumbency, closeness of the race).

Table 1.4 displays the main results. For Senate elections, voters in an entirely in-state market are about 2.3-2.5 percentage points more likely to cast a split ticket than voters in an entirely out-of-state market within the same statewide electoral context. For gubernatorial elections, the estimated coefficient implies a 4.0-4.5 percentage point increase in ticket splitting for voters residing in entirely in-state markets relative to voters living in entirely

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<sup>24</sup>The  $split_{ist}$  variable is based only on the respondent’s vote for president and senator/governor. For respondents in states with both governor and senator elections, as the measure is defined, it is possible for a respondent to cast a split-ticket ballot for one office and a straight-ticket ballot for the other office.

out-of-state markets. Of note, the larger magnitude of the estimated effect of local news coverage for the governor specifications is consistent with the higher level of local TV news coverage that governors garner. The estimated effects for both senators and governors are substantively large: based on estimates from the CCES, in total, about 8 percent of voters in 2012 and 2016 cast a split president-senator ticket and 9 percent of voters cast a split president-governor ticket in 2012 and 2016. Thus, media markets and local news coverage seem to play a meaningful role in ticket-splitting behavior in contemporary U.S. elections.

Table 1.4: Split-Ticket Voting: President-Senator & President-Governor

	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.024*	0.025*	0.022*	0.040*	0.045*	0.040
	(0.010)	(0.009)	(0.010)	(0.020)	(0.020)	(0.022)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	No	Yes	Yes	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes	No	No	Yes
Observations	37,504	37,398	36,962	8,281	8,259	8,156
Clusters	319	319	319	66	66	66

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Dependent variable is coded =1 if an individual votes for the Democrat (Republican) for president and Republican (Democrat) for senator/governor. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include opposite-party incumbent, strength of partisanship, and strength of ideology.

## 1.7 Robustness Checks and Alternative Explanations

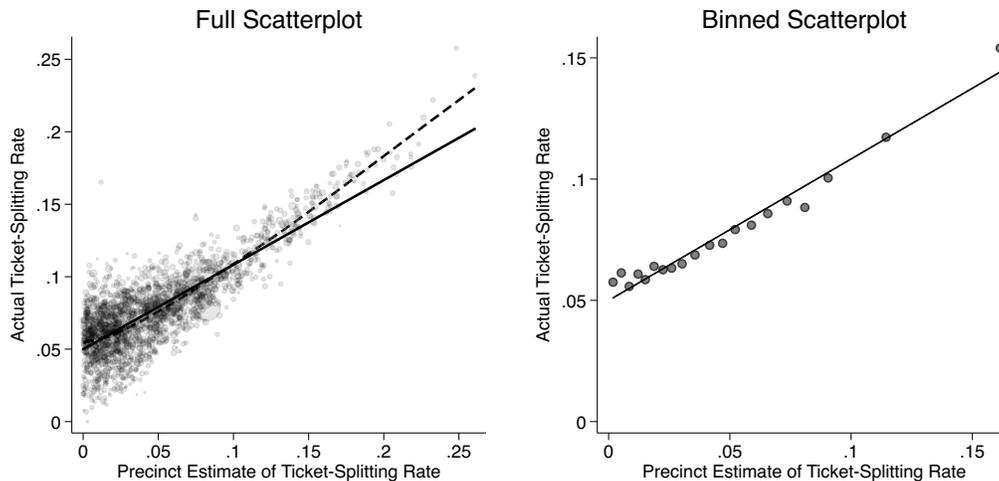
### 1.7.1 Precinct-Level Results: Split-Ticket Voting

To provide further evidence of the effect of local news coverage on ticket splitting, I also examine the relationship between the in-state population share of media markets and aggregate election returns at the precinct level. The aggregate approach guards against potential survey non-response bias and measurement error in recalling vote choice across offices. This approach has a disadvantage in that it requires an aggregate proxy measure of split-ticket voting, which is an individual-level phenomenon. It is worth noting that this

is not a standard ecological inference problem. Voting precincts are nested within counties, which are assigned to media markets. Consequently, the explanatory variable of interest (in-state share of DMA) does not vary within county (or precinct) and, thus, can be observed correctly at the precinct level. However, the outcome variable—split-ticket voting—cannot be observed at the precinct level, so I must use a proxy measure.

The aggregate proxy measure of split-ticket voting is  $|V_G - V_P|$  for governor elections and  $|V_S - V_P|$  for senator elections, where  $V_G$  is the Democratic share of the two-party vote in the gubernatorial election,  $V_S$  is the Democratic share of the two-party vote in the senatorial election, and  $V_P$  is the Democratic share of the two-party vote in the presidential election. The primary issue with this proxy is that it is biased downward, as voters who split their tickets in opposite directions within a geography cancel each other out. Take a simplified example. Suppose there is a precinct with 100 voters in which 10 voters cast a split ticket. If 8 voters cast a Democrat-Republican (president-senator) split ticket, and 2 voters cast a Republican-Democrat split ticket, then the precinct-level measure of split-ticket voting is 6 percent even though the actual rate is 10 percent.

Figure 1.4: **Actual Ticket-Splitting Rate vs. Precinct-Level Measure**



These plots demonstrate the validity of using precinct-level estimates of split-ticket voting. The relationship between the precinct estimate and the actual rate is positive and very strong. The precinct estimate is biased downward overall and, thus, tends to understate the level ticket splitting. The solid line is estimated using OLS, and the dashed curve is estimated from a locally weighted regression (LOESS).

I provide evidence for the validity of the aggregate approach to analyzing split-ticket voting, using ballot-level data from the 2016 South Carolina Election Audit. The South Carolina Election Audit is a rare source of ballot-level data. Using this unique data set, I am able to observe the actual rate of split-ticket voting within each precinct, and I can aggregate the ballot-level data up to the precinct level and calculate the precinct-level measure of ticket splitting for each precinct. Figure 1.4 plots the relationship between the actual rate of split-ticket voting on the vertical axis, and the proxy measure that one would observe from precinct-level election returns on the horizontal axis. As is clear from the figure (as well as the corresponding Table A.9 in the Appendix), the relationship between the precinct-level measure and the actual rate of split-ticket voting is positive, strong, and approximately linear. As is also clear, the precinct-level measure is biased downward (evidenced by most observations being located above where the 45-degree line would be).

Table 1.5: Precinct-Level Analyses, 2012

	Senator		Governor	
	(1)	(2)	(3)	(4)
In-State Share of DMA	0.016 (0.010)	0.015* (0.006)	0.039* (0.012)	0.031* (0.009)
Weighted	No	Yes	No	Yes
State FEs	Yes	Yes	Yes	Yes
Observations	37,560	37,560	11,141	11,141
Clusters	115	115	42	42

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable is the absolute value of the difference between the Democratic share of the two-party vote for president and the Democratic share of the two-party vote for senate/governor. Precinct weights are based on the number of two-party votes cast for president.

Using data from the 2012 Harvard Election Data Archive (HEDA), I examine the relationship between the precinct-level estimate of split-ticket voting and the in-state share of DMA. For 2012, HEDA contains precinct-level election returns from 20 states with senator elections and 8 states with governor elections. Table 1.5 displays the results from this analysis. While the estimated coefficient is not a readily interpretable quantity (due to bias in the aggregated measure of ticket splitting), the positive sign on the coefficient provides further,

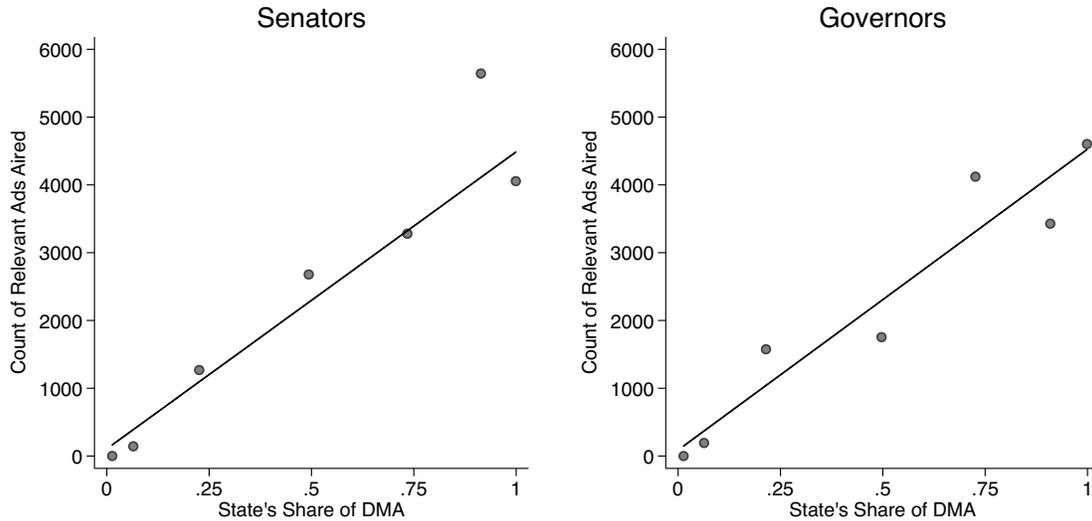
strong evidence of a relationship between split-ticket voting and in-state share of DMA. The primary purpose of this analysis is to demonstrate that the observed relationship between the in-state population share of the media market and split-ticket voting is not driven by some artifact of survey measurement. The positive, significant relationships observed at the precinct level provide strong corroborative evidence that local news coverage increases split-ticket voting. It is also worth noting that, as in the individual-level specifications, the relative magnitude of the estimated coefficient is higher in the gubernatorial specifications than the senatorial specifications, consistent with the higher level of local television news coverage that governors receive relative to senators.

### **1.7.2 Is It Local News Coverage or Campaign Advertisements?**

Thus far, I have demonstrated that the geography of media markets structures local television news coverage and voters residing in areas with greater exposure to local coverage have more knowledge about their governor and senators and are more likely to engage in split-ticket voting. A plausible alternative mechanism, however, is exposure to campaign advertising on television rather than exposure to local news coverage. In this scenario, voters are better able to distinguish candidates from their parties due to higher levels of television advertising in in-state markets. Campaigns are strategic and tend to air more ads in in-state markets than out-of-state markets. Figure 1.5 demonstrates this clear relationship analyzing data from the 2012 Wesleyan Media Project (Fowler, Franz, and Ridout 2015). See Section A.7 for technical details on the analysis. This strong relationship between a state's population share of the market and the number of ads aired in that market is unsurprising: it is a far more efficient use of limited campaign resources to air advertisements in in-state markets in which most viewers are actually able to participate in the relevant electoral contest.

To assess whether voters are primarily acquiring information from advertisements or from local news coverage, I examine the extent to which voters are more knowledgeable about an officeholder who is running for reelection contemporaneously to the administration of

Figure 1.5: Political Ad Airings in 2012 by In-State Share of DMA



Residents of in-state markets receive greater exposure to campaign advertising on television. Each point corresponds to a local mean. The regression line is estimated using OLS.

the survey. Voters are typically exposed to advertisements only during the period in which an officeholder is seeking reelection. Thus, if voters are primarily acquiring information from advertisements rather than local news coverage, large gaps of knowledge should be apparent based on whether or not the officeholder is seeking reelection. Specifically, if exposure to TV advertisements is driving the relationship between in-state share of DMA and voter knowledge, the magnitude of the relationship should be larger for voter knowledge of officeholders running for reelection relative to officeholders not running for reelection during the administration of the survey.

Results for the three measures of voter knowledge—party recall, ability to evaluate, and ability to place ideologically—for both senators and governors are displayed in Table 1.6. The coefficient of interest from this table is the interaction between running (i.e., running for reelection) and the in-state share of DMA. This coefficient indicates if the effect of living in an in-state market on knowledge of officeholders is larger if the officeholder is running for reelection. The estimated coefficient on the interaction term for all three measures across both offices is not significant. While not statistically significant, the interaction term is positive in 5 of the 6 specifications, which is at least suggestive that voters absorb some informational

content from ads.<sup>25</sup> Nevertheless, the coefficient on main effect of in-state share of DMA demonstrates that voters residing in in-state markets have substantially greater knowledge of officeholders compared to residents of out-of-state markets. This provides strong evidence that exposure to local news mostly accounts for the increased voter knowledge rather than exposure to campaign ads.

Table 1.6: Voter Knowledge: Running for Reelection vs. Not Running

	Senator			Governor		
	(1) Recall	(2) Eval.	(3) Ideo.	(4) Recall	(5) Eval.	(6) Ideo.
Running	0.076 (0.019)	0.034 (0.017)	0.027 (0.015)			
In-State Share of DMA	0.092 (0.014)	0.082 (0.013)	0.071 (0.013)	0.104 (0.012)	0.033 (0.007)	0.066 (0.013)
Running x In-State Share of DMA	-0.013 (0.023)	0.011 (0.019)	0.013 (0.018)	0.023 (0.035)	0.030 (0.028)	0.045 (0.032)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	223,529	223,873	222,439	111,894	112,324	111,435
Clusters	333	333	333	333	333	333

Robust standard errors clustered by state-DMA in parentheses.

The dependent variable is whether the individual can recall the party of the senator/governor in specifications (1) and (4), whether the individual is able to evaluate the senator/governor in specifications (2) and (5), and whether the individual is able to place the senator/governor on an ideological scale in specifications (3) and (6). Basic control variables include family income, gender, race, education, age, and marital status.

Another piece of information from Table 1.6 worthy of mention is that the estimated coefficient on the running for reelection variable is positive in all three senator specifications.<sup>26</sup> This result suggests that voters across the entire state, irrespective of the in-state share of the media market, have more knowledge of officeholders running for reelection than officeholders not running at that point in time. Voters are about 8 percentage points more likely to be able to recall a senator’s party, 3 percentage points more likely to make an

<sup>25</sup>It is also possible that voters hold on to knowledge from campaign advertisements aired in past elections. But, if campaigns are providing voters with useful information, they likely exert their strongest effect during the period of time in which they are airing.

<sup>26</sup>This variable is not estimated for the governor models, as there is no within state-year variation; a state’s governor is either running for reelection in a given year or she is not. There is within state-year variation for senators, as there are two senators from each state.

evaluation, and 3 percentage points more likely place a senator on an ideological scale when the senator is running for reelection. This result points to campaigns providing information to voters across different types of media markets through alternative forms of media and communication. In sum, it is apparent that exposure to local news coverage from residing in an in-state market provides voters with large office-specific knowledge advantages compared to residents of out-of-state markets.

## 1.8 Discussion & Conclusions

Overall, the results provide strong evidence that exposure to local television news coverage increases voter knowledge and split-ticket voting for senatorial and gubernatorial races. The geography of television media markets structures the level of relevant local news coverage to which voters are exposed. Voters who live in media markets that are comprised mostly of residents from their own state (in-state markets) are exposed to relatively high levels of relevant local news coverage about their governor and senators. On the other hand, voters who live in media markets that are comprised mostly of residents from another state (out-of-state markets) are exposed to relatively low levels of relevant local news coverage. Instead, these voters are primarily exposed to news coverage relevant to their neighboring state. Exposure to relevant local news coverage has considerable effects on voter knowledge of both officeholders and non-incumbent candidates. For instance, residents of entirely in-state markets with access to relevant local television news coverage are 9 percentage points more likely to recall the party of their senators and 11 percentage points more likely to recall the party of their governor than residents of out-of-state markets with little access to relevant local TV news.

Moreover, exposure to relevant local news coverage increases rates of split-ticket voting. Voters residing in entirely in-state markets are 2.3-2.5 percentage points more likely to cast a split president-senator ticket and 4.0-4.5 percentage points more likely to cast a split president-governor ticket. Given that the overall rate of split-ticket voting in these election

years for both of these offices is 8-9 percent, the estimated effects of exposure to local television news on ticket splitting are considerable. Local television news is a prominent determinant of split-ticket voting. Importantly, these findings are robust across multiple specifications and types of data (individual-level survey data and precinct-level election returns data). Placebo tests and auxiliary analyses all point to local news coverage as a causal factor rather than campaign television ads or possible unobserved confounders.

The results accord with the theoretical expectation that greater exposure to local news coverage provides voters with relevant information about officeholders and candidates, which allows voters to make assessments for down-ballot elections separately from their national, partisan judgment in the presidential race. In the absence of exposure to relevant local news coverage, voters are more likely to apply their national, partisan judgment to down-ballot races. In other words, the results suggest that the decline of local news likely contributes—to an important degree—to the nationalization of U.S. elections. Unfortunately, given the limited time period under examination, it is difficult to assess the degree to which declining local television news audiences account for the decrease in split-ticket voting observed over the past couple of decades. It is quite possible that the magnitude of the relationship between exposure to local television news and ticket splitting was larger in the recent past, prior to the downturn in local newscast audiences. Moreover, to the extent that these results are indicative of the effect of local news coverage beyond television, the broader decline in local news coverage likely contributed substantially to the nationalization of elections. Local newspapers have experienced steep declines in their circulation and revenues, resulting in considerable cuts to staffing and other resources required to provide extensive local coverage. Less speculatively, the results suggest that local TV news has mitigated nationalizing forces in Senate and gubernatorial elections even in the contemporary polarized context.

The nationalization of elections has serious implications for the functioning of representative democracy. On the one hand, to the extent that party labels are more clearly defined across offices and levels of government (i.e., if a party label means the same thing or at least similar things), electoral nationalization could simplify the task of voters and improve

ideological or policy-based representation. On the other hand, nationally meaningful party labels could crowd out important state and local issues in the calculus of voters. Moreover, with highly nationalized elections, the electoral prospects of candidates and officeholders are based less on their (prospective) performance in office (e.g., competence, service delivery, economic performance, etc.) and more on national forces such as the popularity of the president. Unless the performance of officeholders is correlated within party and across levels of government, and these correlations in performance correspond correctly with national electoral forces (e.g., the president’s popularity), it is difficult to hold elected officials accountable based on their performance. In the context of U.S. federalism, the actions and responsibilities of state and local elected officials are mostly distinct from those of federal officeholders.

With that said, scholars in the past have bemoaned indistinct parties and incumbency advantage as problematic for accountability and representative democracy. The parties are now more distinct and the incumbency advantage has declined, which have yielded the present accountability concerns due to increasingly nationalized elections. Thus, it is easy to point to present ills and far more difficult to diagnose potential future ills.<sup>27</sup> More optimistically, U.S. primary elections help to filter out less qualified candidates as well as officeholders involved in scandals even in safe districts in which general elections fail to act as an effective filter (Hirano and Snyder 2018). Of course, the first-order concern with the decline of local news should be its direct impact on being able to hold politicians accountable (e.g., Arnold 2004; Snyder and Strömberg 2010). As local news sources disappear and their resources decrease, it is probably the case that scandals, corruption, and other bad behavior are more likely to go unnoticed. Politicians may also be less deterred with fewer watchdogs, increasing the incidence of such behavior. If the wrong-doing goes undetected, it

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<sup>27</sup>For instance, in *Federalist* 46, Madison (1788) expressed fears that members of Congress and state legislators will both be too focused on local issues at the expense of national issues and the larger national interest: “A local spirit will infallibly prevail much more in the members of Congress, than a national spirit will prevail in the legislatures of the particular States...For the same reason that the members of the State legislatures will be unlikely to attach themselves sufficiently to national objects, the members of the federal legislature will be likely to attach themselves too much to local objects.”

is impossible to hold the officeholder electorally accountable in a primary or general election.

A series of new FCC rules appear to eliminate important protections for localism in broadcast news. First, the FCC eliminated the main studio rule, which required stations to have a physical studio in or near the community of license (Federal Communications Commission 2017c). FCC Commissioner Mignon L. Clyburn, who voted against the rule, issued a strong warning when the rule was proposed: “By tentatively proposing to eliminate the Commission’s main studio rule however, it seems to me that we are embracing a world in which automated national programming is the new normal” (Federal Communications Commission 2017e). The FCC also has loosened restrictions that prevent a single company from owning multiple broadcast television/radio stations and newspapers within the same market area (Federal Communications Commission 2017d). In addition, the Commission has taken steps that effectively loosen restrictions on the national audience reach of a single owner, and it is considering loosening those restrictions further (Federal Communications Commission 2017a).<sup>28</sup> The possible consequence of these rule changes is that a single entity can own television stations with a vast reach throughout the country and pipe into those stations nationally oriented news programming with little local content. Sinclair Media Group is one such company that has acquired several stations recently and thereby expanded its reach.<sup>29</sup> Alarmingly, Martin and McCrain (2018) find that stations acquired by Sinclair Media Group increased their coverage of national politics and reduced their coverage of local politics. The elimination of the main studio rule and the changes and potential changes to ownership restrictions likely present a serious threat to the future of local broadcast news coverage. The direction of new FCC rules is likely only to accelerate the nationalization of elections in the near future.

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<sup>28</sup>The current rule prevents a single entity from owning stations that “reach more than 39 percent of the television households in the country” (Federal Communications Commission 2017a), but the FCC’s reinstatement of the UHF discount effectively raises this cap considerably.

<sup>29</sup>Much to the surprise of experts in the field, the FCC recently halted Sinclair’s attempted acquisition of Tribune Media, another company that owns a multitude of television stations.

## 2 | Media, Senator Ideology, and Retrospective Voting: How Television Enhances Accountability

### 2.1 Introduction

The question of whether voters primarily make choices on the basis of candidate ideology or non-policy considerations has captured the attention of political scientists for several decades. Scholars skeptical of policy voting argue that voters have “non-attitudes” or unorganized preferences (e.g., Converse 1964), make choices at the ballot box in response to various non-policy considerations such as partisan identification (e.g., Achen and Bartels 2016; Campbell et al. 1960), or—to the degree that voters have formed preferences—representatives can ignore these preferences with little or no electoral consequence (e.g., Highton n.d.; Stokes and Miller 1962). The implication from the findings of this camp of scholars is that “elections are capricious collective decisions based on considerations that ought, from the viewpoint of the folk theory, to be largely irrelevant—and that will, in any case, soon be forgotten by the voters themselves” (Achen and Bartels 2016, 16). On the other hand, other researchers argue that voters generally have coherent and organized ideological preferences (e.g., Ansolabehere, Rodden, and Snyder 2008), voters make decisions at the ballot box on the basis of policy considerations, holding representatives accountable for the ideological nature of their behavior in office (e.g., Ansolabehere, Snyder, and Stewart 2001; Ansolabehere and Jones 2010; Canes-Wrone, Brady, and Cogan 2002; Jessee 2009; Key 1966; Page and Shapiro 1992), and voters do not make choices in response to irrelevant

events outside of the control of their elected officials (e.g., Fowler and Hall 2018).<sup>1</sup> For the scholars in this camp, voters are “moved by concern about central and relevant questions of public policy, of governmental performance, and of executive personality” (Key 1966, 8).

These questions—in simple terms: do voters have organized preferences, and do they vote based on these preferences *and* the actions of their legislators—are fundamental to any understanding of the functioning, health, and level of accountability within a representative democracy. Indeed Madison pointed to the crucial role the electorate plays in holding legislators accountable in *Federalist* 57: “The elective mode of obtaining rulers is the characteristic policy of republican government. The means relied on in this form of government for preventing their degeneracy are numerous and various. The most effectual one, is such a limitation of the term of appointments as will maintain a proper responsibility to the people.” These questions and issues are central to the discipline of political science and have been at the heart of many of the most pressing and animated debates in the discipline.

This paper provides strong support for the claim that voters utilize information to hold their senators accountable for the nature of their ideological behavior in office. Specifically, I examine how the interplay between television news coverage and a senator’s roll-call voting record affects the senator’s electoral support. In the words of V.O. Key (1966), “[T]he electorate behaves about as rationally and responsibly as we should expect, given the clarity of the alternatives presented to it and the character of the information available to it.” While previous studies have documented a link between legislator ideology and electoral support, the informational mechanism through which voters infer their representative’s ideological position has received little attention. This paper provides evidence that when voters are exposed to news coverage about their senators, they use it in their choice calculus at the ballot box.

The empirical strategy leverages quasi-random variation in the level of television news

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<sup>1</sup>Of course, most scholars do not neatly fit into two camps. Many scholars, including those cited above, have conditional findings in which voters sometimes demonstrate ideological constraint and sometimes do not or in which voters sometimes effectively hold officials accountable and sometimes not. For some other examples of work that does not fit cleanly into one of the two camps see Erikson, MacKuen, and Stimson (2002) and Adams et al. (2017).

coverage due to the idiosyncratic boundaries of media markets. As Chapter 1 demonstrates, voters situated in a neighboring state’s media market receive little coverage about their own state’s senators, while voters residing in an in-state market receive substantially more coverage. Importantly, voters residing in out-of-state markets and in-state markets are comparable to one another with respect to a host of observable characteristics. The electoral effect of exposure to in-state television varies based on the ideological extremity of the senator’s roll-call behavior. In-state television provides a moderate Senate incumbent with a large, positive electoral boost. As a senator become more extreme, however, the effect of in-state television decreases in size and actually results in a non-negligible electoral penalty. This interactive relationship between in-state television and incumbent ideology is robust to alternative measurement strategies, restricting the sample to counties that are geographically proximate, and allowing for non-linearities in the interactive relationship. I then provide further evidence from analyses of individual-level, survey data that demonstrate that voters exposed to in-state television are more perceptive to ideological differences between their senators. Further analyses of the survey data also strongly suggest that local news coverage, rather than campaign advertisements aired on television, is the primary informational mechanism through which voters perceive senator ideology.

This study makes three important contributions to scholarship on how voters hold elected officials accountable for their behavior in office. First, this paper highlights the role of news media as the informational mechanism through which voters infer the ideological position of officeholders and use this information in making retrospective evaluations at the ballot box. This finding pushes against claims that voters do not consider policy or candidate ideology in making their vote choice. Second, when exposed to information that enhances their perceptions of their senators’ ideological positions, voters reward their incumbent senators for moderate roll-call behavior and punish them for extreme roll-call voting. This finding—that candidates benefit electorally from moderating their position to be closer to the median voter—aligns with one of the main predictions of Downs (1957). Of note, this finding that voters punish extreme candidates during the recent period sharply claims that in recent

years voters have stopped punishing individual candidates for extremism (e.g. Bonica and Cox 2018). Third, this paper revises conventional wisdom on television as a source of incumbency advantage. While the average effect of in-state television on the incumbent’s vote share is minimal, the interplay between television and incumbent ideology reveals that television meaningfully affects incumbent senators’ electoral prospects.

## 2.2 Empirical Strategy, Data, and Measures

The strategy relies on the geographic overlap between media market boundaries and state boundaries. There are several components to this empirical strategy. First, present-day media market boundaries closely approximate where stations’ signals traveled over-the-air in the 1950s and 1960s during the introduction and expansion of television (Gentzkow 2006). Consequently, these boundaries are idiosyncratic, often span multiple states, and are not based on political considerations. Second, residents of a given media market generally only have access to television stations located within their market. This restriction is due to exclusivity contracts between stations and networks that grant stations exclusive content rights within a market. Additionally, the Federal Communications Commission has a series of exclusivity and non-duplication rules that further codify these agreements and provide for a quick enforcement mechanism should another station infringe on these rights (Government Accountability Office 2015; Federal Communications Commission 2016).<sup>2</sup> Third (and, as a consequence), a station’s potential audience is defined by media market boundaries in which the station is located. Fourth, stations attempt to provide news coverage that is of interest and, thus, is relevant to their audiences. Thus, stations located in media markets that cross state boundaries are likely to provide relatively more coverage of a state’s officeholder as that state’s population share of the market increases.

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<sup>2</sup>While these contracts and rules largely prevent multichannel video programming distributors (MVPDs—these are primarily cable and satellite television providers) from importing out-of-market stations into a channel lineup in another market, it is still theoretically possible for consumers to gain access to an out-of-market station through an over-the-air signal. However, the vast majority of television households subscribe to an MVPD (about 90 percent) Federal Communications Commission (2017b). Moreover, the reach of over-the-air signals approximately conform to media market boundaries (Gentzkow 2006).

Chapter 1 demonstrates the validity for this research design. Specifically, media market boundaries are indeed idiosyncratic, resulting in remarkable covariate balance for a host of characteristics between counties in in-state markets and out-of-state markets (see Figure A.3). Additionally, using closed captioning data, this study shows that local television news coverage of a state’s senators and governors increases (approximately linearly) as that state’s residents comprise a larger share of the market’s population. This additional news coverage that residents of a predominantly in-state media market receive translates into greater knowledge of their senators and governor, but, importantly, does not translate into greater knowledge of national politics.<sup>3</sup>

In this paper, I marry aspects of the empirical strategies from Canes-Wrone, Brady, and Cogan (2002) and Ansolabehere, Snowberg, and Snyder (2006) to analyze how the interplay between incumbent ideology and television affects electoral support for incumbent Senate candidates. Canes-Wrone, Brady, and Cogan (2002) assess the extent to which members of the House of Representatives suffer electoral consequences in seeking reelection for engaging in ideologically extreme roll-call voting behavior, and Ansolabehere, Snowberg, and Snyder (2006) examine the degree to which television is a source of the incumbency advantage that senators and governors enjoy. The empirical setup is as follows:

$$\mathbb{E}[V_{ist}] = \lambda_{st} + \rho \cdot P_{ist} + \tau \cdot R_{st}P_{ist} + \beta \cdot N_{ist}$$

where  $V_{ist}$  is the incumbent Senate candidate’s vote share in county  $i$  in state  $s$  in election year  $t$ ,  $\lambda_{st}$  is a state-year fixed effect,  $P_{ist}$  is the in-state population share of the media market for county  $i$ ,  $R_{st}$  is the roll-call ideological extremity of the incumbent Senate candidate from state  $s$  in election year  $t$ , and  $N_{ist}$  is the normal vote. I describe measurement strategies and the data in detail below. The two coefficients of primary interest are  $\rho$  and

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<sup>3</sup>Throughout the paper I refer to individuals or counties being located in *in-state markets* and *out-of-state markets*. For most empirical analyses, the measure is operationalized as a continuous variable that indicates the population share of the market from the same state as the county or individual. For instance, if a media market overlaps with two states and 90 percent of the market’s population is from state  $i$  and 10 percent of the market population is from state  $j$ , residents of that market in state  $i$  reside in a predominantly *in-state market* and residents of that market from state  $j$  reside in a predominantly *out-of-state market*.

$\tau$ . While  $\rho$  indicates the incumbency effect of exposure to relevant local television news for a perfectly moderate incumbent Senate candidate (i.e., an incumbent with a roll-call extremity score equal to zero),  $\tau$  indicates whether more extreme incumbent candidates experience a television incumbency effect of a different magnitude. For instance, a positive  $\rho$  and negative  $\tau$  would mean that moderate incumbents experience a larger electoral benefit from television, while more extreme candidates experience a smaller benefit (and possibly a penalty depending on the magnitude of the parameters). The state-year fixed effects hold constant all of the statewide electoral features in a particular election year (e.g., challenger quality, electoral rules, other statewide races on the ballot, etc.) by comparing counties within the same state and election year.<sup>4</sup>

Below, I detail and describe sources of data and measurement strategies. To measure electoral support, I use county-level election return data from the CQ Voting and Elections Collection. From this data, I construct the measure of electoral support, which is the incumbent candidate's share of the two-party vote at the county-level in Senate races in 2012, 2014, and 2016. I also construct a measure of the county's normal vote, which is based on presidential vote in the most recent two presidential elections. The normal vote is always measured from the perspective of the incumbent candidate's party.<sup>5</sup>

To measure the ideological extremity of incumbent senators' roll-call voting, I use the first-dimension of Nokken-Poole scores from Lewis et al. (2019).<sup>6</sup> Importantly, Nokken-Poole scores allow for members to move ideologically across terms of Congress within their career, so I can specifically examine the effect of roll-call extremity in the period prior to the election.<sup>7</sup> Similar to Canes-Wrone, Brady, and Cogan (2002), the measure is constructed

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<sup>4</sup>Note that the state-year fixed effects absorb the constituent term  $R_{st}$  in the interaction, as this variable is constant within state-year.

<sup>5</sup>Specifically, the normal vote is the average of the de-measured share of two-party presidential vote from the most recent two presidential elections (including the contemporaneous election if the Senate election is in a presidential year). To de-measure the presidential vote, I subtract off the average (turnout-weighted) county in that presidential election year.

<sup>6</sup>For more on how these scores are calculated, see Nokken and Poole (2004).

<sup>7</sup>In the Appendix, I also present results based on a measure constructed from Common Space NOMINATE

such that higher values indicate more extreme roll-call voting behavior (both liberal and conservative), and lower values indicate more moderate roll-call voting. Thus, the measure is calculated simply by taking the absolute value of the roll-call score, which relies on the assumption that a value of zero is perfectly moderate.<sup>8</sup>

The degree of population overlap between a television media market and a state is based on the boundaries to Nielsen’s Designated Market Areas (DMAs) and county-level population data from the Census Bureau.<sup>9</sup> This population overlap measure is referred throughout the paper as *in-state share of DMA* and is measured at the state-DMA level. The in-state share of DMA indicates the proportion of the media market’s population that resides in a given state; the range of the measure is  $(0, 1]$ . If a media market does not span multiple states then the market’s entire population resides in a single state, and the measure is coded = 1 for all counties located within the media market.

All models are estimated using ordinary least squares. The unit of analysis for all analyses that use the county-level election return data is the county-year. Throughout the paper, I present county-level estimates weighted by the number of votes cast in the Senate race in that county and election year. To an important degree, voters and electoral districts are the observational units of interest, and, thus, the weighted estimates reveal more about these units. It is worth emphasizing that the unweighted and weighted estimates are extremely similar.<sup>10</sup> For all analyses, standard errors are clustered at the state-DMA level (i.e., the level at which the treatment is assigned), as counties located within same state and media market have an identical value for in-state share of DMA measure.

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scores, and estimates using this measure are nearly identical to those using the measure calculated from Nokken-Poole scores. See Table B.2.

<sup>8</sup>Of course, the scale of dimension recovered from roll-call based scaling methods is arbitrary, so this assumption is not technically correct.

<sup>9</sup>For more details on the history of Designated Market Areas, the rarity of changes to these boundaries over time, and other information on media markets, see Chapter 1.

<sup>10</sup>See Table B.1 in the Appendix for estimates from unweighted specifications.

## 2.3 Results: Retrospective Voting

The main results are displayed in Table 2.1. Specifications (1) and (2) examine the extent to which incumbent Senate candidates are advantaged by television without accounting for ideological extremity. More precisely, the estimated coefficient on in-state share of DMA indicates the expected electoral boost (or penalty) that incumbent senators experience for a county located in an entirely in-state media market relative to a county located in an entirely out-of-state market, conditional on the normal vote in the county.<sup>11</sup> The estimated coefficient suggests that the average effect of in-state television (i.e., moving from an entirely out-of-state market to an entirely in-state market) on incumbent vote share is quite small: about 1 percentage point.<sup>12</sup> This result is broadly consistent with the results of Ansolabehere, Snowberg, and Snyder (2006) who find a near-zero effect of television on the size of senator and governor incumbency advantage. The similarity in the magnitude of the estimates is notable given that their analysis is of a different time period (the 1950s through 1990s) and uses a somewhat different empirical strategy.<sup>13</sup>

The headline results are shown in specification (2) in Table 2.1. This specification allows for the effect of in-state television to vary by the ideological extremity (as measured by roll-call scores) of the incumbent senator. In this specification with the interaction term between in-state share of DMA and ideological extremity, the estimated coefficient on the base term for the in-state share of DMA now indicates that the expected incumbency advantage from in-state television is about 4 percentage points for a “perfectly moderate” senator (i.e., a

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<sup>11</sup>While the in-state share of DMA is always greater than zero, several counties are located in media markets in which their state’s population share of the DMA is less than 0.01. Thus, the estimated coefficient on in-state share of DMA can be interpreted as moving from an approximately entirely out-of-state market to an entirely in-state market.

<sup>12</sup>Throughout the paper I use the language, “the effect of in-state television” to refer to the estimated coefficient on in-state share of DMA. This language is not precisely correct, but it is cumbersome to repeatedly write, “the effect of moving from an entirely out-of-state market to an entirely in-state market.” I apologize for the imprecision of my language.

<sup>13</sup>Specifically, Ansolabehere, Snowberg, and Snyder (2006) use a county fixed effects strategy so that their comparison is within county over time. The variation they examine is induced by changes in the incumbency status of the candidates. Additionally, they also utilize an indicator variable (rather than a continuous measure) for whether a county is located in an in-state vs. an out-of-state market.

senator with a roll-call extremity score equal to zero).<sup>14</sup> In other words, moderate senators enjoy a sizable electoral benefit from in-state television. However, the negative estimated coefficient on the interaction term implies that the electoral benefit from in-state television diminishes and actually becomes an electoral penalty for increasingly extreme roll-call voting.

Table 2.1: Incumbent Senator’s Vote Share

	(1)	(2)
County Presidential Vote	0.942*	0.942*
	(0.018)	(0.018)
In-State Share of DMA	0.012*	0.053*
	(0.005)	(0.015)
In-State Share of DMA × Roll-Call Extremity		-0.103*
		(0.038)
State-Year Fixed Effects	Yes	Yes
Observations	4,317	4,317
Clusters	318	318

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ . Analyses are weighted based on the total number of votes cast in the Senate election for a county-year. The dependent variable is the Senate incumbent’s share of the two-party vote. County presidential vote is the average share of the two-party presidential vote for the incumbent senator’s party in the most recent two presidential elections, subtracting off the national average in each election. Roll-call extremity is the absolute value of the incumbent senator’s Nokken-Poole score during the Congress in which the election is held.

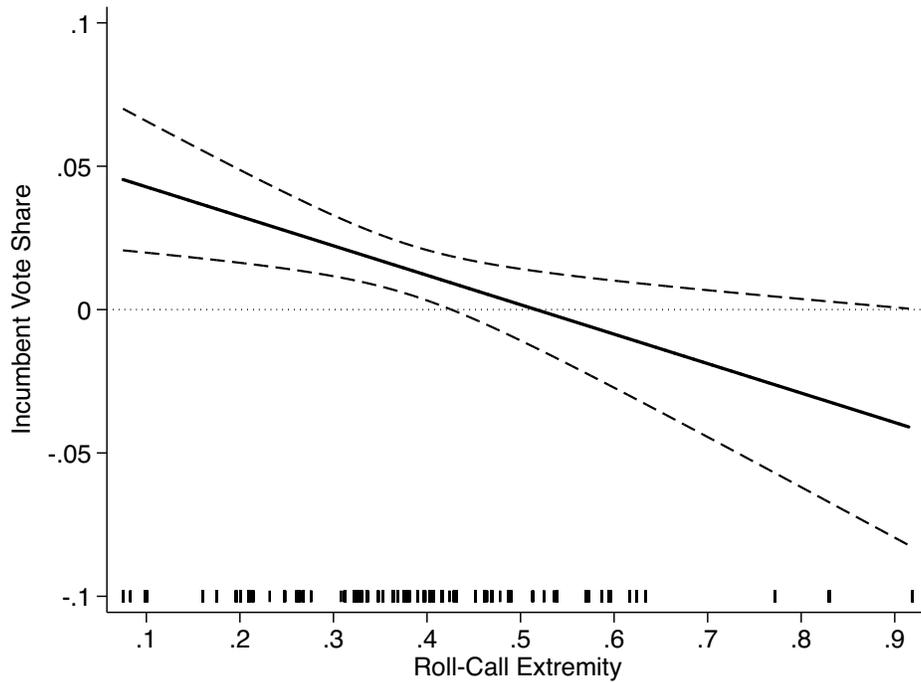
For a moderate senator with a roll-call extremity score at the 10th percentile during this time period, the expected electoral benefit of in-state television is about 2.8 percentage points. On the other hand, the expected electoral effect of in-state television for a more extreme senator with a roll-call score at the 90th percentile is a penalty of about -0.5 percentage points.<sup>15</sup> To provide some useful context for this estimate, during this time period the overall incumbency advantage for senators is estimated to be about 5 percentage points.<sup>16</sup>

<sup>14</sup>For this time period, the lowest observed value for the ideological extremity measure is 0.041 (Senator Joe Manchin from West Virginia in the 113th Congress).

<sup>15</sup>The 10th percentile of the roll-call extremity measure for senators serving during this time period is 0.191 and the 90th percentile is 0.634.

<sup>16</sup>See Section B.2 in the Appendix for the technical details on this estimate of incumbency advantage.

Figure 2.1: Estimated Coefficient on In-State Share of DMA by Roll-Call Extremity



The thick, solid line display the estimated coefficient on in-state share of DMA across the distribution of roll-call extremity, and the dashed lines represent the 95 percent confidence interval. The ticks at the bottom graph display the distribution of the roll-call extremity variable.

Thus, the interplay of in-state television and incumbent ideology plays a considerable role in the overall electoral advantage that Senate incumbents may enjoy. Figure 2.1 provides a visualization to show how the estimated effect of in-state television varies across the distribution of roll-call extremity. As the figure shows, in the moderate end of the roll-call extremity score the effect of in-state television is positive and of a large magnitude, and towards the more extreme end of the roll-call extremity score the effect of in-state television is negative and of a considerable magnitude.

While Chapter 1 demonstrates remarkable covariate balance across the distribution of the in-state share of DMA measure, which provides strong evidence for the validity of the empirical strategy, concerns may persist regarding the comparability of counties located in in-state markets and counties located in out-of-state markets. For instance, counties located in out-of-state markets tend to be geographically proximate to neighboring states,

and they tend to be more rural. To remedy these concerns, Table 2.2 contains a series of robustness checks. Specifications (1) and (2) subset the sample from all counties to a relevant set of geographically proximate counties. In specification (1), the sample is restricted to *border* counties—that is, counties with a border that is adjacent to a neighboring state. In specification (2), the sample is restricted to counties located in out-of-state media markets (i.e., counties with an in-state share of DMA  $< 0.5$ ) as well as counties *adjacent* to these out-of-state-market counties. Both the *border* and *adjacent* specifications help ensure that the comparison is between similar and geographically proximate counties some of which, due to the idiosyncratic nature of media market boundaries, happen to be located in a neighboring state’s media market and some of which happen to be located in their own state’s media market.

Table 2.2: Robustness Checks: Sample Restrictions & In-State Market Indicator

	(1)	(2)	(3)
In-State Share of DMA	0.048*	0.067*	
	(0.016)	(0.019)	
In-State Share of DMA $\times$ Roll-Call Extremity	-0.081*	-0.135*	
	(0.038)	(0.048)	
County Presidential Vote	0.915*	0.982*	0.940*
	(0.034)	(0.024)	(0.018)
In-State DMA Indicator			0.035*
			(0.012)
In-State DMA Indicator $\times$ Roll-Call Extremity			-0.064*
			(0.030)
Sample	Border	Adjacent	Full
State-Year Fixed Effects	Yes	Yes	Yes
Observations	1,686	1,457	4,317
Clusters	270	210	318

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable is the Senate incumbent’s share of the two-party vote. County presidential vote is the average share of the two-party presidential vote for the incumbent senator’s party in the most recent two presidential elections, subtracting off the national average in each election. Roll-call extremity is the absolute value of the incumbent senator’s Nokken-Poole score during the Congress in which the election is held. Specification (1) restricts the sample to counties adjacent to another state (i.e., border counties); specification (2) restricts the sample to counties in which in-state share is  $< 0.5$  or counties adjacent to such a county; and specification (3) includes the full sample of counties and uses a dichotomized version of in-state DMA (coded =1 if in-state share of DMA  $\geq 0.5$ ).

Estimates from both the border counties specification (1) and the adjacent counties specification (2) are of similar magnitude to the headline estimate from Table 2.1. Based on the estimates from the border counties specification (1), a moderate senator with a roll-call extremity score at the 10th percentile is expected to receive a 3.4 percentage point benefit from in-state television. In contrast, a more extreme senator with a roll-call score at the 90th percentile is expected to experience a -0.2 percentage point penalty. The estimates from the adjacent counties specification (2) point to even larger effects. Based on this specification, a moderate senator with a roll-call extremity score at the 10th percentile is expected to enjoy a 4.1 percentage point benefit from in-state television, while, a more extreme senator with a roll-call score at the 90th percentile is expected to experience a -1.9 percentage point penalty. Finally, the third robustness check simply employs an indicator for counties being located in an in-state DMA rather than a continuous measure.<sup>17</sup> Based on this specification, the expected effect of in-state television for a moderate senator at the 10th percentile of roll-call extremity is about a 2.3 percentage point boost, while the expected effect for a more extreme senator at the 90th percentile is about a -0.6 percentage point penalty.

The estimates reported above rely on a linearity assumption in how the in-state television effect varies across the distribution of roll-call extremity. In Table 2.3, I relax this assumption and remedy concerns about a lack of common support.<sup>18</sup> Specifically, I categorize reelection-seeking senators' roll-call extremity score into quartiles based on the distribution of roll-call extremity in the chamber during this time period. These quartile indicators are interacted with the in-state share of DMA measure in specification (1) and the in-state DMA indicator in specification (2). A couple of patterns are immediately apparent from the estimates for both specifications. First, ideologically moderate senators are expected to realize an electoral benefit of substantively important magnitude from in-state television.<sup>19</sup> Second, as the roll-

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<sup>17</sup>The threshold for being located in an in-state market is  $\geq 0.5$ . The indicator is coded = 0 for counties with an in-state share of DMA below the threshold.

<sup>18</sup>For more on these issues in interactive models, see Hainmueller, Mummolo, and Xu (2019).

<sup>19</sup>The reference or omitted category is the bottom quartile. Thus, the estimated coefficient on In-State DMA represents the effect of in-state television for senators in the bottom quartile of roll-call ideologically

call extremity increases—moving from the bottom quartile up to the second, third, and top quartiles—the in-state television effect decreases monotonically. For instance, based on specification (1), a moderate senator in the bottom quartile of the roll-call extremity distribution is expected to experience a 3.4 percentage point gain from in-state television, a senator in the second quartile is expected to experience a 1.6 percentage point benefit, a senator in the third quartile is expected to experience almost no benefit or punishment (0.3 percentage points), and an extreme senator in the top quartile is expected to experience a -1.3 percentage point penalty. Similarly, specification (2), which uses an indicator for presence in an in-state media market (rather than the continuous measure, exhibits this same monotonic pattern.

Overall, the results in this section provide strong evidence that the interplay of in-state television and the incumbent senator’s roll-call ideological extremity has important electoral consequences. While senators who engage in moderate roll-call behavior enjoy a substantial electoral benefit from in-state television, senators who engage in increasingly extreme roll-call voting erase this benefit and eventually experience a sizable electoral penalty from in-state television. In the next section, I explore the extent to which residents of counties located in in-state media markets can more effectively perceive the ideology of their senators compared to residents of out-of-state markets.

## 2.4 Results: Perceptions of Senator Ideology

The results based on aggregate-level election returns in the previous section point to in-state television as an informational tool that allows voters to reward moderate incumbent senators and punish more extremist senators. If in-state television indeed acts as an informational mechanism that allows voters to engage in more effective retrospective voting on the basis of candidate ideology, a minimally necessary precondition is that in-state television must enhance voters’ perceptions of their senators’ ideological roll-call voting

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extremity.

Table 2.3: Incumbent Senator's Vote Share

	(1)	(2)
County Presidential Vote	0.942*	0.940*
	(0.018)	(0.018)
In-State DMA	0.034*	0.028*
	(0.012)	(0.009)
In-State DMA × Roll-Call Extremity, 2nd quartile	-0.018	-0.018
	(0.015)	(0.011)
In-State DMA × Roll-Call Extremity, 3rd quartile	-0.031*	-0.030*
	(0.014)	(0.011)
In-State DMA × Roll-Call Extremity, Top quartile	-0.048*	-0.033*
	(0.017)	(0.013)
In-State DMA Measure	Share	Indicator
State-Year Fixed Effects	Yes	Yes
Observations	4,317	4,317
Clusters	318	318

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Analyses are weighted based on the total number of votes cast in the Senate election for a county-year. The dependent variable is the Senate incumbent's share of the two-party vote. County presidential vote is the average share of the two-party presidential vote for the incumbent senator's party in the most recent two presidential elections, subtracting off the national average in each election. A Roll-call extremity indicator is coded = 1 if a senator's roll-call extremity is in a given quartile (the reference group is the bottom quartile). For specification (1), In-State DMA is the in-state share of DMA measure. For specification (2), In-State DMA is a dichotomized measure (coded =1 if in-state share of DMA  $\geq 0.5$ ).

To test whether in-state television improves voters’ perceptions of senators’ ideology, I use data from the 2012, 2014, and 2016 Cooperative Congressional Election Surveys (CCES).<sup>20</sup> CCES respondents are asked to place their senators on a seven point ideological scale that ranges from “very liberal” to “very conservative” (respondents can also respond “not sure”). I re-scale this measure to range from -1 to 1 where -1 indicates “very liberal” and 1 indicates very conservative.<sup>21</sup> The roll-call scores used in this analysis to measure the ideological positioning of senators are constrained within the unit circle, so re-scaling the perceived ideology measure allows for a simpler interpretation of regression coefficients. Nevertheless, a serious methodological problem remains: a voter’s perception of a senator’s ideology does not directly map onto the ideological space of the roll-call score. In particular, voters’ understanding of what it means to be say, “somewhat liberal” may be anchored at different points on the roll-call scale.

To remedy this problem to an important degree, I investigate whether voters with greater exposure to in-state television are better able to perceive ideological differences between their two senators. To do so, I estimate the following model:

$$\mathbb{E}[\tilde{S}_{1ist} - \tilde{S}_{2ist}] = \lambda_{st} + \rho \cdot P_{ist} + \tau \cdot P_{ist} \cdot (S_{1st} - S_{2st}) + X_{ist} \cdot \beta$$

where  $i$ ,  $s$ , and  $t$  index respondent, state, and year, respectively;  $\tilde{S}_{1ist}$  is the respondent’s ideological placement of her first senator and  $\tilde{S}_{2ist}$  is the respondent’s placement of her second senator;  $\lambda_{st}$  is a state-year fixed effect;  $P_{ist}$  is the in-state population share of the respondent’s media market;  $S_{1st}$  and  $S_{2st}$  are the ideological positions of a state’s first and second senators as measured by the roll-call score; and  $X_{ist}$  is a vector of individual-level

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<sup>20</sup>I also use data from the 2010-12 CCES Panel Study, which includes questions about the direction (“yea” or “nay”) in which respondents think their senators voted on a series of roll-call votes.

<sup>21</sup>To be more precise, in the re-scaled measure,  $-1$  indicates “very liberal,”  $-\frac{2}{3}$  indicates “liberal,”  $-\frac{1}{3}$  indicates “somewhat liberal,”  $0$  indicates “middle of the road,”  $\frac{1}{3}$  indicates “somewhat conservative,”  $\frac{2}{3}$  indicates “conservative,” and  $1$  indicates “very conservative.” Individuals who respond “not sure” are excluded from the analysis. This sample restriction (i.e., conditioning on the ability to make an ideological placement of both senators) induces bias in the estimated effect of in-state television on voters’ perceptions of the ideological differences between their senators. As discussed in further detail below, this sample restriction results in understating the effect of in-state television (i.e., downward bias); Chapter 1 demonstrates that access to in-state television increases the probability that voters are able to make ideological placements of their senators and governor.

control variables.<sup>22</sup> A positive  $\tau$  indicates that the “actual” ideological difference between the two senators has a greater effect on the perceived ideological difference among respondents with greater exposure to in-state television relative to respondents with less exposure.

Table 2.4: Voter Perceptions of Senator Ideology

	(1)	(2)	(3)
In-State Share of DMA $\times$ Ideological Difference	0.096*	0.108*	0.108*
	(0.030)	(0.032)	(0.032)
In-State Share of DMA	0.004	0.009	0.009
	(0.010)	(0.010)	(0.010)
State-Year Fixed Effects	Yes	Yes	Yes
Basic Controls	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes
Observations	110,184	106,100	104,976
Clusters	333	332	332

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable is the perceived ideological difference between the senators from the respondent’s state (scaled to range from -1 to 1). Ideological Difference is the difference between the state’s two senators based on Nokken-Poole scores (ranges from -1 to 1). The constituent term for Ideological Difference is not reported, as it is absorbed by the state-year fixed effects. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include strength of partisanship and strength of ideology.

Table 2.4 displays the estimated results. Across all three specifications, the estimated coefficient on the interaction between the in-state share of DMA and the ideological difference is stable, positive, and statistically significant. These results strongly suggest that in-state television allows voters to be more perceptive to the ideological differences between their senators. One point worth emphasizing is that the sample for this analysis is restricted to individuals who are able to place both of their senators on an ideological scale. Chapter 1 finds that the in-state share of the media market substantially increases the probability that individuals are able to place their senators on an ideological scale. The consequence of this sample restriction is that the analysis almost certainly understates the full effect of in-state

<sup>22</sup>The ordering of a state’s senators as first and second is arbitrary and inconsequential for estimation. The *basic* set of control variables includes family income, gender, race, education, age, and marital status. The set of *partisan/ideology* control variables includes the strength of partisanship and strength of ideology.

television on perceptions of ideological differences.

Table 2.5 again shows estimates for an analysis of voters' perceptions of ideological differences between their senators, but this analysis restricts the sample to state-years in which neither senator was running for reelection. This analysis provides some insight as to whether the informational mechanism through which voters discern ideological differences is local television news coverage or campaign television advertisements. While residents of in-state media markets are exposed to substantially more coverage of their senators, they are also exposed to substantially more campaign ads on television (see Chapter 1). By restricting the sample to state-years in which neither senator is seeking reelection, I can better isolate the effect of local television news coverage from television ads.<sup>23</sup> If in-state television has little or no interactive effect on perceived ideological differences when neither senator is seeking reelection then that would suggest that campaign ads are the primary information source that allows voters to infer ideological differences. On the other hand, if in-state television has a similar-sized interactive effect on perceived ideological differences irrespective of whether the state's senators are seeking reelection, then that would suggest that the primary informational mechanism is local television news.

Examining the estimated coefficient on the interaction term across the three specifications in Table 2.5, the magnitude is somewhat larger than in Table 2.4 (though the difference is not statistically significant). These results strongly suggest that voters residing in in-state media markets are better able to discern senator ideology due to local news coverage and not television ads. If anything, the results point to the possibility that television ads dilute the quality of the supply of information and make voters slightly less perceptive of senator ideology, though such a claim is highly speculative given the level of precision of the estimates.

Finally, I assess the extent to which in-state television increases voters' knowledge about

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<sup>23</sup>Senators, with few or no exceptions, only air campaign ads when they are contemporaneously running for office. By examining voter knowledge when senators are not running, I am able to isolate the informational mechanism to news coverage. That said, it is still possible that past advertisements could (partially) account for voter knowledge of senator ideology.

Table 2.5: Voter Perceptions of Senator Ideology in States without Senate Elections

	(1)	(2)	(3)
In-State Share of DMA × Ideological Difference	0.144*	0.160*	0.161*
	(0.051)	(0.049)	(0.050)
In-State Share of DMA	-0.002	0.004	0.005
	(0.016)	(0.017)	(0.017)
State-Year Fixed Effects	Yes	Yes	Yes
Basic Controls	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes
Observations	53,398	51,035	50,489
Clusters	326	326	326

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The sample is restricted to state-years in which neither senator is seeking reelection.

The dependent variable is the perceived ideological difference between the senators from the respondent's state (scaled to range from -1 to 1). Ideological Difference is the difference between the state's two senators based on Nokken-Poole scores (ranges from -1 to 1). The constituent term for Ideological Difference is not reported, as it is absorbed by the state-year fixed effects. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include strength of partisanship and strength of ideology.

how their senators voted on specific roll-call votes. While the CCES does not ask respondents how their senators voted on roll calls in the 2012, 2014, and 2016 surveys, respondents to the 2010-12 CCES Panel Study are asked how they think their senators voted on a few pieces of legislation.<sup>24</sup> The empirical setup is similar, but the analysis is at the respondent-senator-legislation level:

$$\mathbb{E}[R_{isjk}] = \lambda_s + \rho \cdot P_{is} + X_{is} \cdot \beta$$

where  $R_{ijkst}$  indicates whether respondent  $i$  in state  $s$  correctly answered how senator  $j = \{1, 2\}$  voted on piece of legislation  $k = \{\text{ACA Repeal, 2011 House Budget, Keystone Pipeline, Birth Control Exemption}\}$ ;  $\lambda_{st}$  is a state-year fixed effect;  $P_{is}$  is the in-state population share

<sup>24</sup>Respondents are provided a one or two sentence description of the legislation and then asked how they think each of their senators (and representative) voted on the issues. Specifically, respondents to the 2010-12 CCES Panel are asked how their senators voted on Repeal of the Affordable Care Act, the 2011 House Budget Plan, the Keystone Pipeline, the Birth Control Exemption, and the Simpson-Bowles Budget Plan. However, the Senate did not take a roll-call vote on the Simpson-Bowles Budget Plan, so responses to this question are omitted from the analysis. Additionally, if a senator did not cast a vote on a particular roll call respondents' guesses as to how the senator voted are excluded from the analysis.

of the respondent's media market; and  $X_{is}$  is a vector of individual-level control variables.<sup>25</sup>

The estimates from the analysis of knowledge of how senators voted on roll calls are displayed in Table 2.6. Across the three specifications, the estimated coefficient is stable and statistically significant from zero. However, the magnitude of the estimated coefficient is minuscule in substantive terms. The estimated effect of in-state television is a 2.4-3.0 percentage point increase in the probability of correctly stating how a senator voted on a given piece of legislation. To put the estimates in perspective, the largest point estimate implies that an individual residing in an entirely in-state media market is expected to answer about one additional item correctly for every 33 items asked relative to an individual in an entirely out-of-state media market.<sup>26</sup>

Table 2.6: Voter Statements of How Senators Voted on Roll Calls

	(1)	(2)	(3)
In-State Share of DMA	0.030*	0.025*	0.024*
	(0.012)	(0.012)	(0.012)
State Fixed Effects	Yes	Yes	Yes
Basic Controls	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes
Observations	143,009	142,187	141,134
Clusters	314	314	314

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

The dependent variable is coded =1 if the respondent correctly states how a given senator votes on a given roll-call vote. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include strength of partisanship and strength of ideology.

Overall, while the survey results suggest that in-state television provides voters with minimal additional knowledge on their senators' positions on specific roll-call votes, exposure to in-state television does seem to provide voters with a better general idea of the ideological positioning of their senators. This pair of results nicely illustrates that voters need not learn and remember how their legislators voted on specific issues in order to sense whether their

<sup>25</sup>Because these questions are only asked in a single year, the analysis uses state fixed effects rather than state-year fixed effects, and  $t$  is no longer necessary to index year.

<sup>26</sup>By item, I simply mean how a particular senator voted on a given roll call.

legislators engage in more moderate or more extreme behavior. These findings are consistent with the account of ideological voting that Downs (1957) describes.<sup>27</sup>

## 2.5 Conclusion

This study examines the electoral consequences of the interplay between local television news coverage and senator ideology. The geography of media markets offers a quasi-random source of variation in exposure to relevant local news coverage. When voters live in a media market that is comprised mostly or entirely of residents from their own state, they receive high levels of relevant news coverage about their state’s officeholders. On the other hand, voters who live a neighboring state’s media market tend to receive little coverage about their own state’s elected officials and instead receive irrelevant coverage about their neighboring state’s elected officials. This empirical strategy helps to remedy concerns about selection bias—different types of individuals choose to consume different sources of media—that plague many studies of media effects.

While past research finds minimal effects of in-state television on incumbents’ electoral prospects (Ansolabehere, Snowberg, and Snyder 2006), this research does not test for the possibility of heterogeneous effects with respect to incumbent ideology. In this paper, I again find very modest overall effects of in-state television on the vote share of incumbent senators, but these effects vary meaningfully based on whether the incumbent has engaged in moderate or extreme roll-call voting. Moderate Senate incumbents enjoy a positive and substantively large electoral boost from in-state television of about 3 percentage points. However, as senators engage in increasingly extreme roll-call voting, the incumbency advantage from in-state television diminishes and becomes an incumbency *disadvantage* for those on the upper end of the distribution of roll-call extremity. In other words, extreme

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<sup>27</sup>“Instead of comparing government behavior with opposition proposals, he compares party ideologies and supports the one most like his own. Thus he votes on ideological competency, not on specific issues. Such behavior is rational in two situations (1) having informed himself reasonably well, the voter cannot distinguish between the parties on an issue basis, but can on an ideological basis; or (2) he votes by means of ideologies in order to save himself the cost of becoming informed about specific issues” (Downs 1957, 99)

senators experience a non-negligible electoral penalty from in-state television. For the time period under examination, the size of incumbency advantage is estimated to be about 5 percentage points, so the interplay of television and incumbent ideology plays an important role in the overall electoral prospects of Senate incumbents. This interactive relationship between television and incumbent candidate ideology is robust to alternative measures of ideology, restricting the sample to geographically proximate counties, and alternative model specifications that allow for non-linearities. Additional evidence from survey data analyses demonstrate that voters living in in-state markets are more perceptive of the ideological differences between their senators. These auxiliary analyses also strongly point to local television news, rather than television campaign advertisements, as the primary informational mechanism at work.

This study has serious implications for polarization in Congress. Local sources of news, in particular newspapers and local television news, have experienced dramatic declines in their readerships and audiences (Pew Research Center 2016). As voters have less exposure to local sources of news that provide candidate-specific information, the results from this paper imply that voters will struggle to provide electoral rewards to moderate incumbents and impose electoral penalties on extreme incumbents. If the decline of local sources of news continues, the growth in partisan polarization in Congress is also likely to continue.

## 3 | Partisan Alignment Increases Voter Turnout: Evidence from Redistricting

*Joint work with Bernard L. Fraga and Benjamin Schneer*

“...I can say this — winning’s a lot more fun.” —Richard M. Nixon in his victory speech in 1968<sup>1</sup>

### 3.1 Introduction

Decennial redistricting is among the most contentious political processes in the United States. The state officials tasked with re-drawing electoral district lines face a variety of competing interests, and, depending on the rules laid out in their state’s constitution, may have to deal with concerns that are political (protecting incumbent politicians and the seats held by their political party), legal (maintaining districts of equal population and not diluting political representation of minority groups), and practical (maintaining relatively compact districts and respecting natural boundaries). For these reasons, redistricting invariably leads to shifts in the geographical composition of congressional districts and, more often than not, the partisan composition of those districts as well.<sup>2</sup> These changes hold enormous implications for who wins seats in local, state, and federal elections, and how citizens’ policy preferences are represented in legislatures. As a result, much activity in the courts and among commentators has focused on how redistricting may shift the balance of political power within a state or in the U.S. Congress.

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<sup>1</sup><https://www.nixonfoundation.org/1968/11/victory-speech-1968/>

<sup>2</sup>One exception is states with single at-large districts.

For instance, a recent court decision in Maryland noted that state officials had “targeted Republican voters in the Sixth District by, on net, removing roughly 66,000 of them from the district and adding some 24,000 Democratic voters, thereby effecting a swing of about 90,000 voters.”<sup>3</sup> Redrawing the district lines had transformed the Sixth District from “Solid Republican” to “Likely Democratic”; plaintiffs in the case argued that these changes prevented Republican voters from electing their candidates of choice. When studying redistricting, scholars too have primarily focused on how redistricting affects the partisan composition of legislatures — devising methods for evaluating the relationship between votes cast and seats won (e.g., Kendall and Stuart 1950; Tufte 1973; King and Browning 1987; Gelman and King 1994; Stephanopolous and McGhee 2015) as well as determining whether gerrymandering has altered partisan polarization (McCarty, Poole, and Rosenthal 2009; Chen and Rodden 2013), the party system (Stephanopoulos and Warshaw 2019), or the incumbency advantage (McKee 2013).

Somewhat surprisingly, far less attention has been paid to how changes in the partisan composition of districts influence voters’ behaviors, such as turnout. In most cases, researchers have instead treated voter behavior as fixed rather than responsive to district context.<sup>4</sup> We take up this question: How does a district’s changing partisan composition influence voter turnout? To be precise, does an “alignment” between the partisan composition of the district and an individual’s party affiliation increase citizen participation in an election, and does a “misalignment” created by redistricting inhibit subsequent participation by voters?

In this paper, we examine the interplay between a person’s partisan identity, partisan context, and voter turnout, evaluating several competing explanations that might link these factors together. Redistricting reshuffles the partisan composition of districts, but precisely how redistricting alters voter behavior remains an open question. That said, several the-

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<sup>3</sup>Benisek v. Lamone, 348 F. Supp. 3d 493, 501 (D. Md. 2018).

<sup>4</sup>Research examining electoral effects (such as the effects on incumbency advantage) serve as a partial exception to this general trend (McKee 2013).

oretical traditions offer predictions about how changing partisan context should influence voters. From a theoretical perspective, both the *expressive voting hypothesis* and the *elite mobilization hypothesis* imply that turnout increases when redistricting leads a voter’s partisan identity to align with the partisan composition of the district (i.e., increasing partisan alignment). In the case of *expressive voting*, casting a vote for a winner yields greater expressive benefits, resulting in higher turnout. In the case of *elite mobilization*, higher turnout results from high-quality candidates deploying their plentiful resources to boost turnout among their supporters. The *partisan threat hypothesis*, on the other hand, implies that turnout increases with partisan misalignment as potential voters in districts where there is a chance of losing will compete harder for resources and representation due to “threat” from the other party.

To distinguish which, if any, of these theoretical perspectives stand up to scrutiny, we estimate the effect of changes in district partisanship on changes in rates of participation for individual voters. We present evidence from a real-world electoral setting and largely avoid the internal and external validity concerns that plague previous work. Our results, based on national voter file data tracking over six million individuals before and after the 2012 redistricting cycle, suggest that partisan alignment has a modest, positive effect on turnout, ruling out the *partisan threat* explanation. We then turn to longitudinal survey data to provide secondary evidence and adjudicate the extent to which mechanisms associated with *expressive voting* and *elite mobilization* yield the observed turnout patterns. This secondary evidence provides support for the *expressive voting* hypothesis.

In seeking to understand how redistricting changes both a district’s partisan composition and turnout, we highlight an additional outcome affected by this contentious political process. Participation in elections is a fundamental building block of democracy and is used as a primary indicator of democratic performance (Powell 1982). If the redistricting process nudges some citizens to vote or not, then these changes in behavior are important to know for practitioners of redistricting, scholars of elections, and the legal institutions that permit or sanction certain types of districting schemes. Ultimately, our findings indicate that elec-

toral participation is another competing interest that redistrictors (and, possibly, courts) should consider when evaluating newly drawn electoral jurisdictions.

### 3.2 Voter Turnout and Partisan Context

Early rational choice approaches to voter turnout emphasized voting as a cost-benefit calculation. As understood by Downs (1957), Riker and Ordeshook (1968), and many others, citizens should be motivated to participate when an internal calculus indicates that their vote is likely to influence the election outcome. Subsequent work challenged the empirical validity of the Downsian construct on various dimensions, but most importantly for this study, such an understanding obviates any effect of partisan alignment/misalignment on participation: on average, voters should treat aligned and misaligned districts the same way in their turnout calculus and *not* vote. Yet, competing theories suggest differential effects depending on the partisan divide within a district. These may be grouped into three broad categories: *expressive voting*, *elite mobilization*, and *partisan threat*.

The *expressive voting hypothesis* is straightforward: the expressive benefit to casting a ballot for a winning candidate is likely greater than the expressive benefit to casting a ballot for a losing candidate. For instance, Ashworth, Geys, and Heyndels (2006) find higher turnout rates in Belgian municipalities with a single dominant party, which they attribute to expressive voting. More generally, some evidence suggests that voters prefer to be a part of the winning team (e.g., Niemi and Bartels 1984; Bartels 1988; Kenney and Rice 1994).<sup>5</sup> A close relative to *expressive voting* is the *voter empowerment hypothesis*. Scholars often cite the empowerment hypothesis when examining the racial and ethnic composition of districts (Gay 2001; Barreto, Segura, and Woods 2004), where being in a district with more same-race citizens is associated with higher turnout (Hayes and McKee 2012; Fraga 2018). Applying this hypothesis to partisanship, citizens whose partisan leanings align with the partisan composition of their district grow politically empowered and more likely to feel

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<sup>5</sup>Others, such as Mutz (1997), are skeptical that voters “have an inherent desire to be on the winning team.”

effectively represented. Indeed, citizens have greater trust in their representatives and feel more efficacious in places where their party wins and the election was not close (Brunell and Buchler 2009), and across democracies we see evidence that those consistently on the losing side of electoral contests develop negative attitudes toward government in general and their own political efficacy in particular (Anderson et al. 2005).

While the hypotheses discussed so far are voter-centric, the *elite mobilization hypothesis* focuses on the behavior of officeseekers and their agents. Contact by partisan elites is at the heart of mobilization-based understandings of turnout (Wolfinger and Rosenstone 1980; Rosenstone and Hansen 1993), and empirical evidence suggests campaigns have substantively large effects on turnout in recent elections, especially for targeted groups of voters (Enos and Fowler 2016). Party and candidate contact increases turnout in congressional elections as well (Wielhouwer and Lockerbie 1994; Caldeira, Aage, and Patterson 1990), and may explain higher turnout in close U.S. House elections (Cox and Munger 1989; Jackson 1996).<sup>6</sup> Under this hypothesis, strongly partisan (safe) districts elect representatives who are likely to keep winning in the future and become incumbents. In turn, these incumbents draw on their experience and abundant resources to campaign effectively and to turn out their bases of support (Jacobson and Kernell 1983). Higher-quality incumbent candidates will expose their supporters to increased direct voter contact, media advertising, and outside endorsements, boosting co-partisan turnout. In a district where a subset of the same voters comprised a political minority whose party had essentially no chance of winning the election, these outreach efforts would exist to far less a degree or not at all. For the majority group in such districts, campaign targeting should also be facilitated (Oberholzer-Gee and Waldfogel 2005). Because the change in behavior in this theory occurs at the level of party officials, politicians, and campaign staffers — all keenly aware of the composition of the district — elite mobilization theory does not require citizens themselves to be informed about the composition of their districts.

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<sup>6</sup>Moskowitz and Schneer (2019) cast doubt on the existence of a causal relationship between competitiveness and turnout in recent U.S. House elections.

The above hypotheses predict increases in turnout for changes in district composition that align with an individual citizen’s partisanship. A final theoretical explanation — “threat” — goes in the opposite direction. If potential voters feel that they must compete harder for resources and representation when they are part of a minority in the district (due to “threat” from the opposing group), then they will be more likely to turn out after shifts in the district that make them a political minority.<sup>7</sup> This theory has primarily been applied in the context of race, where scholars have found evidence for the threat hypothesis at both a macro (Key 1949) and micro level (Enos 2016). The same logic could be applied for party, and while attempts to establish empirical evidence for explicitly *partisan* threat have not been confirmatory so far (Barber and Imai 2014), the notion that potential voters respond to the increased presence of an out-group with higher levels of participation bears further investigation.

Empirical tests to adjudicate which of these processes dominates are fraught with issues of internal and external validity. For one, strategic redistricting (whereby district lines are purposefully drawn to include or exclude voters with certain characteristics) poses a fundamental problem for making firm inferences from cross-sectional studies that focus on the effects of redistricting. Hunt (2018), in an analysis of the impact of redistricting in Florida, finds suggestive evidence that partisan alignment (misalignment) increases (decreases) turnout, but the analytical framework for this paper focuses on a single year and a single state with a highly contentious redistricting process. Other observational studies may suffer from selection problems. For example, if individuals self-select into homogeneous communities with respect to ideology or party (Motyl et al. 2014), or if members of one party sort (Cho, Gimpel, and Hui 2013; Mummolo and Nall 2016), then partisan alignment may correlate with important observed and unobserved voter characteristics that are also related to turnout. Finally, disaggregating the effects of partisan alignment from the ef-

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<sup>7</sup>Alternatively, we may consider “threat” to be greatest when the risk of loss is greatest, as in highly competitive districts. In this case, turnout should be highest in the most competitive districts, and less high in both heavily-aligned and heavily misaligned districts. We thank an anonymous reviewer to alerting us to this possibility.

fects of district heterogeneity is also not trivial, as past research has suggested heterogeneity within a district (Kaniovski and Mueller 2006) or neighborhood (Gimpel, Dyck, and Shaw 2004) may have a negative effect on turnout; or alternatively, conflict aversion may depress participation in areas with partisan and ideological diversity (Mutz 2002). Under such a framework, electoral jurisdictions would play no precise role in affecting turnout, except insofar as they reflect the underlying heterogeneity of a community.

While research in a laboratory setting sidesteps many of these problems, it faces other potential stumbling blocks for drawing broader conclusions. On the basis of experimental research, there is reason to suspect that placing voters in a lopsided district aligned with their partisanship may increase their propensity to turn out compared with voters in a misaligned lopsided district. This research finds that individuals are more likely to report their intent to participate in elections and are more likely to participate in simulated elections when they think their preferred choice will win (de Bock 1976; Ansolabehere and Iyengar 1994; Agranov et al. 2018); these findings that align best with the expressive voting hypothesis. However, the extent to which findings from a lab setting extend to voters making real-world decisions is unclear.

We attempt to balance the tradeoffs between internal and external validity by drawing on methodologies that leverage features of the redistricting process that approximate a natural experiment (Dunning 2012; Sekhon and Titiunik 2012). Importantly, we use a longitudinal approach that uses multiple snapshots of an individual’s participation before and after a change in district context. This limits the possibility that self-selection or other features tied to the types of individuals who live in a particular partisan context drive our results. Similar approaches have been used to study the impact of racial/ethnic context on voter turnout (Fraga 2016; Keele and White 2018; Henderson, Sekhon, and Titiunik 2016) and the impact of competition on voter turnout (Moskowitz and Schneer 2019). We extend these advances to the study of partisan context, providing increased internal and external validity in our empirical tests with national data probing changes in voter behavior, awareness, and campaign contacts that result from redistricting.

### 3.3 Data

As noted above, the first set of explanations suggest that turnout should be higher for individuals voting in districts where their party dominates the district, *ceteris paribus*. The *partisan threat* hypothesis, on the other hand, implies that turnout should be higher in the opposite circumstance: partisans feel threatened when their majority status is eliminated, and they are mobilized to participate. As a first step, we seek to establish the *magnitude* and *direction* of the effect of district partisan composition on voter turnout, adjudicating both between these hypotheses and the possibility of no impact of district partisan alignment. We then investigate the potential *mechanisms* behind the effects we find, further refining our understanding of what motivates individuals to vote.

To study the turnout behavior of individuals across the redistricting process, we use data from Catalist, LLC.<sup>8</sup> Catalist is a data vendor whose primary product is a “unified national voter file,” which they compile from numerous state-level and county-level voter lists across the United States. Catalist standardizes the publicly available information from voter lists, such as registration, turnout history, age, residence, gender, and race, and they routinely update the database with new information such as turnout records and changes in registration status.<sup>9</sup> Catalist further supplements the publicly available data from these voter lists with proprietary commercial data. Relevant to our purposes, Catalist tracks the individuals in their unified file across time, even as their registration may lapse or they move addresses.<sup>10</sup> Most of Catalist’s clients are progressive organizations, political action committees, and Democratic candidates, but several academic studies use Catalist data (e.g., Ansolabehere and Hersh 2012; Fraga 2016; Hersh and Nall 2016). For this study, we

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<sup>8</sup>See here for basic information on Catalist: <http://www.catalist.us/data/>.

<sup>9</sup>Some of these characteristics (e.g., race/ethnicity) are only available in certain states’ voter files.

<sup>10</sup>“[L]ongitudinal analysis of individual-level registration or turnout is a great challenge to researchers wishing to avoid contracting with a third-party organization, despite the public availability of the voter file” (Fraga 2016).

utilize a sample of the Catalist database that contains 6.4 million individuals.<sup>11</sup>

In order to determine if an individual resides in a partisan aligned or misaligned district, we require a measure of the partisan composition of the congressional district. The primary measure of district partisanship we employ is derived from the Cook Political Report’s Partisan Voting Index (PVI). The PVI is the average of the mean-deviated two-party vote share from the most recent two presidential elections. On a basic level, the PVI indicates, relative to the average congressional district, how many percentage points more Democratic or Republican a given congressional district is. A PVI of 0 indicates a 50/50 district, D+10 or R+10 represents a 60/40 district that favors the Democratic candidate or Republican candidate, respectively. For purposes of classifying districts as partisan aligned or misaligned, we use the 55/45 threshold.<sup>12</sup> An individual registered as a Republican residing in a D+6 district would be coded as “misaligned,” while a registered Republican in an R+6 district would be coded as “aligned.”

We also present secondary evidence based on the analysis of survey data. We use data from the 2010-2014 Cooperative Congressional Election Study (CCES) Panel. The 2010-2014 CCES Panel allows us to examine the extent to which voters demonstrate awareness of the partisan composition of their congressional districts as well as whether voters in partisan aligned districts report additional campaign contact. The CCES Panel is a sample of 9,500 respondents who are surveyed during the election season in 2010, 2012, and 2014. This information allows us to examine the mechanisms behind the turnout effects extracted from the voter file.

### 3.4 Research Design

Using a panel constructed from the voter files, we evaluate the evidence on whether voters situated in districts with a partisan composition aligned with their partisanship

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<sup>11</sup>This data set is a product intended for the use of academic researchers.

<sup>12</sup>Districts within the D+5 through R+5 interval are considered competitive. Districts D+6 and “greater” are considered favoring Democrats, and districts R+6 and “greater” are considered favoring Republicans.

turn out at higher rates than voters in misaligned districts. We conceptualize the “treatment” as occurring when voters reside in a district in which the partisan composition aligns with their individual partisanship. Formally, when an individual  $i$  resides in a district matching her party registration in year  $t$  and state  $s$ , then the binary indicator variable  $\text{Partisan Alignment}_{ist} = 1$ ; when this condition is not met, it takes the value 0. Redistricting provides the quasi-experimental variation in treatment. For example, by tracking individual voting behavior in 2008, which is pre-redistricting, and in 2012, which is post-redistricting, we observe how voters respond when placed into new districts through the redistricting process. Some of these voters experience a change in the partisan composition of their district and, thus, their individual partisan alignment. Importantly, we can also restrict the sample so that we only make comparisons between two voters of the same party who reside in the same congressional district in the first period.<sup>13</sup> This approach ensures that treatment and control units reside in roughly the same geographic area with the same pre-treatment electoral experience (with respect to congressional elections); their experience differs only insofar as the redistricting process shuffles one into an aligned district and one into a misaligned district.

Perhaps the greatest threat to internal validity when relying on redistricting to provide variation in partisan alignment is the possibility of strategic redistricting. Specifically, one might worry that state legislatures with partisan interests systematically move partisans into or out of a district based on characteristics including propensity to vote. For example, a Democratic legislature might go out of their way to move Republican voters who reside in a competitive district and who have a high propensity to turn out into a strong majority Republican district, while leaving Republican voters with a lower turnout propensity in the original, competitive district. If this were the case, then we might lack a valid comparison group for the individuals placed into the strong majority Republican district. The primary empirical approach that we take is designed to address these concerns. We use “block” fixed effects to ensure that comparisons occur between voters who start in the same district, share

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<sup>13</sup>Building on the framework provided in Sekhon and Titiunik (2012).

a party affiliation, have identical voting histories, and have similar demographic profiles with respect to age, sex, and race. These are the characteristics observable to those drawing district boundaries, so comparing within groups that share these characteristics guards against concerns about strategic redistricting.

To find the effect of competitiveness on turnout using block fixed effects, we estimate the turnout decision in a post-redistricting year as follows:

$$E(\text{Turnout}_{ibst}) = \alpha + \lambda_{st} + \gamma_b + \delta \cdot \text{Partisan Alignment}_{ibst} \quad (3.1)$$

where  $\alpha$  is a constant term,  $\lambda_{st}$  is a state-year fixed effect,  $\gamma_b$  is a block fixed effect, and  $\text{Partisan Alignment}_{ibst}$  is a binary indicator defined as above. We determine blocks by matching individuals exactly on the following observable characteristics: congressional district pre-redistricting, party registration, Black, Hispanic, Asian, female, age group (18-24, 25-34, 35-44, 45-54, 55-64, and over 65), turnout in 2008, and turnout in 2010.

We complement this framework with a difference-in-differences approach, in which we take advantage of the over time and across individual variation in the panel data. Consider a reduced form empirical model of the turnout decision:

$$E(\text{Turnout}_{ist}) = \alpha + \lambda_{st} + \delta \cdot \text{Partisan Alignment}_{ist} + \text{Vote Propensity}'_i \cdot \psi \quad (3.2)$$

where  $\alpha$  is a constant term,  $\lambda_{st}$  is a state-year fixed effect,  $\text{Partisan Alignment}_{ist}$  is a binary indicator defined as above, and  $\text{Vote Propensity}'_i$  is an individual's unobserved underlying tendency to vote.<sup>14</sup> By imposing the assumption that unobserved propensity to vote remains constant over time, then we can let  $\gamma_i = \alpha + \text{Vote Propensity}'_i \cdot \psi$  and estimate the model:

$$E(\text{Turnout}_{ist}) = \gamma_i + \lambda_{st} + \delta \cdot \text{Partisan Alignment}_{ist} \quad (3.3)$$

where we identify the effect of residing in a partisan aligned district based upon variation in partisan alignment over time due to redistricting. By taking this approach, we deal explicitly with the critique that the tendency to reside in a partisan aligned district might

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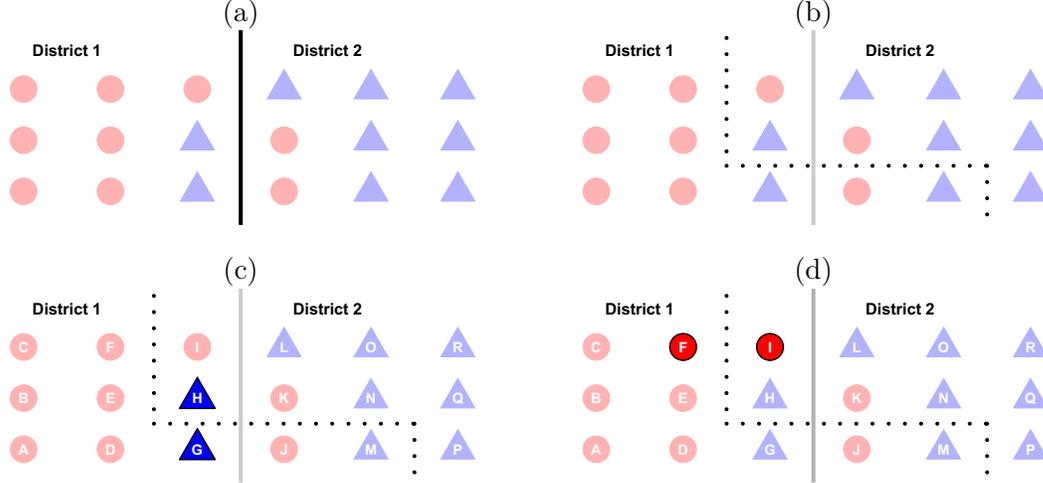
<sup>14</sup>The state-year fixed effects control for state-specific political conditions in a given election year such as gubernatorial and senatorial elections, changes to election laws, etc.

be correlated with observable and unobservable fixed individual characteristics that also affect turnout. For example, a district comprised primarily of a densely populated urban area with a high concentration of low-income voters might tend towards including many citizens whose party registration matches the partisan composition of their district (possibly boosting turnout) while also having other observable (e.g., socioeconomic status) and unobservable characteristics (e.g., lack of time/political resources) known to lower turnout. If turnout choices systematically vary with these characteristics, then estimates of the effect of individual partisan alignment on turnout would be biased if we did not condition on these variables. Since we cannot possibly measure all potential confounders, we instead employ individual and state-year fixed effects to difference out all time-invariant covariates (both measured and unmeasured) that influence turnout.

We can apply these approaches to make several different types of comparisons. Figure 3.1 provides a simplified graphical presentation. Figure 3.1a displays two districts with District 1 comprised of 7/9 Republicans and 2/9 Democrats, and District 2 comprised of 2/9 Republicans and 7/9 Democrats in the pre-redistricting period. For the sake of simplicity, suppose that Republicans (Democrats) in District 1 match on the observable characteristics that comprise a block with the other Republicans (Democrats) in the district. Part b of the figure illustrates the district composition after redistricting. In this case, the partisan composition of each district remains the same pre- and post-redistricting, but several individual voters switch districts.

First consider the comparison between citizens who experience partisan misalignment with citizens who experience partisan alignment. In this case, we identify individuals who start out (pre-redistricting) in the same, uncompetitive district where the majority of residents have an opposing partisan affiliation. This occurs in Figure 3.1c, which shows that both voters  $G$  and  $H$  begin in the pre-redistricting period as misaligned Democrats in a majority Republican district. However, after redistricting occurs,  $G$  remains in a misaligned district but  $H$  is located in the new District 2, which is aligned with  $H$ 's partisanship. Thus, in the first period both voters have a treatment status (i.e., partisan alignment) equal to

Figure 3.1: Graphical Representation of the Research Design



zero; in the second period,  $G$ 's treatment status remains zero but  $H$ , who now resides in a partisan aligned district, has a treatment status equal to one. To estimate the effect of moving from a partisan misaligned district to a partisan aligned district, we can calculate the difference in turnout in the second period, i.e.,  $\delta_{m,a}^{BFE} = H_2 - G_2$ , in which the subscripts on  $G$  and  $H$  refer to their district in the pre- versus post-redistricting periods and  $\delta_{m,a}^{BFE}$  refers to the estimate from the block fixed effects approach of the effect of moving from a partisan misaligned to a partisan aligned district. Alternatively, *if we did not form blocks* based on observable characteristics, we could also calculate a difference-in-differences estimate of  $\delta_{m,a}^{DID} = (H_2 - H_1) - (G_2 - G_1)$ .<sup>15</sup>

The two key features of these approaches are that (1) they account for time-invariant covariates that might be correlated with both partisan alignment and with turnout; and, (2) they make it possible to compare only individuals who begin in the same district and belong to the same party and, through redistricting alone, experience a changed electoral context.

We can also estimate the effects of moving from a partisan aligned district to a partisan misaligned district. Figure 3.1d illustrates a similar exercise for voters  $F$  and  $I$ , Republicans

<sup>15</sup>Further, note the link between these two estimates: because in the block fixed effects approach, blocks are formed based upon previous turnout history, we know that by definition  $H_1 - G_1 = 0$ , which implies that when looking within any one block  $\delta^{BFE} = \delta^{DID}$ .

who both begin in partisan aligned districts. The block fixed effects estimate is  $\delta_{a,m}^{BFE} = F_2 - I_2$  and the difference-in-differences estimate of partisan alignment is provided by  $\delta_{a,m}^{DID} = (F_2 - F_1) - (I_2 - I_1)$ . Finally, in instances with competitive districts (i.e., not a clear majority of Republican or Democratic voters in a district), then we may also estimate the effects of moving from a competitive district to a partisan aligned district ( $\delta_{c,a}$ ) or the effects of moving from a competitive district to a partisan misaligned district ( $\delta_{c,m}$ ) using a similar approach.

### 3.5 Main Results

Our expectation is that partisan alignment matters and, further, that it boosts rather than depresses turnout. That is, we expect that one of the elite mobilization or expressive voting hypotheses (which predict positive effects), rather than the partisan threat hypothesis (negative effects), prevails.

More formally, we are interested in testing the following hypothesis:

$$H_0 : \delta = 0 \tag{H1a}$$

$$H_A : \delta \neq 0$$

where  $\delta$  is either  $\delta_{a,m}$  or  $\delta_{m,a}$  and we perform a test with the null being that the partisan alignment/misalignment treatment has no effect. Additional evidence of a positive effect of increasing alignment can occur when examining the transition from competitive to aligned or misaligned districts. If moving from a competitive to a partisan aligned district ( $\delta_{c,a}$ ) has a greater effect than moving from a competitive district to a partisan misaligned district ( $\delta_{c,m}$ ), then this pattern helps confirm our expectations. We therefore test:

$$H_0 : \delta_{c,a} = \delta_{c,m} \tag{H1b}$$

$$H_A : \delta_{c,a} \neq \delta_{c,m}$$

A second order question is whether the effects of partisan alignment/misalignment oper-

ate symmetrically; that is, does moving from a partisan aligned to misaligned district have the same magnitude effect as moving from a partisan misaligned to aligned district? Does a move from a competitive to a partisan aligned district have roughly equal magnitude and opposite sign to a move from a competitive to a partisan misaligned district? We test for symmetry in terms of the magnitude of effects:

$$H_0^a : \delta_{m,a} = \delta_{a,m} \text{ and } H_0^b : \delta_{c,a} = -\delta_{c,m} \quad (\text{H2})$$

$$H_A^a : \delta_{m,a} \neq \delta_{a,m} \text{ and } H_A^b : \delta_{c,a} \neq -\delta_{c,m}$$

To test these hypotheses, we devise four different sets of comparisons. In the first comparison, we examine individuals who start in a partisan misaligned district in the first period; in the second period, some remain in a misaligned district and some, through the redistricting process, are placed into a partisan aligned district. In the second comparison, we examine the converse of this scenario. In the first period, all voters reside in a partisan aligned district; however, in the second period, some voters continue in the aligned district, while others are placed in a misaligned district. For the third and fourth scenarios, we instead examine individuals who are in a competitive district in the first period (i.e., one where each party’s vote share falls in the  $\pm 5$  range). We then study what happens when, in the second period, some voters remain in a competitive district and some are placed into districts with a new partisan composition (in the third scenario a partisan match and in the fourth a partisan mismatch).

Table 3.1 reports the results from each of these approaches for our preferred model specification, which uses block fixed effects to estimate effects for pooled post-redistricting election years (2012, 2014 and 2016). Panel A includes all districts, whether or not any of the residents of a particular district are shifted into a different partisan context due to redistricting — in effect, including individuals in “unredistricted” districts as controls. Panel B instead restricts the sample to pre-redistricting districts where at least some voters end up in a different partisan context post-redistricting; individuals redistricted to a new partisan

context are matched with individuals from the same initial district who do not experience a change in partisan context. Table 3.1 indicates that across all specifications, estimated effects are in the expected direction. Columns 1 and 2 provide the most straight-forward estimates; for these columns, across the years we examine, we observe effect sizes that range from slightly more than a third of a percentage point to a 1.5 percentage point increase in turnout rates attributable to partisan alignment. A 99% confidence interval does not overlap with zero for our estimates of the effect of moving from a misaligned to an aligned district; however, we cannot say the same for the estimates of moving from an aligned to a misaligned district. The direction, magnitude, and significance of these point estimates provide initial evidence supporting Hypothesis H1a.

We also can examine Hypothesis H1b, which indicates differences in the effects for individuals moving from competitive to aligned districts versus those moving from competitive to misaligned districts. If the effect of the former is larger and we can reject the null of no difference in effects, then this provides additional evidence in support of either the expressive voting or elite mobilization theories. Running this hypothesis test, we can indeed reject the null of no effect for both Panels A and B (at  $p \leq 0.05$  and  $p \leq 0.01$ , respectively). For these specifications, moving from a competitive to an aligned district has a meaningfully different effect on turnout than does moving from a competitive to a misaligned district and, specifically,  $\delta_{c,a} \geq \delta_{c,m}$ . Overall, then, in each panel, two of three hypothesis tests allow us to reject the null that partisan (mis)alignment has no effect on turnout in favor of the finding that partisan alignment has a positive effect and the finding that partisan misalignment has a negative effect.

Table 3.1: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, All Years with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
<b>Panel A: All Districts</b>				
Partisan	0.0149	0.00371	0.00698	
Aligned	(0.00495)	(0.00423)	(0.00785)	
Partisan Misaligned				-0.0149 (0.00571)
Observations	655716	1083330	578658	551433
$R^2$	0.057	0.064	0.066	0.065
<b>Panel B: Redistricted</b>				
Partisan	0.0138	0.00519	0.0106	
Aligned	(0.00347)	(0.00374)	(0.00596)	
Partisan Misaligned				-0.0189 (0.00633)
Observations	471510	820128	538329	511143
$R^2$	0.434	0.407	0.410	0.443
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Panel B matches observations in treatment group to controls from same first period CD.

All elections in Louisiana are excluded from the sample due to their unusual rules.

To test the symmetry hypothesis (H2), we have two pieces of evidence to examine. First, we can compare the estimates from models 1 and 2 (i.e., examining  $\delta_{m,a}$  versus  $\delta_{a,m}$ ). Second, we can compare the magnitudes of the estimates from models 3 and 4 (i.e., examining  $\delta_{c,a}$  versus  $-\delta_{c,m}$ ). The results here are more mixed. Comparing the effects from models 1 and 2, we can reject the null of no difference in the estimates for Panel A and for Panel B at  $\alpha = 0.10$  but not at  $\alpha = 0.05$ . Yet when comparing the magnitudes of the estimates from models 3 and 4, we cannot reject the null hypotheses in either panel. Thus, we have only limited evidence that moving from a misaligned to an aligned district has a more pronounced effect than moving in the opposite direction. That said, even though the results are not significant when comparing models 3 and 4, we observe effects of a slightly larger magnitude when moving from a competitive to a misaligned district, which cuts against the notion that moving to an aligned district is uniquely important for turnout. Based on this mixed evidence, we think it is safest to conclude that the magnitude of the effects of alignment versus misalignment are not tremendously different from one another.

In addition to the results in Table 3.1, we also estimated effects when separating midterm and presidential election years, and using the difference-in-differences approach instead of block fixed effects. We present the raw results of these analyses in the Online Appendix and summarize the effects with regard to our hypotheses in Table 3.2. The first column in the table indicates whether the tests were performed using turnout data from all election years, midterm election years only (2010 and 2014), or presidential election years only (2008 and 2012 as well as another set of results using 2008 and 2016). The second column of Table 3.2 indicates the modeling approach: difference in differences (DID) versus block fixed-effects (BFE). The “Right Direction” columns refer to whether or not the sign of the effect is in the hypothesized direction for a given hypothesis. The  $p \leq 0.05$  columns refer to whether or not the null hypothesis can be rejected at standard levels of statistical significance.

Across each combination of year and specification, we counted the number of estimates that support Hypotheses 1a and 1b (in the columns labelled Hyp. 1) and the number of estimates that support Hypothesis 2. When we evaluate Hypothesis 1, which states that

Table 3.2: Summary of Regressions of Turnout on Partisan Alignment

Year	Spec.	Hyp. 1		Hyp. 2	
		Right Direction	$p \leq 0.05$	Right Direction	$p \leq 0.05$
All Years	BFE	6/6	4/6	4/4	0/4
All Years	DID	6/6	1/6	3/4	1/4
Mid-Term	BFE	5/6	2/6	1/4	0/4
Mid-Term	DID	0/6	0/6	0/4	0/4
Pres.	BFE	8/12	7/12	4/8	0/8
Pres.	DID	12/12	7/12	7/8	0/8

moving to (away from) partisan aligned districts increases (decreases) turnout, we find that the effects operate in a direction that overall supports either the expressive voting or elite mobilization hypotheses more than three quarters of the time. When formally testing each hypothesis for a given year/specification combination, we can reject the null at  $p \leq 0.05$  slightly less than half of the time. Looking more closely across years and specifications in Table 3.2, several patterns emerge worth noting. First, we observe stronger effects of partisan alignment in presidential election years, with notably more mixed results in midterm election years. Second, the results are more likely to allow rejection of the null hypothesis of no effect under the block fixed effects approach as compared to the difference-in-differences approach, with the most notable differences occurring in midterm years.

Initially, we thought that a “learning” effect might explain the larger and more robust effects observed in the presidential election years; in this case, the effects of switching to a new district would be more mobilizing/demobilizing in the first election year after redistricting than in later years. For example, after a redistricting period, candidates might work harder to reach out to new partisans in their district thereby boosting turnout, efforts that are not as strong in subsequent contests. Such a phenomenon would explain larger effects in 2012 than in 2014. However, the results from 2016 do not appear to bear out this pattern. In fact, rather than observing effects in line with 2014 we instead observe effects for 2016 more similar in direction and magnitude to 2012.<sup>16</sup>

<sup>16</sup>Again, the raw results used to build Table 3.2 may be found in the Online Appendix.

Instead, when the House contest coincides with the presidential election campaign cycle we see more substantial effects. Either of two possibilities may be at work. First, complementarities between presidential and House campaigns might be responsible for the larger, positive effects in presidential election years. Second, potential voters may be easier to mobilize in presidential election years due to heightened attention through media coverage and the general perception of higher stakes in presidential election years. We sought to distinguish between these two options by examining whether the effect of alignment was larger in battleground versus non-battleground states during presidential elections.<sup>17</sup> However, results here were inconclusive, as we found a slight *positive* effect for battleground states in 2012 and a slight *negative* effect in 2016. It is possible that both of these mechanisms are at work without one necessarily overwhelming the other, but future work should examine the nature of this heterogeneity more fully.

When examining midterm years only, we also discover one specification that might support a version of the partisan threat hypothesis. In Table 3.2, we see that the difference-in-differences approach, when applied to 2014 turnout only, suggests a decrease in turnout when voters are assigned to misaligned districts. To determine whether this is a mere statistical artifact or if something more meaningful is at work, ideally we would have data from an additional midterm election year. Given that this data is not yet available to us, we cannot say for sure whether the negative effects we observe in this combination of year and specification fit with a larger pattern or not. However, given that we do not observe similar results for the block fixed effects specification for these same years, we do not think we have robust evidence in favor of the partisan threat hypothesis.

We also test whether the observed effects operate symmetrically using these alternative years and specifications. As with our main specification presented in Table 3.1, we do not find convincing evidence that the effects of alignment differ in magnitude from misalignment. In the Table 3.2 column labelled “Right Direction” under Hypothesis 2, we count the share of instances in which the effects of alignment are positive and the effects of misalignment

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<sup>17</sup>We determined battleground status by referring to the classifications made in Enos and Fowler (2016).

are negative. This occurs the majority of the time, with the notable exception of the midterm difference-in-differences approach mentioned above. When we test Hypothesis H2 formally, which compares the magnitudes of the positive versus negative effects, we cannot reject the null in the vast majority of cases. This finding suggests that, for these additional specifications and subsets of the data, we do not have much evidence to suggest that partisan alignment or misalignment operate very differently from one another (i.e., alignment appears to be as mobilizing as misalignment is demobilizing).

Taking all of the evidence presented so far, we appear to be able to reject the null hypothesis of no effect of partisan alignment. Furthermore, most estimates have values consistent with positive effects of partisan alignment on turnout. Though the results are less clear for one type of specification in midterm election years, in the vast majority of our analyses we see results consistent with one of the two hypotheses (elite mobilization or expressive voting) that predicts a boost in turnout resulting from partisan alignment.

While the empirical approach here explicitly addresses most threats to inference surrounding redistricting, several challenges remain. Perhaps chief among these challenges is the idea that redistricting represents more than just a change in partisan composition but rather a bundle of different treatments. For example, redistricting may change the partisan composition of a district along with the racial composition. If racial composition changes in tandem with partisan composition, then the former might drive changes in behavior rather than the latter — and we might attribute changes in turnout to changes in partisan composition when in fact racial composition was the salient factor.

### **3.6 Secondary Evidence: Voter Awareness & Campaign Contact**

Section 3.5 provides evidence that assignment to a partisan aligned district leads to a modest increase in turnout as compared to residing in a misaligned district. In this section, we utilize survey data to examine the mechanisms producing our finding and, to the extent

possible, adjudicate between the *expressive voting* and *elite mobilization* hypotheses laid out in Section 3.2. Specifically, survey data allows us to explore whether patterns of voter awareness and self-reported campaign contact coincide with the increases in turnout that we have observed. Are voters aware of their district’s partisan composition? Do voters experience more campaign contact when they are situated in a partisan aligned district relative to a misaligned district?

We begin by using data from the 2010-2014 Cooperative Congressional Election Study (CCES) Panel to investigate if citizens are aware of the partisan composition of their congressional districts. The 2010-2014 CCES Panel tracks 9,500 respondents to the 2010 CCES through the 2014 election.<sup>18</sup> In 2012 and 2014, CCES Panel respondents were asked: “How would you describe the new Congressional District you live in?” They could respond: “Most people are Democratic,” “Most people are Republican,” or “My district is a mix with no single dominant party.”<sup>19</sup> We create a perception of district partisanship variable that takes a value of  $-1$  if the respondent states that the district is mostly Republican,  $0$  if the respondent states that the district is mixed, and  $1$  if the respondent says the district is mostly Democratic.

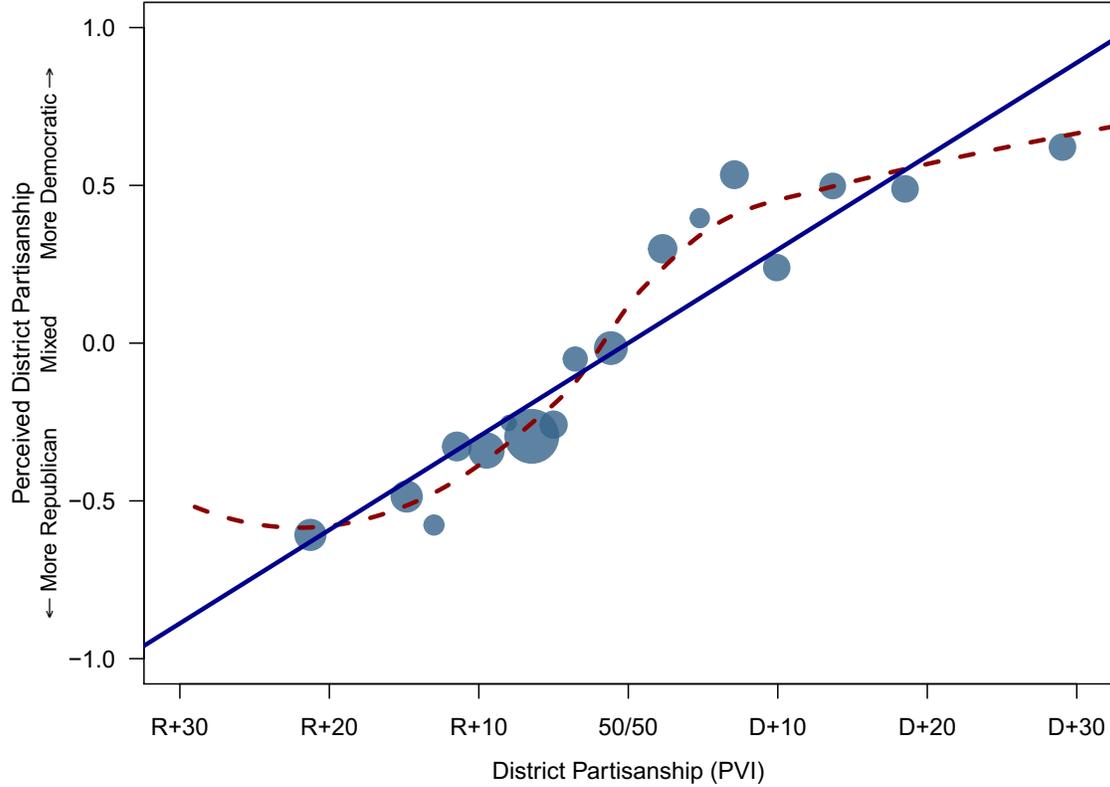
Figure 3.2 plots respondents’ perceptions of their district partisanship in 2012 against a measure of the actual district partisanship (PVI) in 2012. We see that voters are aware of their district’s partisanship to an impressive degree: respondents situated in Democratic districts tend to indicate that they are in mostly Democratic districts, those in competitive districts largely indicate that there is a mix, and respondents in heavily-Republican districts correctly indicate that most people in their district are Republicans. On its face, therefore, respondents seem able to infer when they are in a circumstance in which their party is likely to win, versus congressional districts in which their party is likely to lose; a key requirement for expressive voting to take place.

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<sup>18</sup>The 2010 CCES had 55,400 respondents.

<sup>19</sup>As in our analyses using voter file data, we restrict to respondents who did not move throughout the period of the panel. This question is only asked to panel respondents (i.e., it is not included in the CCES Common Content for 2012 or 2014).

Figure 3.2: Voters' Perceptions of District Partisanship, 2012



This figure demonstrates that voters are largely aware of their congressional districts' partisan composition. This binned scatterplot is based on tabulations of 2012 data from the 2010-2014 CCEs Panel. Each point in the figure corresponds to a local mean and is proportional in size to the number of observations within the locale. The dark blue, solid line is based on a linear regression and the red, dashed line is based on a locally weighted regression.

The analysis in Figure 3.2 indicates that voters correctly identify the partisan composition of their congressional district. However, such a relationship is not definitive evidence that voters are able to perceive the *changes* in district composition that produce the turnout effects we find above. Perhaps voters (correctly) guess the the partisan composition of their immediate surroundings (due to sociodemographic features of their neighborhood, for instance), and use this information as a proxy for the partisan composition of their congressional districts. Based on our research design in Section 3.5, variation in congressional district partisanship is induced by changes to district boundaries. If our turnout effects are attributable to expressive voting, therefore, voters must also be aware of changes to the

partisan composition of their district that result from redistricting.

In Table 3.3, we conduct a more stringent test of voters' ability to judge their congressional district's partisan composition. While model (1) shows the results for the linear regression in Figure 3.2, models (2) and (3) account for the partisanship of each respondent's *pre*-redistricting congressional district. In model (2), we include fixed effects for respondents' "old" congressional district. By conditioning on the pre-redistricting congressional district, we can isolate voters' perceptions of their current district partisanship from the partisanship of their previous district. In model (3), we take a different approach with the same objective in mind: we condition on the partisan composition of the pre-redistricting congressional district ("lagged PVI"). While the magnitude of the estimated coefficients from models (2) and (3) is somewhat smaller than the naive estimate from model (1), models (2) and (3) still indicate a statistically (and substantively) significant ability of respondents to perceive their congressional district's partisan composition independent of the partisanship of their previous district.<sup>20</sup> In sum, citizens demonstrate awareness of the partisan composition of their congressional district, even when they are in a "new" partisan circumstance in the first election after redistricting.<sup>21</sup>

While awareness of the partisan composition of the congressional district is a necessary precondition for individuals to engage in expressive voting, there may be other observable implications of this mechanism. For instance, individuals who engage in expressive voting should also have greater awareness of their party's candidate, that is, their "team leader." In all three years of the 2010-2014 CCES panel, respondents are asked to rate the Democratic and Republican House candidates in their district in terms of competence and personal integrity. Respondents are also asked to place these candidates on an ideological scale.<sup>22</sup> For

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<sup>20</sup>Moving from a 60/40 pro-Republican district (R+10) to a 60/40 pro-Democratic district (D+10) is associated with about a 0.4 increase in perceived district partisanship.

<sup>21</sup>While we focus on perceived district partisanship in 2012, results are extremely similar for perceptions of district partisanship in 2014. See Figure C.1 and Table C.15 in the Online Appendix.

<sup>22</sup>The text of the competence and personal integrity question is as follows: "Please rate the following characteristics of the [Democratic/Republican] candidate [INSERT NAME] for the U.S. House in your district...[Competence / Personal integrity]." Respondents then rate how strong or weak the candidate is on

Table 3.3: Perceived Partisan Composition of District | 2012

	(1)	(2)	(3)
PVI	0.0296 (0.0014)	0.0213 (0.0023)	0.0205 (0.0029)
Lagged PVI			0.0102 (0.0029)
Constant	0.0013 (0.0193)	-0.0087 (0.0075)	0.0019 (0.0192)
Observations	7611	7611	7611
$R^2$	0.257	0.444	0.264
Old District FEs	No	Yes	No

Standard errors, clustered by congressional district, are in parentheses.

all three of these questions, respondents can choose the option: “Not sure.” We investigate whether individuals situated in partisan aligned districts are better able to make an evaluation of their party’s candidate (i.e., less likely to respond “not sure”).<sup>23</sup> It is worth noting that, for a given characteristic, candidate, and year, approximately 35-40 percent of all individuals are unable to make an evaluation. Because this question is asked both pre- and post-redistricting, we can examine whether the same individual is more or less likely to evaluate her party’s candidate under varying conditions of partisan (mis)alignment.<sup>24</sup> By including individual and state-year fixed effects, we implement a research design similar to our study of turnout reported in Section 3.5 with many of the same benefits (e.g., protecting

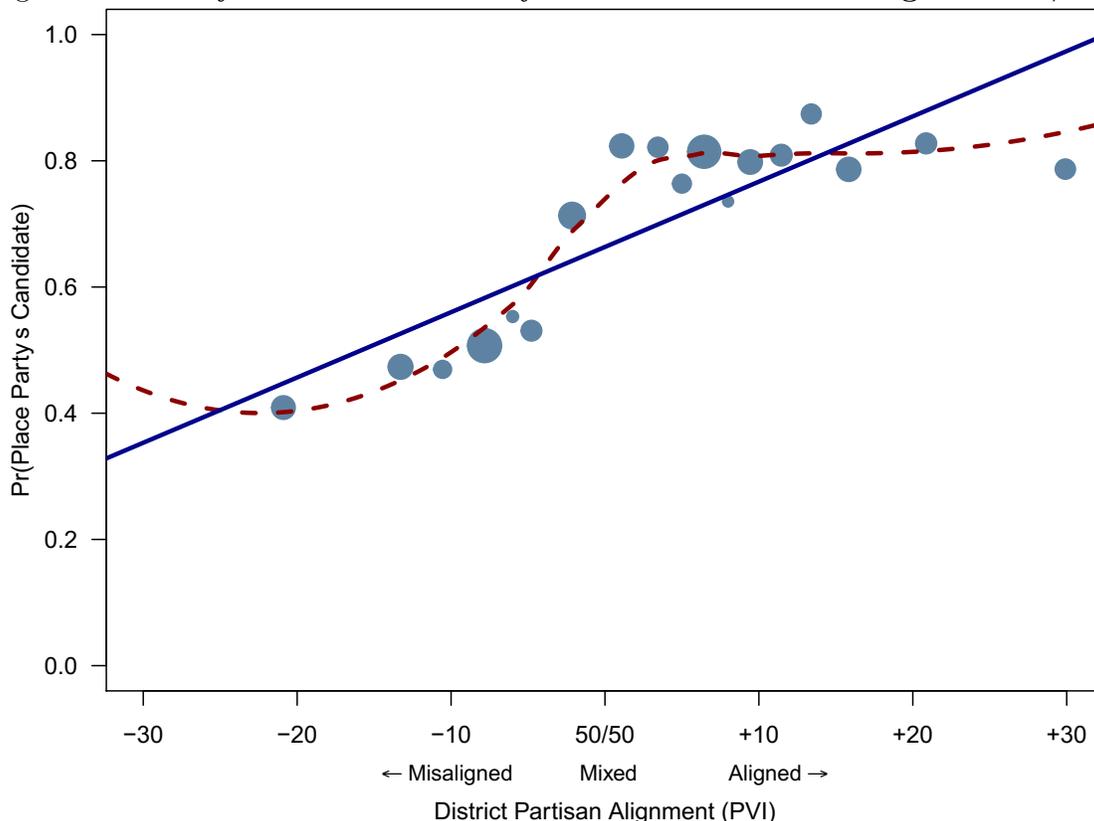
a seven-point scale, or they respond: “Not sure.” For placing the candidate on an ideological scale, respondents are asked: “How would you rate each of the following individuals and groups...[INSERT NAME].” Respondents then rate the candidate on a seven-point scale from very liberal to very conservative, or they respond: “Not sure.”

<sup>23</sup>In other words, if the respondent makes an evaluation of her party’s candidate for a given characteristic in a given year,  $Evaluation_{ict}$  is coded = 1 (where  $i$  is the index for respondents in the CCES Panel,  $c$  is the set of characteristics to be evaluated: {competence, personal integrity, ideology}, and  $t$  is the set of years in the CCES Panel: {2010, 2012, 2014}). If the respondent cannot make an evaluation of her party’s candidate and instead responds not sure,  $Evaluation_{ict}$  is coded = 0.

<sup>24</sup>The partisan-aligned district variable is coded = 1 if the respondent is in a district aligned with her partisan identity (D+6/R+6 or greater) and coded = 0 otherwise; the competitive district variable is coded = 1 if the respondent is in a district where the PVI is within the interval of R+5 through D+5 and coded = 0 otherwise; and the set of misaligned voters is the reference group for the regression. We define respondents’ partisan identities based on their 2010 responses in the CCES Panel. Partisan leaners as well as weak and strong partisans are all included. Non-leaning independents are not included in these analyses.

against time-invariant confounding).<sup>25</sup>

Figure 3.3: Ability to Place Their Party's Candidate on an Ideological Scale, 2012



This figure demonstrates that voters in congressional districts aligned with their partisanship are better able to place their party's candidate on an ideological scale. This binned scatterplot is based on tabulations of 2012 data from the 2010-2014 CCES Panel. Each point in the figure corresponds to a local mean and is proportional in size to the number of observations (accounting for sampling weights) within the locale. The dark blue, solid line is based on a linear regression and the red, dashed line is based on a locally weighted regression.

We begin by reporting the cross-sectional relationship between the degree of partisan alignment and the probability respondents can place their party's candidate on an ideological scale.<sup>26</sup> Relative to voters residing in misaligned districts, voters in partisan aligned districts

<sup>25</sup>These results are based on candidate evaluations made in 2010 and 2014. We opt to use 2010 and 2014 because the 2010-2014 CCES Panel does not have candidate evaluations for 2008. As in the previous section, we exclude respondents who experience uncontested races, races between two Democrats or two Republicans, and respondents living in Louisiana due to their unique electoral rules.

<sup>26</sup>While we show the relationship between partisan alignment and ideology for 2012, the patterns are generally similar for other characteristics and other years.

Table 3.4: Ability to Evaluate Their Party’s Candidate

	(1)	(2)	(3)
	Competence	Integrity	Ideology
Partisan-aligned district	0.1728 (0.0551)	0.1121 (0.0524)	0.1424 (0.0634)
Competitive district	0.0705 (0.0510)	0.0084 (0.0569)	0.0729 (0.0584)
Observations	10734	10782	10650
$R^2$	0.069	0.066	0.070
Individual FEs	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses. The dependent variable is coded =1 if the respondent is able to make evaluation about their party’s candidate.

have a greater ability to place their party’s candidate on an ideological scale. In Table 3.4, we report the more rigorous test using our panel research design for ideological placement, competence, and integrity. Here the omitted category is a misaligned district. We see that, relative to being situated in a misaligned district, individuals placed in a partisan aligned district are 17 percentage points more likely to make a competence evaluation, 11 percentage points more likely to make a personal integrity evaluation, and 14 percentage points more likely to make an ideological placement. Voters situated in an aligned district have greater awareness of their party’s candidate, again supporting the expressive voting hypothesis.

While voters’ greater ability to “say something” about their party’s candidate (i.e., make an evaluation) accords with expressive voting, it could also be consistent with elite mobilization. Perhaps voters have greater knowledge about their party’s candidate directly as a result of the campaign efforts of that candidate. Importantly, the results in Table 3.4 do not imply that voters’ awareness of their party’s candidate is higher in competitive districts, which are the places where incentives to raise candidate salience are strongest. Nevertheless, we search for any evidence in support of the elite mobilization hypothesis.

Elite mobilization-based explanations of partisanship tend to focus on voter mobilization and contact. Conveniently, CCES panel respondents report if campaigns contacted them

during the election and, if so, the methods through which they received contact.<sup>27</sup> Focusing in particular on this channel of direct voter outreach from campaigns to voters makes sense given that Enos and Fowler (2016) find evidence of very large turnout effects from this mode of campaigning. On the other hand, an abundance of studies suggest that television advertising has minimal or no effect on turnout (Ashworth and Clinton 2007; Huber and Arceneaux 2007; Krasno and Green 2008; Vavreck 2007). As a result, if elite mobilization is responsible for the turnout effects reported in the previous section, it likely manifests via direct voter outreach. Again, we first show the cross-sectional relationship between the degree of partisan alignment and the probability respondents report any contact from a campaign in 2012 in Figure 3.4.<sup>28</sup> As is clear from both the locally weighted regression and the very flat linear regression, individuals residing in aligned districts do not appear to report greater campaign contact; contact increases slightly in more competitive districts but is relatively lower in both aligned and misaligned districts.<sup>29</sup>

Results from our more rigorous panel research design for the “any” campaign contact outcome as well as the individual methods of contact outcomes are reported in Table 3.5. For nearly all of the outcomes, being situated in an aligned district relative to a misaligned district seems to have little or no effect on reported campaign contact. With one exception, the estimated coefficients have a substantively small magnitude and are not significant from zero. We do find that being situated in either a partisan aligned district or a competitive district increases the probability of reporting campaign contact via email or text message by about 8 percentage points relative to misaligned districts. While 8 percentage points might at first glance seem like a large effect, given the relatively small turnout effects from most impersonal forms of campaign contact, an 8 percentage point increase in reported email/text

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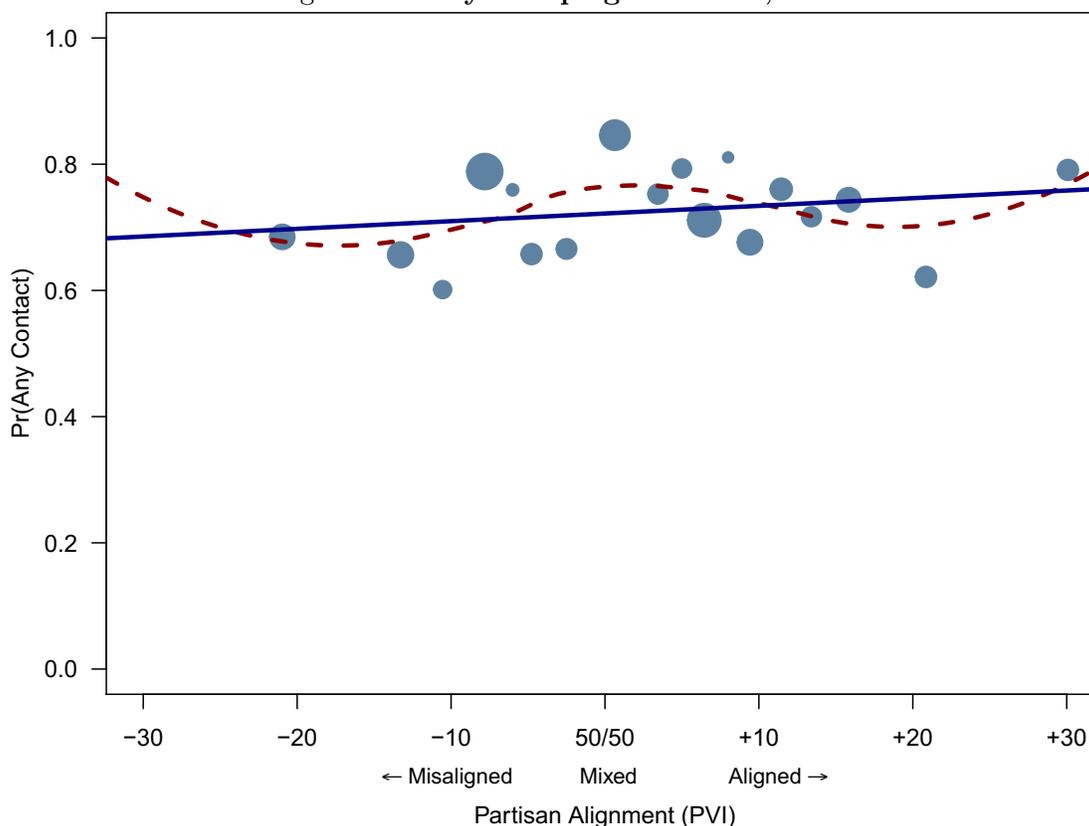
<sup>27</sup>Respondents are asked: “Did a candidate or political campaign organization contact you during the [INSERT YEAR] election?” If respondents answer “yes,” they are then asked: “How did these candidates or campaigns contact you...[in person / phone call / email or text message / letter or post card.]”

<sup>28</sup>We show this relationship for 2010, 2012, and 2014 in the same plot in Figure C.2 in the Online Appendix.

<sup>29</sup>The loess curve begins to increase again in the extreme ends of both aligned and misaligned districts, but very few CCES respondents are in these parts of the distribution.

message contact likely only translates into a minuscule increase in turnout.<sup>30</sup>

Figure 3.4: Any Campaign Contact, 2012



This figure demonstrates that voters in congressional districts aligned with their partisanship do not report substantially more campaign contact than voters in misaligned districts. This binned scatterplot is based on tabulations of 2012 data from the 2010-2014 CCES Panel. Each point in the figure corresponds to a local mean and is proportional in size to the number of observations (accounting for sampling weights) within the locale. The dark blue, solid line is based on a linear regression and the red, dashed line is based on a locally weighted regression.

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<sup>30</sup>In a meta-analysis of both published and unpublished experimental studies on turnout, Green, McGrath, and Aronow (2013) finds that most impersonal interventions have substantively small effects on turnout. Unfortunately, there are relatively few studies of the turnout effects of emails and text messages. In the case of email, few studies find positive effects on turnout. The evidence on text messages is sparse, but Malhotra et al. (2011) finds a 0.79 percentage point increase in turnout with a “cold” text message, while Dale and Strauss (2009) reports an extremely large 4.1 percentage point (treatment-on-the-treated) effect on turnout. Green, McGrath, and Aronow (2013) describes the “effectiveness of text messaging [as]...an intriguing anomaly” given that “impersonal tactics...tend to produce weak effects...” Nevertheless, our estimated 8 percentage point increase in reporting email/text message contact would only translate into a 0.3 percentage point increase in turnout, based on an estimated turnout effect of 4.1 percentage points of text messages. Of course, some of the respondents who reported contact via email or text message received emails rather than text messages.

Table 3.5: Reported Campaign Contact

	(1)	(2)	(3)	(4)	(5)
	Any	In-Person	Phone	Mail	Email/Text
Partisan-aligned district	-0.0017 (0.0452)	0.0353 (0.0369)	-0.0467 (0.0499)	0.0519 (0.0609)	0.0793 (0.0394)
Competitive district	0.0083 (0.0284)	0.0253 (0.0243)	0.0007 (0.0313)	0.0335 (0.0483)	0.0799 (0.0362)
Observations	12444	12444	12444	12444	12444
$R^2$	0.060	0.034	0.071	0.036	0.046
Individual FEs	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting CD level, are in parentheses.

The dependent variable is coded =1 if the respondent reported campaign contact.

In sum, the analyses of survey data presented in this section provide both corroboratory evidence for the effect of partisan alignment on turnout and indications for which mechanisms likely produce the turnout boost. Voters demonstrate strong awareness of the partisan composition of congressional districts, and are more likely to rate their party’s candidate when situated in a partisan aligned district relative to a misaligned district or a competitive district. On the other hand, voters do not consistently report more campaign contact when situated in partisan aligned districts, implying that greater campaign activity is unlikely to be the explanation for increased turnout. Taken together, these findings provide relatively strong support for the expressive voting hypothesis and limited support for the elite mobilization hypothesis.

### 3.7 Discussion

When analyzed appropriately, the redistricting process approximates a natural experiment that induces considerable variation in the partisan composition of voters’ congressional districts. The re-drawing of district boundaries allocates some voters into districts aligned with their partisanship and other voters into misaligned districts. Analyzing panel data tracking over 6 million voters pre- and post-redistricting, we determine how turnout changes in response to shifting conditions of partisan (mis)alignment. We find a positive

effect of about a 0.3-1.5 percentage point increase in turnout for voters assigned to a partisan aligned district relative to those assigned to a misaligned district. Examining potential mechanisms behind this effect, our analyses of survey data show that voters are highly perceptive of their congressional district's partisan composition, and individuals situated in partisan aligned districts are much more likely to be able to evaluate their party's candidate. However, voters placed in aligned districts, for the most part, do not report significantly more campaign contact relative to voters in misaligned districts. On the whole, evidence from both the voter file and survey data is supportive of the *expressive voting hypothesis* and the limited evidence of additional campaign contact in partisan aligned districts casts doubt on mechanisms associated *elite mobilization hypothesis*. The positive effect of partisan alignment on turnout is at odds with the *partisan threat hypothesis*, whether we think that threat is greatest in lopsided districts in favor of the other party or competitive districts: when individuals are redistricted into districts where co-partisans make up a larger share of the electorate, turnout increases.

Our findings thus provide a notable update to recent research (Moskowitz and Schneer 2019) that finds no discernible effect of competition on turnout. One possible explanation for this inconsistency with previous work is that the partisan alignment of districts and voters affects turnout in more subtle ways. Measures of competitiveness account for the closeness of an election outcome, but as elections become less close these measures do not account for the direction that the district is moving from the perspective of individual voters. Our findings suggest that we might observe no overall changes in turnout as the composition of a district moves, for example, from 50/50 to 60/40 Republican, but the levels of turnout between partisans are likely still shifting under the surface, with Republican turnout increasing and Democratic turnout decreasing. We find the most convincing support for this account in the models where we separately study the effects of moving from a competitive district to a partisan aligned district and moving from a competitive district to a partisan misaligned district. In these instances, we find effects with opposite signs, consistent with the account that changes in competition could have near zero effects overall due to effect heterogeneity

depending on an individual's partisan alignment.

The potential asymmetry in the effects we find also deserves further attention given its implications for our understanding of voter turnout. If researchers can show convincingly that movements from an aligned to misaligned district have smaller magnitude effects than movement from misaligned to aligned districts, then such a finding might provide some evidence in favor of habit formation as applied to voting (Meredith 2009). The logic here is that partisan alignment might provide an initial boost to turnout that persists due to habit even after a switch to a mismatched districts where the expressive benefits of casting a vote for the winning side no longer exist.

These findings have broader implications for debates about redistricting. A cursory glance at our findings would seem to support research by Brunell and Buchler (2009) and Brunell (2010) that suggests “packing” partisans into uncompetitive, homogeneous districts may improve citizens’ perceptions of the representation they receive. Given findings that “packing” is an optimal strategy for those seeking partisan advantage through redistricting (Friedman and Holden 2008), the turnout increases we find with partisan alignment do indeed suggest partisan gerrymandering can yield unexpected benefits. But to whom will these benefits accrue? While courts have struggled to determine a clear standard for partisan gerrymandering (Lowenstein 2006; Stephanopolous and McGhee 2015), understanding turnout effects is important for courts evaluating partisan gerrymandering on basis of free speech or equal protection arguments. If voting is a protected form of speech, and presence in a misaligned district reduces turnout, then partisan gerrymandering systematically benefiting one party over the other (which cuts to the heart of gerrymandering’s aim) may violate this principle.

Clashes in democratic goals also persist when thinking about partisan representation. A previously unappreciated trade-off may exist between state- or national-level election outcomes and congressional election outcomes. Gerrymanders that pack partisans into just a few districts may have the effect of increasing residents’ turnout above expected levels. Such a gerrymander would limit the number of congressional seats won by the party, but due

to the higher turnout, may also *help* the chances of the “gerrymandered” party in statewide elections. Court cases that evaluate the effects of redistricting often consider the probability of a party or candidate winning an election given different potential changes in district lines. However, the statistical models used in these efforts all assume that turnout does not change as a result of the redistricting process itself. By estimating how partisan context influences turnout, the results of this paper could be useful for evaluating the true impact of a hypothetical districting plan.

Finally, our results provide evidence of the challenge to democratic legitimacy produced by “losers” in the electoral process. Building on Hirschman (1970), Anderson et al. (2005) indicate that consistent losers may “exit” from the political process, undermining the legitimacy of subsequent election outcomes. We show that those likely to end up on the losing side of elections as a result of redistricting do indeed “exit,” dropping out of the electoral process in subsequent elections. Thus if the potential turnout boost for partisans placed in heavily-favorable districts must be offset by a decrease in voting among out-partisans, the normative implications of our work indicate that it may be better to produce competitive jurisdictions where no single group always ends up on the losing side of the electoral divide. Winning might be more fun, but an inclusive electoral process may yield long-run benefits that outweigh the turnout gains that we find.

The boundaries of electoral districts often determine who gets what in politics. We demonstrate that the partisan consequences of redistricting extend beyond the representation individuals receive, shaping individual involvement in the political process itself. As affective polarization increases and the electorate increasingly views democracy as a clash of identities (Mason 2018; Iyengar et al. 2019), we provide yet another piece of evidence that party shapes political life.

# A | Appendix to Chapter 1

## A.1 Data Sources

- In-State Share of DMA
  - Nielsen DMA boundaries
  - Census Bureau’s Annual Estimates of the Resident Population by county<sup>1</sup>
  - Population data from the 2010 Census for ZIP Code Tabulation Areas<sup>2</sup>
- National rate of TV households subscribing to multichannel video programming distributor
  - Nielsen estimates compiled from FCC Annual Reports on the Status of Competition in the Market for the Delivery of Video Programming<sup>3</sup>
- County-level rate of households subscribing to multichannel video programming distributor
  - SNL Kagan MediaCensus

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<sup>1</sup>See <https://www.census.gov/data/tables/2017/demo/popest/counties-total.html#tables>.

<sup>2</sup>Data from the 2010 Census Summary File 1 at the ZCTA level retrieved through American FactFinder ([factfinder.census.gov](http://factfinder.census.gov)), Table P1.

<sup>3</sup>See [https://apps.fcc.gov/edocs\\_public/attachmatch/DA-17-71A1.pdf](https://apps.fcc.gov/edocs_public/attachmatch/DA-17-71A1.pdf), [https://apps.fcc.gov/edocs\\_public/attachmatch/FCC-15-41A1\\_Rcd.pdf](https://apps.fcc.gov/edocs_public/attachmatch/FCC-15-41A1_Rcd.pdf), [https://apps.fcc.gov/edocs\\_public/attachmatch/FCC-07-206A1.pdf](https://apps.fcc.gov/edocs_public/attachmatch/FCC-07-206A1.pdf), [https://apps.fcc.gov/edocs\\_public/attachmatch/FCC-05-13A1.pdf](https://apps.fcc.gov/edocs_public/attachmatch/FCC-05-13A1.pdf), [https://apps.fcc.gov/edocs\\_public/attachmatch/FCC-02-338A1.pdf](https://apps.fcc.gov/edocs_public/attachmatch/FCC-02-338A1.pdf), [https://apps.fcc.gov/edocs\\_public/attachmatch/FCC-01-1A1.pdf](https://apps.fcc.gov/edocs_public/attachmatch/FCC-01-1A1.pdf), <https://transition.fcc.gov/Bureaus/Cable/Reports/fcc96496.txt>, <https://www.fcc.gov/Bureaus/Cable/Reports/fcc95491.zip>, <https://transition.fcc.gov/Bureaus/Cable/Reports/fcc98335.pdf>, [https://www.tvb.org/Portals/0/media/file/TV\\_Households.pdf](https://www.tvb.org/Portals/0/media/file/TV_Households.pdf), and [https://apps.fcc.gov/edocs\\_public/attachmatch/FCC-12-81A1\\_Rcd.pdf](https://apps.fcc.gov/edocs_public/attachmatch/FCC-12-81A1_Rcd.pdf).

- Five-year estimates (2012-2016) on the number of households from the Census Bureau’s American Community Survey county-level summary file<sup>4</sup>
- Local television news coverage of governors and senators
  - The Internet Archive’s Television News Archive<sup>5</sup>
- Survey data on voter knowledge, ticket splitting, and individual-level characteristics
  - 2012 and 2016 Cooperative Congressional Election Studies<sup>6</sup>
- Precinct-level measure of split-ticket voting
  - 2012 Harvard Election Data Archive<sup>7</sup>
- Ballot-level data to assess validity of aggregate measures of split-ticket voting
  - 2016 South Carolina Election Audit<sup>8</sup>
- Airings of political advertisements
  - 2012 Wesleyan Media Project<sup>9</sup>
- Universe of television stations affiliated with major networks, studio addresses, and status as a parent, satellite, or translator station
  - FCC Public Inspection Files<sup>10</sup>
  - Kantar Media’s Standard Rate & Data Service (SRDS) Media Planning Platform<sup>11</sup>
  - TVNewsCheck<sup>12</sup>

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<sup>4</sup>Data retrieved through American FactFinder (<https://factfinder.census.gov>), Table S1101.

<sup>5</sup>Data retrieved through the GDELT API (<https://blog.gdeltproject.org/gdelt-2-0-television-api-debuts/>).

<sup>6</sup>See <https://cces.gov.harvard.edu/>.

<sup>7</sup>See <https://hdl.handle.net/1902.1/21919>.

<sup>8</sup>See <https://www.scvotes.org/election-audits-south-carolina>.

<sup>9</sup>See Fowler, Franz, and Ridout (2015) and <http://mediaproject.wesleyan.edu/>.

<sup>10</sup>See <https://publicfiles.fcc.gov/>.

<sup>11</sup>See <http://next.srds.com/home>.

<sup>12</sup>See <https://tvnewscheck.com>.

## A.2 Media Markets: Nielsen Designated Market Areas

The Nielsen company assigns counties to Designated Market Areas (DMAs) based on the television viewing patterns of residents in each county. In the past, Nielsen exclusively utilized viewer diaries to collect data on viewing patterns (Federal Communications Commission 1999). Nielsen increasingly utilizes electronic measurement methods to collect data on viewing patterns, especially in large markets (Nielsen Company 2014). Nielsen DMAs generally define the boundaries of local markets for stations' exclusive rights to network and syndicated content (Government Accountability Office 2015). The FCC also uses DMAs to define market areas for mandatory carriage as well as limits on ownership of broadcast stations. Prior to 1999, the FCC used Arbitron's television market boundaries (Areas of Dominant Influence), but transitioned to Nielsen's DMA boundaries after Arbitron "ceased its designation and publication of ADI market areas" (Federal Communications Commission 1995).

Designated Market Areas are non-overlapping geographies, which means that each county is assigned to a single DMA. It is important to note that Nielsen only rarely splits counties into multiple DMAs. Of the 3,130 counties / county equivalents assigned to a DMA, 16 are split into multiple markets. In other words, 99.5 percent of all counties are not split. While Nielsen updates boundaries annually based on county viewing patterns, boundaries rarely change over time. Nielsen assigned only 30 counties (less than 1 percent of all counties) to different DMAs in 2008 and 2016. In terms of population, these 30 counties that switched DMAs comprise about 0.1 percent of the U.S. population. Thus, Nielsen reassigns only a minuscule share of viewers to different media markets over time.

The in-state share of DMA measure is calculated using population data at the county and zip code tabulation area level (for the few counties split into multiple DMAs). The measure indicates the proportion of a DMA's population from a given state:

$$\text{In-State Share of DMA}_{sj} = \frac{P_{sj}}{\sum_{s=1}^S P_{sj}}$$

where In-State Share of DMA $_{sj}$  indicates the population share of DMA  $j$  from state  $s$  and  $P_{sj}$  indicates the population in the area of overlap between state  $s$  and DMA  $j$ . Thus, the numerator is the population in a given state-DMA (i.e., area of overlap between a state and DMA) and the denominator is the total population in the DMA.

As a result of both contracts between stations and networks as well as FCC rules, Nielsen’s DMA boundaries restrict which stations cable, satellite, and other multichannel video programming distributors (MVPDs) are permitted to carry in their channel lineups. While out-of-market stations are occasionally available over-the-air, the vast majority of television households rely on MVPDs rather than over-the-air signals; since 2011, about 90 percent of television households subscribe to an MVPD (see Figure A.1). If, for any reason, residents of out-of-state media markets are less likely to subscribe to an MVPD than residents of in-state markets, then it is possible that residents of out-of-state-markets have more access and exposure to in-state local news than otherwise thought. Over-the-air access to out-of-market stations likely depends on a number of factors, including the distance from the station or its nearest translator, terrain, buildings and natural barriers (e.g., trees), weather, and reception technology/hardware. I examine whether households residing in out-of-state markets are less likely to subscribe to an MVPD using data from the SNL Kagan MediaCensus.<sup>13</sup>

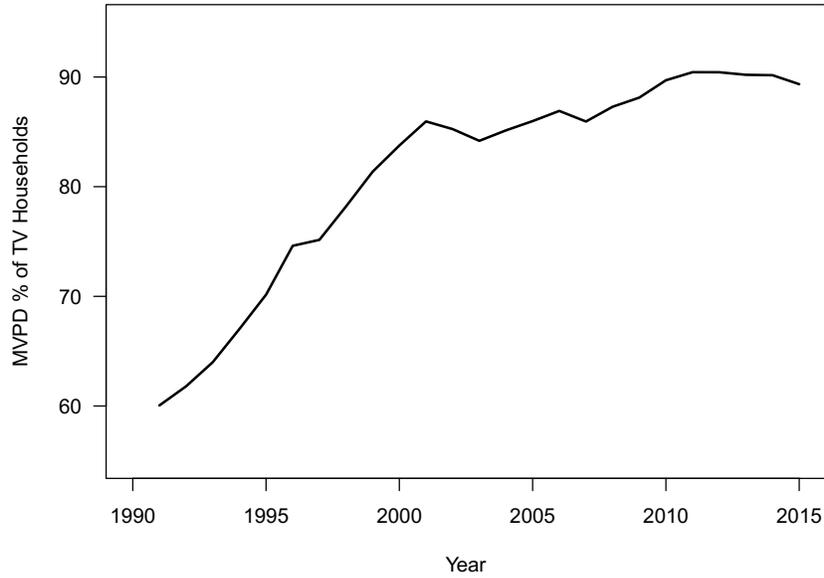
Figure A.2 displays the relationship between the county-level MVPD penetration rate (i.e., the share of households in a county that subscribe to an MVPD) and the in-state share of the DMA to which the county is assigned. The locally weighted regression (LOESS) indicates an extremely flat relationship; there are only negligible differences in subscriber rates across the entire distribution of the in-state share of DMA. The MediaCensus data are not perfect; for instance, a small number of counties have more MVPD subscribers than households in the data. Regardless of how these counties are treated (excluded from the

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<sup>13</sup>According to SNL Kagan (2012), “MediaCensus, which provides geographic subscriber estimates for the multichannel space, channel lineups and must-have competitive intelligence, is the most accurate and complete information of its kind, carefully constrained to public documents, provider footprints, technical capabilities and other exclusive sources.”

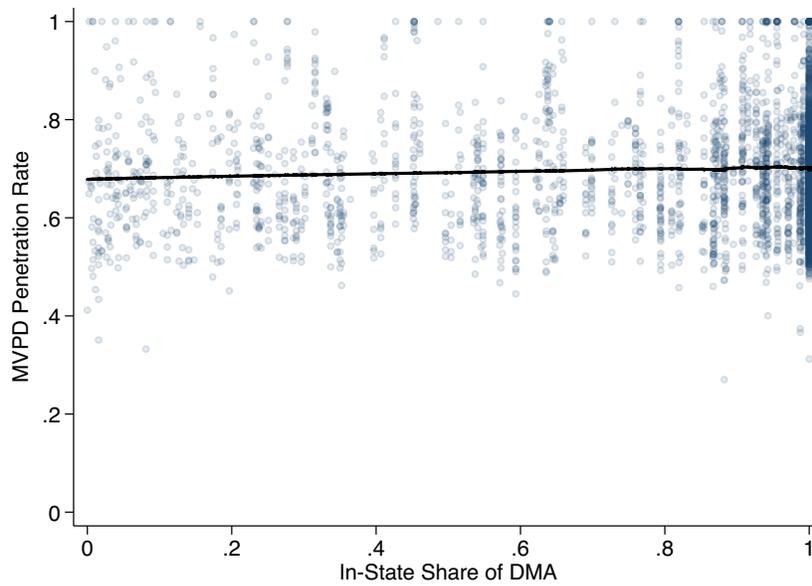
sample, top-coded to have a rate = 1, or allowed to have a rate > 1), the relationship between in-state share of DMA and MVPD penetration rate is unchanged. The results strongly suggest that differences in subscription rates to MVPDs by type of media market (i.e., in-state vs. out-of-state market) are minimal.

Figure A.1: **Share of TV Households with MVPD Broadcast Service, 1991-2015**



Nielsen estimates compiled from various FCC Annual Reports on the Status of Competition in the Market for the Delivery of Video Programming.

Figure A.2: **Share of Households with MVPD Broadcast Service by In-State Share of DMA**



The MVPD penetration rate is calculated based on county-level subscriber data from the SNL Kagan MediaCensus and the number of households from the Census Bureau's American Community Survey county-level summary file, five-year estimates (2012-2016). The curve is from a locally weighted regression (LOESS).

## A.3 Measuring Local Television News Coverage

### About the TV News Archive

The Internet Archive’s TV News Archive’s “mission is to build and preserve comprehensive collections of the world’s most important television programming and make them as accessible as possible to researchers and the general public” (Hanamura 2018).<sup>14</sup>

The Internet Archive collects the television content by recording channels through hardware located in each of the markets they are monitoring (third parties mostly own the hardware), the video content and closed captioning data is then transferred, archived, and stored on the Internet Archive’s servers. The process is not without occasional problems: “A lot can go wrong here. Storms can affect reception, packets can be lost or corrupted before they reach our servers. The result can be time shifts or missing content. But most of the time the data winds up sitting comfortably on our hard drives unscathed”(Schultz 2016).

I access the data through the GDELT API, which splits the content into a 15-second clips.<sup>15</sup> I pull from the API the number of 15-second clips that match the search term(s) as well as the total number of non-entertainment 15-second clips stored in the archive by station and by day.<sup>16</sup> My specific search query for governors is:

*“last name” AND context:“governor”*

And, similarly, my search query for senators is:

*“last name” AND context:“senator”*

The context operator searches for the context term (e.g., “governor”) in the 15-second clips that immediately precede and follow. For instance, if the search is for Governor John Kasich (“Kasich AND context:“governor”), a given 15-second clip that includes the word “Kasich” but not the word “governor” only counts as a mention if the 15-second clip that immediately

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<sup>14</sup>Sources of funding for the archive include the John S. and James L. Knight Foundation, Democracy Fund, Rita Allen Foundation, among others.

<sup>15</sup>See here: <https://blog.gdeltproject.org/gdelt-2-0-television-api-debuts/>.

<sup>16</sup>While closed captioning text from sitcoms and purely entertainment shows is not included in the archive, the archive is not restricted to local newscasts. For instance, it can include national newscasts, talk shows, and other types of shows.

precedes the given clip contains the word “governor” or the 15-second clip that follows the given clip contains the word “governor.” If a 15-second clip contains both “Kasich” and “governor,” it counts as a mention. If one 15-second clip includes the word “Kasich” multiple times, it only counts as one mention. Results are similar if I search for the term, “Governor *last name*,” rather than utilize the context operator.

I search for mentions of each governor (senator) for each station located in a market that overlaps with that governor’s (senator’s) state. So, for instance, I search for mentions of Governor Chris Christie for stations in the New York and Philadelphia markets, but I do not search for mentions of Christie for stations in, for instance, the Charlotte market.

### **In-State Share of DMA & Local News Coverage**

Across a variety of specifications, the population share of media market from the officeholder’s state has significant and substantively meaningful effects on local news coverage of both senators (see Table A.1) and governors (see Table A.2). A governor in an entirely in-state market is expected to receive 1.3-1.6 additional mentions per hour relative to a governor in an entirely out-of-state market, while the equivalent quantity for a senator is an additional 0.3-0.6 mentions.

One potential concern is that the mentions in the closed captioning transcripts are from campaign advertisements rather than news coverage. However, the vast majority of political ads do not include closed captions: “Closed captions are required for most U.S. TV programs, but not for advertisements. Shockingly, most political ads are not captioned” (Schultz 2016). I attempt to identify possible mentions in campaign ads by utilizing the following search query:

“*last name*” AND context:(“approve this message” OR “responsible for the content”)

While not all candidates’ advertisements contain closed captions, the query for potential ad mentions reveals that some candidates and groups in fact do caption their ads. To remedy this potential problem, I control for whether the senator/governor was seeking reelection or another office during 2016 (see specifications (2) and (5) in Tables A.1-A.2. In addition, I

Table A.1: Local News Coverage of Senators

	(1)	(2)	(3)	(4)	(5)
In-State Share of DMA	0.648*	0.492*	0.245	0.338*	0.499*
	(0.222)	(0.181)	(0.157)	(0.149)	(0.214)
Seeking Reelection		0.779*		0.817*	
		(0.259)		(0.283)	
Pres/VP Candidate		1.217*		1.211*	
		(0.448)		(0.467)	
Constant	0.097	-0.145	0.311*	-0.073	0.176
	(0.091)	(0.092)	(0.073)	(0.128)	(0.111)
Station Fixed Effects	No	No	Yes	Yes	No
Senator Fixed Effects	No	No	No	No	Yes
Observations	370	370	370	370	370
Clusters	46	46	46	46	46

Robust standard errors clustered by state-DMA in parentheses.

\*  $p < 0.05$ . Dependent variable is the number of 15-second clips that mention a senator per hour of non-entertainment content.

Table A.2: Local News Coverage of Governors

	(1)	(2)	(3)	(4)	(5)
In-State Share of DMA	1.338*	1.334*	1.599*	1.540*	1.479*
	(0.248)	(0.247)	(0.434)	(0.392)	(0.346)
Seeking Reelection		0.168		0.300	
		(0.423)		(0.318)	
Seeking Senate		0.460*		0.537*	
		(0.063)		(0.128)	
Pres/VP Candidate		1.233*		1.417*	
		(0.477)		(0.608)	
Constant	0.200	0.022	0.061	-0.124	0.125
	(0.103)	(0.081)	(0.217)	(0.196)	(0.180)
Station Fixed Effects	No	No	Yes	Yes	No
Governor Fixed Effects	No	No	No	No	Yes
Observations	185	185	185	185	185
Clusters	46	46	46	46	46

Robust standard errors clustered by state-DMA in parentheses.

\*  $p < 0.05$ . Dependent variable is the number of 15-second clips that mention a governor per hour of non-entertainment content.

include specifications that omit senators/governors seeking reelection or election to another office in 2016 (e.g., presidential candidates in the primary). The estimates from these specifications in Table A.3, restricting to senators and governors not running for anything in 2016, rule out the idea that the observed relationship is driven entirely by campaign ads.

Table A.3: Local News Coverage Excluding Election Seekers

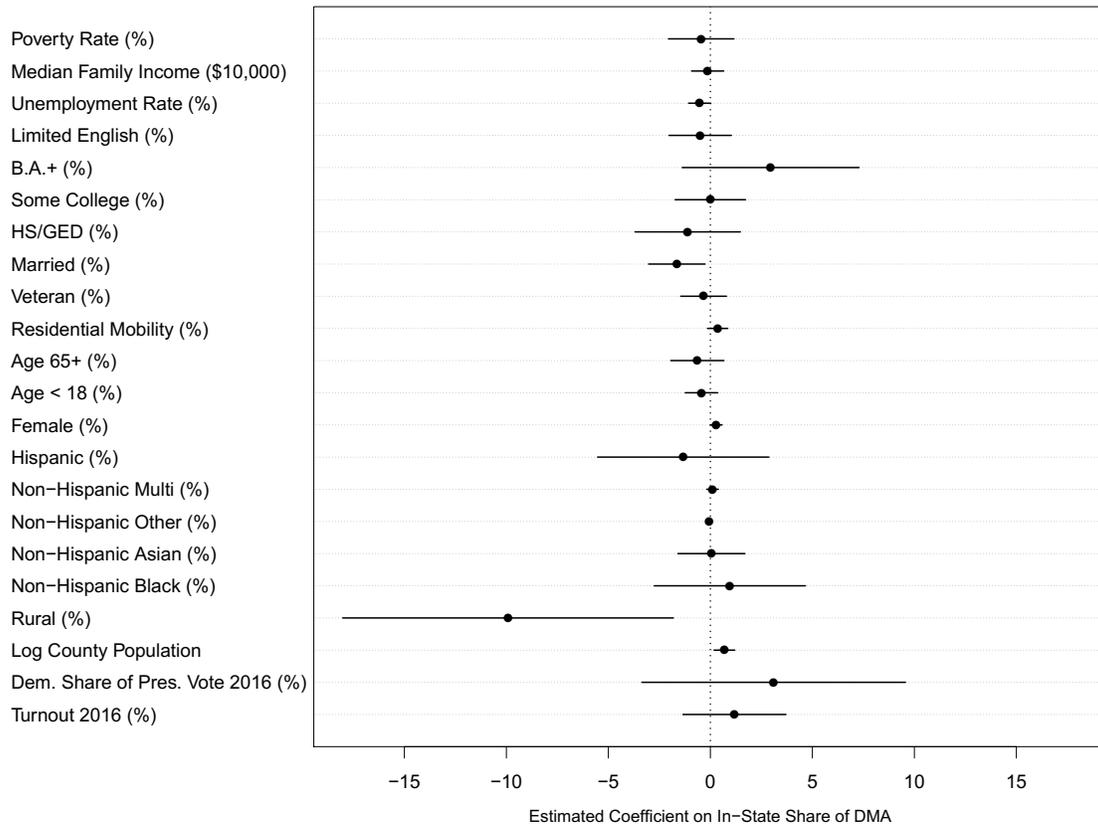
	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.194*	0.170*	0.127*	1.308*	1.103*	1.800*
	(0.044)	(0.056)	(0.027)	(0.252)	(0.285)	(0.341)
Constant	0.025	0.037	0.059*	0.036	0.142	-0.220
	(0.017)	(0.030)	(0.015)	(0.057)	(0.133)	(0.187)
Station FEs	No	Yes	No	No	Yes	No
Senator/Governor FEs	No	No	Yes	No	No	Yes
Observations	248	248	248	141	141	141
Clusters	43	43	43	35	35	35

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Dependent variable is the number of 15-second clips that mention a senator/governor per hour of non-entertainment content. Sample is restricted to senators/governors who were not seeking reelection or involved in the presidential race in 2016.

## A.4 Covariate Balance across In-State Share of DMA

Figure A.3: County-Level Covariate Balance



This plot displays the estimated coefficient and 95% confidence interval for the regression of each county characteristic on in-state share of DMA, partialling out the state-year fixed effects.

## A.5 Individual-Level Results

### About the CCES

I use data from the 2012 and 2016 Cooperative Congressional Election Studies (CCES). The CCES is a large, national, stratified-sample survey conducted by YouGov/Polimetrix. YouGov/Polimetrix constructs the sample using sample-matching methods, and the sample is nationally representative of U.S. adults with appropriate survey weights (Ansolabehere and Schaffner 2013; Ansolabehere, Schaffner, and Luks 2017). The sample size for 2012 is about 55,000 respondents, and the sample size for 2016 is about 65,000 respondents. Importantly for this study, “the state-level samples are sufficiently large as to measure with a reasonable degree of precision the distribution of voters’ preferences within most states” (Ansolabehere and Schaffner 2013). Given the study’s focus on the nationalization of elections and split-ticket voting, I utilize data from the CCES for presidential years only.<sup>17</sup>

Based on the recommendation of Ansolabehere, Schaffner, and Luks (2017), for all specifications using data from the pre-election survey (these are specifications assessing voter knowledge), I weight the sample using “commonweight\_vv,” and for all specifications using data from the post-election survey (these are the split-ticket specifications), I weight the sample using “commonweight\_vv\_post.” In addition, for the split-ticket voting specifications, I restrict the analysis to validated voters (Ansolabehere and Hersh 2012). However, in Table A.7, I present results without restricting to validated voters (this specification includes all respondents who self-reported that they participated even if their turnout is not validated), and the results are very similar.

The basic control variables from CCES used include family income, gender, race/ethnicity, education, age, and marital status. The partisan/ideological control variables include strength

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<sup>17</sup>I do not include the 2008 CCES, as the sampling weights do not appear to be comparable to the weights from 2012 and 2016. Both the 2012 and 2016 CCES include separate weights for the pre- and post-sample and weights constructed before and after vote validation. For 2008, only one set of common content weights are available. In addition, weights for 2008 appear to be trimmed differently than weights for subsequent years. Finally, as Grimmer et al. (Forthcoming) point out, the match rates to Catalist for vote the validation change substantially after 2008. Nevertheless, including the 2008 CCES in the pooled sample does not appear to alter the results meaningfully.

of partisanship and the strength of ideology. CCES respondents are provided with response options for all of these questions. To allow for non-linear relationships, I utilize indicator variables for each of the response options with one omitted reference category.<sup>18</sup> For variables with more than two response options, the variables are defined as follows:

- **family income** – <\$10,000, \$10,000-\$19,999, \$20,000-\$29,999, \$30,000-\$39,999, \$40,000-\$49,999, \$50,000-\$59,999, \$60,000-\$69,999, \$70,000-\$79,999, \$80,000-\$99,999, \$100,000-\$119,000, \$120,000-\$149,999, \$150,000+, and prefer not to say
- **race/ethnicity** – white, black, Hispanic, Asian, multiple races selected, and “other”
- **education** – less than high school, high school graduate, some college, two-year college degree, four-year college degree, and post-graduate degree
- **age** – 18-25, 26-30, 31-35, 36-40, 41-45, 46-50, 51-55, 56-60, 61-65, 66-75, and 76+
- **marital status** – single, married, domestic partner, separated, divorced, and widowed
- **opposite party incumbent** – the partisanship of the incumbent senator/governor is the “opposite” party of the voter’s choice for president
- **strength of partisanship** – non-leaning independents or not sure, independents leaning towards parties, weak partisans, and strong partisans
- **strength of ideology** – middle of the road or not sure, somewhat liberal/conservative, liberal/conservative, and very liberal/conservative

## Voter Knowledge

Tables A.4-2.4 demonstrate that residents of in-state markets are able to recall the party, evaluate, and place their senators and governor on an ideological scale at higher rates. Specification (2) from all three tables is displayed in Table 1.1. The results from these

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<sup>18</sup>Some response options are combined to ensure comparability across years or in cases in which there are very few responses to a particular option.

three tables show that the estimated effect of exposure to local news on knowledge is not sensitive model specification. Across all three specifications for each measure of knowledge, the estimated coefficient is remarkably stable.

For party recall, the question wording is as follows: “Please indicate whether you’ve heard of this person and if so which party he or she is affiliated with...” If an individual responds, “never heard of person,” “not sure,” or with the wrong party, then individual is *not* considered to have party recall knowledge. If the individual responds with the correct party, then the individual has party recall knowledge.

Table A.4: Voter Knowledge: Party Recall of Senators & Governors

	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.096*	0.088*	0.086*	0.113*	0.106*	0.104*
	(0.014)	(0.012)	(0.012)	(0.014)	(0.012)	(0.012)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	No	Yes	Yes	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes	No	No	Yes
Observations	111,846	111,517	110,014	112,225	111,894	110,383
Clusters	333	333	333	333	333	333

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

For senators, the dependent variable is coded =1 if the individual can recall both senators’ parties, =0.5 for recall one senator’s party, and =0 for recall of neither senators’ parties. For governor, the dependent variable is coded =1 if the individual can recall the governor’s party and =0 if the individual cannot recall the party. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include strength of partisanship and strength of ideology.

For the ability to make an evaluation measure, the question wording is as follows: “Do you approve of the way each is doing their job...” If the individual responds with “not sure,” then the individual is not able to make an evaluation. If the individual responds, “strongly approve,” “somewhat approve,” “somewhat disapprove,” or “strongly disapprove,” then the individual is able to make an evaluation.

Finally, for the ability to place on an ideological scale measure, the question wording is as follows: “How would you rate each of the following individuals and groups?” If the individual responds with “not sure,” then the individual is unable to place the official/candidate on

Table A.5: Voter Knowledge: Ability to Evaluate Senators & Governors

	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.089*	0.085*	0.085*	0.037*	0.035*	0.034*
	(0.013)	(0.012)	(0.012)	(0.008)	(0.007)	(0.007)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	No	Yes	Yes	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes	No	No	Yes
Observations	112,087	111,765	110,269	112,655	112,324	110,794
Clusters	333	333	333	333	333	333

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

For senators, the dependent variable is coded =1 if the individual makes an evaluation for both senators, =0.5 if the individual is able to evaluate one senator, and =0 if the individual is unable to evaluate either senator. For governor, the dependent variable is coded =1 if the individual can make an evaluation of the governor and =0 if the individual cannot evaluate the governor. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include strength of partisanship and strength of ideology.

an ideological scale. If the individual responds, “very liberal,” “liberal,” “somewhat liberal,” “middle of the road,” “somewhat conservative,” “conservative,” or “very conservative,” then the individual is able to make an ideological placement.

Table A.6: Voter Knowledge: Ideological Placement of Senators & Governors

	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.081*	0.076*	0.074*	0.075*	0.069*	0.068*
	(0.014)	(0.012)	(0.012)	(0.014)	(0.012)	(0.012)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	No	Yes	Yes	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes	No	No	Yes
Observations	110,914	110,594	109,201	111,759	111,435	110,094
Clusters	333	333	333	333	333	333

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

For senators, the dependent variable is coded =1 if the individual can place both senators on an ideological scale, =0.5 if the individual is able to place one senator, and =0 if the individual is unable to place either senator. For governor, the dependent variable is coded =1 if the individual can place the governor on an ideological scale and =0 if the individual cannot place the governor. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include strength of partisanship and strength of ideology.

### Additional Specifications: In-State Share of DMA & Split-Ticket Voting

For the main results in the paper, I restrict the sample to validated voters (Ansolabehere and Hersh 2012). Nevertheless, results are not particularly sensitive to this decision. Table A.7 contains results for a sample that does not impose the restriction. The sample includes all respondents who self-report participating even if their turnout is not validated.

Table A.7: Split-Ticket Voting Including Non-Validated Voters

	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.032*	0.034*	0.032*	0.033*	0.035*	0.031*
	(0.009)	(0.008)	(0.008)	(0.015)	(0.016)	(0.015)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	No	Yes	Yes	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes	No	No	Yes
Observations	49,844	49,696	49,083	10,798	10,769	10,629
Clusters	320	320	320	66	66	66

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Dependent variable is coded =1 if an individual votes for the Democrat (Republican) for president and Republican (Democrat) for senator/governor. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include opposite-party incumbent, strength of partisanship, and strength of ideology.

Additionally, for the main analyses in the paper, I exclude from the sample counties that are split into multiple DMAs as well as counties assigned to different DMAs in 2008 and 2016. As noted above in Section A.2, split counties and changes to DMA boundaries are very rare. Table A.8 contains results for all counties, including split counties and those assigned to different DMAs in 2008 and 2016. Unsurprisingly, due to the rarity of these counties, estimates are insensitive to their inclusion.

Table A.8: Split-Ticket Voting Including All Counties

	Senator			Governor		
	(1)	(2)	(3)	(4)	(5)	(6)
In-State Share of DMA	0.024*	0.025*	0.022*	0.039*	0.045*	0.040
	(0.010)	(0.009)	(0.010)	(0.019)	(0.020)	(0.022)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Basic Controls	No	Yes	Yes	No	Yes	Yes
Partisan/Ideology Controls	No	No	Yes	No	No	Yes
Observations	38,793	38,682	38,226	8,302	8,280	8,177
Clusters	324	324	324	66	66	66

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Dependent variable is coded =1 if an individual votes for the Democrat (Republican) for president and Republican (Democrat) for senator/governor. Basic control variables include family income, gender, race, education, age, and marital status. Partisan/ideological controls include opposite-party incumbent, strength of partisanship, and strength of ideology.

## A.6 Validity of Using Precinct-Level Data to Measure Ticket Splitting

Table A.9 displays the estimated regression coefficients that correspond to Figure 1.4 in the paper. This exercise demonstrates the validity of using a precinct-level proxy measure of split-ticket voting. As is clear from the table, the relationship between precinct-level proxy measure and the actual

	(1)	(2)
Precinct Estimated Rate	0.618*	0.583*
	(0.009)	(0.009)
Constant	0.047*	0.050*
	(0.001)	(0.001)
Weighted	No	Yes
Observations	2,067	2,067

Standard errors in parentheses. \*  $p < 0.05$ .

The dependent variable is the actual rate of split-ticket voting in each precinct based on ballot-level data from South Carolina in 2016. The independent variable is the aggregate estimated rate of ticket splitting for precincts. Weights are based on the number of two-party votes cast for president.

## A.7 Campaign Advertisements

I use data from the 2012 Wesleyan Media Project (WMP) on television advertisement airings across all media markets in the U.S. (Fowler, Franz, and Ridout 2015). Kantar Media’s Campaign Media Analysis Group (CMAG) tracks airings of each advertisement, including the station, DMA, date, time, and television program.<sup>19</sup> WMP then codes each ad based on its content and the funding source (candidate, party, or interest group).

Using the WMP 2012 data, I calculate the number of ads aired for each senator and governor race in each market. In other words, an observation is an electoral contest-DMA. Because there is at most a single governor and single senator race in each state in 2012, the measure is the number of governor/senator ads aired in each state-DMA. I include all general-election ads aired, including those sponsored by candidates, parties, coordinated groups, and outside groups.<sup>20</sup> Based on Table A.10, it is apparent that campaigns and other entities air more ads as the state comprises a larger share of the DMA’s population. As a result, residents of predominantly in-state markets likely view far more ads than residents of predominantly out-of-state markets. Campaign ads are, thus, a potential alternative explanation for the results in the paper. However, in Section 1.7.2, I present evidence that local news coverage, not television ads, indeed primarily accounts for the paper’s results.

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<sup>19</sup>“CMAG gathers such data by using a market-based tracking system, deploying ‘Ad Detectors’ in each media market in the U.S. In addition to all local advertising activity, these detectors track advertisements on the major national networks, as well as national cable networks” (Fowler, Franz, and Ridout 2015).

<sup>20</sup>I exclude the few ads that purportedly aired in markets that lack any population overlap with the state of the electoral contest.

Table A.10: Relevant Campaign Television Ads Aired, 2012

	Senator		Governor	
	(1)	(2)	(3)	(4)
State's Share of DMA	4381.580*	5662.484*	4437.896*	4285.558*
	(778.422)	(626.538)	(671.469)	(569.721)
Constant	106.183	-716.508	88.511	171.847
	(585.921)	(459.093)	(452.237)	(373.375)
State FEs	No	Yes	No	Yes
Observations	224	224	70	70

\*  $p < 0.05$ . An observation is a state-DMA. The dependent variable is the count of senator/governor ads aired in the state-DMA.

## B | Appendix to Chapter 2

### B.1 Robustness Checks

#### Unweighted Analysis

Throughout the paper the analyses reported weight county-years based on the number of votes cast in the county in a given election. It makes sense to weight based on votes cast, as the county is not the unit of interest to researchers. Instead, voters and election outcomes are of more immediate interest to scholars. Nevertheless, estimates using an unweighted sample are of similar magnitude and reveal the same pattern. Table B.1 displays these unweighted estimates. As in the weighted analyses, specification (1) implies a small, positive average effect of in-state television (not accounting for incumbent ideology) on the incumbent's vote share.<sup>1</sup> And, the estimates from specification (2), which examines how the effect of in-state television varies across the distribution of roll-call extremity, is nearly identical to the estimates of the corresponding weighted analysis (see Table 2.1).

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<sup>1</sup>While the estimate is about 1 percentage point in both the weighted and unweighted analyses, it is statistically significant in the weighted analysis but not the unweighted analysis.

Table B.1: Unweighted Specifications

	(1)	(2)
County Presidential Vote	0.920*	0.919*
	(0.010)	(0.010)
In-State Share of DMA	0.006	0.046*
	(0.005)	(0.013)
In-State Share of DMA $\times$ Roll-Call Extremity		-0.099*
		(0.034)
State-Year Fixed Effects	Yes	Yes
Observations	4,317	4,317
Clusters	318	318

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Analyses are weighted based on the total number of votes cast in the Senate election for a county-year. The dependent variable is the Senate incumbent's share of the two-party vote. County presidential vote is the average share of the two-party presidential vote for the incumbent senator's party in the most recent two presidential elections, subtracting off the national average in each election. Roll-call extremity is the absolute value of the incumbent senator's Nokken-Poole score during the Congress in which the election is held.

## Alternative Measure of Roll-Call Extremity

This section presents the main results using a roll-call extremity measure based on Nokken-Poole scores (which is used throughout the paper) as well as a roll-call extremity measure based on Common-Space NOMINATE scores. The method used to calculate Nokken-Poole scores allow legislators to move across two-year Congresses across their career. On the other hand, Common-Space NOMINATE scores are static, which means that members of Congress occupy a single ideal point throughout their entire career (except in the rare instances of party switches). As is clear from Table B.2, the estimates from specification (1), using the Nokken-Poole measure, and specification (2), using the NOMINATE measure, are extremely. These results suggest that the estimates are not very sensitive to the particular measure of roll-call extremity used.

Table B.2: Alternative Measure of Roll-Call Extremity

	(1)	(2)
In-State Share of DMA	0.046*	0.047*
	(0.013)	(0.013)
In-State Share of DMA $\times$ Roll-Call Extremity	-0.099*	-0.102*
	(0.034)	(0.035)
County Presidential Vote	0.919*	0.919*
	(0.010)	(0.010)
Roll-Call Extremity Measure	Nokken-Poole	NOMINATE
State-Year Fixed Effects	Yes	Yes
Observations	4,317	4,317
Clusters	318	318

Robust standard errors clustered by state-DMA in parentheses. \*  $p < 0.05$ .

Analyses are weighted based on the total number of votes cast in the Senate election for a county-year. The dependent variable is the Senate incumbent's share of the two-party vote. County presidential vote is the average share of the two-party presidential vote for the incumbent senator's party in the most recent two presidential elections, subtracting off the national average in each election. Roll-call extremity is the absolute value of the incumbent senator's roll-call score during the Congress in which the election is held.

## B.2 Incumbency Advantage in the Senate

To estimate the size of incumbency advantage for the Senate between 2012 and 2016, I use modified version of the measure from Gelman and King (1990).<sup>2</sup> Specifically, I estimate the following model using ordinary least squares:

$$\mathbb{E}[V_{st}] = \beta \cdot I_{st} + \tau \cdot P_{st} + \alpha \cdot N_{st} + \gamma_t$$

where  $V_{st}$  is the Democratic Senate candidate's share of the two-party vote in state  $s$  and election year  $t$ ;  $I_{st}$  is the candidate incumbency status—coded = 1 if the Democratic candidate is the incumbent, = -1 if the Republican candidate is the incumbent, and = 0 for open-seat races;  $P_{st}$  is the party incumbency status for the Senate seat—again, coded = 1 if a Democrat currently holds the seat, = -1 if a Republican currently holds the seat, and = 0 if an Independent holds the seat (e.g., Joe Lieberman in 2012);  $N_{st}$  is the normal vote, which is measured by taking the average of the de-means Democratic share of the two-party presidential vote from the most recent two elections; and  $\gamma_t$  are year fixed effects to account for partisan tides.

In addition, I estimate a model that omits the term for the party incumbency status:

$$\mathbb{E}[V_{st}] = \beta \cdot I_{st} \alpha \cdot N_{st} + \gamma_t$$

where the notation has the same meaning as above. All variables are measured at the state-year level, and an observation is a state-year with a U.S. Senate election. To make the estimates comparable to those reported in the paper, I include an unweighted specification and a specification weighted by the number of votes cast in the Senate race for both models. Senate races in which the major party vote constitutes less than 80 percent of the total vote are omitted as are Louisiana Senate races (due to unusual election rules), and as are races between two Democrats or two Republicans.

Table B.3 displays the estimates. Across all four specifications, the estimated size of incumbency advantage is about 5-6 percentage points. The two weighted estimates are

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<sup>2</sup>Jacobson (2015) also uses a modified Gelman-King measure.

more comparable to the weighted estimates presented in the paper, which suggests that incumbency is worth about a 5 percentage point boost for senators during this time period.

Table B.3: Senate Incumbency Advantage, 2012-2016

	(1)	(2)	(3)	(4)
Candidate Incumbency Status	0.064*	0.050*	0.060*	0.049*
	(0.014)	(0.012)	(0.008)	(0.007)
Party Incumbency Status	-0.004	-0.001		
	(0.012)	(0.011)		
Presidential Vote	0.783*	0.865*	0.782*	0.862*
	(0.066)	(0.070)	(0.064)	(0.065)
Constant	0.508*	0.502*	0.507*	0.502*
	(0.009)	(0.007)	(0.009)	(0.007)
Weighted	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	91	91	92	92

Standard errors in parentheses. \*  $p < 0.05$ .

## C | Appendix to Chapter 3

### C.1 Alternative Modeling Strategies, Effect of Partisan Alignment on Turnout

In the main text we explain that we use a block fixed-effects (BFE) approach to model the effect of partisan alignment on voter turnout. In Table 3.1 we provide estimates demonstrating that partisan alignment increases turnout and partisan misalignment decreases turnout under a variety of comparisons (i.e., aligned versus misaligned and competitive versus aligned/misaligned) and when including all districts in the sample as opposed to just districts that were redistricted. We also include Table 3.2 in the main text, which provides a summary of our findings under alternative specifications. Below we produce the raw estimates shown in Table 3.2.

Tables C.1 to C.6 use the same BFE approach as in Table 3.1, but separate midterm (2010-2014) and presidential (2008-2012 or 2008-2016) election years. As compared to including all years, the results are generally slightly noisier when looking at just a single pre and post year. Examining the effects when moving from misaligned to aligned/misaligned (column 1 in the tables), we observe results that range from essentially zero (in Table C.1) on the low end to 0.0353 on the high end. The results are slightly weaker when examining individuals who move from aligned to aligned/misaligned districts, ranging from -0.0066 on the low end to 0.008 on the high end. Examining movements out of competitive districts serves as another means of assessing our hypotheses. Here, the results fall generally in line

with our expectations but with a notable exception of one negative point estimate when moving from competitive to competitive/aligned districts for mid-term years (significant at  $p \leq 0.10$ ). We also observe two of six point estimates that are positive and significant at  $p \leq 0.01$ . Looking at this subset of the data also speaks to the literature on competitiveness and turnout. The prevalent theme in the literature is that competitiveness leads to higher turnout. Under this framework, we would expect to see negative results for both column 3 and column 4, since each indicates the effect of moving from competitive to uncompetitive districts. Echoing the findings in some other recent research (Moskowitz and Schmeer 2019), this straightforward story is not exactly what we observe. Instead, 4 of 6 results for column 3 are positive. Interestingly, the lone negative results are for mid-term years. A plausible explanation for this is that when the competitiveness effect plays a role large enough to outweigh the effects of partisan alignment, it occurs in mid-term years, where House races sit closer to the top of the ticket and therefore have more salience. For individuals redistricted from competitive to misaligned districts (column 4), all six point estimates suggest a negative effect on turnout, including in midterm years. In some sense, this is not surprising, because in this case the possible effects of competitiveness and of partisan alignment/misalignment should be operating in the same direction to depress turnout. The estimates in column 4 range from -0.0047 on the low end (in terms of magnitude) to -0.0220 (significant at  $p \leq 0.01$ ) on the high end in terms of magnitude.

Table C.1: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Mid-Term Years with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	-0.000135 (0.00432)	0.00810 (0.00395)	-0.00925 (0.00516)	
Partisan Misaligned				-0.0220 (0.00479)
Observations	275898	474500	215916	205643
$R^2$	0.433	0.405	0.424	0.427
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.2: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Mid-Term Years (Redistricted Districts Only) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.00550 (0.00452)	0.00653 (0.00436)	-0.00432 (0.00550)	
Partisan Misaligned				-0.0220 (0.00577)
Observations	196609	358385	199210	189814
$R^2$	0.442	0.413	0.488	0.442
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.3: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2012) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.00900 (0.00313)	-0.00122 (0.00310)	0.00384 (0.00426)	
Partisan Misaligned				-0.00470 (0.00427)
Observations	283978	495273	217766	207411
$R^2$	0.511	0.479	0.502	0.500
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.4: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2012, Redistricted Districts Only) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0103 (0.00329)	-0.0000824 (0.00334)	0.00168 (0.00391)	
Partisan Misaligned				-0.0135 (0.00664)
Observations	209317	357594	203925	192891
$R^2$	0.529	0.498	0.521	0.599
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.5: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2016) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0353 (0.00550)	-0.00660 (0.00455)	0.0214 (0.00816)	
Partisan Misaligned				-0.00742 (0.00750)
Observations	292183	510944	220329	209465
$R^2$	0.348	0.312	0.329	0.329
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.6: Block FE: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2016, Redistricted Districts Only) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0290 (0.00514)	-0.00220 (0.00502)	0.0218 (0.00773)	
Partisan Misaligned				-0.0116 (0.00833)
Observations	226176	407912	203262	192589
$R^2$	0.362	0.323	0.351	0.389
State-Year FEs	Yes	Yes	Yes	Yes
Block FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Tables C.7 to C.14 use a difference-in-differences approach, either pooling all years or separating midterm and presidential years. In all cases, we separate results for all districts versus redistricted districts only. The difference-in-differences results generally support the narrative advanced in this paper, but do exhibit some notable departures from expectations. Tables C.7 (all districts) and C.8 (redistricted only) report results when pooling all years. Here we observe noisier results in general than under the block fixed effects approach. The combination of the severe sample restrictions (i.e., restricting to only certain types of districts based on levels of competitiveness) and including individual fixed effects is particularly taxing on the data. Nonetheless, 7 of 8 point estimates in these tables are in the hypothesized directions. The largest effect we observe is for movements from competitive to competitive/misaligned districts at -0.0391. By far the biggest departure we observe from our hypotheses occurs in Tables C.9 and C.10, which report results for the difference-in-differences specification in midterm years. In these cases, we observe negative effects across the board. The most puzzling results are those for columns 1 and 2, where we observe slightly negative results for movements between aligned and misaligned districts. That said, only 1 of the 4 point estimates is statistically significant at  $p \leq 0.10$ . So, it is at least possible to interpret these as primarily null results that reflect noise in the data. The results for movements out of competitive districts in Columns 3 and 4 are more rationalizable. In these cases, the effects of competitiveness may outweigh the effects of partisan alignment for the subset of voters we are examining. Including voters who moved in the opposite directions (i.e., from aligned/misaligned districts to competitive districts) might, due to the idiosyncrasies of these elections attenuate the negative results.

The results for presidential election years are generally much more in line with expectations. Tables C.11 to C.14 report these results.

Table C.7: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, All Years with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.00759 (0.00690)	0.00368 (0.00397)	-0.0000883 (0.00412)	
Partisan Misaligned				-0.00669 (0.00421)
Observations	1092860	1805550	964430	919055
$R^2$	0.090	0.092	0.093	0.094
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.8: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, All Years with State-Year FEs (Redistricted Districts Only)

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.00670 (0.00785)	0.00409 (0.00449)	0.00255 (0.00424)	
Partisan Misaligned				-0.0391 (0.0148)
Observations	785850	1366880	897215	851905
$R^2$	0.107	0.105	0.095	0.155
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.9: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Mid-Term Years with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	-0.00970 (0.00582)	-0.00123 (0.00518)	-0.0185 (0.00489)	
Partisan Misaligned				-0.0140 (0.00526)
Observations	551796	949000	431832	411286
$R^2$	0.024	0.022	0.014	0.014
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.10: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Mid-Term Years with State-Year FEs (Redistricted Districts Only)

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	-0.00974 (0.00638)	-0.000333 (0.00577)	-0.0242 (0.00542)	
Partisan Misaligned				-0.0129 (0.00546)
Observations	393218	716770	398420	379628
$R^2$	0.026	0.024	0.016	0.015
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.11: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2012) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0120 (0.00424)	0.00392 (0.00334)	0.00661 (0.00371)	
Partisan Misaligned				-0.00261 (0.00434)
Observations	567956	990546	435532	414822
$R^2$	0.006	0.005	0.004	0.003
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.12: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2012, Redistricted Districts Only) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0119 (0.00434)	0.00727 (0.00335)	0.00697 (0.00456)	
Partisan Misaligned				-0.0234 (0.0237)
Observations	418634	715188	407850	385782
$R^2$	0.005	0.006	0.003	0.013
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.13: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2016) with State-Year FEs

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0243 (0.00863)	0.00865 (0.00655)	0.0183 (0.00633)	
Partisan Misaligned				0.000232 (0.00634)
Observations	584366	1021888	440658	418930
$R^2$	0.008	0.011	0.008	0.006
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

Table C.14: Differences in Differences: Individual Regressions of Turnout on Partisan Alignment, Aligned versus Misaligned Districts, Presidential Years (2008 & 2016, Redistricted Districts Only) with State-Year FEs (Redistricted Districts Only)

	Misaligned to Aligned/ Misaligned	Aligned to Aligned/ Misaligned	Competitive to Competitive/ Aligned	Competitive to Competitive/ Misaligned
	(1)	(2)	(3)	(4)
Partisan Aligned	0.0214 (0.00818)	0.00743 (0.00694)	0.0197 (0.00640)	
Partisan Misaligned				-0.0668 (0.0420)
Observations	452352	815824	406524	385178
$R^2$	0.008	0.014	0.010	0.065
State-Year FEs	Yes	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes	Yes

Standard errors, clustered at the pre/post redistricting Party-CD level, are in parentheses.

The sample is comprised of contested general elections by a D and R candidate.

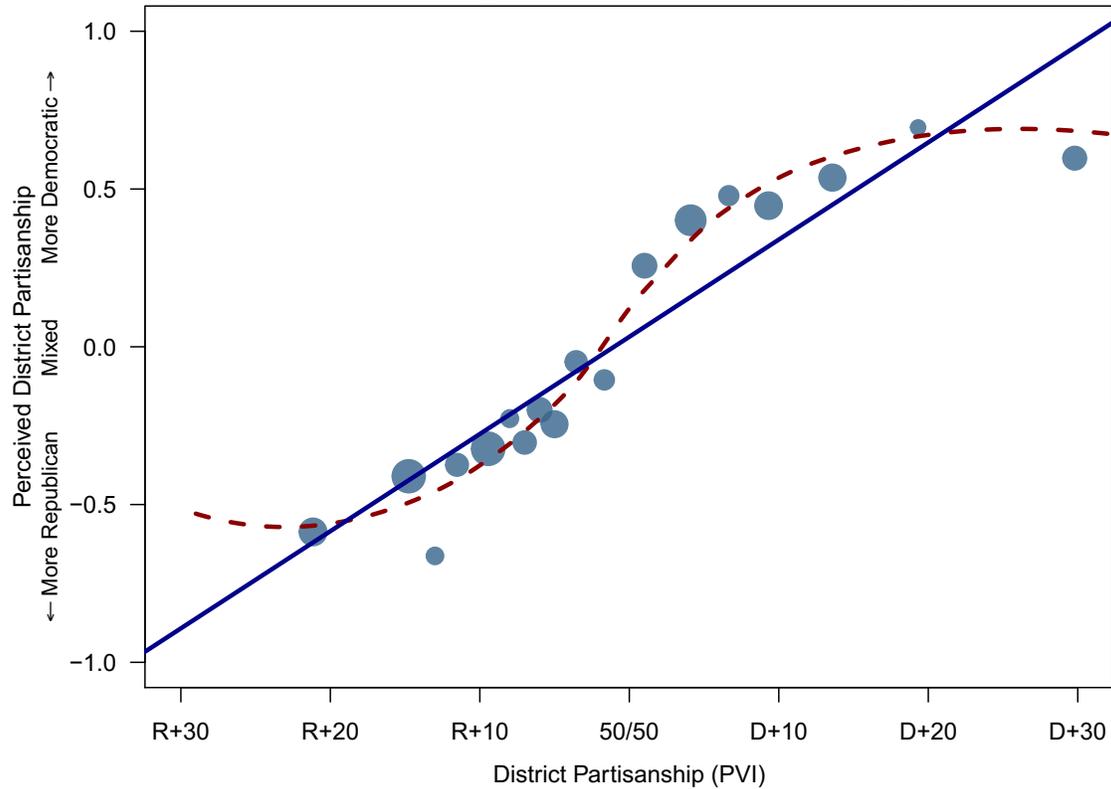
Treatment occurs in second period.

All elections in Louisiana are excluded from the sample due to their unusual rules.

## C.2 Perceptions of District Partisanship

Secondary evidence for expressive voting in the main text takes the form of CCES panel survey results spanning the redistricting cycle. There we choose to focus on results for 2012 perceptions of the partisan composition of districts and self-reported campaign contact in the 2012 election (for the cross-sectional plots). Below we provide results for 2014 perceptions of district partisanship, where we see patterns that are strikingly similar to 2012 perceptions, even after accounting for the partisan composition of individual's pre-redistricting district.

Figure C.1: Voters' Perceptions of District Partisanship, 2014



This figure demonstrates that voters are largely aware of their congressional districts' partisan composition. This binned scatterplot is based on tabulations of 2014 data from the 2010-2014 CCES Panel. Each point in the figure corresponds to a local mean and is proportional in size to the number of observations within the locale. The dark blue, solid line is based on a linear regression and the red, dashed line is based on a locally weighted regression.

We find little evidence that campaign contact increases substantially in aligned versus misaligned districts. In the main text, we visually show this evidence for 2012. In Figure

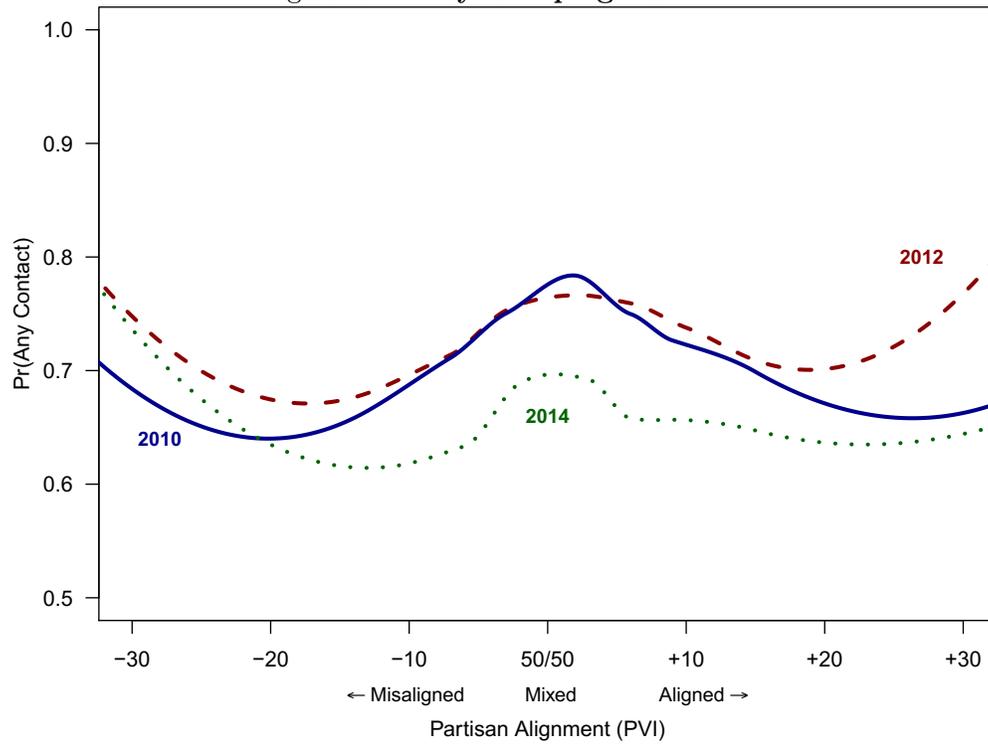
Table C.15: Perceived Partisan Composition of District | 2014

	(1)	(2)	(3)
PVI	0.0294 (0.0018)	0.0227 (0.0023)	0.0204 (0.0046)
Lagged PVI			0.0100 (0.0042)
Constant	0.0100 (0.0215)	0.0014 (0.0074)	0.0110 (0.0210)
Observations	7647	7647	7647
$R^2$	0.242	0.463	0.249
Old District FEs	No	Yes	No

Standard errors, clustered by congressional district, are in parentheses.

C.2, we see that this holds across years.

Figure C.2: Any Campaign Contact



This figure demonstrates that voters in congressional districts aligned with their partisanship do not report substantially more campaign contact than voters in misaligned districts. Each curve is fit from a locally weighted regression based on CCES Panel data from each respective year.

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