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The Reaches and Limits of Nationalization in U.S. Politics

A dissertation submitted in partial satisfaction

of the requirements for the degree

Doctor of Philosophy in Political Science

by

Derek Edward Holliday

2023

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ABSTRACT OF THE DISSERTATION

The Reaches and Limits of Nationalization in U.S. Politics

by

Derek Edward Holliday

Doctor of Philosophy in Political Science

University of California, Los Angeles, 2023

Professor Christopher N. Tausanovitch, Chair

Recent research in political science documents the “nationalization” of U.S. state and local politics; the down-ballot results of partisan elections tend to reflect the outcomes of presidential contests. The three papers of this dissertation examine this phenomenon in greater detail. The first paper critically assesses nationalization as a top-down force. I decompose a large set of election results into candidate specific, partisan, and idiosyncratic components. While it is true election results are tied increasingly to partisanship, I find it is not true that presidential elections are most strongly tied to partisanship. In the second paper, I utilize a supervised machine learning technique to determine the extent to which gubernatorial rhetoric mirrors that of presidential candidates, finding candidates largely speak on topics germane to their own jurisdictions. Finally, the third paper uses a survey experiment to find that voters use national policy signals when choosing between candidates for state and local office. I also find voters use state and local policy signals when evaluating national candidates. The stronger the partisan signal, the greater the effect on behavior.

The dissertation of Derek Edward Holliday is approved.

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2023

*To my parents, Ann and Joe,
and my wife, Lisa,
whose love and support made all this possible*

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Chapter 1

Is All Politics Presidential? Decomposing partisan patterns in U.S. election outcomes across offices, 1972-2020

A growing body of research finds the electoral successes of candidates for state and local office appear increasingly linked to copartisan presidential candidates (Abramowitz and Webster 2016; Hopkins 2018; Jacobson 2015a; Sievert and McKee 2019). This apparent relationship is called “nationalization.” Broadly, this work assumes a top-down relationship between presidential and down-ballot results, with the evidence for such a relationship often being increasing correlations between copartisan candidate vote-shares across contests. The representational consequences of nationalization are presented in bleak terms; if voters make electoral decisions based simply on the attributes of presidential candidates or antipathy toward members of the other party, how can officeholders be held accountable for their actions or the unique demands of their elected positions (Abramowitz and Webster 2016)?

In this paper, I argue that the conclusion that nationalization is the result of presidential candidates influencing down-ballot results is not supported by the prior work, which has used data and estimation strategies unable to distinguish top-down mechanisms from others. In particular, previous strategies fail to distinguish presidential influence from the growing strength of partisan

political preferences over all elections. I present an alternative theoretical framework for understanding nationalization. Election results across contests within the same electorate can differ for three reasons. First, candidates may differ in *valence*, performing systematically better or worse than copartisan candidates in other contests across all voting districts (counties and precincts). Second, the effect of a district's *partisan lean* on voting outcomes may differ across contests. Finally, the *stochastic*, idiosyncratic forces present in all elections can lead to differences in voting outcomes. Under this framework, nationalization in its strongest form would imply a complete lack of variance in all elements of elections. Put simply, nationalization is the decreasing variety of how voter preferences are translated into vote choice in aggregate across different elections.

I operationalize this theoretical framework with a measurement-model approach, estimating separate parameters for candidate valence, the partisan lean of voting districts, and how that partisan lean is translated into vote-shares. I apply the model to three contexts: over time, across statewide contests, and in sub-state elections. Over time, I analyze county-level presidential, senate, and governor results from 1972-2020, focusing on the post-Southern realignment time period. For statewide contests, I analyze 476 statewide elections across 26 offices from 2016-2020 using precinct-level data. Finally, for contests deeper down-ballot, I utilize precinct-level results for all partisan contests in Maricopa County, Arizona, from 2008-2020. These data represent the most comprehensive set of elections evaluated for nationalization to date.

Across all contexts, I find partisanship is an increasingly strong component of election results. Variation in candidate valence and the effect of a district's partisan lean have decreased, and are particularly low in deeper down-ballot races. However, presidential contests are not the strongest manifestation of partisan voting. Like other races with greater media attention, presidential elections are often noisy translations of district partisanship into vote-shares. This suggests the increasing correlation of election outcomes is the result not of top-down, presidential influence but of the growing strength of partisanship across all elections. These results have consequences for how we evaluate the quality of political representation in state and local contexts.

The paper proceeds as follows. First, I review existing research on nationalization, assessing

differences in theoretical understandings of the concept and how it is operationalized. Second, I propose my theoretical and methodological approach, detailing its advantages over previous approaches. I then introduce the data for analysis and descriptive results from the measurement model over-time and cross-sectionally, considering the relative influence of presidential and partisan forces in election outcomes. I end with a discussion of how the results shape our understanding of representation in a federal system.

1.1 Nationalization in Theory and Practice

In its broadest conceptualization, the “nationalization” of U.S. elections refers to the growing tendency of national and state/local election results to appear increasingly similar. As Hopkins (2018) states when describing nationalization as it manifests in voting outcomes, “If voters’ choices in state and local races *echo those in national races*, their voting is nationalized” (emphasis added). In this sense, nationalization describes an easily observable aggregate-level outcome, and all published work on the concept concurs on this fundamental pattern. Disagreement arises, however, when describing the underlying *mechanisms* of nationalization. Beyond the similarity of election outcomes, what does it mean for state and local races to “echo” national races?

Many scholars describe nationalization as a top-down force, which I will refer to as “presidentialization.” As Sievert and McKee (2019) understand it, “nationalization refers to an increasing linkage between presidential voting patterns with subpresidential contests at the federal, state, and local level.” Moskowitz (2021) concurs; “United States House, U.S. Senate, gubernatorial, and other state and local election outcomes have grown increasingly tied to presidential election outcomes.” So goes the presidency, so too go down-ballot contests. A stronger claim in this genre implies an almost causal relationship between presidential contests and down-ballot outcomes. Sievert and McKee (2019) elaborate their understanding further:

“The most visible and salient election contest, that for the White House, sets the agenda for most other American elections. For years, southern politics scholars recognized and

emphasized the role of presidential elections in leading to Republican electoral success in lower offices (Black and Black 1987), and this was dubbed top–down advancement (Aistrup 1996). Nationalization is a more expansive form of top–down advancement that is expected to permeate all regions of the United States.”

If presidential candidates are seen as the standard-bearers for their respective political parties, voters are easily able to connect candidate attributes and performance across offices without forming unique preferences or criteria for less salient offices (Carsey and Wright 1998).

Anecdotally, the presidentialization mechanism of nationalization seem particularly popular in media descriptions of down-ballot races. Under headlines such as “Newsom’s Anti-Trump Recall Strategy Offers Republicans a Warning for 2022” and “GOP seeks to nationalize gubernatorial elections,” journalists describe the use of Donald Trump as a campaign tool for gubernatorial candidates (Manchester 2019; J. Martin 2021). In California Governor Gavin Newsom’s case, successfully connecting his Republican opponent to a President deeply unpopular in the state was seen as a savvy campaign strategy. In 2019, however, in gubernatorial races in Republican-leaning states such as Kentucky, Mississippi, and Louisiana, leading candidates took the opposite approach. Trump personally visited many campaign events in these states, explicitly focusing the elections on his ongoing impeachment probe. As the President became more involved in these contests, the media described them as more “nationalized” in the “presidentialization” respect.

Alternatively, nationalization is sometimes portrayed in terms of party loyalty, stemming perhaps from voters’ more national policy preferences. Abramowitz and Webster (2016) contend “Growing party loyalty and straight-ticket voting have led to the nationalization of elections in the United States: there is a much closer connection between the results of presidential elections and the results of House, Senate and even state legislative elections today than in the past.” Similarly, Jacobson (2015a) extends the argument to the decrease in incumbency advantage among US House candidates: “Over the last three decades... party loyalty has risen steadily, the articulation between congressional and presidential elections has strengthened, and electoral politics have grown increasingly nationalized.”

Of course, suggesting presidential and down-ballot contests are linked in the presidentialization conception doesn't mean that linkage is not informed by political preference, although many scholars do not analyze this claim. Indeed, Hopkins (2018) theoretically conceives of nationalization in a more spatial manner, arguing "national and local politics are fought over related dimensions, and the scope for disagreement in national politics is much wider. As a consequence, national political divisions infuse subnational politics, and political engagement is primarily national in orientation." Significant research supports this conclusion, but has not directly linked nationalized electoral outcomes to the homogenization of preferences over issues and offices. Caughey, Dunham, and Warshaw (2018), for example, provide evidence of cross-state ideological variation within each party declining sharply since the 1950s. Further evidence exists for more "vertical" nationalization, with N. Lee, Landgrave, and Bansak (2022) finding similar patterns of party sorting across a range of national and local issues among local elected officials. Others find similar relationships, but also find dimensions of local politics that remain independent of national policy debates (Bucchianeri et al. 2021; Jensen et al. 2021). Evidence from conjoint experiments that voters are motivated primarily by policy preferences over appeals to negative partisanship further suggests any relationship between presidential and down-ballot vote totals is likely deeper than simple skin-deep appeals to partisan identity (Costa 2021).

Despite deeper theoretical underpinnings and varieties of potential mechanisms, extant nationalization research often uses the same set variables: the two-party vote share of the Democratic presidential candidate and the two-party Democratic vote share of the down-ballot office of interest.¹ These two are often linked using either simple linear regression or a correlation coefficient (Amrani and Algara 2021; Hopkins 2018; Jacobson 2015b; Sievert and McKee 2019; Weinschenk et al. 2020; Weinschenk 2022). While certainly a convenient way of eliciting a connection between two offices, this measurement approach cannot differentiate between different mechanisms underlying the correlations. Correlational approaches particularly obscure outcomes arising from variation in candidate valence; instances where one party's down-ballot candidates perform uniformly better

¹A detailed summary of previous methodological approaches is given in Appendix 4.1.1.

across voting districts relative to the presidential candidate appear identical to instances of equal or worse performance.

Despite this, scholars make strong conclusions about the nature of the relationship: “evaluations of the president play an increasingly important role in structuring Americans’ attitudes about the president’s political party and his co-partisans in other elected offices” (Sievert and McKee 2019). An obvious lurking confounding variable in these sorts of models is partisan political preference; as partisanship becomes a stronger driver of behavior across all contests, results will appear more similar. Conceptualizing presidential voting behavior as an organizing force in U.S. politics is, of course, not entirely novel. Scholars have used district-level presidential vote-shares as measures of district partisanship for years (Ansolabehere, Snyder Jr., and Stewart 2001; Canes-Wrone, Brady, and Cogan 2002; Erikson and Wright 1980). However, as Levendusky, Pope, and Jackman (2008) note, elections are merely indicators of underlying preferences and subject to short-term forces.

1.1.1 Formalizing Shortcomings of Previous Approaches

In Figure 1.1, I construct four hypothetical elections to demonstrate the shortcomings of correlational and bivariate regression approaches to measuring nationalization. In each panel, every point represents a fictional voting district (county or precinct, for example). The two-party margin of victory for the Democratic candidate is given on the y-axis, and the general leaning of the district toward or against Democratic candidates is given on the x-axis. This is meant to capture a more general partisan dimension of politics across all elections. In previous approaches, it is assumed the two-party Democratic Presidential candidate’s vote share is an error-less manifestation of partisan preference; the x-axis in each of the plots could simply be replaced by presidential vote-share.

Note that the vote-shares panels 1, 3, and 4 are correlated with each other with a correlation coefficient of 1, and panels 1, 2, and 3 all have identical slopes. These similarities exist despite systematic differences between each of the elections. Consider panels 1 and 2. While each share the same slope and intercept, panel 2 exhibits much greater residual error, suggesting the election was a noisier manifestation of general district partisan leaning. Alternatively, panels 1 and 3 have

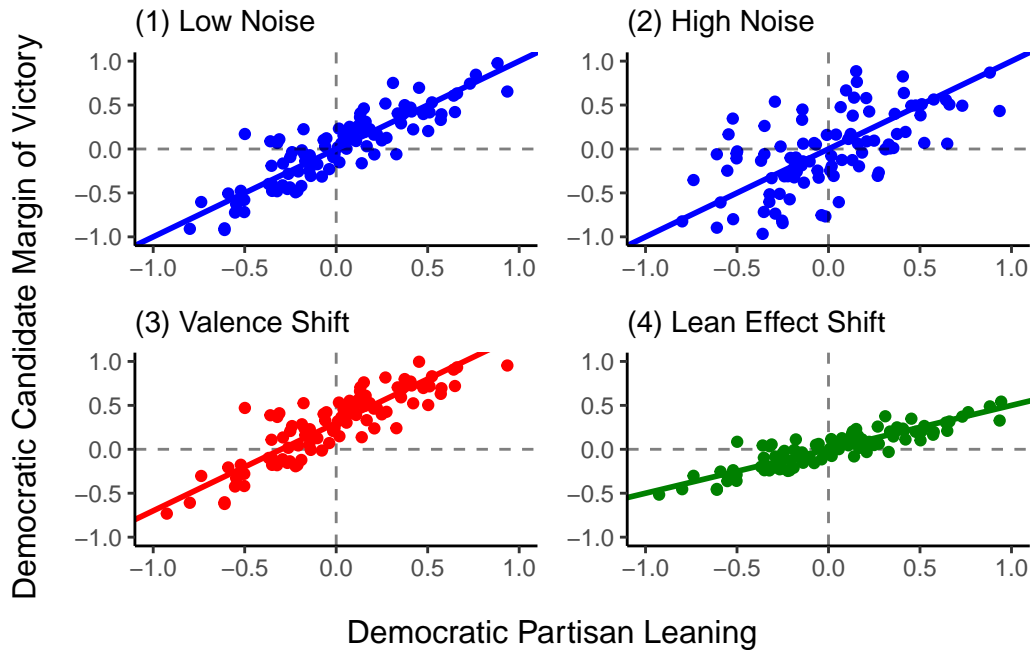


Figure 1.1: Correlations and Slopes Mask Distinct Election Forces

the same residual error and slope, but the Democratic candidate in panel 3 is performing much better across all districts. For the remainder of the paper, I call this a change in candidate valence. Finally, while the voting outcomes in panels 1 and 4 are correlated with correlation coefficient 1 and have similar residual error, the relationship between the partisan lean of a district and the voting outcomes are substantially different; the rate of translation between partisan lean and vote-share is reduced considerably.

Election outcomes, then, can appear identical in correlational and bivariate approaches, but belie three sources of variation: residual error as a result of idiosyncratic noise in an election, valence shifts from relative candidate over- and under-performance, and differences in how district partisan lean is translated into votes. Therefore, I propose a more complete operationalization of nationalization as the reduction in variation across all election forces. As idiosyncratic noise, valence shifts, and differences in the effect of partisan lean collapse toward similar values, elections have “nationalized” in the sense that forces structuring outcomes across all levels of government have grown more similar.

Unlike previous approaches, this operationalization of nationalization is agnostic toward the un-

derlying mechanism behind the growing similarity of election forces. It does, however, allow for such mechanisms to be explored. In this paper, I will focus on the “presidentialization” of elections implied by much of the literature; that is, presidential candidates and voting outcomes increasingly structure down-ballot results. Specifically, the “presidentialization” implies a number of patterns that can be measured in the above operationalization of nationalization.

First, if the top-down, “presidentialization” mechanism of nationalization is accurate, we would expect the relationship between partisan lean and vote share to be tightest in presidential contests relative to other contests. Presidential contests are the preferential and partisan locus, and all other contests are noisier manifestations of such preferences. In the strongest version of presidentialization, the partisan leaning and presidential voting behavior of districts are indistinguishable; in Figure 1.1, this would manifest as district results appearing in a perfectly straight line without deviation. Conversely, if mechanisms behind nationalization are more akin to partisan sorting across offices, the tightness of the relationship between preference and vote share should appear fairly similar across contests. Partisan preference is no more likely to be predictive in presidential contests than any other contest; it acts as a latent dimension structuring **all** behavior up and down the ballot.

Second, presidentialization suggests presidential candidate choice should be the strongest predictor of down-ballot vote choice. Being just a manifestation of presidential preference, using district partisan lean as a predictor should yield noisier results. Alternatively, if elections are structured primarily by underlying partisan preference, then such latent partisanship should be the strongest predictor, as the same dynamics underlying presidential races structure other down-ballot races.

Finally, the further down-ballot a contest, the more inoculated it should be against presidentialization. Top-of-ballot contests share more similarities with and opportunities of influence from presidential contests; many higher-office candidates consider presidential runs themselves, comment directly on national policy issues, and receive endorsements from presidential candidates. If nationalization is a manifestation of top-down forces, the contests with more connections to the “top” should be the most nationalized. If nationalization is a manifestation of stronger partisan

influence, we should expect the opposite. The lower salience the election, the fewer signals voters have to translate preference into votes besides party identification. Nationalized elections, then, are less about the functions of offices and more about the signals voters have access to when making decisions.

1.1.2 Decomposing Variation in County and Precinct Election Returns

To distinguish between top-down and partisan mechanisms, I propose measuring nationalization through a decomposition of electoral outcomes into the following form:

$$\text{DemMargin}_{ij} = \alpha_j + \beta_j \text{Partisan Lean}_i + \epsilon_{ij}, \quad (1.1)$$

for electoral district i and contest j . DemMargin_{ij} is the Democratic candidate's vote share margin of victory in the i^{th} electoral district and j^{th} contest, using the two-party vote share in the race.² The intercept α_j can be theoretically understood as the *valence* of the Democratic candidate in the j^{th} contest, or the partisanship-independent candidate effect. By construction, this is the Democratic candidate's margin of victory when the electoral district partisan lean equals zero. Positive numbers are associated with better performance from Democratic candidates while negative numbers are associated with worse performance, relative to their Republican opponents.

The electoral district-level variable for partisan lean is centered at zero such that positive numbers are associated with more Democratic-leaning electoral districts and negative numbers with more Republican-leaning electoral districts. The slope parameter β_j modifies this electoral district partisan lean, and can be understood as the rate of translation of partisanship into Democratic votes. For the model to be identified, I set the presidential rate of translation of partisanship into vote-share to 1. Values less than one signify a weaker relationship between preference and vote share than in the most recent presidential election, and values greater than one signify a stronger relationship.³ The stochastic element of elections is accounted for by ϵ_{ij} .

²For example, if the Democratic candidate in a contest receives 75 votes, the Republican candidate receives 25, and an Independent candidate receives 10, DemMargin equals $75 - 25 = 50$.

³It is theoretically possible for slope values to be negative in cases where partisan lean and Democratic vote shares are

This approach to measuring nationalization has a number of other theoretical and methodological advantages over previous work. First, it allows for presidential elections to be treated simply as another manifestation of political preference being translated into votes along with other electoral contests instead of as directly influencing down-ballot behavior as an independent variable. This makes the approach more consistent with the deeper theoretical understanding of nationalization as down-ballot contests being contested over the same partisan dimension as national contests, regardless of jurisdictional (dis)similarity.

In addition, this approach allows me to separate preference and candidate effects through the two parameters α_j and β_j (the candidate valence and partisan lean effect). While a purely Downsian approach would assume votes are simply a function of ideological distance between candidate and voter, we know certain non-ideological forces (such as a scandal during the campaign) also influence vote choice. This is another reason for moving presidential vote shares onto the same side of the equation as other contest vote shares; candidates for president vary year to year, and therefore so too does the translation of district partisanship into votes. Each presidential contest is its own manifestation of preference, which this operationalization is able to track. These measures are not independent, however, nor should we expect them to be. On average, as the absolute value of candidate valence increases, the magnitude of the partisan lean effect will decrease. This is largely due to valence setting a floor or ceiling for the performance of the Democratic candidate, which by construction limits the range of variation over which the partisan lean effect operates.

Methodologically, parameterizing nationalization in terms of a linear transformation of partisan lean makes clearer the interpretation of each of the coefficients. In previous approaches where down-ballot vote shares were functions of presidential vote shares, the slope parameter of interest has an ambiguous meaning. Clearly a value of one would mean a strong relationship between presidential and down-ballot contests, but how should one compare values on opposite sides of one? Would a value of 0.95 be as “nationalized” as a value of 1.05? Both values indicate a close but imperfect relationship between the two vote shares, and in purely functional terms the latter indicates

inversely related, or in a case where a Democratic candidate was coded as a Republican and vice-versa. However, this does not occur in my analysis.

each percentage-point gain by the presidential candidate is associated with a 1.05 percentage point gain by the down-ballot candidate. Determining which is more “nationalized” in terms of how the contest is connected to preference is not immediately clear, however. This is not an ambiguity shared by this decomposition approach. The partisan lean effect β_j can be directly understood as a translation of aggregate partisanship into votes. The greater the slope, the greater the rate of translation. More importantly, my focus is on the level of variation in underlying contest dynamics and the degree to which contests are structured by partisan dimension.

1.2 Estimation

Because I only observe DemMargin_{ij} in my formulation, the estimation of α_j , β_j , and Partisan Lean_i is a nontrivial task. Thankfully, the political science measurement model literature has been particularly adept in solving similar problems, such as ideology scaling in congress and measuring district-level liberalism (Kernell 2009; Levendusky, Pope, and Jackman 2008; Poole and Rosenthal 1985; Tausanovitch and Warshaw 2014). I utilize a maximum likelihood method used by Groseclose, Levitt, and Snyder Jr. (1999), which itself is similar to Aldrich and McKelvey (1977) and Poole (1998). In their paper, the authors are concerned with comparing interest groups scores for members of Congress over time and across chambers, where the scale for the scores are known to shift and stretch over time based on the composition and agendas of the chambers. By setting a single year as a reference point and explicitly modeling the stretch (slope) and shift (intercept) of the underlying preferences of the representatives, this comparison across time and chambers becomes possible.

This approach fits nicely with the theoretical problem of nationalization. In my application, the “shift” parameter is the candidate valence α_j and the “stretch” parameter is the partisan lean effect β_j . For the model to be identified, I must set one contest as a “reference” against which other contests are shifted and stretched. As previously discussed, I use the most recent presidential contest as this reference point, at which I set $\alpha_{\text{president}} = 0$ and $\beta_{\text{president}} = 1$. The choice of the

reference point is methodologically inconsequential (any contest could be chosen), but important for the substantive interpretation of the effects, as the other parameters will be interpreted as relative to the reference point. This works nicely with the theoretical underpinnings of nationalization, as we want to explore how the effect of partisanship is shifted and stretched relative to a national reference point. Other modeling approaches, such as in Levendusky, Pope, and Jackman (2008), assume constant effects across offices, making them unsuitable for operationalizing nationalization.

Specifically, the estimation process involves representing contests as a matrix (one for each state-time period), where each row is a precinct, each column is a contest, and each cell is populated by the Democratic margin of victory. I then iteratively perform singular value decompositions (hereafter SVD), where each decomposition yields estimates for α_j , β_j , and Partisan Lean_{*i*}. These values are then used to populate missing democratic margin values (precinct-contests with missing data) in the matrix for the next iteration.⁴ This process repeats until convergence (when the mean absolute difference between the starting and ending values for estimated partisan lean is less than $1 \cdot 10^{-8}$). See Appendix 4.1.2 for a comparison of this estimation technique to Markov Chain-Monte Carlo, which yields almost identical results.

In isolation, each parameter estimated in this process has a clear functional interpretation in the model. However, certain cases can lead to interactions making substantive interpretation more difficult. For example, in cases where the the mean partisan lean is far from 0 and β_j is substantially greater than 1, the estimate for the intercept α_j can be far outside the substantively meaningful bounds of -1 and 1. This almost never occurs in contemporary elections, but happens frequently during the Southern realignment when presidential partisan voting results were inversely correlated with results for state office. Therefore, I use a transformed parameter μ_j to represent candidate valence, where $\mu_j = \alpha_j + (\beta_j - 1) \cdot \overline{\text{Partisan Lean}}$. This yields the difference in the expected outcome between office *j* and the presidential election in the district with the average partisan leaning. By construction, since I set the presidential contests to have $\alpha = 0$ and $\beta = 1$ for identifiability, $\mu_{president} = 0$. I use μ in place of α for the remainder of this paper.

⁴Typically only one iteration is needed, as most state-time periods do not have missing data. The iterative process is only necessary because singular value decomposition requires no cells in a matrix be missing.

1.3 Data and Results

One of the shortcomings of current research on nationalization is the piecemeal approach to contests; typically, presidential election results are only compared to one or two other contests, such as governor, senate, or state judicial races. This makes direct comparisons of results difficult and artificially limits our understanding of the reaches and limits of nationalization in U.S. politics. To resolve this, I analyze the most complete set of elections to date simultaneously. In this section, I draw on the following data sources:

- Amlani and Algara (2021): provides county-level election results for presidential, senate, and governor elections from 1872-2020. Allows for analysis of top-of-ballot contests over a longer time horizon. For the purposes of this paper, I focus on the elections starting at the end of the Southern realignment (1972 onward) for a more consistent understanding of party composition/ideology.
- OurCampaigns.com: provides crowd-sourced county-level data for state secretary of state and attorney general elections from 1972-2020. These data have been used in a number of recent studies (de Benedictis-Kessner and Warshaw 2020; Sides, Vavreck, and Warshaw 2022) and augment the top-of-ballot results from Amlani and Algara.
- Voting and Election Science Team (VEST) Dataverse: provides precinct-level general election results for statewide contests from 2016-2020. Allows for a more focused analysis on downballot races in a reduced timeframe (Voting and Election Science Team 2022).

1.3.1 Nationalization Over Time

Using data from Amlani and Algara (2021) and OurCampaigns.com, I analyze county-level general election vote shares for presidential, senate, governor, secretary of state, and attorney general contests from 1972 to 2020 using the SVD estimation strategy. I split the data into state-four-year intervals, beginning each interval with a presidential election year and using that presidential

election as the reference point in the analysis. The analysis covers 2,057 distinct contests; 13 presidential, 789 senate, 637 governor, 280 secretary of state, and 338 attorney general⁵. Figure 1.2 plots the raw estimates for the absolute candidate valence effects over time for all offices, where every point is a single election. Recall candidate valence effects are *intercept shifts* relative to the Presidential contest.

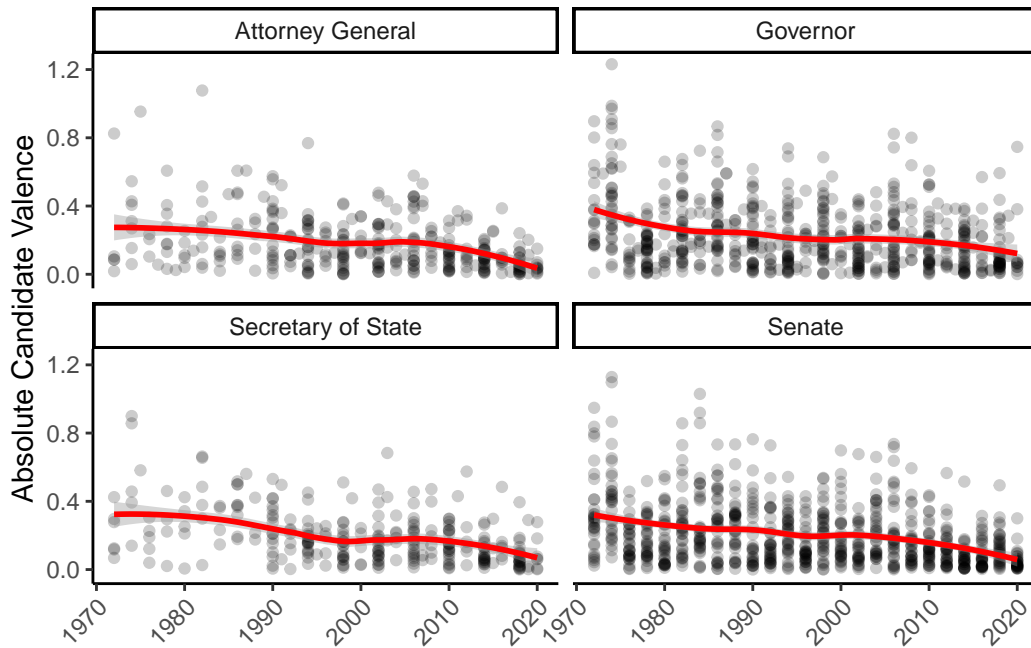


Figure 1.2: Candidate Valence Effects have Decreased. Senate, Governor, Secretary of State, and Attorney General, 1972-2020

Across all contests, the average absolute candidate valence has decreased since 1972. This suggests systematic advantages for certain candidates are smaller now than in the past. These decreases have been notably similar across offices and keep a fairly linear trend, with average annual decreases of about 0.004. Over the 48 year time period, these effects are substantial, and easily enough to alter election winners.

Figure 1.3 plots the corresponding partisan lean effects across all contests for every office. These values are the *slope* values, evaluated relative to the Presidential contest where the slope is set to 1. Note the margins of the y-axis in Figure 1.3 are constrained to more clearly visualize the bulk of the

⁵Data coverage for secretary of state and attorney general elections is sparser in earlier election years due to limitations in the OurCampaigns.com data.

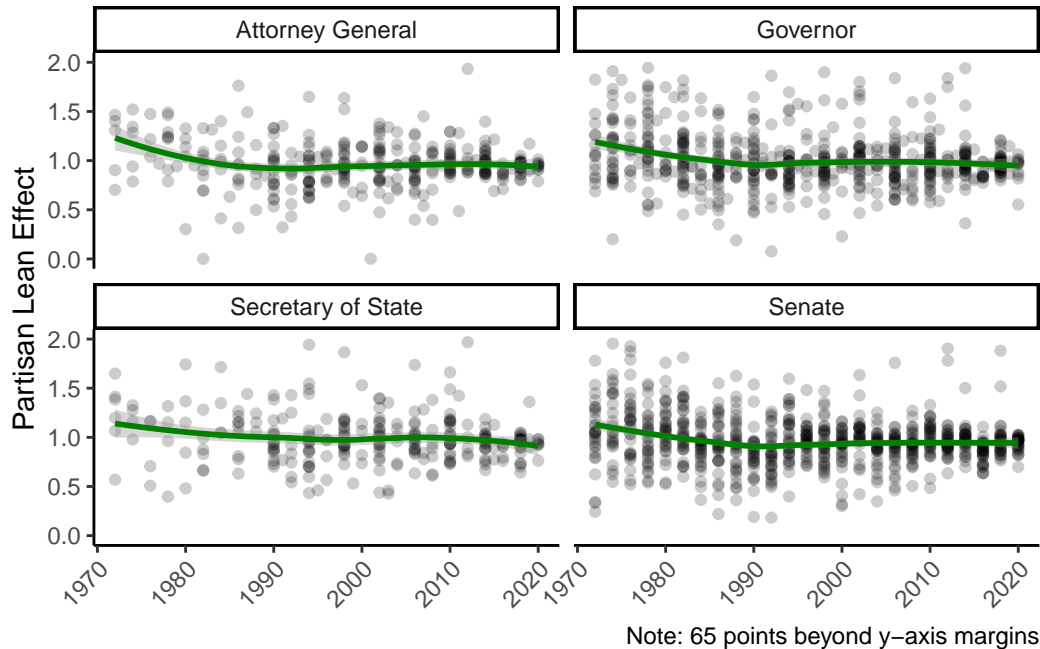


Figure 1.3: Partisan Lean Effects have Converged. Senate, Governor, Secretary of State, and Attorney General, 1972-2020

data, but 65 observations have values beyond the limits that influence the estimation of the trend line. The results are similar to the candidate valence effect: average partisan lean effects have trended toward those of Presidential contests since 1972. However, the bulk of this movement happens from 1972 to 1990. While candidate valence effects have continuously decreased over time, the decreases in partisan lean effects have dissipated since 1990, suggesting the realignment of contests around a common dimension was mostly complete by that time. This is an important advancement of our understanding of nationalization, as previous work has documented a mostly linear increase of nationalization over time without reference to variance in the underlying forces structuring the phenomenon.

While the average values of candidate valence and partisan lean effects help us understanding the degree to which contests are *centered* around a common point, we need to directly measure the *variance* in the effects to understand the relative similarities of such contests over time. I plot the standard deviations of both the absolute candidate valence and partisan lean effects in Figure 1.4, pooling across all contests in four-year intervals beginning with the most recent Presidential elec-

tion. For both election forces, we see a steep decline in variance from a high point in the early 1970s, with smaller but mostly linear decreases subsequently.

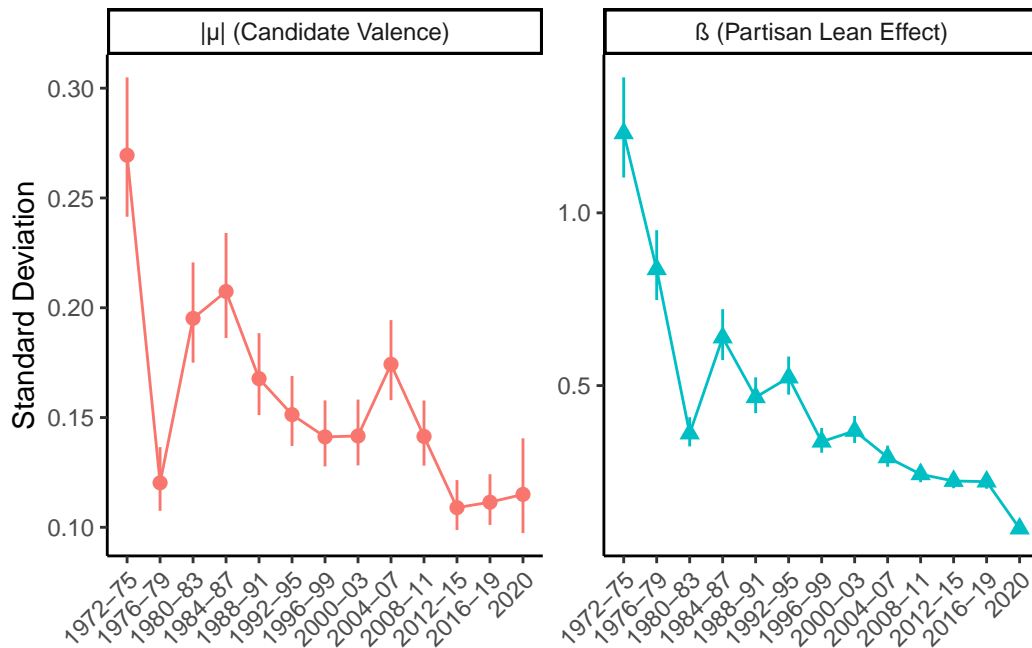


Figure 1.4: Variation in Election Forces has Decreased. Pooled Senate, Governor, Secretary of State, and Attorney General, 1972-2020

The final facet of nationalization I measure is the prevalence of stochastic, idiosyncratic noise in each election. As residual variance left unexplained by the candidate valence and partisan lean effects decreases, elections results across contests become more similar to each other. The decrease in idiosyncratic noise is shown in Figure 1.5, this time with the inclusion of Presidential contests.⁶ The residual standard error resulting from regressing the two-party Democratic candidate margin of victory on district partisan lean is shown as a point for each contest. These results are again remarkably similar across offices. While there has been a general downward trend in residual standard error, that trend appears steepest after 2000. This is in contrast to previous facets of nationalization shown above, where most of the movement occurs from 1972 to 1990. This suggests an almost two-step nationalization process: in one period, the factors shaping election outcomes snap into place, and in the other period the voting patterns fall in line more and more.

⁶Presidential contests were excluded from previous figures because they have consistent values of 0 and 1 for the valence and lean effects, respectively.

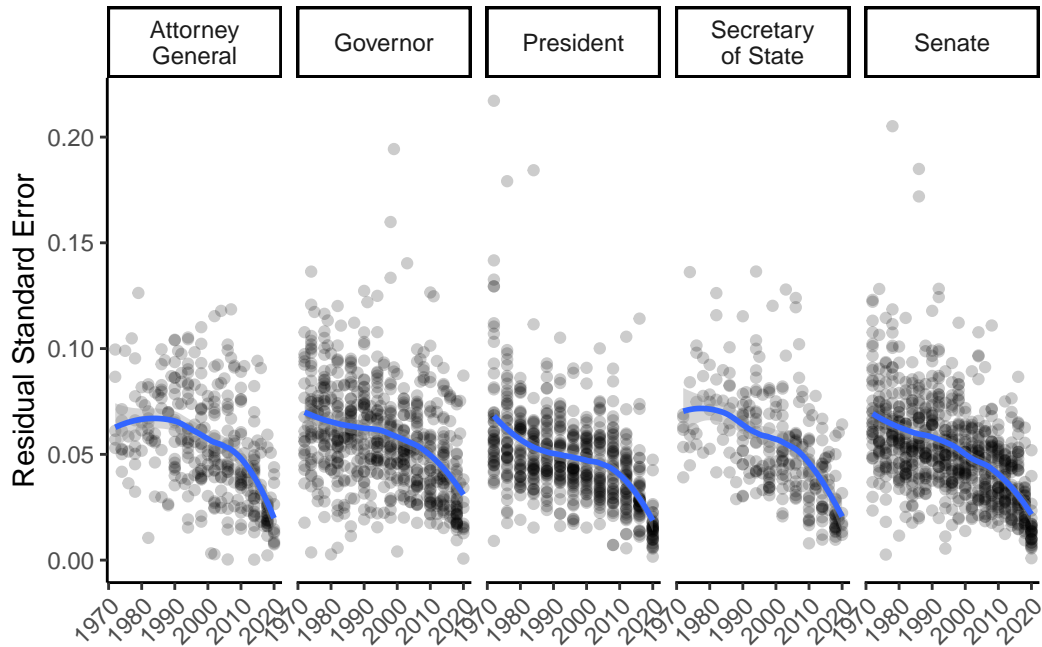


Figure 1.5: Idiosyncratic Noise has Decreased. President, Senate, Governor, Secretary of State, and Attorney General, 1972-2020

The preceding figures have documented a much more nuanced and complete picture of the nationalization of U.S. politics. As election forces and noise associated with those forces have decreased over time, U.S. politics has become more nationalized. This does *not* necessarily mean elections have become more “presidentialized,” or that the fundamental structuring forces in elections is the office of the President. Using the decomposition approach, I am able to distinguish such presidentialization from the more general phenomenon of nationalization. Figure 1.5 already offers some insight into this distinction, as it is *not* the case that the average error in Presidential contests is significantly less than the error in down-ballot contests. If U.S. elections are increasingly referenda on the president, we would expect the level of noise in Presidential contests to be lowest, especially in the contemporary period, but that does not appear to be the case.

As previously discussed, another observable implication of presidentialization within my theoretical framework is that presidential voting patterns should be **as good a predictor** of down-ballot patterns as a more general measure of district-level partisan leaning. Methodologically, we can treat this like any other prediction problem by horse-racing the two models against each other on

held-out data and comparing the level of residual error. I mimic this sort of cross-validation approach with the SVD method by holding out results for each down-ballot office, estimating the latent partisan lean of each district using the remaining results, then regressing the Democratic two-party margin of victory of the heldout office on the estimated district partisan lean. I obtain the presidential model comparison by simply regressing the two-party Democratic margin of victory in the down-ballot contest on the two-party Democratic margin of victory in the most recent presidential contest.

The results of this process are given in Figure 1.6. In the left panel, I plot the mean residual standard error for each model (using Presidential voting or estimated partisan lean) in each 4-year interval. Across all years, partisan lean is a better predictor of down-ballot voting outcomes than presidential voting patterns, with lower mean residual standard error. While the absolute difference between the two models appears fairly similar over time, more recent elections simply have less noise as a starting point. To better understand relative model performance over time, then, I plot how much better the partisan lean model performs in terms of percentage in the right panel of Figure 1.6. For example, if the presidential vote and partisan lean models had RSEs of 0.1 and 0.09, respectively, the partisan lean model is performing 10% better.⁷ Given the lower baseline rate of error in more recent elections, this means partisan lean acts as a much better relative predictor now than in earlier elections.

These over-time results suggest U.S. elections have nationalized, but not presidentialized. The general tendency for districts to vote for one party or another is a better predictor of down-ballot results than Presidential voting. While the forces shaping elections have largely homogenized over time, those forces have not congealed around the presidential election, at least for top-of-ballot statewide elections. In the next section, I extend my analysis to a larger array of statewide offices.

⁷Formally, $(1 - (0.09/0.1)) \times 100$

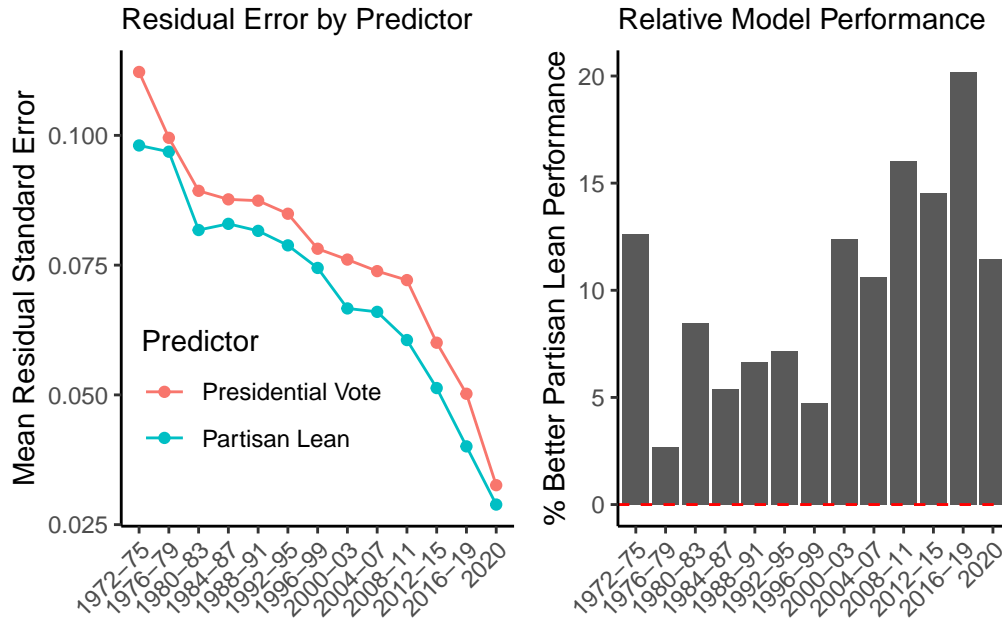


Figure 1.6: Partisan Lean is a Better Predictor than Presidential Voting

1.3.2 Nationalization in Statewide Contests

While a significant body of research has emerged surrounding the nationalization of senate and governor races over time, a significantly smaller amount of research has been done on contests further down-ballot. Extant research on these races tend to analyze one contest type at a time, such as State Supreme Court or school superintendent contests (Weinschenk et al. 2020; Weinschenk 2022). To fill this gap, I use data from the Voting and Election Science Team (VEST), part of the United States Election Project. These data provide precinct-level results for statewide races from 2016-2020, allowing for a much deeper look at how far nationalization reaches in contemporary U.S. politics. Table 1.1 gives summary descriptions of the VEST data; across 26 non-presidential contests, I analyze 300 state-contests and 476 unique contests. For comparability, I limit my analysis to partisan contests in general elections with at least one Democratic candidate and one Republican candidate. Results from these data give us a deep cross-sectional look into how nationalization acts as a homogenizing force in present-day politics.

I estimate the candidate valence and partisan lean effects (μ and β parameters) in an identical

Table 1.1: VEST Data Summary for Statewide Races

Office	Number of States	Total Contests
Federal		
US Senator	48	99
US House (At Large)	7	19
Total	55	118
State Executive		
Governor	49	62
Attorney General	39	49
Secretary of State	33	41
Treasurer	29	35
Auditor	20	25
Lieutenant Governor	16	20
Public Service Commissioner	5	15
Insurance Commissioner	8	11
Agriculture Commissioner	9	10
Superintendent of Public Instruction	7	9
State Controller/Comptroller	7	8
Commissioner of Public Lands	4	5
State University Regent	2	5
Labor Commissioner	3	4
Railroad Commissioner	1	3
Chief Financial Officer	1	1
Commissioner of School and Public Lands	1	1
State Board of Education	1	1
State Mine Inspector	1	1
Tax Commissioner	1	1
Total	237	307
State Judicial		
State Supreme Court	4	21
State Appeals Court	2	21
State Court of Criminal Appeals	1	8
Clerk of the Supreme Court	1	1
Total	8	51
Total	300	476

process to the over-time application. Each state-four-year interval is estimated separately, starting in 2016 (meaning the 2020 period includes only that year). Because the estimation process assumes consistent voting district boundaries over time, and precincts occasionally change such boundaries, I use areal weighted interpolation for consistent geographies over years (see Appendix 4.1.3 for

details). The resulting parameter estimates are shown in Figure 1.7. Presidential reference lines are plotted for μ and β at 0 and 1, respectively. I report the absolute values of μ , focusing on the magnitude of the candidate valence instead of the direction. The mean contest-parameter value is given by a cross.⁸

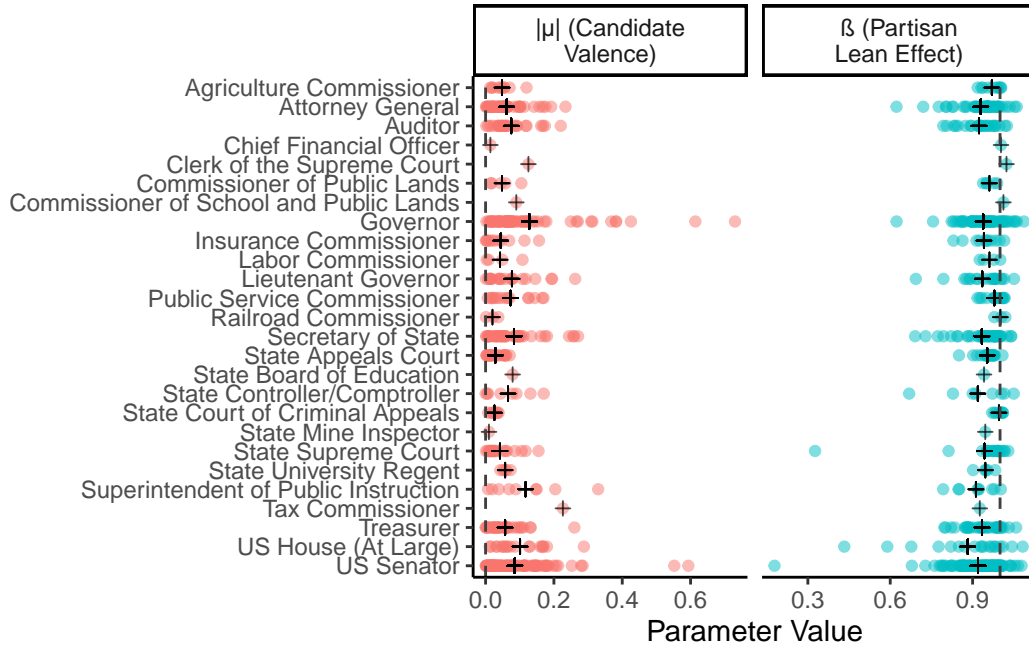


Figure 1.7: Parameter Values, Statewide Races 2016-20

Overall, mean parameter values across all contests are close to the presidential reference points, with generally limited variation surrounding those points. What variation does exist seems to do so in contests that are either Federal (US Senator and at-large House elections) or typically considered higher-salience elections (Governor, Lt. Governor, or Attorney General). This is consistent with theories of nationalization in which better information access about candidates allows voters to make decisions not based solely on the party identification of the candidates, but also paints a troubling representational picture down-ballot. In the races that are nominally the least like presidential elections (Railroad Commissioner, Public Service Commissioner, etc.), the voting behavior is the most similar. This result supports the formulation of nationalization as a homogenizing partisan force rather than a top-down, presidential force, as the offices with the most direct connections to the

⁸Appendix 4.1.4 directly compares my results to those generated by previously-used approaches.

presidency show the greatest amount of variation.⁹ When there are few cues other than partisanship to guide decisions, aggregate voting outcomes tend to closely follow partisan preference. As contests become more nationalized, μ and β converge to 0 and 1, respectively. Presidential elections are *not* consistently the elections with the strongest relationship to partisan lean. 77 down-ballot elections have greater partisan lean effects β than their most recent presidential counterparts, of which 13 are contests for governor and 12 for senate.

Certain observations merit closer consideration and explanation. First, consider the 2016 Alaskan US Senate election, which is the contest with the lowest partisan lean effect of 0.179 (and corresponding candidate valence of -0.6). Incumbent Republican Senator Lisa Murkowski won the election with 44.4% of the total vote, whereas Democratic challenger Ray Metcalfe received only 11.6% of the vote, placing him fourth behind Libertarian Joe Miller (29.2%) and Independent Margaret Stock (13.2%). It is therefore no surprise the rate at which estimated partisan lean is translated into Democratic votes is very low; many of those votes are not going to the Democratic candidate. This is a consistent feature of the estimation procedure; very successful third-party candidates relative to the third-party candidates in the presidential election can heavily influence the estimated parameters. In the contemporary U.S. context, however, “spoilers” like this are rare. In my sample, the average absolute difference between precinct-level Democratic vote share and two-party Democratic vote share for non-presidential races is only 0.01 (standard deviation 0.02). In the 2016 Alaskan Senate election, this difference was 0.08.

Another outlier is the 2016 North Carolina State Supreme Court election, with partisan lean effect of 0.326 (Democratic candidate handicap of 0.16). The winning Democratic candidate received 54.5% of the final vote, while the incumbent Republican candidate received 45.5%. Likely contributing to the outlier parameter results is the race being officially non-partisan; Democratic and Republican affiliations did not appear on the ballot, although the candidates were officially registered with their respective parties and had known affiliations (which is why I include them in the dataset).

⁹See Appendix 4.1.5 for additional summary descriptions of the parameter estimates.

How do the parameters vary across office categories? In Figure 1.8, I break offices into four categories: Federal (Senate, US House), State Executive - High (Governor, Lieutenant Governor, Secretary of State, and Attorney General: offices that are typically higher salience), State Executive (the rest of the statewide offices), and State Judicial. I then plot the regression line representing the relationship between partisan lean and vote-share for each election, with the intercept as the absolute candidate valence and the slope as the partisan lean effect.

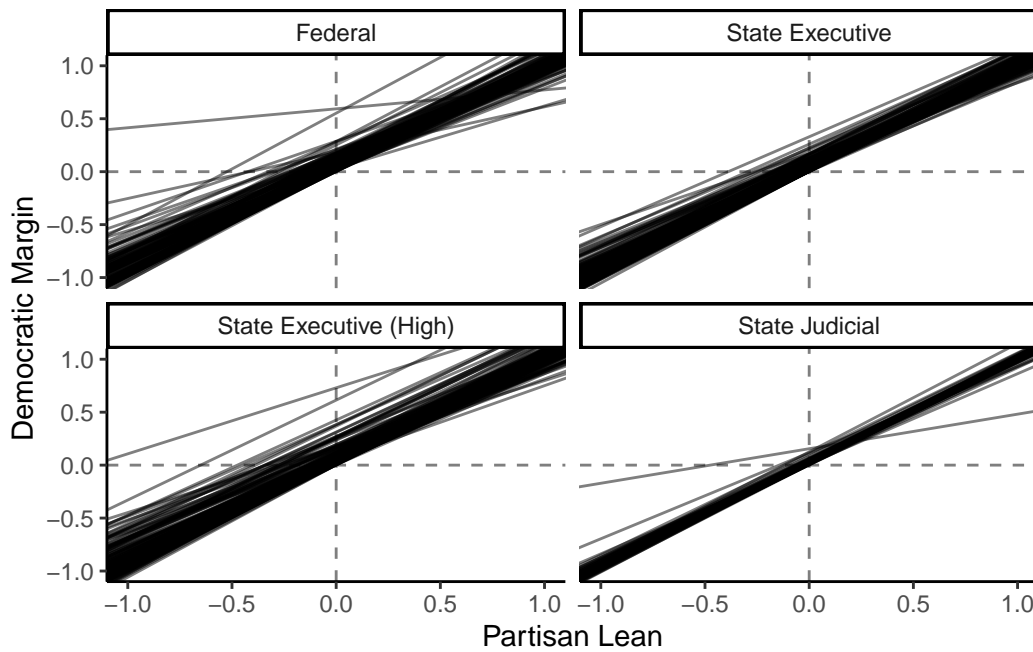


Figure 1.8: Variation in Election Forces Highest in Higher Office

The results confirm offices generally considered to be of high salience have the largest variation in the elements underlying election outcomes. Federal elections consistently show higher variation in both the intercepts (candidate valence) and slopes (partisan lean effect) of the lines. This matches expectations about high-salience elections; voters have easier access to information about candidate valence, and are also potentially able to make decisions based on dimensions of politics slightly different from simple partisan attachment. Higher state executive offices also show higher levels of variance, but moreso in candidate valence than the effect of partisan lean. Lower state executive offices show significantly less variance. These are typically offices with lower media coverage but still contested with partisan candidates, even if the offices themselves are responsible for fairly

narrow policy jurisdictions. The overall conclusion from these patterns at first seems counter-intuitive; the offices least like the presidency have elections results most similar to it, and the offices most like the presidency have the greatest differences in such results. Viewed through the lens of partisanship becoming a stronger organizing force in elections, however, such as result is expected. Insofar as voters see all politics as primarily related to partisanship and have little other information on down-ballot candidates to make decisions, we should expect variation in election factors to mostly be constrained to the stochastic elements of elections. Additional sources of variation among parameters (incumbency and news media) are discussed in Appendix 4.1.8.

Again, these cross-sectional results suggest elections have “nationalized” in that the forces shaping outcomes appear similar to each other across offices and elections. To determine if elections are “presidentialized,” I consider two observable implications of presidentialization under my theoretical framework: (1) partisan preference and vote choice should be most tightly linked in presidential contests, and (2) presidential vote share should be a stronger predictor of down-ballot vote shares than partisan lean. Figure 1.9 shows the results of two analyses measuring those implications. In the left panel, I regress the precinct-level Democratic candidate margin of victory on the estimated precinct partisan lean within all contests in the VEST data and recover the residual standard error. To account for compositional differences in contests between states (where some states hold contests for certain offices but not others), I normalize the RSE within state by dividing by the RSE in the presidential election. I then report the mean normalized residual standard error for each office type as a measure of model fit; the greater the residual standard error, the less variance explained by underlying preference. Compared to other contests, presidential races actually have the second highest mean residual standard error. Mechanically, this means the translation of partisan lean to presidential vote share is not error-less. Other factors beyond the latent partisan dimension are significant movers of vote share. This evidence gives relative support to the understanding of nationalization as the homogenization of election dynamics around a common partisan dimension. The linkage between partisan preference and vote choice does vary across offices, but is generally fairly similar.

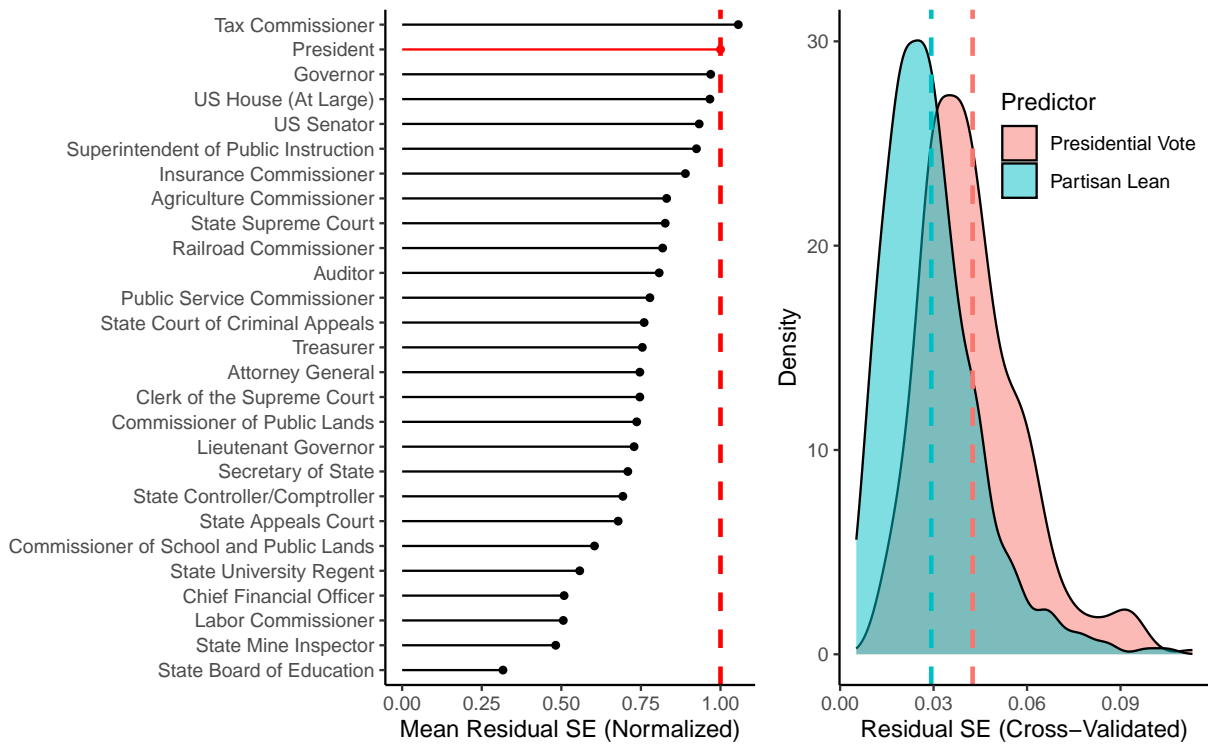


Figure 1.9: Presidential Elections are the Most Idiosyncratic and Less Predictive than Partisan Lean

In the right panel of Figure 1.9, I plot two distributions of residual standard errors. The blue distribution is created using my SVD approach to measuring latent preference and mirrors the analysis conducted in the left panel; I regress the Democratic margin of victory in each contest on the latent partisan lean and record the residual standard errors (excluding the presidential races). I again use a cross-validation approach where the summary statistics for any given office is calculated after leaving that office out of the initial estimation of partisan lean. The red distribution is created using the standard bivariate approach to measuring nationalization, where the down-ballot Democratic margin of victory is regressed on the Democratic presidential candidate’s margin of victory. Here I compare how well each measure does in predicting down-ballot vote shares. According to the presidentialization hypothesis, presidential voting should be the strongest predictor and therefore have the smallest residual standard error. The right panel shows this is not the case; the mean residual standard error using latent partisan preference as a predictor is significantly less than the mean residual standard error using presidential voting as a predictor. This again suggests voting behavior

is not structured by simple reference to presidential candidates, but by latent partisan preference across all offices.

1.3.3 Nationalization in Local Elections and Ballot Measures

My theoretical framework and methodological approach allow for additional analyses to be performed in a wide array of contexts, data-permitting. In this section, I consider two different applications in a large U.S. county (Maricopa, Arizona), focusing on local-level contests and ballot measures, respectively.

One of the advantages of using precinct-level election results is the ability to make inferences about local-level contests. I demonstrate this using election data from Maricopa County, Arizona, obtained directly from the county elections department. Maricopa has a number of features making it a particularly useful case study. First, it has become an important swing county in presidential contests over the previous election cycles. Joe Biden narrowly carried the county with 50.1% of the total vote in 2020. Second, it encompasses substantial demographic diversity. The county has a fairly dense urban center surrounded by sprawling suburbs and very rural outskirts. The 2020 Census estimates Maricopa residents are 53.4% non-Hispanic white, and the population has a comparable poverty rate (11.6%) and education rate (33.4% over age 25 with a bachelor's degree) to that of the U.S. as a whole (11.4% and 37.9%, respectively). It is the fourth largest county in the U.S. by population, with over 4 million residents. Finally, it has numerous partisan down-ballot races, making it an ideal candidate county for analysis.

Similar to the VEST data, the Maricopa results span the contemporary period, but this time cover 2008-2020. The data cover seven elections periods (every two years), 19 distinct office categories, and 203 unique contests. I estimate the parameter values similarly to the previous iterations, grouping into 4-year time periods and setting the reference category to the most recent presidential election.

Figure 1.10 shows the county-wide results for all partisan races in Maricopa. Similar to the statewide election results, the mean parameter estimates are concentrated around their presidential

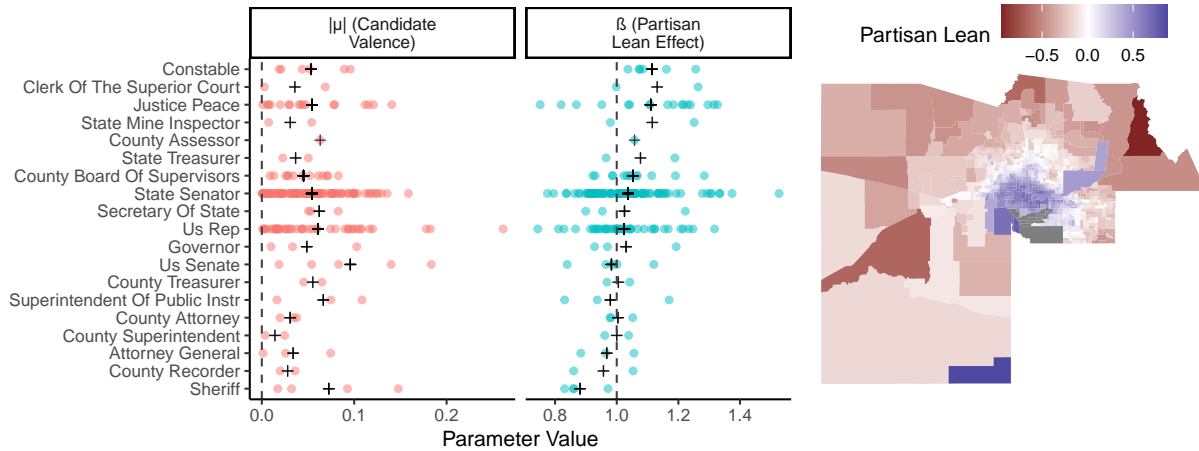


Figure 1.10: Maricopa, AZ Parameter Estimates, 2008-2020

reference points, but variation exists for many races. State senator elections and U.S. House elections have some of the widest ranges, although even they tend to fall within 0.2 of the presidential reference point.¹⁰ One election of note is the 2016 county sheriff contest, with the eight-lowest partisan lean effect (β) value (0.83) and sixth-highest candidate valence (μ) value (0.15) of the races analyzed in the county. Democratic challenger Paul Penzone defeated six-term incumbent Joe Arpaio by 11.2 percentage points. Arpaio had become nationally prominent for hard-line immigration stances, was charged with criminal contempt for ignoring a judge’s ruling in a racial profiling case, and was a vocal proponent for Donald Trump’s presidential campaign. While the race was “nationalized” in the sense that it gained considerable media attention linking Arpaio with Trump, the results were markedly different from the modal partisan contest. This challenges the typical president-centered notion of nationalization where down-ballot contests are tied to presidential outcomes.

The right panel of Figure 1.10 shows the geographic distribution of precinct partisan lean in the county for the 2020 election. The results align with expectations; the urban center is markedly more Democratic-leaning than the rural outskirts, with a more moderate suburban ring separating the two. Two unique precincts are the more rural but Democratic-leaning precincts to the northeast

¹⁰Arizona’s lower house uses multi-member state representative districts where the candidates with the two highest vote totals are elected, so I exclude them from my results here.

of the urban core and at the southern edge of the county. These are portions of the Salt River Pima–Maricopa Indian Community and Tohono O’odham Nation Reservation, respectively. The missing geography in gray is an uninhabited area including a nature preserve and the northern edge of the Gila River Indian Reservation.

To determine if local elections are potentially more “presidentialized” than statewide elections, I perform the same set of analyses with the residual errors measured by contest and predictor as in the cross-sectional statewide context. The results are presented in a similar fashion in Figure 1.11, with almost identical results. Across 12 years of elections and 19 different offices, presidential elections in Maricopa have the third highest mean residual standard error, behind only state treasurer and county sheriff elections. This suggests presidential elections are among the most idiosyncratic in the county, whereas other contests more closely follow a common latent dimension of partisan lean. The story is similar in the right panel of Figure 1.11; partisan lean is again a significantly better predictor of down-ballot (statewide and local) election outcomes than presidential voting. While potentially nationalized, local contests are not presidentialized.

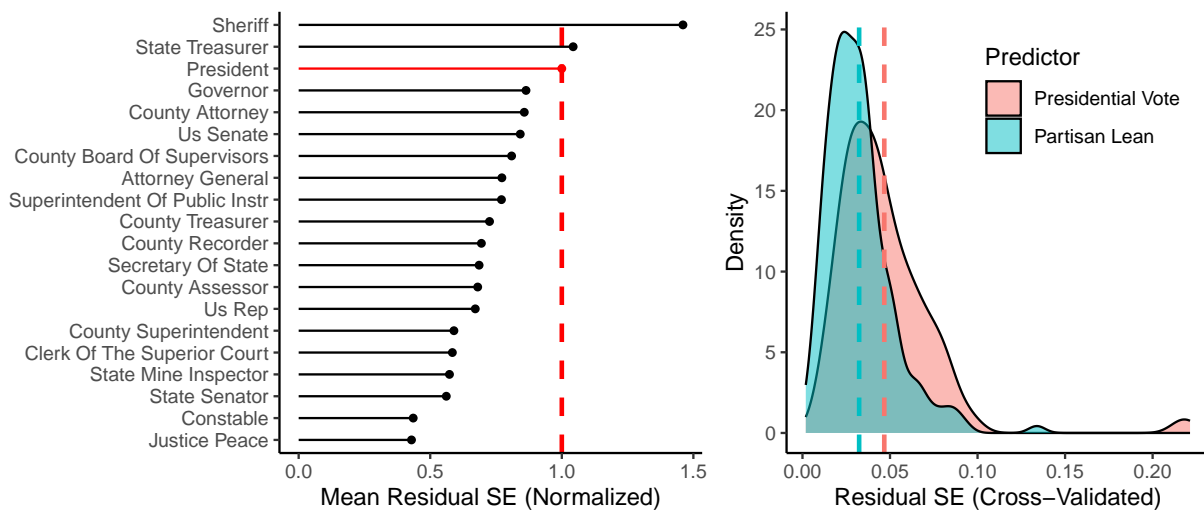


Figure 1.11: Prediction Error and Predictor Comparison in Maricopa, AZ, 2008-2020

While research of nationalization often focuses on candidate elections, some work has been done suggesting nationalization has policy consequences as well. Burke (2021), for example, finds states with more nationalized election outcomes (measured by regressing the state’s two-party vote for

Democratic legislative candidates on the Democratic presidential candidate's two-party vote share) have legislative agendas focusing more on divisive national issues (such as abortion) versus local issues (such as education and transportation). This may suggest a high level of elite partisan sorting, but do such dynamics exist in the broader electorate? More broadly, are the political dimensions underlying candidate and policy questions similar?

Because the decomposition approach yields a precinct-level measure of partisan lean, I am able to analyze outcomes of ballot measure contests in manner similar to candidate contests. Instead of using the two-party margin of victory for the Democratic candidate, I can just use the "Yes" margin of victory for any particular proposition. This gives the resulting parameters slightly different interpretations. The partisan lean effect β can still be interpreted as a translation of partisanship, but the translation is of partisan lean into "yes" votes. This means more Republican-leaning ballot measures will have a negative slope. The candidate valence μ becomes a "yes" valence, or how far ahead the "yes" position is when precinct partisan lean equals zero. For simplicity, I present the absolute value $|\mu|$ as a measure of valence magnitude.

I apply the approach to 38 statewide propositions in Arizona from 2008-2020, using precinct-level results from Maricopa County. These propositions cover a range of policy dimensions, including the legalization of marijuana (propositions 203, 205, and 207), the legal definition of marriage (102), payday loan industry regulation (200), and the right to hunt (109). Many of these propositions don't align neatly with preexisting partisan splits. For full descriptions of each proposition, see Appendix 4.1.7. To determine how closely proposition outcomes track candidate-election outcomes, I use the predicted precinct-level partisan lean estimated using all partisan elections from above as the predictor variable for the precinct-level "yes" margin of victory. This yields the slope parameter as the partisan lean effect β and the intercept as the valence μ . The results are shown in Figure 1.12, with successful propositions shown in green and failed propositions in red.¹¹

The results in Figure 1.12 show a very different set of outcomes relative to candidate contests. Most obviously, the range of outcomes for $|\mu|$ and β have changed dramatically. The valence pa-

¹¹One proposition (208) increasing taxes on individuals making more than \$250,000 to fund an increase of teacher salaries was later ruled unconstitutional by the Arizona State Supreme Court.

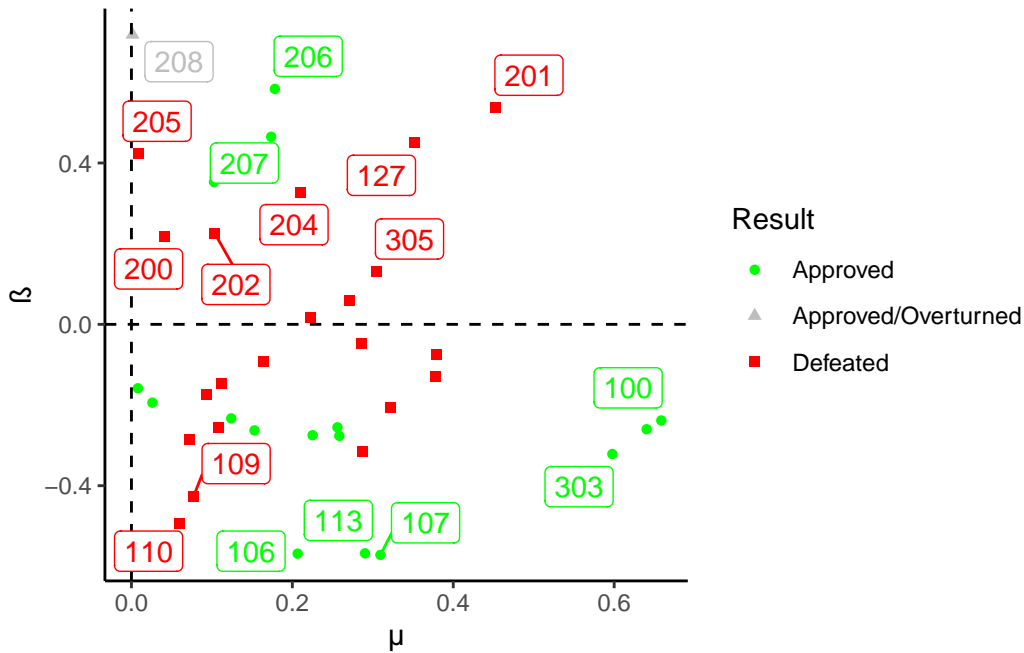


Figure 1.12: Statewide Proposition Results, Maricopa, AZ, 2008-2020.

parameter $|\mu|$ now ranges from 0 to above 0.6, a much broader range, while the partisan lean effect β ranges from roughly -0.6 to 0.5, a broad range but one that falls well short of the rate at which partisan lean was translated into Democratic candidates votes in candidate elections. This suggests that individual issues, especially at the state level, have a much looser connection between partisan lean and positions on the issues themselves. Certain propositions do have relatively high/low partisan lean effects, such as Proposition 106 ($|\mu| = 0.2, \beta = -0.57$), which prohibited rules against participation in specific healthcare, and Proposition 107 ($|\mu| = 0.3, \beta = -0.57$), which banned preferential acceptance to public employment (seen as an affirmative action ban). Both of these have clearer national party positions, but even these more extreme values fall short of most preference modifiers in candidate elections.

While the parameter estimates are substantially different between candidates and ballot measure contests, the estimates for precinct-level partisan lean are not. Using the SVD approach, I separately estimate the precinct partisan lean for all precincts in Maricopa, Arizona in the time intervals 2008-11, 2012-15, 2016-19, and 2020 using two sets of contests: (1) all partisan candidate contests and (2) all ballot measures. I include the most recent presidential contest in each as the reference

point. The correlation between the precinct partisanship estimates is 0.98, suggesting a strong latent dimension of preference underlying both sets of contests. This continues to be the case even when using a non-presidential race as the reference point for the ballot measure estimation process; if I include county sheriff in the ballot measure estimation instead of the presidential contest, the correlation stays remarkably high at 0.94. Even if I remove the sheriffs' race from the candidate contest estimation altogether and compare entirely disjoint sets of contests, the correlation remains 0.94. This point bears emphasizing; with my decomposition of voting patterns, I obtain highly correlated estimates for precinct-level preference over a 12-year period using a set of partisan elections (for offices as different as President and Justice of the Peace) and a set of ballot measures (with questions ranging from same-sex marriage to the salaries of state legislators) with no explicit partisan labels.

How do the partisan lean estimates derived from ballot measures perform against presidential voting when predicting vote margins? In this context, the evidence is mixed. I perform similar analysis to those performed previously in Figure 1.9, comparing the performance of presidential voting and partisan lean derived from ballot measure behavior as predictors. The distributions of the residual standard errors across all contests are plotted in Figure 1.13, with the dashed vertical lines representing the mean values for each estimation method.

Presidential voting slightly outperforms the partisan lean estimates derived from ballot measure results. This is true across all office categories except for constable, although the differences between the two estimation processes are most pronounced in county-level offices and most similar in statewide offices. Given the SVD estimation process is using data devoid of partisan labels (besides the presidential reference point), however, the similarity between the two distributions is quite striking. It is not entirely unexpected that the ballot measure estimates may yield a dimension of preference slightly different to the partisan dimensions that structure behavior in partisan contests. Indeed, the questions considered in many of the ballot measures are not obviously partisan in nature, and the precinct-level partisan leans are estimated using relatively few ballot measures per interval of time (38 propositions across 12 years). As expected, when I compare the accuracy

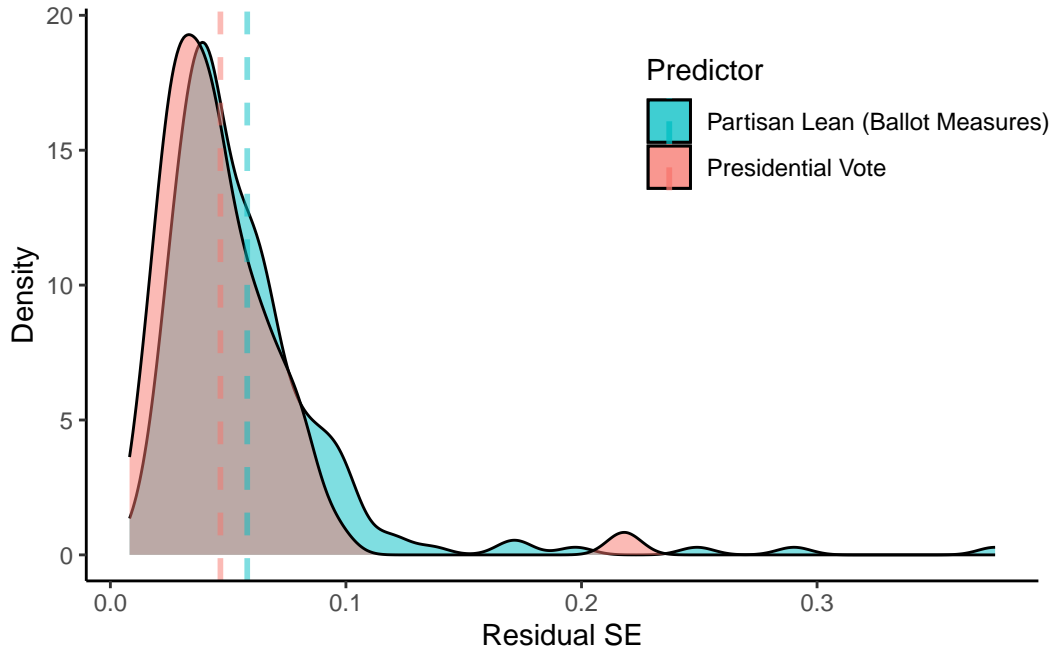


Figure 1.13: Presidential Voting Outperforms Ballot Measures as a Predictor of Downballot Results. Maricopa, AZ, 2008-2020

of the ballot-measure SVD preference estimates versus presidential vote shares when predicting ballot measure outcomes, the ballot measure approach significantly ($\alpha = 0.001$) outperforms the bivariate approach.

Substantively, these results suggest the contours of political behavior in ballot measure contests are slightly noisier and less well-defined than such behavior in partisan contests. In that sense, behavior in such contests is less “nationalized” than behavior in partisan contests, as the dimensions over which such issues are contested have yet to completely homogenize around partisan lean. This is likely partly due to the issue-specific nature of ballot measures. Where partisan contests are inherently “bundled treatments” insofar as candidates and their parties take positions on multiple issues, ballot measures simply ask voters to respond yes or no to a single question, inviting higher variance in behavioral outcomes. However, while beyond the scope of this paper, the results suggest the partisan leaning of a district is not devoid of policy preferences. Insofar as the dimensions of politics underlying both candidate and policy suggestions are fairly similar, it seems more likely the policy dimension informs partisan decisions than partisan attachment informs non-partisan policy

behavior.

1.4 Discussion and Conclusion

The results of this paper provide a more theoretically grounded and substantively holistic picture of nationalization in contemporary U.S. politics using data from the most granular level of aggregation possible in the measurement of true voting behavior. The over-time descriptive results demonstrate how the alignment of presidential and down-ballot results is largely a function of decreasing variability in how preference is translated into votes rather than (1) purely top-down influence from presidential candidates or (2) the sole homogenization of candidate effects. The results for statewide contests from 2016-2020 show the current reaches of nationalization, with both candidate and preference effects being very similar on average across all offices. I find evidence supporting the conclusion that partisanship has become a stronger organizing force across all US politics. My results also belie substantial variation. While this variation is limited in comparison to previous periods of U.S. politics, it allows us to consider cases where the connection between partisan preference and vote choice is weaker. These results extend to even the furthest of down-ballot races analyzed in Maricopa, Arizona. Overall, all contests, from county recorder to President of the United States, are explained by similar translations of partisan preference to vote choice and fairly minimal candidate-level effects. Future work should expand the set of elections to include more years of statewide contests and a greater diversity of local contests, with the latter allowing for a more granular consideration of sources of variation in nationalization.

While all elections appear as manifestations of similar forces, presidential elections appear as some of the most idiosyncratic in U.S. politics. They are some of the noisiest elections with regard to the translation of district partisanship into partisan votes. Therefore, while undoubtedly nationalized, U.S. elections are likely not “presidentialized” to the extent many observers have diagnosed. This should not be entirely surprising, as presidential contests involve some of the most idiosyncratic features of our political system.

These results invite a deeper conversation about the representational consequences of nationalization. The primary concern of research on nationalization is one of accountability; if voters are making decisions for state and local offices using criteria unrelated to the demands of the office or the qualities of the candidates, how can elected politicians be held electorally accountable? This is fundamentally a concern about the performance of U.S. federalism and voters' ability to navigate a slate of offices ranging from President to local dogcatcher.¹² Evidence is mixed on voters' abilities to assign functional responsibility of certain policies to the appropriate offices (Arceneaux 2006; Brown 2010; de Benedictis-Kessner and Warshaw 2020; Maestas et al. 2008). It is more than understandable voters cannot name the responsibilities of the dozens of office contests they vote in, let alone name the positions taken by particular candidates.

Using partisan identification as a heuristic in this situation seems natural. My results suggest this is generally the case across all offices; underlying partisanship translates into votes in an almost one-to-one manner regardless of contest. The deeper question is whether such a dimension is *appropriate* for choices in these elections. While the case could be easily made many facets of state and federal politics are correlated (and the issue domains themselves permeable), such claims become more tenuous at more local levels of government and especially in more specialized office capacities. Tausanovitch (2019) poses the question of why subnational governments are so responsive despite their many institutional and behavioral afflictions: off-cycle elections, low turnout, sparse information, and issues that don't neatly fit on a simple left-right issue dimension. Perhaps the answer is the dimensions of subnational and national politics are sufficiently correlated to produce representation without knowledgeable accountability. The adage "there is no Republican or Democratic way to collect garbage" is not entirely accurate; Democrats may be more willing to dedicate larger portions of the municipal budget toward trash collection, or Republicans may prefer a privatized garbage collection arrangement. Future work should explore the ideological connection between nationalization and policy representation in greater depth and consider the consequences of potential "representation by mistake."

¹²Until March 2019, town dogcatcher was still an elected position in the town of Duxbury, VT. The last officeholder was a 15-year incumbent.

My results raise a related question: what are the consequences of persistent variation in *candidate valence effects* for quality representation? These effects persisted (at least for gubernatorial and senatorial elections) since 1974 and pose a complicated problem for representation. If the partisan dimensions on which voters are making decisions for down-ballot office are inappropriate for the offices in question, we would perhaps prefer to see greater candidate effects. However, if these effects are informed by equally dubious facets of politics or require sufficient candidate-specific information in low-salience elections, what are the consequences for representation? Future work should consider additional sources of variation in these effects, such as campaign dynamics, endorsements, and candidate demographics, and investigate their connections to representation.

In conclusion, the results of this paper raise important questions regarding the quality of representation and performance of federalism in U.S. politics. By considering separately the partisan and candidate-level dimensions of contests across a variety of contexts and time periods, we are better able to understand the fundamental drivers of mass voter behavior. Future avenues of research are plentiful in regard both to theoretical advances and advances in data availability, and should be pursued with renewed interest.

Chapter 2

Nationalized Elections, Localized Campaigns? Classifying gubernatorial rhetoric, 2000-2018

A growing literature in political science focuses on the “nationalization” of U.S. politics. Generally, this phenomenon refers to national political actors and issues influencing state and local political activity (Abramowitz and Webster 2016; Hopkins 2018; Sievert and McKee 2019). The most prominent results in this literature point to the increasing correlation between vote shares of presidential and down-ballot candidates of the same party. This nationalization of election results has potentially problematic effects on the quality of representation from the winners of such down-ballot contests. If voters are evaluating candidates not based on job demands specific to the elected office (which at the state and local level is in many cases orthogonal to the contours of national politics) but simply on the partisan affiliation of the candidates, holding an office-holder accountable becomes significantly more challenging.

A large portion of the nationalization literature links nationalized election results to a lack of information specific to down-ballot races (G. J. Martin and McCrain 2019; Moskowitz 2021). If voters have access to distinct down-ballot political information, they are more likely to make voting decisions less directly influenced by co-partisanship with national candidates. While these studies

focus predominantly on the media environment, one source of political information remains understudied; the content of political campaigns themselves. A growing narrative has emerged within popular media that the content of down-ballot political campaigns has grown more similar to the campaigns of candidates for national office. By referencing the talking points of prominent national candidates, down-ballot candidates are perhaps able to easily identify themselves to voters who are already more familiar with the national candidates.

In this paper, I evaluate the extent to which the rhetoric of gubernatorial candidates reflects the topics referenced by their national counterparts. I consider a broad array of political speech, spanning televised electoral debates from 2000 to 2018, televised political advertisements from 2004 and 2008 election campaigns, and the Twitter activity of incumbent Governors and Members of Congress in 2018. I approach the measurement of nationalized rhetoric as a text classification problem by constructing a classification model based on topic representations of speech known to be of state or national providence. The results show a more complete and nuanced picture of gubernatorial campaign rhetoric; gubernatorial candidates overall tend to reference topic distinct from their national counterparts, but are more likely to “nationalize” their speech in televised ads and social media. These results have significant consequences for voter behavior; if voters are effectively exposed to gubernatorial campaign messaging, they are at least nominally able to make voting decisions that aren’t as susceptible to nationalized pressures. However, voters are also susceptible to appeals that *are* more nationalized, and if these messages are the ones highlighted by media outlets, then nationalized voting behavior may follow.

The paper proceeds as follows; first, I briefly review existing literature on nationalized elections and their underlying mechanisms, following this discussion with anecdotal and theoretical extensions to campaign rhetoric. Second, I present my methodological approach to measuring the nationalization of political rhetoric. Third, I apply my approach to three corpuses: debates, advertisements, and Twitter. I conclude with a discussion of the results and implications for future research.

2.1 Nationalized elections

Many scholars have documented an increasing correlation between Presidential and down-ballot vote shares for co-partisan candidates since the 1970s. These correlations extend to gubernatorial, U.S. Senate, and House of Representatives elections, with correlations coefficients reaching and exceeding 0.8 (Hopkins 2018; Jacobson 2015a; Sievert and McKee 2019). These trends are also apparent in lower-salience down-ballot elections such as State Supreme Court and Superintendent of Public Instruction (Weinschenk et al. 2020; Weinschenk 2022). This phenomena has been dubbed “nationalization” by scholars, gesturing toward the theory that these increasing correlations are largely driven by top-down forces.

What specific mechanisms do scholars propose for such nationalization? Some point to the primacy of partisan identity as an affective group identity. Huddy and Bankert (2017) and Iyengar, Sood, and Lelkes (2012) view voting as an expression of in-group favoritism and out-group disfavor. As polarization worsens, the value of the partisan label for distinguishing between in- and out-group members increases. This is particularly important with regard to out-group antipathy; higher rates of such antipathy are associated with higher rates of straight-ticket voting (Abramowitz and Webster 2016). These associations extend to feelings of anger as well (Webster 2020). Of course, voting is not a purely emotional exercise. Costa (2021) and Mummolo, Peterson, and Westwood (2021) both use conjoint designs to show voters use policy position information to make decisions between candidates, even candidates have known party affiliations or prioritize appeals to partisan affect. This makes partisanship a bundled treatment of both emotional attachment and policy information.

Other scholars emphasize this policy information aspect of partisanship when explaining nationalization. As parties at the state and national levels become more unified entities (Caughey, Dunham, and Warshaw 2018; Hopkins 2018) and state and national ideological dimensions reduce to one (Caughey and Warshaw 2016; Shor and McCarty 2011), partisan signals at all levels become meaningful indicators of policy positions. This is likely exacerbated by decreasing access to both

televised local news and the shuttering of many local newspapers, making state- and local-specific information more costly to obtain (Abernathy 2018; Hayes and Lawless 2018). As such access wanes, nationalized behavior seems to increase (G. J. Martin and McCrain 2019). When access is available, however, straight ticket voting becomes more likely (Moskowitz 2021).

2.2 Nationalized campaigns?

The strength of party identification, relative lack of prolonged media attention in down-ballot contests, and correlation between state and national political dimensions may incentivize candidates for subnational office to make nationalized rhetorical appeals during their campaigns to amplify their candidacy. The mechanisms discussed above largely focus on voter-level factors that influence the electorate's propensity to vote in a nationalized manner. While elite behavior certainly pushes the trend in a similar manner through cross-state homogenization of party platforms and the more rigorous sorting of partisans along ideological lines, the effects candidates themselves can have in any given election remains understudied.

Media portrayals of gubernatorial campaigns have stressed their “nationalized” content. For example, national media outlets characterized the 2019 gubernatorial races in Kentucky, Louisiana, and Mississippi as being nationalized due to Donald Trump’s personal involvement in some of the races and an emphasis on impeachment of Trump as a campaign issue (Manchester 2019; J. Martin 2019; Rojas and Alford 2019). Other outlets gave similar appraisals of other races, including Washington in 2016 (“Inslee... was happy to nationalize the governor’s race, sounding at many events like he was running against Trump”), West Virginia in 2011 (where the Republican Governor’s Association spend \$3.5 million in ad buys in an attempt to link the democratic candidate to Obamacare), and Texas in 2010 (“Mr. Perry turned the race into a referendum on federal spending” (Brunner 2016; Catanese 2011; McKinley Jr 2010). Some Governors have engaged in nationalized rhetoric themselves, with Governor Gavin Newsom of California characterizing supporters of the 2021 recall election as “a partisan, Republican coalition of national Republicans, anti-vaxxers,

Q-Anon conspiracy theorists and anti-immigrant Trump supporters.” At the very least, candidates for state office do not feel bound only address or espouse policies, individuals, and organizations exclusive to their own states.

Some empirical evidence exists to suggest political rhetoric has broadly nationalized in the same manner elections have. Das et al. (2022) analyze the tweets of incumbent Members of Congress, Governors, and mayors in 2018 utilizing a topic modeling approach to ascertain the level of semantic similarity between the different office holders. They find Members of Congress and Governors are almost indistinguishable in terms of their topical similarity, while mayors still seem to tweet about distinct topics. These findings give pause to the “all politics is national” hypothesis, at least at the local level, but still suggest gubernatorial rhetoric has nationalized parallel to the nationalization of electoral results. It is important to note, however, that the Twitter activity analyzed by Das et al. (2022) is not specific to campaigning, focuses on sitting incumbents, and may also contain content that is apolitical in nature. For example, New Jersey Governor Phil Murphy has, since February 2022, tweeted about his daily Wordle score, the Saint Peter’s University men’s basketball team, and changing the state bird of New Jersey to the middle finger for April Fools.

Both the identity and information mechanisms prove potentially useful in explaining this potential nationalization of campaign rhetoric. If voters are predominantly motivated by simple party identification, nationalizing one’s campaign appeals in gubernatorial contests may boost signals of partisan type by linking candidates to more traditional, national-level policy positions. This makes candidates more “identifiably” Republican or Democrat. Alternatively, nationalizing campaign appeals may have the effect of diluting the pool of locale-specific information available to voters, instead focusing the information environment on national signals of partisan type.

While there are many plausible reasons to nationalize a gubernatorial campaign, there are equally plausible reasons to keep a campaign localized. The most obvious reason is voters may recognize a candidate running on nationalized appeals has no jurisdiction over the issue being discussed. Current research is divided on the extent to which voters hold politicians accountable for conditions under their jurisdiction; Arceneaux (2006) finds survey respondents tend to attribute

credit/blame to offices which they (fairly accurately) assign functional responsibility to, whereas Brown (2010) finds partisanship moderates the attribution of functional responsibility and subsequent credit/blame. De Benedictis-Kessner and Warshaw (2020) find some evidence for both conclusions using time series, cross-sectional models; voters routinely hold the president’s party responsible for local economic conditions, but also hold governors accountable for such conditions. Therefore, the incentives for candidate to nationalize gubernatorial campaigns seems mixed.

Additionally, rhetorical context may influence the content of campaign appeals. Such differences may emerge from the perceived audiences of different medium and constraints on message length or content (Bossetta 2018; Owen 2014; Stier et al. 2018). From a nationalization perspective, we may expect messages broadcast through less geographically-defined medium to emphasize more national political themes, perhaps as a means to fund raise outside of one’s jurisdiction (Reckhow et al. 2017). As the available space or time for the message decreases, we may also expect appeals to homogenize toward more familiar appeals to national partisanship, while longer-form messages can explore locale-specific details of certain issue dimensions. To fully understand the dynamics of nationalized rhetoric, we must therefore consider a broader array of rhetorical contexts.

2.3 Methodological approach

I approach the potential nationalization of political rhetoric during campaigns as a supervised text classification problem; the collection of words spoken or otherwise disseminated during a campaign can be categorized as having either national or state content. This approach allows me to measure the presence of nationalized rhetoric across a broad array of rhetorical contexts.

The workflow involves (1) defining training data where document “class” (national or state prov-idence) is known, (2) quantitatively representing text using a topic-modeling approach, (3) fitting a classification model using the training data and quantitative text representation, and (4) predicting the class of test data using the trained classification model. I describe each step of the workflow in more detail below. The result of this workflow is a state/national classification rate of political

campaign rhetoric at the state level, where a high national classification rate signifies campaign activities sharing similar characteristics to national-level rhetoric and where a high state classification rate signifies activities sharing similar characteristics to state-level rhetoric.

2.3.1 Training data

A classification approach requires a model to be fitted with data of known classes (state or national). This precludes the possibility of using words from campaign activities as part of the training process; by design, these activities are of ambiguous “class,” being potentially more state or national in content than their providence would suggest. Additionally, the training data must be substantively representative of the classes I aim to predict. To this end, I synthesize a corpus of Presidential speeches (State of the Union addresses and opposition responses, inaugural addresses, official statements, and national party platforms) and gubernatorial speeches (State of the State addresses and budget addresses) representing national and state political content, respectively. The final training corpus contains 1,038 speeches and documents, 227 national and 811 state, spanning 2000-2018.

This training corpus is meant to distinguish between national and state political content via the policy discussions in each respective sphere. State of the Union/State speeches are particularly useful in this context, as they often involve explicit references to policy accomplishments and goals. However, this does not prevent certain words and phrases from existing in either the state or national contexts that are highly predictive of a particular class but devoid of policy content. For example, most gubernatorial State of the States addresses include the state names themselves and the names of residents for those states (such as “Californians” or “Hoosiers”). Including these words during the model fitting process would potentially allow for the model to “cheat” and accurately predict class not from the policy content of a speech but from these cheap signals of state providence. Therefore, during standard text pre-processing of the training data (stopwording, lemmatization, removal of very short or very rare words), I also remove all state names, names of state residents (including nicknames), references to the level of office (besides presidential), common audio transcription tags (laughter, applause), and common words without policy meaning (year, will, thank, etc.).

2.3.2 Quantitative text representation

To predict class, the text of any document needs to be quantitatively represented. In classic supervised learning approaches, the choice of quantitative text representation is driven by best out-of-sample prediction accuracy. In my application, however, the substantive meaning of the text representation is equally important, as I want to interpret the classification rate as a meaningful indicator of overall national content. Therefore, I represent all text in this paper as estimated topic proportions using a structural topic model (STM) approach.

STM treats texts as “bags of words.” Like the latent Dirichlet allocation (LDA) approach, STM assigns words to topics and topics to documents probabilistically (Roberts, Stewart, and Airoldi 2016). The output gives word probabilities associated with each topic and the topic proportions for each document. STM builds upon LDA by allowing topic probabilities to vary according to researcher-specified covariates, allowing for resulting topics to more closely approximate the theorized data-generating process. In this application, I allow topics to vary as a function of providence (state or national) and year (binned in two-year intervals). Functionally, this means every document x_i is represented by a length k vector of topic proportions $\theta_{1\dots k}$, which are then used as the features of the classification model.¹

The application of the STM process in this paper can be thought of as a text-as-data manifestation of other dimension-reduction techniques (such as principal components analysis) in machine learning. Methodologically, the process alleviates problems resulting from high-dimensional data such as data sparsity, computational complexity, and overfitting. Substantively, STM provides a more interpretable output than raw term frequencies and more closely captures the theoretical thrust of the nationalization hypothesis; certain collections of words are more “national” in nature than others. This is significantly more theoretically meaningful than any single word being an indicator of state or national providence.

¹ k is a researcher-defined hyperparameter determining the number of documents. The results presented in this paper use $k = 40$, which was determined to have the best balance of semantic coherence, held-out likelihood, and minimization of residuals. See Appendix 4.2.1 for details and results with alternative k specifications.

Table 2.1: Classification Model Performance on Heldout Documents

Model	National Documents		State Documents		Accuracy	AUC
	Correct	Incorrect	Correct	Incorrect		
Logistic Regression	39	1	167	1	0.990	0.999
Naive Bayes	36	4	162	6	0.952	0.984
Lasso	39	1	166	2	0.986	0.999
XGBoost	28	12	168	0	0.942	0.994
Support Vector Machine	40	0	167	1	0.995	0.999

2.3.3 Classification model

I train an assortment of classification models using the STM-generated topic proportions of the training data: logistic regression, naive Bayes, regularized logistic regression (lasso), support vector machine (SVM), and boosted gradient descent (XGBoost). Before fitting the models, I randomly hold out 20 percent of the training data as a validation set to evaluate out-of-sample model performance. Models requiring hyperparameter tuning are first evaluated for performance using area under the ROC curve (AUC) with 10-fold cross-validation. Furthermore, because of the class imbalance within the training data (significantly more state documents than national documents), I use a bootstrap-based synthetic oversampling technique called ROSE (Random Over-Sampling Examples) to prevent models from always predicting the majority class. The performance of each model for predicting the class of the documents in the validation set are shown in Table 2.1.

All the models perform remarkably well, indicating there is sufficient textual differentiation between state and national rhetoric to perform classification in this manner. This also alleviates a potential concern that the state documents of the training data may themselves contain nationalized rhetoric. While such rhetoric may exist, the models are able to successfully determine which topics are most associated with state and national origin. I use the unpenalized logistic regression model as the final prediction model for the remainder of this paper because it has the joint-highest AUC and easily interpretable coefficients, but see Appendix 4.2.2 for prediction results from the other models.

2.4 Prediction results

The final step of the workflow is to fit the trained model to text from political campaigns to predict the class of each document. I consider three different mediums through which political candidates communicate with voters: televised debates, TV advertisements, and social media (Twitter). There are both theoretical and technical reasons to believe classification performance should vary by medium, which I will discuss in greater depth below.

2.4.1 Televised debates

I first consider the potential nationalization of rhetoric during televised political debates. Specifically, I analyze an original corpus of 397 electoral debates (86 presidential and 311 gubernatorial) between 2000 and 2018 retrieved from closed-captioned transcripts from the C-SPAN video archives, which were originally broadcast either directly on C-SPAN or through local public affiliates.²

Research on gubernatorial debates is rare, but the few studies that have been conducted conclude candidates largely focus on policy positions rather than character (Benoit, Brazeal, and Airne 2007) and viewers of debates are often able to correctly identify the eventual winner of the contest (Benjamin and Shapiro 2009). Research on the effects of presidential debates largely conclude such events have some short-term effect on candidate preference (Hillygus and Jackman 2003) and issue knowledge/salience (Benoit, Hansen, and Verser 2003). Practically, the debate context helps control for candidate-level confounders such as ideology, campaign resources, or campaign activity level that may bias results in a different context (such as television advertisements or social media).

While there is no particular reason why the messaging content of debates would deviate substantially from other mediums, there are reasons to believe there would be a high hurdle to find evidence of nationalization. The length of debates (typically at least an hour long) allows for greater depth

²Transcripts were retrieved through a combination of headless web browsing and scraping. Transcripts for non-closed captioned videos are not available.

and breadth of discussion across policy issues, making state-specific content perhaps more likely to appear in gubernatorial contests. The moderator of the debate (typically a member of the local media) may push candidates to give positions on more local issues of interest to that media market. The candidates themselves may believe the audience of such debates to be fairly well-informed, makes less detailed or policy-oriented appeals less effective. Still, media coverage of the debates the next day may focus on the headline-grabbing nationalized appeals made during the debates, and candidates are easily able to answer the questions they want to answer instead of the questions that are asked of them.

Figure 2.1 shows the results of applying the trained classification model to the C-SPAN debates corpus. The left panel shows the confusion matrix of the classification model, the upper right panel shows the average predicted probabilities of presidential and gubernatorial debates of being of national class over time, and the lower right panel shows the predicted class counts for just gubernatorial debates over time.

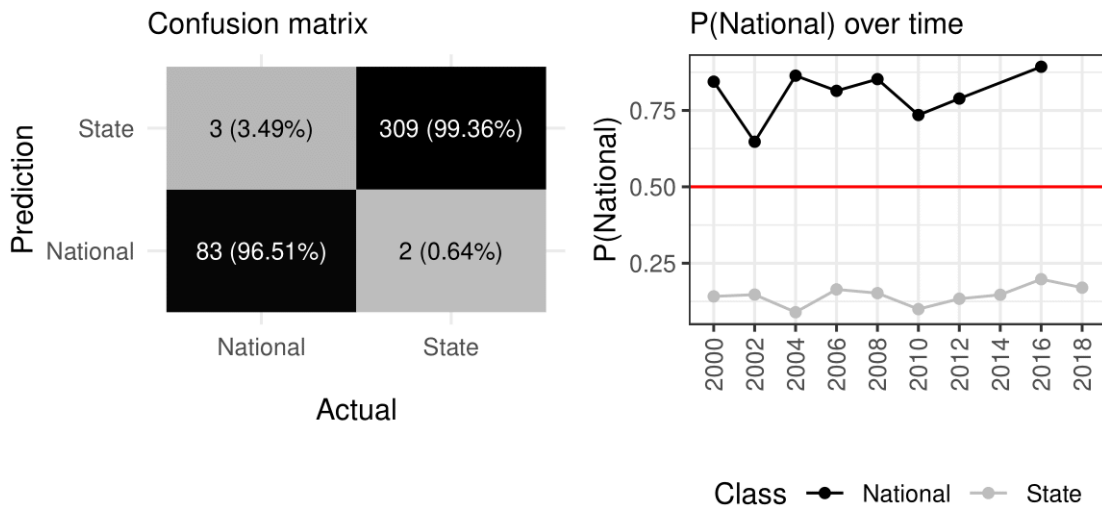


Figure 2.1: C-SPAN debate predictions, pooled and over time

These results give a consistent picture of rhetoric in debates; candidates predominately discuss topics germane to their jurisdictions. In purely statistical terms, collections of words more indicative of state (national) content are significantly more common in gubernatorial (presidential)

debates, and the prevalence of those topics has not changed significantly over time.

It is important to note that these results, particularly the predicted probability of national origin, are not meant to indicate the exact proportion of content in a debate that reflects state or national topics. The debates themselves *are* quantitatively represented as different proportions of topics, but the weights with which those topic proportions are translated into predictions of class are not uniform. An alternative method of quantitatively representing texts may break debates into more granular pieces (such as sentences), classify each individual sentence as either state or national, then tally the number of sentences in each predicted class. The advantage of the method I use in this paper is the overall sense of content for each document; in general, are the topics discussed in the document more consistent with documents of known origin?

For example, consider the 2002 New Mexican gubernatorial debate between Republican John Sanchez, Democrat Bill Richardson, and Green Party candidate David Bacon. The classification model gave this debate a predicted national probability of 16.9%, but this of course masks the full heterogeneity of topics covered during the debate, which touched on national topics such as NAFTA and the war in Iraq. Figure 2.2 gives a more detailed view of the debate as a treemap of estimated topic proportions, with the size of each tile (labeled with the topic number) representing the size of the proportion and the color representing the model estimate of its relative “state-ness” or “national-ness” (and coefficients that weren’t significant predictors of either). Here, topics that lean “national” make up a larger proportion of the total debate than the 16.9% total suggested by the predicted probability, but the predictive weight of the state topics lowers shifts the prediction to the state side.

2.4.2 TV advertisements

Next, I consider the potential nationalization of rhetoric in televised campaign advertisements. This medium is perhaps the modal form of campaigning in the eyes of constituents and the most commonly studied campaign messaging medium in political science. It does, however, present some unique challenges to the classification methodology utilized in this paper.

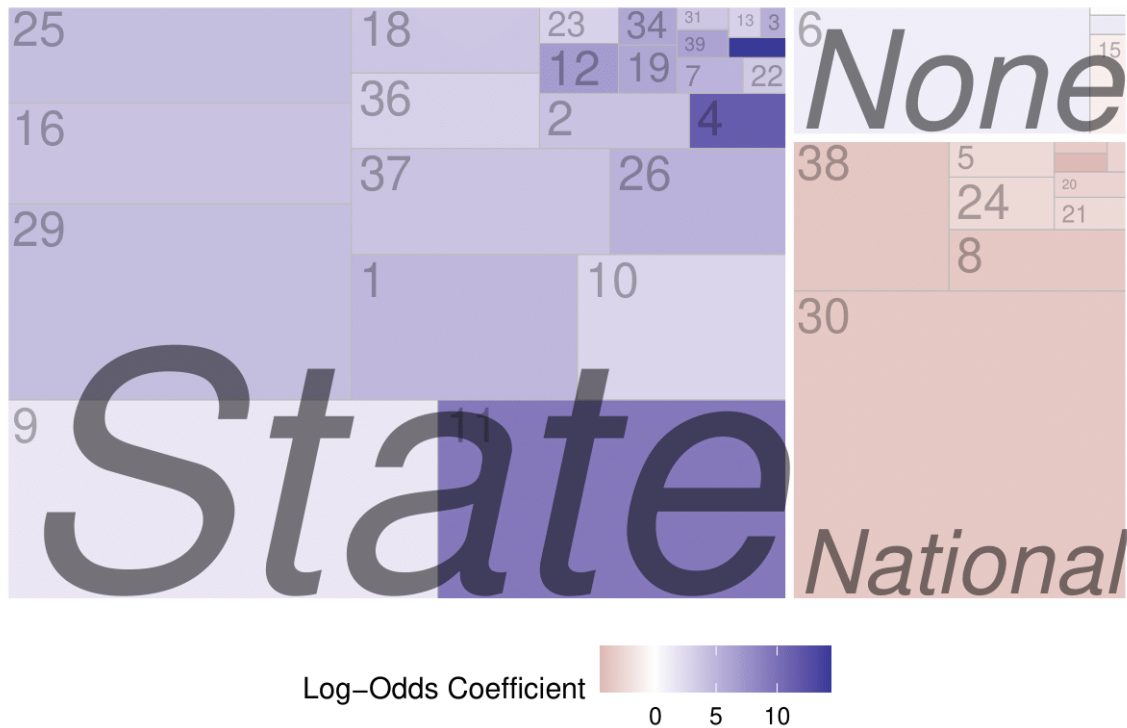


Figure 2.2: Topic proportions of 2002 New Mexico gubernatorial debate

Most obviously, the quasi built-in controls for candidate ideology and campaign resources in the debate context are absent from televised ads. Candidates with larger war chests may be able to air more ads referencing a broader range of topics, whereas more cash-restricted candidates could be forced to focus their message around just a few talking points. A related problem is the unique content of ads overall; they are much shorter than debates, tend to be more negative, and, while they still speak predominantly about policy, they do often incorporate references to general candidate character or background. Furthermore, candidates increasingly have the ability to target advertisements to particular audiences for particular purposes. Certain messages may be broadcast to swing voters as persuasive content, while other messages may be broadcast to candidates' bases to turn out the vote. This is all to say that the content of advertisements is likely substantially different from the content of debates, which has consequences for the potential for nationalized campaigning strategies.

For this paper, I analyze 2,334 televised advertisements from presidential (1,528 ads) and gu-

gubernatorial (806) campaigns in 2004 and 2008. These ads are provided by the Wisconsin media project, with transcripts scraped from PDF storyboards.³ These ads include those run in both the primaries and general elections and by both candidates and interest groups.

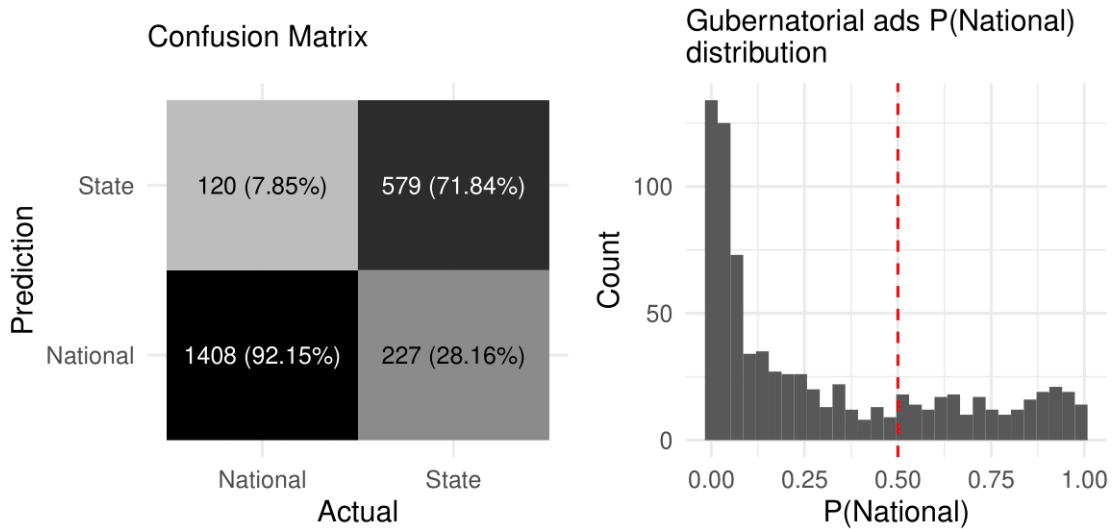


Figure 2.3: Televised advertisements predictions

Figure 2.3 shows the results of applying the trained classification model to these televised advertisements. While advertising content still predominately consists of topics germane to candidates' jurisdictions, there is a higher proportion (26.9%) of gubernatorial ads that are classified as being national. The left panel shows a similar classification rate of presidential rhetoric as in the debate context, which indicates the televised ad medium is not necessarily biased toward more state-like content. The right panel of Figure 2.3 shows the distribution of the predicted probabilities of national classification for just the gubernatorial ads. Unsurprisingly, most of the predictions are strongly in the state directly, but a large number are classified as very national in content.

To give an example of one such gubernatorial ad, the Alliance for North Carolina ran an attack ad on Pat McCrory in October 2008 that the classification model assigned an 87% probability of being national in content. The brief transcript reads:

³Only the data from 2004 and 2008 are presented in PDF storyboards with embedded text. While storyboards are available in 2000 and 2002, the text can only be extracted via more advanced techniques such as optical character recognition, which is a potential source of error. Years after 2008 do not included textual data.

The big developers, energy companies, and the banking industry just love Pat McCrory and George Bush. Why? Because McCrory and Bush have the same economic philosophy. Less regulation and less oversight to help these companies make even more profit. The result, economic collapse and a Wall Street Bailout. Who ends up paying? You the middle-class. Pat McCrory, stop supporting Bush economics and start supporting more regulation and oversight of big business.

This ad clearly attempts to link McCrory to Bush policies with fairly little state-specific information, instead using terms that would be equally applicable in any other state (“Wall Street bailout” and “middle-class”). While this is a compelling example, a majority of advertisements are still classified as being predominantly state content.

2.4.3 Twitter

Finally, I analyze a more modern form of campaign rhetoric; social media. Specifically, I analyze the Twitter of Members of Congress and Governors in office during 2018 using the Das et al. (2022) corpus of tweets. This corpus contains 952,425 tweets from sitting Members of Congress and 101,546 tweets from incumbent governors.

This corpus is unique in this paper for many reasons. First, the tweets are not specific to the campaign timeframe, and therefore don’t explicitly count as “campaigning.” Second, the tweets only account for incumbents and do not include the tweets of their challengers. Third, the “national” comparison in this context is Members of Congress, not communication from Presidents. This is important because Members of Congress operate at the national stage while being beholden to district even more localized than their gubernatorial counterparts, so we might expect communication to be split between national and state topics. Finally, and perhaps most importantly, Twitter represents a fundamentally different avenue through which politicians communicate with supporters. Twitter can and is used to campaign, but can also be used for ostensibly non-political activity, like cheering on a local basketball team or engaging in more general political hobbyism. Outside of

terms of service violations, there are really no restrictions on what can or can't be said on Twitter by politicians.

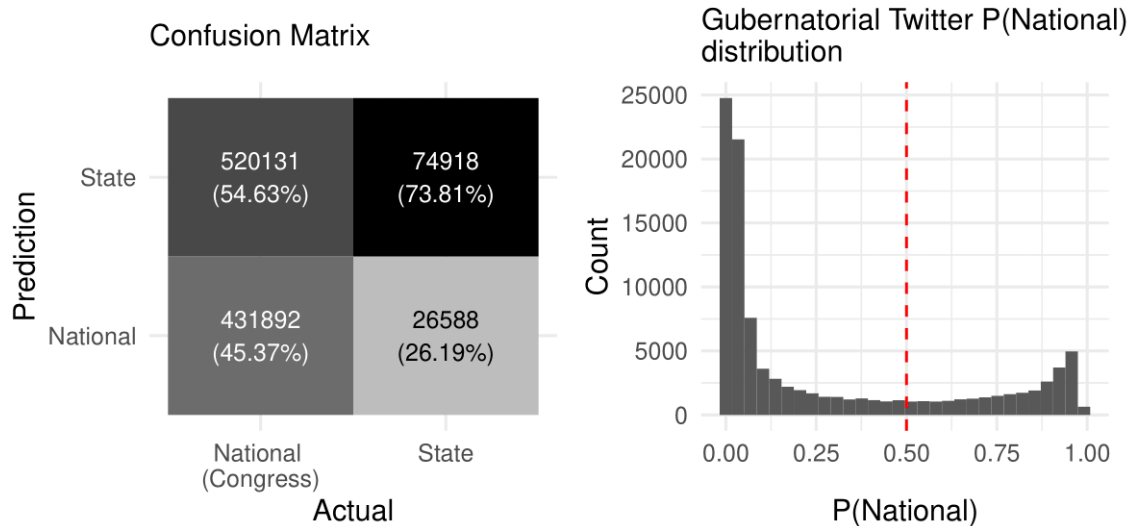


Figure 2.4: Twitter predictions

Figure 2.4 shows the results of the Twitter analysis. Similar to rhetoric in televised ads, governors still communicate more on topics related to state politics, but engage significantly in national political topics as well. Members of Congress are more evenly split between national and state topics, which is likely a function of their accountability to district-level pressures. The right panel of Figure 2.4 shows the distribution of predicted probabilities for national classification of gubernatorial tweets. Most tweets show strong “state-ness,” but there does seem to be a slight bimodal distribution, with a large portion of tweets being classified as heavily national.

The results of Figure 2.4 share similarities with those discussed by Das et al. (2022), but are difficult to directly compare. Das and coauthors’ primary analysis uses a topic similarity approach, wherein 100 different topic models are trained and the resulting distributions of topic proportions can be compared using a distance metric. While the authors focus on the difference between national and mayoral Twitter, they do present results comparing Governors and Members of Congress. They find the median topic distance between Governors and Members of Congress is about 14% greater than the median distance between Governors and other Governors, indicating a small but

not insignificant difference in the topics discussed by the two sets of politicians. Similarly, I find the “national” and “state” classification rates between Governors and Members of Congress differ by about 19%. Again, these numbers cannot be directly compared, but the general trend suggests some similarity but not complete overlap between the two levels of government.

2.5 Discussion

The preceding results indicate an overall picture of gubernatorial campaign rhetoric that gives pause to the “all politics is national” hypothesis. Across all mediums, a comfortable majority of communications were classified as primarily consisting of state topics. This approached almost 100% in debates, but closer to 75% in televised advertisements and social media posts on Twitter. The lower state classification rates in the latter two mediums suggest gubernatorial candidates do engage in some degree of nationalization when they have the flexibility to do so. The debate context is fairly constrained, so when those constraints are lifted and the field of possible topics expands beyond topics presented by a debate moderator, we would expect to see somewhat higher rates of nationalization.

While this paper has focused largely on the classification of single pieces of communication in isolation, it is possible that the real engine of information nationalization is the media environment reporting on, circulating, and commenting on the communications. Media plays a major role in how voters engage with campaign materials. It is possible that while most campaign messaging from gubernatorial candidates focuses on state topics, the few communications that *are* nationalized are circulated more widely by the media. During the gubernatorial race in Kentucky in 2019, for example, coverage from national outlets like the New York Times largely focused on the Republican incumbent’s (Matt Bevin) allegiance with and affinity for Donald Trump. In the debate between the Bevin and his Democratic challenger Andy Beshear, the topic of impeachment *did* arise, but it was constrained to a single question. For the most part, the rest of the debate revolved around issues germane to Kentucky politics.

Further work must be done to both extend the corpus of text analyzed for nationalized content and determine if there exists a relationship between nationalized rhetoric and nationalized results. While I address a wide variety of mediums here, campaigns devote resources to many more. These include radio, flyers, websites, and other social media outlets such as Facebook and TikTok. Linking nationalized rhetoric to the nationalization of results is beyond the scope of this paper, but future work should consider the media markets in which differentially nationalized content is utilized by campaigns. Finally, future work should consider “downstream” nationalization occurring through media coverage of the campaigns. While the initial campaign rhetoric may be fairly germane to state topics, state-level media outlets may focus significantly more on the nationalized aspects of those campaigns.

The results of this paper have consequences for how we understand voter interaction with campaigns. These campaign activities *do* offer a source of information to voters that is functionally distinct from national politics. How voters then process this information is of subsequent importance to better understand the information environment voters must navigate within a nationalized context.

Chapter 3

D.C. On My Mind: National Considerations in State and Local Political Decisions

Do voters make state- and local-level electoral decisions based on national political information? A growing body of research suggests state and local elections have become “nationalized.” That is, national political actors and issues increasingly influence state and local political activity, a claim supported by an increasing correlation between Presidential and down-ballot election results since the 1970s (Hopkins 2018; Sievert and McKee 2019). These findings are a matter of some concern for those who expect quality representation in a federal system: federal institutions are designed to divide power and responsibility between the national, state, and local governments, so if voters treat state and local elections simply as extensions of national elections, their ability to hold state and local officials accountable for actions pertinent to state and local government is strained.

Observational evidence for nationalized political behavior is often measured using correlations in electoral performance between national and state- and local-level candidates of the same party, typically at the county or state level.¹ These measurement strategies obscure heterogeneity in individual-level behavior. If the partisan contours of national, state, and local policy debates are highly correlated (i.e. people who prefer one party’s platform at the national level also prefer the party’s state and/or local platform), nationalized political outcomes pose little threat to quality rep-

¹See Kuriwaki (2020), however, for an example of ballot-level data on nationalization in state and local elections.

resentation. In this case, apparent aggregate measures of nationalization reflect the genuine state- and local-level preferences of informed voters rather than from the byproduct of national-level preferences of uninformed voters. And to the extent national, state, and local political dimensions aren't independent, national-level issues position can provide voters with useful signals for determining ideological similarity.

We contend a homogenization of individual preferences over issue dimensions is in part responsible for the nationalization of electoral results. Specifically, because voters see issue positions across jurisdictions as connected, every issue position taken by a candidate acts as a useful signal of candidate type. Crucially, this is true even when candidates take issue positions on policy domains outside of their jurisdictional responsibilities.

In this paper, we use an experimental approach to directly measure how voters make decisions in national, state, and local political contests. We ask respondents to choose between a pair of hypothetical candidates running in a national- or state/municipal-level election. Each candidate is represented by a battery of policy positions, which are also drawn from a pool of national- or state/municipal-level issues. Because some policy positions are ostensibly irrelevant to the jurisdiction of the candidate, any effect of their inclusion necessarily comes from the respondent's interpretation of that position as indicative of candidate type. We find the office of the candidate has almost no effect on the preference of voters; regardless of whether candidates take issue positions on policies inside or outside of their jurisdiction, voters in agreement (disagreement) with their policy stance are more (less) likely to select them as their preferred candidate. The size of the policy effect does not vary by the office of the candidate. However, we find national-level policies have a larger effect on candidate selection in both state/municipal and national-level contests. The inclusion of labels specifying the partisan affiliation of candidates has little influence on the size of policy effects and does not change the similarity of such effects across municipal, state, and national offices. However, the clearer the partisan signal associated with each policy, the greater the effect of that policy on candidate selection.

Our findings provide some of the first individual-level causal effects in the nationalization liter-

ature and provide more detail on the potential mechanisms behind nationalized political behavior in the U.S. While nationalized issue positions certainly sway voters, they do not do so at the complete expense of state and local issue positions. Indeed, even state/local issue positions have effects on candidates for national office. Our results are consistent with information-seeking behavior in national, state, and local domains where the ideological dimensions are correlated, and are consequential for how we frame discussions of representational quality at the state and local level.

3.1 Nationalized Behavior, Federal System

Nationalization researchers have found an increasing correlation between presidential and state/local partisan vote shares. From 1968 to 2012, the correlation between Democratic two-party vote shares in presidential and gubernatorial midterm elections (measured at the county level) has risen from less than 0.3 to around 0.7 (Hopkins 2018). Sievert and McKee (2019) similarly find the rate at which the same party won both the Presidential and Senatorial contests in a given state rose from 52% in 1980 to 84% in 2012, with Jacobson (2015a) finding similar trends in U.S. House elections. Examining state Supreme Court elections, Weinschenk et al. (2020) find a nearly 1-to-1 relationship between county-level Democratic Presidential and state Supreme Court vote shares from 2000 to 2018 in partisan elections.

The nationalization of U.S. politics extends beyond election results as well, as many scholars note the behavioral alignment of state and local political elites with their national counterparts. State party platforms have become increasingly homogeneous across state boundaries (Hopkins 2018). State legislative agendas also display signs of homogenization (Burke 2021). In as local a venue as school board elections, Reckhow et al. (2017) find that national funding networks play a significant role. Das et al. (2022) find striking semantic similarity between the public communications (tweets) of Governors and Congressional representatives.

Popular media portrayals of gubernatorial campaigns also stress nationalization. During the 2019 gubernatorial contests in Kentucky, Louisiana, and Mississippi, multiple outlets highlighted

Donald Trump’s personal involvement in the contests, with Trump’s impeachment being a particularly salient campaign issue (Manchester 2019; J. Martin 2019; Rojas and Alford 2019). Such nationalized appraisals extend to gubernatorial races in Washington, West Virginia, and Texas in 2016, 2011, and 2011, respectively (Brunner 2016; Catanese 2011; McKinley Jr 2010). Governors themselves use nationalized rhetoric, including when Governor Gavin Newsom of California characterized supporters of the 2021 gubernatorial recall election as “a partisan, Republican coalition of national Republicans, anti-vaxxers, Q-Anon conspiracy theorists and anti-immigrant Trump supporters.” At a minimum, candidates for state offices do not feel bound to engage in policy debates or address controversies exclusive to their own jurisdictions. Candidates for local office increasingly appear and speak at national rallies, including Joe Arpaio (former Sheriff, Maricopa County, Arizona), a fixture at Trump campaign rallies. National politicians also bring local issues into the national spotlight, as Joe Biden did when he called for the resignation of three Los Angeles City Council members after they were recorded making disparaging and racist comments about a colleague and his family.

Nationalized rhetoric is accompanied by nationalized behavior: state politicians, particularly Attorneys General, take their mandate as spanning both state and federal issue portfolios. Texas Attorney General Ken Paxton sued multiple battleground states won by Joe Biden in the 2020 Presidential election for “exploit[ing] the COVID-19 pandemic to justify ignoring federal and state election laws.” Hawaii Attorney General Doug Chin sued the Trump administration in 2017 after the implementation of a travel ban on refugees and travelers from certain Muslim-majority countries. In these cases and others, states are not merely defending their federally designated roles, they are actively weighing into inherently national issues.

We turn our attention from political elites to the American electorate. Scholars have proposed a number of mechanisms by which the electorate could become nationalized. We categorize them as belonging to two (non-mutually exclusive) categories: identity and information. The *identity mechanism* views nationalization as an extension of partisanship and polarization; that is, partisanship is an affective, expressive identity, so we should expect voters to vote according to their party

ID (either for their preferred party, or against their non-preferred party) in any context, as an expression of in-group solidarity and/or out-group antipathy (Huddy and Bankert 2017; Iyengar, Sood, and Lelkes 2012). For example, Abramowitz and Webster (2016) note an association between high levels of out-party antipathy (negative partisanship) and an increase in straight-ticket party voting. Webster (2020) finds a similar association between straight-ticket voting and personal anger. Although strong, the extent to which voters are willing to make voting decisions based purely on party identification may be bounded. Using a conjoint design similar to our own, Mummolo, Peterson, and Westwood (2021) find voters punish excessive deviation from preferred positions on salient policies by co-partisan candidates. Costa (2021) also uses a conjoint design to find voters prefer candidates who provide substantive representation and constituency service over partisan affect. So, while party labels convey information about a candidate's ideology, they do so only partially in the eyes of voters.

Information mechanisms propose that voters operate in an environment of limited information, and given a lack of meaningful information (or the costliness of obtaining information that exists) about state and local political contests, voters use national information as a shortcut, defaulting to the candidate of their preferred party. Hayes and Lawless (2018) note the steep decline in access to local news in the last decade, with a 10% reduction in issue coverage and a 33% reduction in the coverage of candidate traits in U.S. House of Representatives contests between 2010 and 2014. From 2004 to 2018, one in five newspapers has closed (Abernathy 2018). This decline in access to local news is associated with increase in nationalized news content and voting behavior (G. J. Martin and McCrain 2019). The more information voters have besides the party identification of the candidates, the more likely they are to make split-ticket decisions (Moskowitz 2021).

Both of these proposed mechanisms stress the importance of any available signal, including partisanship and policy stances, to form judgments on candidates. Party platforms have homogenized and national and state parties are seen as more singular than separate (Caughey, Dunham, and Warshaw 2018; Hopkins 2018). The dimensions of state politics now also largely mirror the left-right contours of national politics (Caughey and Warshaw 2016; Shor and McCarty 2011). Given this,

it stands to reason that nationalized signals convey real, not just illusory, information about state and local contexts. Still, the more divorced the voter's landscape is from the national one, the less informative the signal. Jensen et al. (2021) note many local development policies seem to defy the partisan sorting and polarization seen in national politics, and Bucchianeri et al. (2021) find that city council voting has displays a more complex (higher dimensional) spatial structure than state and national contexts.

Why should political scientists care about nationalized vote behavior? Because it puts real pressure on political representation in a federal system. While local, state, and national institutions often overlap and trade jurisdiction over policies areas from year to year, there still remain areas of functional responsibility unique to each (Beer 1978; Kousser 2014). Democracy requires that voters can hold officials accountable for their actions in office. If the contours of local, state, and national politics are truly highly correlated in a given election, then nationalized voting may be rational and informed. But to the extent politics diverges across venues, nationalized voters may attribute credit or blame to the wrong elected officials. Current research primarily relies on aggregated, observational data to measure group-level voter preferences, which renders the discipline unable to distinguish potentially rational behavior from failures of representation, and unable to determine whether nationalization as a whole improves accountability or misplaces blame.

Previous research on behavior in federal systems is inconclusive regarding voters' abilities to assign functional responsibility to the appropriate level of government. Arceneaux (2005) is optimistic, concluding from survey data that voters do tend to expect policy solutions from the level of government they deem responsible for an issue. Arceneaux (2006) also finds that voters are significantly more likely to sanction officials who deviate from their preferred policy positions over which they are functionally responsible, but this effect is constrained to the most prominent policy issues. Brown (2010) finds that voters evaluate the state economy through a partisan lens, leading them to attribute responsibility to state officials accordingly. If the state economy is doing well (poorly) and the governor is a co-partisan (non-co-partisan), they attribute the success (failure) to the governor, but not otherwise. This evaluation of responsibility through a partisan lens extends

to disaster relief as well, with partisans blaming failures on non-co-partisans and attributing successes to co-partisans, regardless of level of government or functional responsibility (Maestas et al. 2008). While this pattern is consistent, it is not entirely fixed; both Democrats and Republicans update their attitudes when given objective (or at least, non-partisan) information about economic conditions (Malhotra 2008).

In summary, state political outcomes are now more likely to mirror national political outcomes; and voters and officials alike are more likely to invoke national contexts. Voters seem to use the best signals of candidate type available to them in order to make their decisions, whether that information is partisan identification of a candidate or the policy positions they take. Voters have displayed an inconsistent ability to correctly attribute credit or blame to offices for their areas of responsibility.

Given these findings, we argue that electoral nationalization is driven in part by the homogenization of preferences over issue dimensions spanning national, state, and local politics. Because these issue positions are correlated in the minds of voters, information that appears irrelevant in a state contest because it speaks to an issue which is a national responsibility is actually quite useful in making decisions according to one's preferences. Not all signals are equally useful; some policy positions are surely orthogonal to the main ideological dimension. This implies a bidirectional perfusion of issues: if national signals are useful to voters in state and local contests, so too are state and local signals useful in national contexts. Our conception of nationalization, then, is not a top-down force that dominates lower levels of government. Instead, we view it as a homogenizing force which impacts all levels of politics.

By answering the question of how voters make state- and local-level decisions in nationalized contexts, our design fills gaps in the extant literature. Namely, our understanding of nationalized political behavior has been limited largely to either aggregated voting outcomes or surveys without causal effect attribution due to biases in self-reported preferences. The mechanisms of nationalization should occur at the individual voter level, so we view it as an important task to conduct experiments using individuals as the unit of analysis.

3.2 Design

We conduct a series of survey experiments consisting of nationally representative and weighted samples.² Each respondent to one of our surveys is given a series of 10 conjoint (forced-choice) prompts, which ask them to choose between two hypothetical candidates for office whose attributes are varied randomly. Respondents are assigned to one of two *level conditions* (comparing national issues to either state or local issues) and one of two *partisan label conditions* (revealing or hiding the party label of the candidate), yielding four distinct analysis groups.

Our analysis groups are as follows:

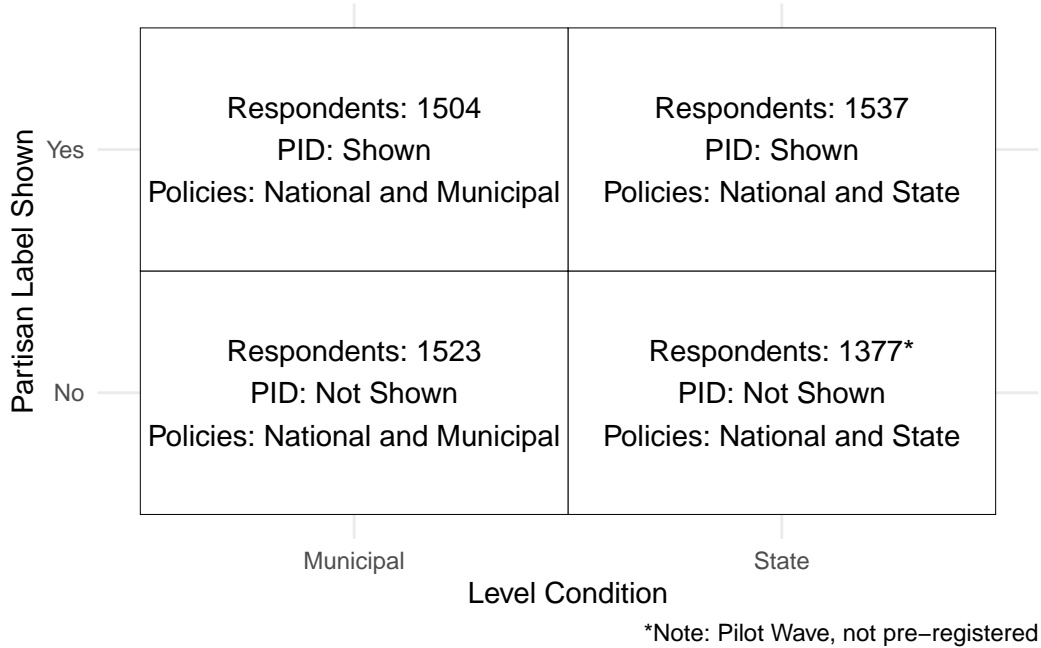


Figure 3.1: Details of survey design

Within each experiment, every respondent takes part in two *candidate office conditions*: respon-

²The survey was fielded online in two waves. The pilot wave, consisting of the state level, no party ID condition, was conducted August 25-27, 2021. The remaining wave, consisting of the other three conditions, was conducted October 10-17, 2022. The survey was conducted on the Qualtrics survey platform. All respondents were weighted to reflect a nationally representative sample, the parameters of which are described in Appendix 4.3.6.1. We discuss theoretical concerns about the inferential impact of our weighting scheme in Appendix 4.3.6.3. Our core result is robust to alternate weighting schemes or to dropping weights entirely (results presented in Appendix 4.3.6.4). The 2022 survey wave is pre-registered with the Center for Open Science (DOI 10.17605/OSF.IO/ASVMN). We observe no major deviations from our pre-registration. The 2021 survey wave, which served as a pilot, was not pre-registered.

dents are given five conjoint prompts in which they are asked to choose their preferred candidate for the federal House of Representatives and five conjoint prompts where they are asked to choose their preferred candidate for the relevant lower-level election (either state assembly or city council depending on the level condition). Before answering, respondents are given the following preamble to consider:

Candidates for political offices often have opinions on policies at all levels of government. Below are two sets of policy positions held by two candidates, A and B, running for the [**federal House of Representatives/state assembly/city council**]. Some policies are able to be enacted by the [**state government/municipal government**], and others are able to be enacted by the federal government (given in parentheses next to each policy). Please choose the candidate you would prefer if the candidates were running for the [**federal House of Representatives/state assembly/city council**].

Neither set may perfectly reflect your preferences. If this happens, just pick the candidate set you most prefer even if it isn't perfect.

Each conjoint prompt offers a respondent two hypothetical candidates whose platforms are represented by four randomly chosen policies. The four policies are chosen from a list of 29: 10 where the primary responsibility for the policy domain rests with the federal government; 10 where the primary responsibility rests with the state government; and 9 where the primary responsibility rests with the municipal government. The set of policies chosen from depends on the level condition (i.e. respondents assigned to the state level condition have policies chosen from national and state issues, while respondents assigned to the municipal level condition have policies chosen from national/municipal issues). Each policy has one of two settings: an affirmative and a negative setting, for instance "Mandate the use of body cameras for state police" and "Do not mandate the use of body cameras for state police." Both candidates are assigned the same four policies, but they vary with respect to the settings chosen for each policy, simulating the kind of comparisons real voters make.

Thus, if the body camera policy is chosen, both candidates may support mandating body cameras; both may oppose mandating body cameras; or candidates may have opposing positions. The set of all policies was selected to cover a number of salient issues, and include policies for which the affirmative wording is liberal, conservative, or not obviously ideological. A full list of policies and settings are given in Table 3.1.³ Respondents assigned to the partisan label condition are given a fifth piece of information: a party label for each candidate, randomly assigned to be “Democrat” or “Republican”.

Table 3.1: Conjoint Policies

Policy	Category	Level	Positive Setting	Negative Setting
military_size	military	National	Substantially reduce the size of the U.S. military	Not substantially reduce the size of the U.S. military
israel	israel	National	Withdraw military support from the state of Israel	Not withdraw military support from the state of Israel
path_to_citizen	immigration	National	Create a path to citizenship for all undocumented immigrants	Not create a path to citizenship for all undocumented immigrants
dreamers	immigration	National	Create a path to citizenship for undocumented immigrants brought here as children	Not create a path to citizenship for undocumented immigrants brought here as children
deportation	immigration	National	Deport all undocumented immigrants	Not deport all undocumented immigrants
tariffs_china	trade	National	Substantially increase tariffs on imports from China	Not substantially increase tariffs on imports from China
tariffs_eu	trade	National	Substantially increase tariffs on imports from the European Union	Not substantially increase tariffs on imports from the European Union
saudi_weapons	weapons	National	Stop the sale of weapons to Saudi Arabia	Not stop the sale of weapons to Saudi Arabia

³The national policy items are adapted from survey items in the Democracy Fund + UCLA Nationscape Survey. A number of the state and local items are adapted from randomized policy items in Jensen et al. (2021).

medicare_for_all	healthcare	National	Provide government-run health insurance to all Americans	Not provide government-run health insurance to all Americans
public_option	healthcare	National	Provide the option to purchase government-run health insurance to all Americans	Not provide the option to purchase government-run health insurance to all Americans
teacher_pay	education	State	Mandate a substantial pay raise for state public school teachers	Not mandate a substantial pay raise for state public school teachers
state_pre_k	education	State	Create a state-run pre-kindergarten program	Not create a state-run pre-kindergarten program
charter_schools	education	State	Substantially increase state funding of public charter schools	Not substantially increase funding of public charter schools
private_prisons	corrections	State	Ban the use of privately operated prisons	Not ban the use of privately operated prisons
court_fees	courts	State	Eliminate state court fees for defendants	Keep state court fees for defendants
body_cameras	police	State	Mandate the use of body cameras for state police	Not mandate the use of body cameras for state police
use_of_force	police	State	Substantially increase funding for use-of-force trainings for state police	Not substantially increase funding for use-of-force trainings for state police
highways	transportation	State	Fund major state highway improvements with additional toll revenue	Not fund major state highway improvements with additional toll revenue
redistricting	elections	State	Create a non-partisan state redistricting commission for the drawing of electoral boundaries	Not create a non-partisan state redistricting commission for the drawing of electoral boundaries
occ_licensing	licensing	State	Substantially reduce state occupational licensing requirements for non-medical occupations	Not substantially reduce state occupational licensing requirements for non-medical occupations
affordable_house	housing	Municipal	Substantially increase spending on affordable housing	Not substantially increase spending on affordable housing
public_transit	transit	Municipal	Substantially increase spending on public transportation projects	Not substantially increase spending on public transportation projects

public_safety	police	Municipal	Substantially increase spending on policing	Not substantially increase spending on policing
business_tax_break	development	Municipal	Use tax breaks and subsidies to attract new businesses	Not use tax breaks and subsidies to attract new businesses
housing_loans	housing	Municipal	Make grants or loans available to buy, build, or renovate multi-family housing in the area	Not make grants or loans available to buy, build, or renovate multi-family housing in the area
height_restriction	height	Municipal	Implement a height restriction on new residential and commercial development in the area	Not implement a height restriction on new residential and commercial development in the area
population_limit	population	Municipal	Establish a population ceiling to maintain neighborhood character	Not establish a population ceiling to maintain neighborhood character
parking	parking	Municipal	Eliminate minimum parking space requirements for new businesses	Not eliminate minimum parking spaces requirements for new businesses
sanctuary	immigration	Municipal	Enact a 'Sanctuary City' policy forbidding local authorities from cooperating with federal agents on immigration issues	Do not enact a 'Sanctuary City' policy forbidding local authorities from cooperating with federal agents on immigration issues

Conjoint experiments, which were popularized in Political Science by Hainmueller, Hopkins, and Yamamoto (2014), extract average marginal component effects (AMCE). Given the random assignment of policies, settings, and partisanship to candidates, these are interpreted as the causal effect of including a given policy/party alternative in a candidate profile on voter selection (Hainmueller, Hopkins, and Yamamoto 2014). Because conjoint experiments capture effects for a variety of simultaneously randomized treatments, they are efficient and externally valid ways to measure candidate choice: real candidates present baskets of policies, and real elections involve the forced choice between those candidates.

We apply three basic constraints to the random selection of policies and settings. First, every candidate pair must be assigned at least one federal policy and at least one state/local policy (according to level condition). Thus, every choice set includes at least one position on an issue relevant

to the candidate office condition, and at least one position that is not ostensibly relevant to their candidate office condition. Second, candidates must differ by at least one setting among federal policies, and at least one setting among state/local policies. These constraints ensure that every respondent contributes at least some information towards our estimation.⁴ Third, some policy pairs would create mutual contradiction (e.g. “Deport all undocumented immigrants” and “Create a path to citizenship for all undocumented immigrants”); when this occurs, only one policy is chosen.⁵

For respondents who are not assigned to the partisan label condition, our design is purposely built to be a “hard” test for nationalization: those respondents are not given party labels, demographic attributes, or any information other than the office being sought, the policies the candidates advocate for, and the level of government responsible for enacting the policy. If nationalization is really caused primarily by the information gleaned from party labels, then these respondents are not given the precursor required to make nationalized vote choices. On one hand, the absence of partisan labels limits the external validity (real-world generalizability) of our results to the typical two-party contest setting seeing in many U.S. state and national elections. But this design still has clear real-world analogues. Primaries, top-two general elections, and certain runoff elections can all involve candidates of the same party with differences in policy positions. By contrast, respondents assigned to the partisan label condition do see party labels, which allow us to estimate the effects of policy nationalization *net of partisanship*. Put differently, do policy positions taken by candidates have any additional influence on behavior when respondents already have access to the candidates’ partisanship?

After excluding respondents who failed a simple attention check, respondents who completed the entire survey module in less than 30 seconds, and those for whom demographic information was incomplete or insufficient to weight to our preferred population targets, our survey yields 58,750

⁴In a conjoint setting, offering respondents the choice between two identical candidates (thus forcing the respondent to choose at random between “left” and “right”) would simply attenuate the regression model’s estimated coefficients on each selected policy.

⁵We do allow for highly improbable combinations of policies, just not directly contradictory ones. While such improbable combinations may pose a threat to the external validity of our design, we show in Appendix 4.3.7 that limiting our analysis to *only* choices involving policy settings consistent with regard to partisanship does not change our main results. We place no constraints on the random assignment of party identifications.

completed conjoint responses (117,500 choice sets).

3.3 Theoretical Expectations

We previously noted that the nationalization of U.S. politics leaves open a range of possibilities with regard to its effect on the quality of democratic representation. Here, we describe three potential results which are consistent with these theoretical mechanisms:

The first potential result is that *only national policy positions affect candidate selection* (or else that national policy positions completely dominate state and local ones). This is most consistent with identity-driven nationalization wherein national policy positions offer the strongest signal as to the status of the candidate as an in-group or out-group member. Only state policies with similar levels of polarization would be significant.

The second potential result is that of *the responsible federalist*: both the national and state/municipal policy effects are significant and comparably large in magnitude, but only for the candidate office condition that matches the responsible level of government. Because respondents are given access to policy information germane to the office they must make a decision for, they are able to, if they prefer, discard the non-germane policy information. If respondents understand and prioritize the functional responsibility of the office the candidate seeks on certain policy areas, this result will occur.

The final potential result lies somewhere in between the two previously mentioned; *both the national and state/municipal policy effects are significant, but there is no difference in effects by candidate office*. This would occur if voters treat all information as valuable signals of type. There is variance in effect magnitude still, as some policy stances may be stronger signals of type than others, but the value of those signals is not limited to the ones with high polarization. Respondents act not as blind partisans but as information-seekers making decisions with limited resources.⁶

⁶Other results are theoretically possible: perhaps no policy positions have any effect on candidate choice at all, or perhaps state or local policies dominate national policies, or respondents care more about policies for which the candidate office condition makes explicit the candidate has no power over (i.e. “exactly wrong” voting). These would be inconsistent with previous theory and results in a dramatic way, and we choose not to give them detailed

3.4 Results

We estimate the average marginal component effect (AMCE) split by office condition. Rather than recording whether the respondent selected the candidate with the affirmative or negative policy setting, we rely on the respondent's preferred position when asked outright to determine whether the candidate's position accords with the respondent's and condition on this accord. AMCE are estimated using ordinary least squares regression with standard errors clustered at the respondent level. An AMCE represents the average change in probability of selecting a candidate when that candidate holds that policy position. Positive (negative) coefficients indicate that respondents are more (less) likely to select candidates. AMCE are bounded between -1 and 1.⁷ If respondents randomly chose between the two candidates, the model intercept coefficient would equal 0.5 and the coefficient estimate would be 0 for each policy. In the case of perfect separation, i.e. respondents *always* pick the side with a particular attribute level, the intercept coefficient would equal 0 and the coefficient estimate would be 1 for that policy.

We condition our AMCE on shared policy stance because unconditional effects may (mechanically) reflect either respondent indifference to the issue or else the existence of a bimodal preference distribution, where respondents are highly animated by the presence of the issue position but in opposite directions.⁸ We can disambiguate between these two causes by measuring a respondent's baseline position on an issue and measuring accord of the candidate's position with the respondent's (Hanretty, Lauderdale, and Vivyan 2020). Suppose the respondent sample is split evenly between people who believe all undocumented immigrants should be deported and those who believe undocumented immigrants should not be deported such that every respondent always chooses the candidate with their preferred position on the matter (regardless of other positions). In this case,

consideration in our text.

⁷These are the absolute theoretical limits of the AMCE. In practice, however, the limits attenuate toward zero because of (a) cases where both choice sets contain the same attribute level: in these cases, the selected candidate and the rejected candidate both contribute in opposite directions, bounding the effect, and (b) the cumulative total of the effects of other included attributes. In effect, then, the bounding of an AMCE is design-specific.

⁸We provide pooled AMCE estimates in Appendix 4.3.2. We also present results in Appendix 4.3.3.2 which condition on agreeing with the affirmative or negative setting of the policy (rather than accord between candidate and respondent).

an AMCE estimate that does not condition on accord would be 0 *despite* there being highly intense preferences on the policy. Because this is frequently the case for real-world political items, some researchers condition conjoint results on party identification. Our preferred approach improves on this by directly measuring respondent preference.⁹ We expect the improvement will be most salient for issues where parties do not display polarization, which is likely the case for several of our state and local policy items.

The results of the non-partisan label condition, where no candidate party identification is provided, are given in Figure 3.2. The top row shows the AMCE from the state government level condition, while the bottom row shows the AMCE from the municipal government level condition. Because AMCE are calculated over the joint distribution of attributes in the design, the AMCE of the national policies must be calculated separately in the state and municipal conditions.

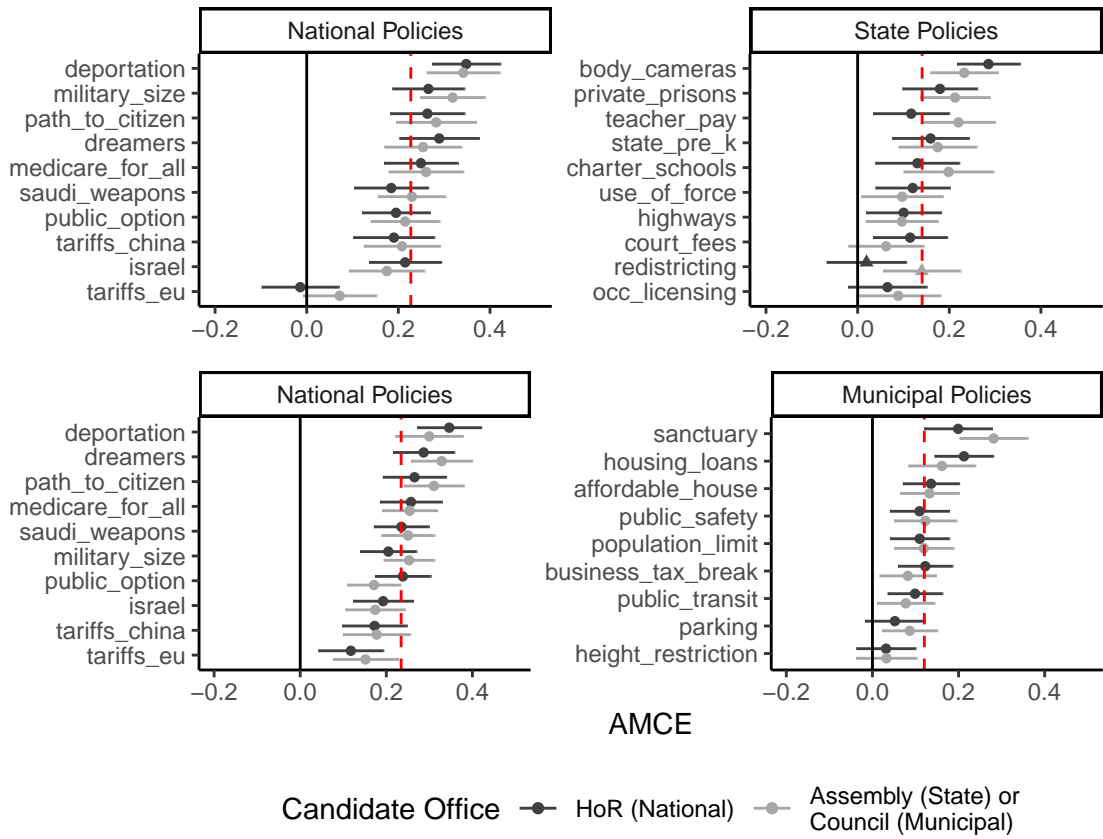
The initial results indicate that many policies across federal, state, and municipal levels are significant drivers of candidate selection. For example, respondents who agreed with a candidate's position on mandating the use of body cameras by police officers were roughly 25% more likely to select that candidate. Put differently, candidates who share a respondent's position on body cameras are selected about 62.5% of the time, whereas candidates that do not are selected roughly 37.5% of the time.¹⁰ Many of the national policy AMCEs are larger in magnitude than the state policy AMCEs, with the deportation of undocumented immigrants as the largest national effect across both government level conditions.¹¹

In all but one case (creating a non-partisan redistricting commission), there is no significant difference in the policy AMCE by candidate office. Substantively speaking, it does not matter whether the policy position taken by the candidate is under that candidate's potential jurisdiction or not. Instead, respondents seem to treat *all* policy information as useful when making decisions between

⁹See Appendix 4.3.3.1 for additional analyses conditioning on party identification rather than direct policy preferences.

¹⁰This is the "marginal mean" interpretation of AMCE, which is possible to use in this conjoint design because each attribute (policy) has only two settings (positive/negative), so the reference category being used in the regression for each policy position is the negative setting of the policy (Leeper, Hobolt, and Tilley 2020).

¹¹Note the survey fielding period of the pilot wave (August 25-27, 2021) overlapped with the U.S. military withdrawal from Afghanistan, which may have created an ephemeral exogenous uptick in the salience of this issue.



Candidate Office ● HoR (National) ● Assembly (State) or Council (Municipal)

Triangles denote statistically significant (α = .05) difference between office conditions
 Red lines represent mean AMCE

Figure 3.2: Conditional AMCE of Non-Partisan Waves

candidates. While this supports the “all politics is national” findings from previous research, it also leads to the surprising conclusion that many state and municipal policy stances also drive voter behavior when selecting candidates for national office. National, state, and municipal politics are not clearly divided in the minds of voters: if an issue matters for one office, it matters for them all.

However, we also observe that national policies have larger average AMCE than state and municipal policies. So while voters may not have different evaluations for candidates competing for state and municipal versus national offices for any given policy, the effect of a national policy on voter decision making is greater than the effect of state and municipal policies. Another potential explanation for the difference in AMCE is simply that national policy issues are more salient, familiar, or exciting for voters. While we attempt to select from the most important policies in the exclusive domains of state and national policy making, we are unable to evaluate if we successfully did so without an exhaustive inclusion of all potential policies.¹²

Next, we consider whether the inclusion of candidate party labels alters respondents’ use of policy information in candidate selection. In the partisan label condition, we conduct the same forced choice survey experiment, but include randomized party labels (Democrat, Republican) above the series of policy positions taken by candidates. If respondents were using policy positions only to triangulate the partisanship of candidates but had no substantive preferences over the policies themselves, we should expect all policy AMCE to attenuate toward zero while the party identification effect remains large. Previous work suggests this should not occur: respondents have substantive preferences over policies and infer more than just partisanship from policy position-taking (Costa 2021; Mummolo, Peterson, and Westwood 2021). Regardless, this formulation of our design allows us to determine the effects of policy positions *net of partisanship*.

Results of our partisan condition are given in Figure 3.3, and are almost identical to those of the non-partisan condition. The effect of party identification itself (shown under the national policies)

¹²We also note that the institutions our candidates are being elected to differ. While the federal House of Representatives is common to all respondents, respondents exist in states whose state houses have different purviews, and municipal councils vary substantially across setting. The degree of deference granted to bureaucrats, the strictures of state constitutions, and many other institutional features differ across our sample. We consider this a case of comparing tangerines and oranges.

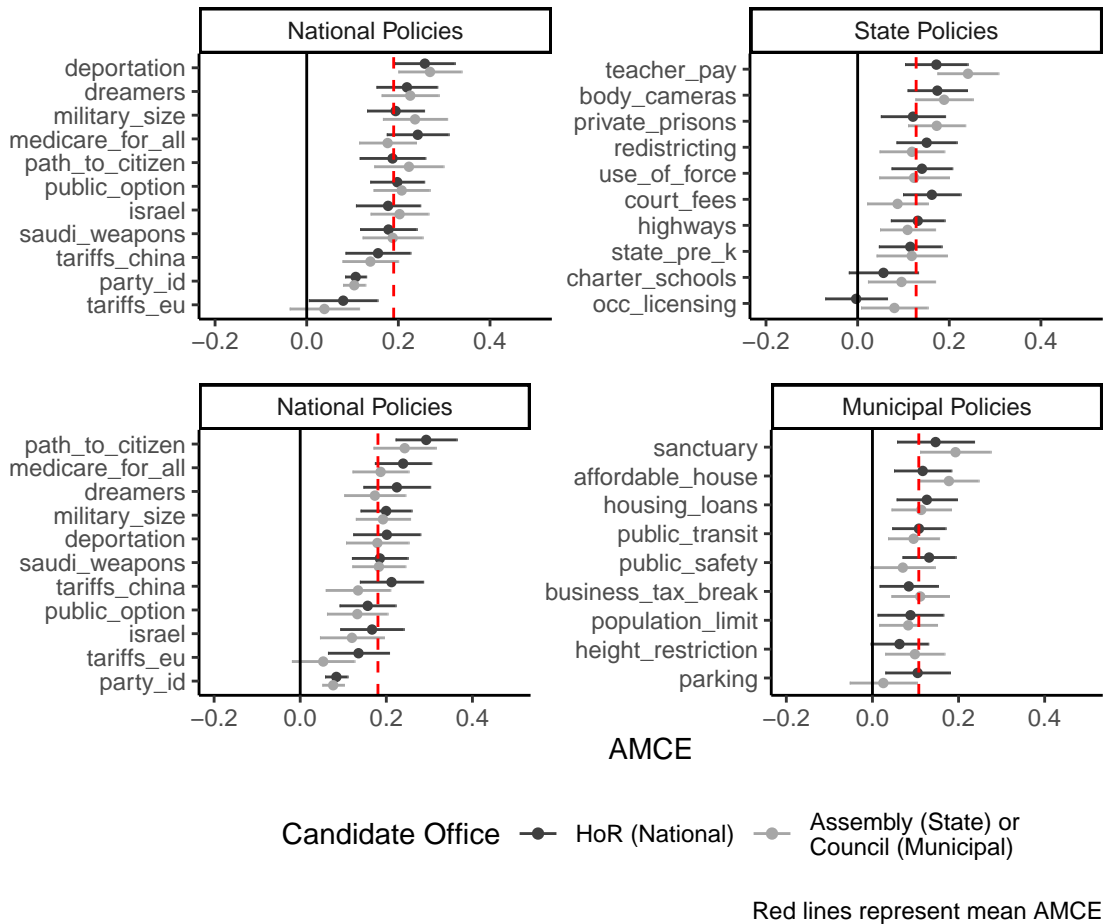


Figure 3.3: Conditional AMCE of Partisan Waves

is small in comparison to national policies, but roughly similar to the state and municipal policy effects.¹³ Across all conditions, we see an average decline in AMCE of just 0.03. This difference is driven mostly by national policies, with an average decrease of 0.046 (compared to state and municipal policies, which both have an average decrease of about 0.01).¹⁴ Again, these decreases do not change the similarity of effects across office conditions or the significance of the AMCE. These results indicate that respondents may use policy positions as indicators of party, but their behavior is still mostly driven by preferences over those policies instead of shared partisanship with a candidate.

The absence of difference between office conditions is a result of substantive interest, but it is important to note our statistical tests are statistically “conservative” in the sense they are weighted toward finding null results. To more directly test the equivalence of the point estimates, we can invert our understanding of significance. Instead of assuming a null where there is no difference between office conditions, we can instead assume there *is* a difference between the offices conditions and quantify how large that difference could be given our result. Hartman and Hidalgo (2018) offer a formal statistical equivalence test that does precisely this. Their formulation is meant to evaluate balance and placebo tests in causal inference. We implement their framework but use our estimated effects between office conditions as the two values being evaluated for equivalence, using the default equivalence ranges suggested by Hartman and Hidalgo (2018) due to a lack of prior benchmark. Per the authors, resulting equivalence confidence intervals can be interpreted as “the smallest equivalence range supported by the observed data.” The maximum values are the points at which we can reject the null of difference at $\alpha = 0.05$.

The full results of the equivalence test are given in the Appendix 4.3.8, but we note here that most of our results lay within an equivalence range of -0.05 and 0.05, with a maximum range for redistricting (an effect where we did observe a significant difference) being just between -0.1 and 0.1. While these ranges temper claims of exact equivalence between estimates, it is critical to

¹³Note the precision of the point estimate for partisan identification is greater than that of policy effects due to its inclusion in *all* conjoint profiles under the partisan condition.

¹⁴All differences are reported in full in Appendix 4.3.4.

compare this range to the overall size of the policy effects. Almost all AMCE were greater than 0.1, including half of the national policy effects being greater than 0.2. Even if the differences in effects *are* at the limits of their equivalence ranges, the individual AMCEs are still significant. In sum, policy positions held by candidates that aren't germane to the candidate's jurisdiction are still used by respondents to make voting decision.

Our results thusfar suggest that almost any policy signal is useful to those who hold opinions on that policy, regardless of the level of government at which that policy is implemented or if the candidates contesting the office have jurisdiction. The question remains what the content of that signal is. If the nationalization literature is correct that politics at all levels of government are contested over the same single-dimensional policy space, then signals that better position a candidate in that policy space are likely to be more informative and persuasive to voters. That is, the more clearly a policy can be associated with the left or right of the political spectrum, especially through associations with the Democratic and Republican parties, the stronger the effect of that policy will be in determining vote choice.

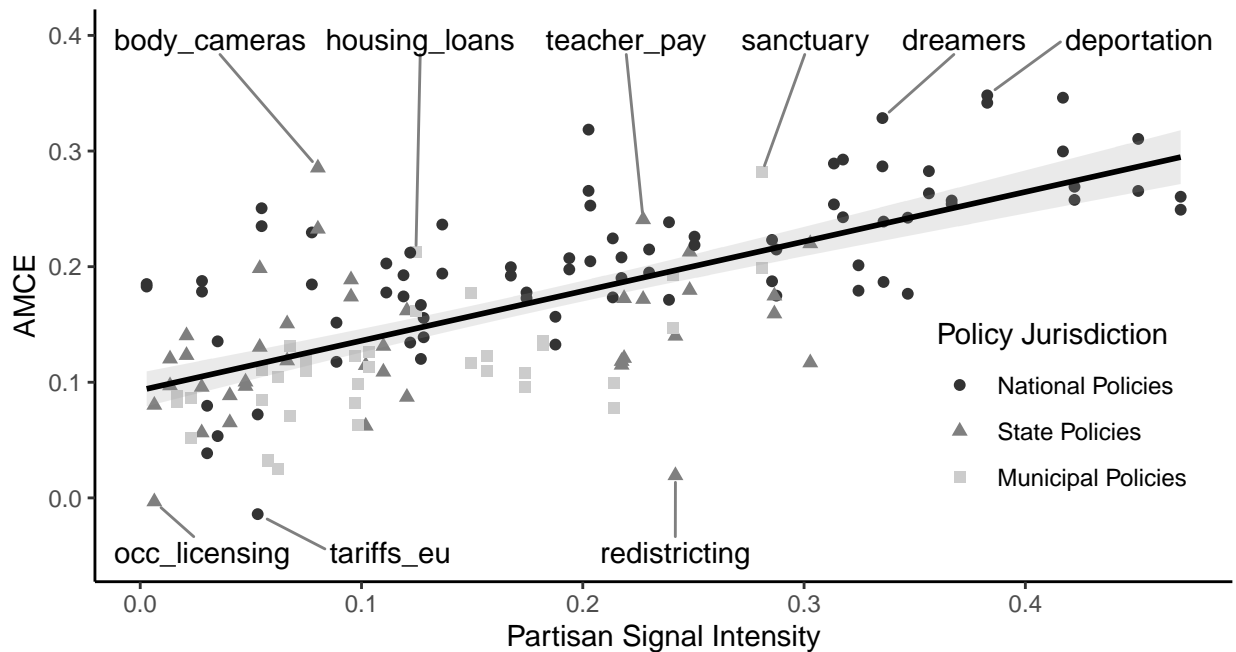


Figure 3.4: AMCE versus Partisan Signal Intensity

We investigate this conjecture in Figure 3.4. Using data we collect prior to the conjoint portion

of the survey, where respondents are directly asked their policy preferences, we construct a measure of partisan signal intensity by taking the absolute value of the difference in the percentage of Democrats and Republicans that agree with the policy’s affirmative setting.¹⁵ The greater the difference, the more partisan polarization exists for this policy and the more clearly respondents can identify the policy with its position on the ideological spectrum. We then plot the partisan signal intensity against the AMCE from Figure 3.2 and Figure 3.3. To account for differences across government level and partisanship conditions, we estimate partisan signal intensity separately within each condition, yielding 156 point estimates (equal to the total number of AMCE).¹⁶

The results of our analysis in Figure 3.4 suggest that the stronger the partisan signal, the greater effect the policy has on candidate selection, supporting the hypothesis that strong signals of type are particularly useful to voters. This pattern persists even when splitting the data by either *office condition* or *policy type*; regardless of whether candidates are contesting national, state, or municipal office or if the policies are national, state, or municipal in nature, partisan signal intensity is positively associated with candidate selection.¹⁷

3.5 Discussion

In combination, our results suggest a different picture of nationalized behavior than has previously been articulated by the nationalization literature, which is made possible by our ability to analyze individual-level preferences leveraging an experimental design. Almost all policy signals, regardless of jurisdiction or functional relevance to the office being contested, are useful to voters. Our results suggest voters leverage whatever information they have to better triangulate the type of the

¹⁵We gather party identification using the standard 7-point scale and then collapse it to a 3-point scale by collapsing leaners into the party they lean towards. The resulting party identification can take the values “Democrat”, “Republican”, or “Independent” in our sample.

¹⁶In Appendix 4.3.5.1, we consider an alternative estimation strategy where we created a pooled estimate of partisan signal intensity ($n = 29$) and average the AMCE of each policy item within each government level condition. The results are identical.

¹⁷If we condition further by both the government level and partisanship conditions, 7 of the 8 estimated relationships are positive and significant. Only for state policy AMCE estimated without partisanship is the relationship is significance “lost”. Full results are given in Appendix 4.3.5.2.

candidates they are evaluating. This casts doubt on the “responsible federalist” view of voters who see distinctions between offices insofar as distinguishing information is available to them.

On average, national policy positions have greater effects on voter behavior than state and local policy positions. We interpret this as national policy signals likely being better indicators of type. This conclusion is supported by the positive relationship between policy effect and the strength of the partisan signal attached to the policy. Voters do not necessarily behave as blind, tribal partisans, but partisanship *does* provide compelling information as to the type of candidate they are willing to vote for.

Our design is not without limitations. Our list of policy positions is not exhaustive, and there may be plausible overlap in jurisdiction between some of the policy areas (which we attempt to mitigate through the explicit labeling of policies as either being of state or national jurisdiction): we invite further experimentation with different baskets of policies. Additionally, particular policy bundles may present apparently incongruous policies, and although we limit explicitly contradictory policies through the category constraint, not all candidates might be candidates that we could plausibly expect people to see in real elections – though we assess a version of this criticism in Appendix 4.3.7 and find little evidence that it drives our results (de la Cuesta, Egami, and Imai 2022).

Our results are important for future discussions of representation in a nationalized context. We tend to think of accountability as a two-step process of (1) understanding what elected officials have done and are responsible for and (2) acting upon relevant information. This is complicated when voters have access to “nationalized” information that acts as an important but imperfect signal of candidate type. In this sense, accountability can be loosely achieved even with limited information, but the quality of such representation is opaque. Thus, our results speak to a larger literature of state and local government responsiveness, namely how sub-national government can be responsive without voters having access to high-quality state-level information (Tausanovitch 2019). Further work is needed to more deeply understand the nature of the accountability structure and what pressures nationalization puts on the future of representation.

Chapter 4

Appendices

4.1 Appendix for Chapter 1

4.1.1 Summary of Previous Research

Table 4.1: Nationalization Literature

Author	Year	Main DV	Main IV	Method	Offices	Timeframe
Abramowitz and Webster	2016	Party loyalty; Dem 2-party vote share	Feeling thermometer; Dem Presidential 2-party vote share	Regression; Correlation	Senate; US House; State House	1972-2014
Amlani and Algara	2021	Dem 2-party vote share	Dem Presidential 2-party vote share	Regression (Spatial Lag)	Senate; Governor	1872-2020
Hopkins	2018	Dem 2-party vote share	Dem Presidential 2-party vote share	Regression	Governor	1928-2014
Jacobson	2015	Standard deviation of district inter-election vote swings; Dem 2-party vote share	Dem Presidential 2-party vote share	Proportions over time; Correlation	Senate; US House	1952-2014

Jacobson	2015	Incumbency advantage; Split-ticket voting; Shared variance	NA	Proportions over time	US House	1952-2012
Knotts and Ragusa	2016	GOP 2-party vote share	Presidential approval	Regression	US House (special elections)	1995-2014
Moskowitz	2021	Split-ticket voting	Percent of Media Market In-State	Regression (Causal)	Senate; Governor	2012-2016
Sievert and McKee	2018	Dem 2-party vote share	Dem Presidential 2-party vote share; incumbency	Regression	Senate; Governor	1980-2015
Weinschenk	2022	Dem 2-party vote share	Dem Presidential 2-party vote share	Regression	Superintendent of Public Educaiton	2000-2021
Weinschenk et al.	2020	Dem 2-party vote share	Dem Presidential 2-party vote share	Regression	State Supreme Court	2000-2018
Zingher and Richman	2018	State legislative partisan balance	Relative national polarization	Regression	State House	1994-2014

4.1.2 MCMC Estimation

The singular value decomposition approach is just one of many techniques that could be used to estimate parameter values for the linear model provided in the paper. Alternatively, one could estimate these parameters using Bayesian techniques via Markov Chain-Monte Carlo. I do so for a subset of state-periods below.

I perform MCMC estimation using Stan via CmdStan 2.29.2 with the following priors:

$$\begin{aligned}\alpha &\sim \text{Normal}(0, 1) \\ \beta &\sim \text{Normal}(1, 0.5)\end{aligned}\tag{4.1}$$

$$\text{Partisan Lean} \sim \text{Normal}(0, 1)$$

with a flat prior over the variance parameter σ for DemMargin. Similar to the SVD approach, I set $\alpha_{president} = 0$ and $\beta_{president} = 1$. Using 4 parallel chains with 2000 warmup iterations and 4000 sampling iterations, the estimation process takes 1489 seconds to estimate the 21 non-presidential 2016-2019 North Carolina contests (the most of any state-period). The resulting α and β parameters are plotted in Figure 4.1 below against the same parameters estimated via singular value decomposition.

The estimates are nearly identical, indicating either estimation strategy can be use. The advantage of the SVD approach, however, is speed. The SVD approach takes less than 2 seconds to complete the same process. Because MCMC becomes inefficient with large numbers of parameters (such as all of the precinct partisan lean estimates), SVD is the obviously preferred method.

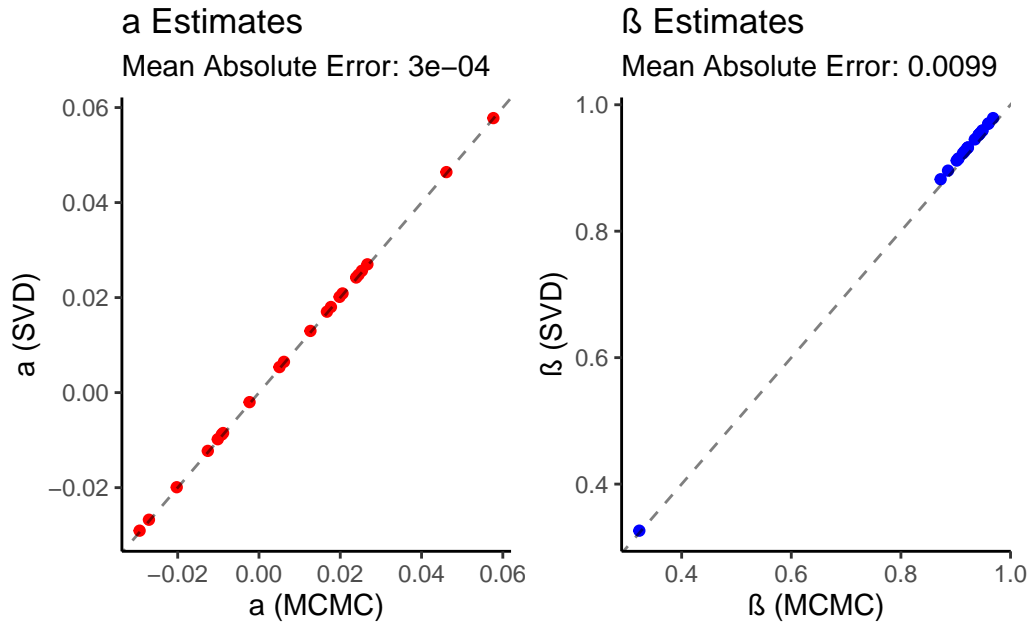


Figure 4.1: MCMC vs. SVD Estimates for North Carolina 2016-2019

4.1.3 VEST Precinct Cleaning

VEST provides shapefiles of precinct-level election data for statewide races between 2016-2020. Unfortunately, precinct boundaries often change (albeit slightly) between election years, making the consistent estimation of precinct-level partisan lean and its effects on vote shares more challenging. To provide constant precinct identifiers for the 2016-2020 period, I use areal weighted interpolation to estimate the number of votes received by Democratic and Republican candidates in each statewide contest in the voting precincts defined during the 2020 redistricting cycle. This allows me to say each precinct observation refers to the same geography in 2020 as it did in 2016, something that would not be possible otherwise.

Areal weighted interpolation involves 4 steps, performed by the R package `areal`. These steps are documented in greater detail in Prener, Revord, and Fox (2022), but explained briefly here. In the first, `areal` calculate the intersections of the source (original VEST data) and target (2020 redistricting results) shapefiles. The target shapefiles are drawn from the US Census Bureau via the R package `tidycensus`. Areal weights are calculated in the second step, such that:

$$W_i = \frac{A_i}{A_j} \quad (4.2)$$

where W_i is the areal weight for intersected feature i , A_i is the area of intersected feature i , and A_j is the total area of source feature j .

In step 3, areal estimates the population value E of the intersected feature i :

$$E_i = V_j \cdot W_i \quad (4.3)$$

where V_j is the population value for source feature j . These estimates are then summarized in step 4 to create the sum of estimated values G for target feature k :

$$G_k = \sum E_{ik} \quad (4.4)$$

where E_{ik} is the estimated values from the intersected features in i within target feature k .

Areal weighted interpolation makes one important assumption about the precincts; population is distributed uniformly within precincts. We know, of course, this is not true. However, given the relatively small changes in precincts from year-to-year and the generally small precinct sizes, the relative gains of more complex areal interpolation methods such as Curiel and Steelman (2018), who overlay the source and target shapefiles atop a smaller grid of atomic-level Census geography, are minimal.

4.1.4 Comparison to Previous Work

An important question is how do the results yielded from the decomposition approach differ from those yielded by previous work and, perhaps more importantly, where do those differences arise. In Figure 4.2 below, I plot my α and β estimates for the gubernatorial elections from the Amlani and Algara (2021) county-level data, 1972-2020, on the y axis. On the x-axis, I estimate the same values using the most common approach of regressing the down-ballot Democratic candidate's two-party vote share on the Democratic presidential candidate's two-party vote share, plotting their respective intercept and slope values against my own. Typically, the intercept parameter is unreported or analyzed, but because the two linear forms are comparable, it is useful to see the connection.

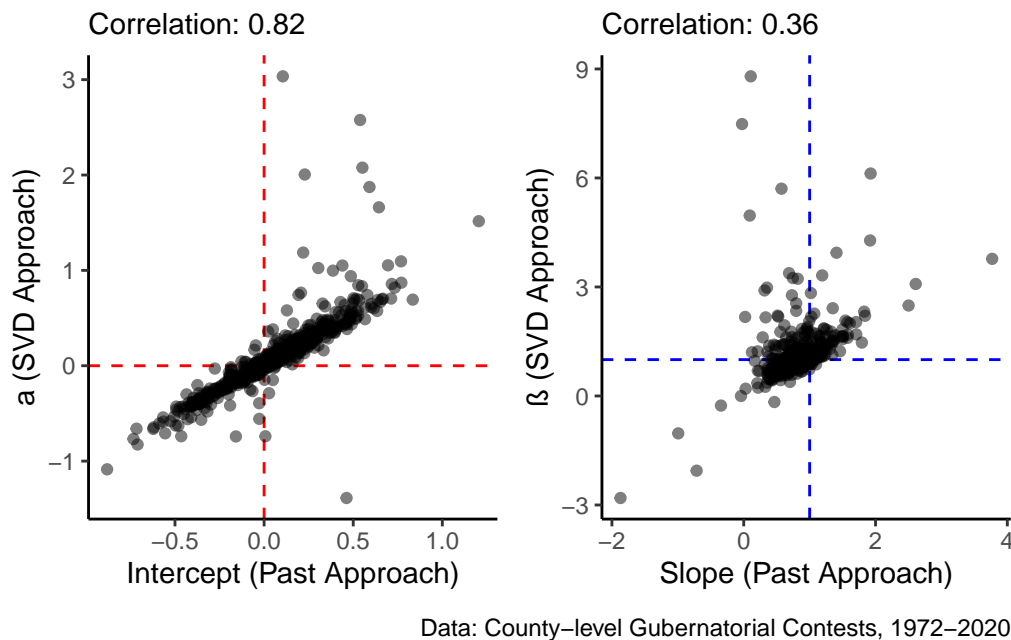


Figure 4.2: SVD Approach vs. Past Approach Results

Because both approaches share the same linear form, the results have an expected correlation, especially among the α /intercept parameters. The range of values from the decomposition approach is greater, and one could ask if that additional variance offers any more information than the linear approach. I'll consider some of the more extreme off-diagonal estimates from the decomposition approach. For example, in the right panel, the highest value of β is 8.8 for the 1978 Alabama guber-

natorial election between Democrat Fob James (winning 72.6% of the total vote) and Republican H. Guy Hunt (25.9%). In the previous 1976 presidential election, Jimmy Carter carried the state with 55.7% of the total vote compared to Gerald Ford’s 42.6%. The simple bivariate regression method used in previous research yields a very different slope parameter of 0.11, giving the impression the two are unrelated. As it relates to preference, however, this would be an inaccurate conclusion, and one that masks a deeper dynamic. While the vote shares may be unrelated, that does not necessarily mean there is no structure in the underlying variance. Figure 4.3 documents this dynamic.

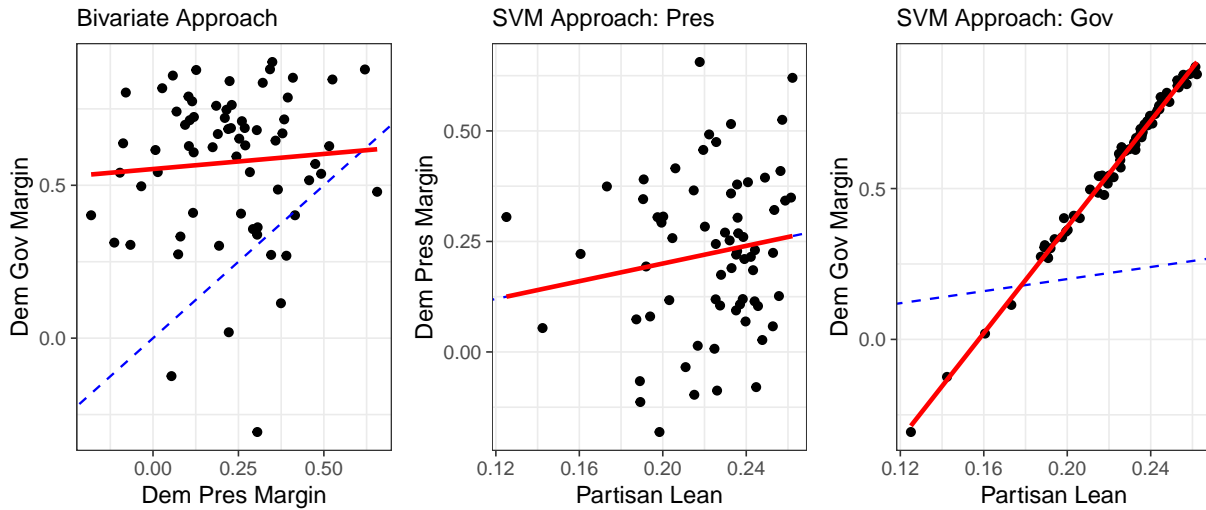


Figure 4.3: 1978 Alabama Results Comparison

The left panel of Figure 4.3 shows the simple bivariate approach, regressing gubernatorial margin on presidential margin, with the resulting weak relationship in red and the reference one-to-one translation of presidential votes to gubernatorial votes (intercept = 0, slope = 1) in blue. In the middle panel, I show the relationship between the same presidential vote margin and the estimated county partisan lean, with the relationship scaled to $\alpha = 0$ and $\beta = 1$, as is done with my SVD approach. Finally, in the right panel, I show the gubernatorial vote margin plotted against the same county-level partisan lean. The relationship, relative to the presidential relationship plotted with the blue line, is much tighter and steeper. This is consistent with our understanding of partisan behavior in the American South during the southern realignment; there was as very tight relationship between

preference and vote shares, where the presidential results were often “noisier.”

While the realigning South is certainly a major source of high variance in outcomes, differences between the two approaches are not limited to this setting. Consider a more contemporary example from 2008. In 2008, Democratic presidential candidate Barack Obama narrowly carried Indiana with 49.9% of the total vote to John McCain’s 48.8%. Democratic gubernatorial candidate Jill Long Thompson, however, lost handily to Republican Mitch Daniels, with 40.1 and 57.8% of the total vote, respectively. The bivariate approach estimates a slope value of 0.81, but my method yields a value of 1.33. Figure 4.4 shows these differences in more detail.

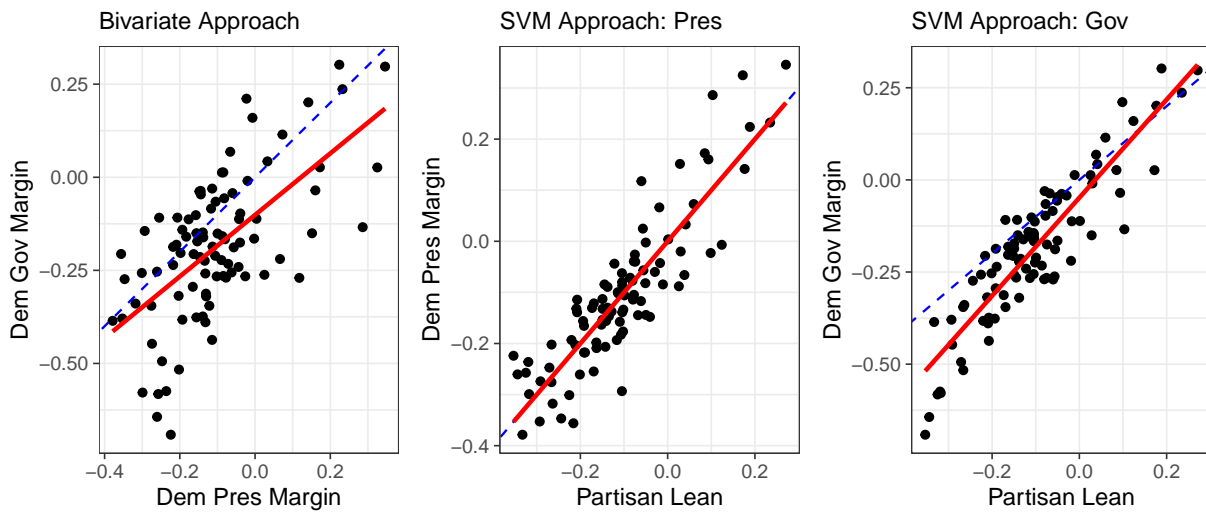


Figure 4.4: 2008 Indiana Results Comparison

The results are presented in the same order as Figure 4.3. While the relationships aren’t as dramatic for Indiana 2008 as Alabama 1978, the outcomes still illuminate the relative shortcomings of the simple bivariate approach. In the left panel, the noisiness of the margins, especially at the most Democratic/Republican ends of the distribution, drive a fairly imprecise result, whereas the relationship with latent partisanship in the right two panels is much tighter. Additionally, we can see that the gubernatorial candidate is actually outperforming Obama slightly in the most Democratic-leaning districts, but underperforming in the most Republican-leaning, driving the steeper translation of partisan-lean into votes.

4.1.5 Descriptive parameter results

Figure 4.5 shows summary descriptions of the estimated parameters, with the top two panels showing the relationship between α and β and the bottom panel showing the distribution of estimated precinct partisan-lean by 4-year interval. The top left panel confirms the hypothesized relationship between the two parameters; greater absolute values of the Democratic candidate handicap are generally associated with smaller preference modifier values ($\rho_{|\alpha|\beta} = -0.49$). Again, this is likely due to α imposing a lower ceiling or higher floor on the performance of Democratic candidates, limiting the remaining variation explained by β . This relationship isn't deterministic, however. Furthermore, the top right panel of Figure 4.5 shows how greater state-level variation in α is associated with greater variation in β ($\rho_{\sigma_\alpha\sigma_\beta} = 0.59$). The results in the bottom panel showing the distribution of precinct partisan-lean demonstrates the relative consistency of the estimates between 4-year intervals. It is important to note that this figure does not show the ideological distribution of the US voting population, just the distribution of precinct partisanship. The tendency for a large number of very small, very Democratic-leaning precincts with Democratic margins of victory over 0.9 to predominate many urban centers accounts for the higher density toward the upper limit of partisan lean.¹ Partisanship in my application is largely a “nuisance” parameter, however, as the main quantities of interest relate to how partisan lean is translated into vote shares.

¹These very Democratic and very Republican precincts also lead to limited cases (0.3%) having estimated partisan lean of greater than 1 or less than -1. Because partisan lean is bounded by construction by the range of possible Democratic margins of victory (-1 to 1), these cases are the typical example of predictions for linear models fit for bounded dependent variables lying outside the possible range of the variable. Removing these precincts from the analysis or forcing them to be 1 or -1 does not change the presented results.

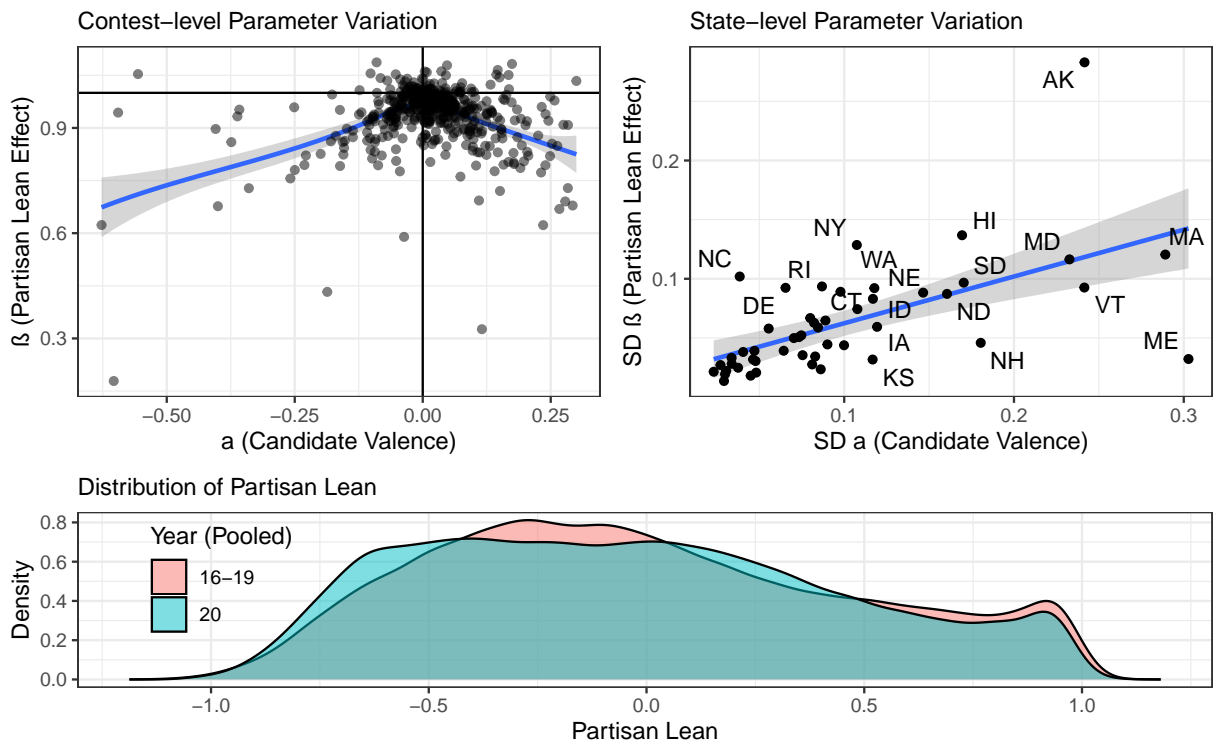


Figure 4.5: Parameter Variation

4.1.6 Additional model fit diagnostics (normalized)

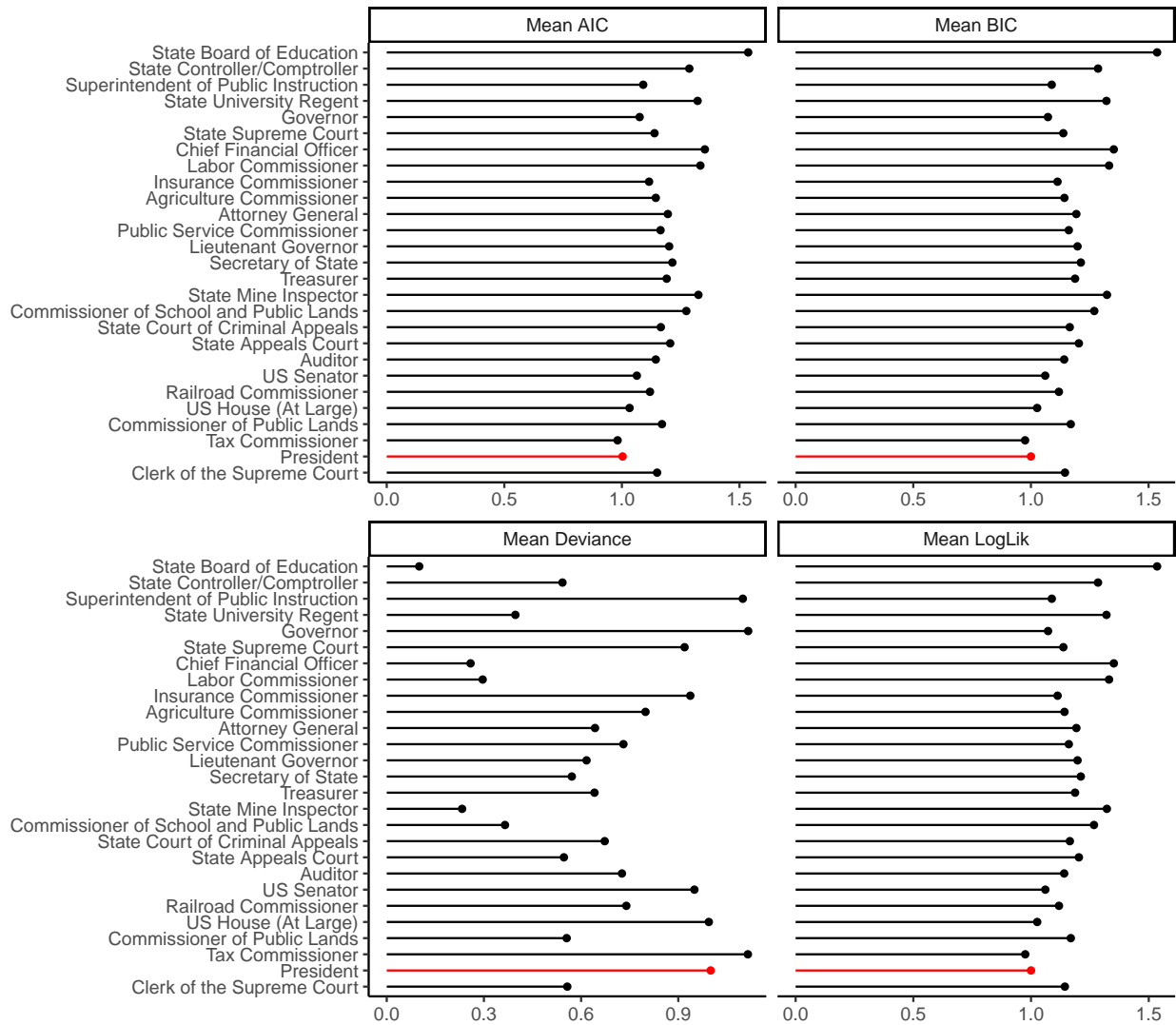


Figure 4.6: Goodness-of-fit Statistics

4.1.7 Proposition Descriptions

The following table shows the parameter estimates and descriptions for each of the proposition analyzed in Figure 1.12. These descriptions are provided courtesy of Ballotpedia.

Table 4.2: Arizona Statewide Proposition Descriptions

Proposition	Subject	Description	Alpha	Beta
100	Taxes	Block enactment of real estate transfer tax	0.535	-0.238
101	Healthcare	Goal: ""Prevent socialized medicine""	-0.002	-0.146
102	Marriage	Marriage is only between one man and one woman	0.098	-0.275
105	I&R	Increase vote needed to approved tax-imposing initiatives	-0.320	0.018
106	Healthcare	Prohibit rules against participation in specific healthcare	0.050	-0.568
107	Affirmative action	Ban preferential acceptance to employment	0.153	-0.571
109	Hunting	Would give a constitutional protection to the right to hunt in Arizona	-0.219	-0.426
110	Natural resources	Authorizes exchange of state trust lands in order to protect military installations.	-0.089	-0.494
111	Admin. of gov't.	Re-name the position of Secretary of State to Lieutenant Governor.	-0.233	-0.256
112	Direct democracy measures	Change petition drive deadline by two months earlier than current deadline.	-0.056	-0.286
113	Labor	Extend the right of Arizonans to use a secret ballot in union elections	0.134	-0.568
114	Law enforcement	Prohibits crime victims from being subject to a claim for damages for causing death or injury.	0.572	-0.260
115	Judiciary	Relating to the modification of the Appellate and Trial Court Commissions.	-0.437	-0.075
116	Taxes	Give tax break to businesses with newly acquired equipment.	-0.157	-0.175
117	Taxes	Limit annual growth in limited property value of locally assessed properties.	0.084	-0.263
118	Budgets	Yearly Permanent Fund distribution to be 2.5% of monthly market values of the fund from 5 previous years.	-0.039	-0.194

119	Property	Lets legislature enact a process to exchange trust land if related to protecting military installations.	0.188	-0.255
120	Environment	Would declare state sovereignty over state natural resources based on the argument of ""equal footing.""	-0.387	-0.207
121	Admin. of gov't.	Implement a top-two style open primary system.	-0.322	0.059
125	Pension	Allow for adjustments to the Elected Officials' Retirement Plan and Corrections Officer Retirement Plan	0.024	-0.158
126	Taxes	Prohibits the government from increasing taxes on services in the future	0.276	-0.276
127	Energy	Requires 50 percent of energy to come from renewable resources by 2030	-0.344	0.450
200	Business	Regulations on payday loan industry	-0.119	0.218
201	Property on the ballot	Minimum 10-year warranty on new homes.	-0.499	0.537
202	Immigration	Penalties on businesses that bypass immigration laws	-0.180	0.226
203	Marijuana	Legalization of medical marijuana	0.038	0.353
204	Taxes	Would renew the sales tax increase approved in 2010.	-0.246	0.328
205	Marijuana	Legalize marijuana for individuals older than 21 years of age	-0.001	0.422
206	Minimum wage	Minimum wage increase; paid sick time	0.184	0.583
207	Marijuana	Legalizes the recreational possession and use of marijuana	0.205	0.465
208	Taxes	Increases the tax on incomes exceeding \$250,000 for teacher salaries and schools	0.018	0.718
300	Legislature	Increase state legislative salaries to \$30,000	-0.273	-0.093
301	State budgets	Transfer money from a land-conservation fund to the general fund	-0.490	-0.128
302	State budgets	Measure to repeal First Things First education program	-0.418	-0.315
303	Healthcare	Allows terminally ill patients access to medical treatments which have completed phase one of a clinical trial, but are not yet approved by FDA	0.526	-0.321
304	Gov't Salaries	Increases salaries of state legislators by \$11,000 to \$35,000 annually	-0.343	-0.047
305	Education	Upholds SB 1431, expanding Empowerment Scholarship Accounts program	-0.292	0.131

306	Elections	Designates unlawful contributions from clean election accounts and removes commission exemption from rulemaking requirements	0.141	-0.233
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4.1.8 Correlates of Parameter Values

Substantively, the average down-ballot race has similar underpinnings as the presidential race. However, the averages belie substantial variation in the parameter estimates. While there isn't nearly as much variation as in previous decades, there are enough contest-to-contest differences to merit a deeper analysis of their correlates. In this appendix, I utilize common correlates of nationalization in existing research to first validate the measures of nationalization generated by the decomposition approach and, second, examine how the dynamics of the relationships change in down-ballot contests.

4.1.8.1 Incumbency and Candidate Quality

A significant body of research exists regarding how incumbency and generalized candidate quality influence voting behavior, consistently finding the proportion of votes received by incumbent politicians are significantly greater than the number received by the incumbent party if the incumbent does not run (Ansolabehere and Snyder Jr. 2004; Ashworth and Bueno de Mesquita 2008; Gelman and King 1990; D. S. Lee 2008). This literature spans many levels of government, with Ansolabehere and Snyder Jr. (2002) finding evidence of incumbency advantage across federal and statewide races and Trounstein (2011) finding similar evidence in city council elections. Many sources of such an advantage have been hypothesized and measured, including office benefits (including fundraising), candidate quality, and opposition candidate deterrence (Fournaies and Hall 2014; Hirano and Snyder Jr. 2009).

The connection between nationalization and incumbency advantage is noted by Jacobson (2015a); as elections become more party-centered around presidential contests, straight ticket voting increases, and incumbents in opposition-leaning districts have a harder time currying a personal vote. The approach used in this paper yields measures particularly useful in measuring how incumbency influences nationalization; if we understand incumbency as a buffer against more partisan voting, I expect contests with an incumbent running to have greater absolute

candidate effects and smaller preference modifiers. I separately analyze the statewide results from 2016-2020 along with the over-time results from 1972-2020 in Figure 4.7. In the top panel, I regress the estimated parameters on the incumbency status for the contest, which is a factor variable with three levels: no incumbent, Republican incumbent, and Democratic incumbent (effects are estimated in reference to no incumbent). These incumbency data (and later candidate quality data) were gathered manually for all contests in the statewide election data. I control for logged state population and use cluster-robust standard errors at the state-four-year level. I also present an auxiliary measure for nationalization in these results: the break-even percentile of the Democratic candidate. This summary measure takes the value of Partisan Lean_i for the j^{th} contest where the expected Democratic candidate margin of victory equals zero $\left(\frac{-\alpha_j}{\beta_j}\right)$ and reports the percentile of that value in the national distribution of precinct partisan lean in that four-year interval.

The results in the top panel support the hypothesized relationship between incumbency and preference. For both Democratic and Republican incumbents, the candidate valence and break-even percentile move in the expected direction; Democratic (Republican) incumbents have greater (smaller) valence μ and smaller (greater) break-even percentiles in partisan lean. The estimate for candidate valence for Democratic incumbents is only statistically significant at $\alpha = 0.1$, however. Contests with Democratic incumbents also have a smaller partisan lean effect β than contests without incumbents, while contests with Republican incumbents have no such difference. This preference-modifying difference between Democrats and Republicans is consistent with Jacobson's (2015a) finding that the decline in incumbency advantage has been most acutely felt by Democrats, who had held elected office in more "uncongenial" districts than Republicans. My results suggest this relative decline of Democrats is not due to lopsided candidate effects, but the closer alignment of down-ballot voting behavior with partisan preference.

In the second panel, I consider the over-time data for senate and governor contests from 1972-2020. Because the data are coded as a simple binary measure of whether the seat is open or vacant (no incumbent running) instead of a party-specific factor, it is necessary to appropriately scale

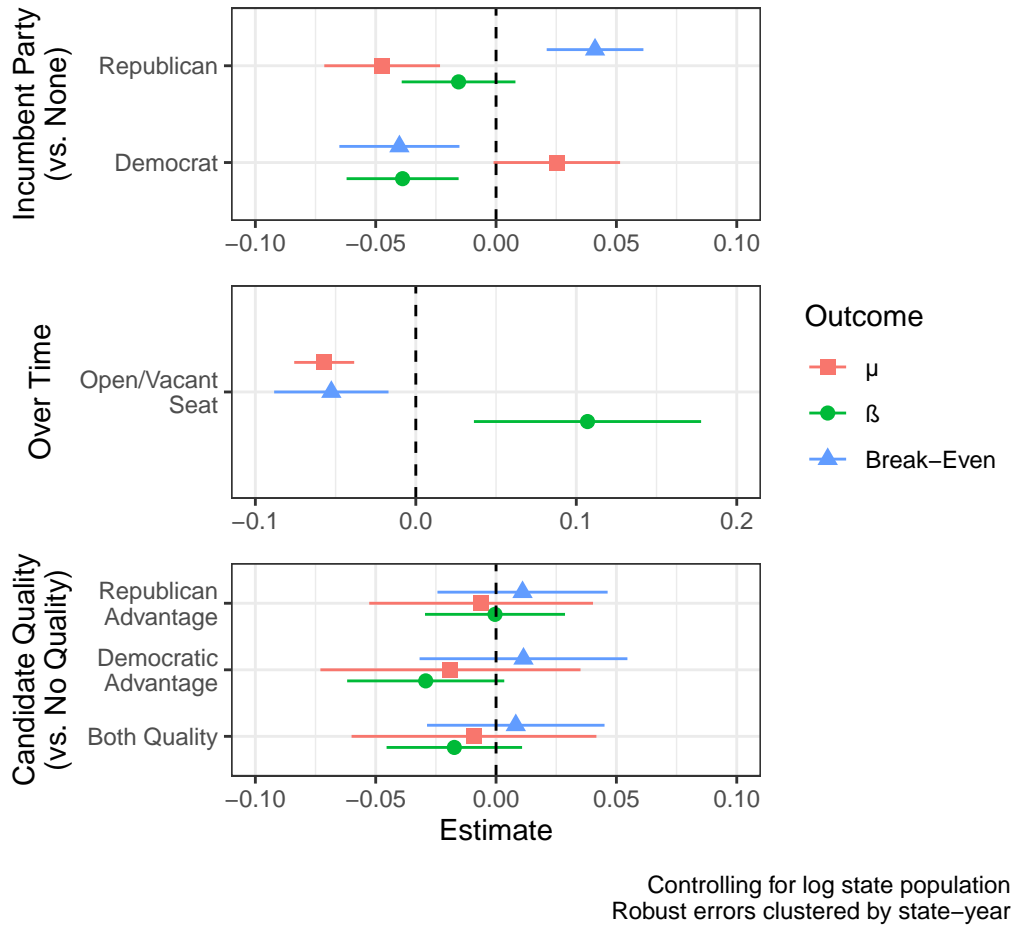


Figure 4.7: Incumbency and Quality Results

the outcome variables such that I am measuring the absolute magnitude of the effect instead of the direction, which will vary by party. For μ I simply take the absolute value of the effect. For the break-even percentile, I take the absolute difference of the original value and the break-even partisan lean percentile of the reference presidential candidate. This can simply be interpreted as how different the break-even precincts are in terms of partisanship for down-ballot and presidential candidates; smaller differences indicate candidates breaking even at relatively similar levels of partisanship. I control for the lagged parameter value and a binary indicator for Southern states. The results are similar in the top panel. In open seat elections without incumbents, absolute candidate valence and break-even percentiles are less than in races with incumbents, and the partisan lean effect exerts a greater effect.

Finally, in the third panel, I consider a specific characteristic of candidates thought to operate similar to/within incumbency advantage: candidate quality. As Carson, Engstrom, and Roberts (2007) note, candidate quality is most obviously understood as the ability of such candidates to be skilled and well-known campaigners. I again consider this phenomenon in the 2016-2020 statewide contests, subsetting to open seats to determine the independent effect of candidate quality from incumbency.² I code candidate quality similarly to incumbency, noting which party has the “advantage” in quality in the contest and using cases where neither candidate is a quality candidate as the reference level, where quality is defined as having previously held elected office. The results are insignificant across all outcome variables and notably more imprecise due to the relatively low number of cases (143) with no incumbent.

In incumbency results merit deeper exploration, and the variety of offices covered by the statewide election data allow for such analyses. In Figure 4.8, I split the effect of incumbency by office type using the four previously discussed categorizations: Federal, State Executive - High, State Executive, and State Judicial. The same regression analysis split by office category is shown

²Carson, Engstrom, and Roberts (2007) use a two-equation approach with lagged indicators for incumbent party and candidate quality to consider the effect of candidate quality in both open and non-vacant seats. In my application, I lack the over-time data to estimate such a model with sufficient precision, as the four-year terms beginning in 2016 end in 2020, meaning I would only have one year of observations. This is also why I am unable to employ the regression-discontinuity designs used by D. S. Lee (2008) and Trounstein (2011) for causally estimating the effects of incumbency. As more precinct-level data become available, these methods will become more plausible.

in the left panel. In the right panel, I plot the distribution of μ and β parameters, split by office category and incumbent party, to visualize the variation in results.

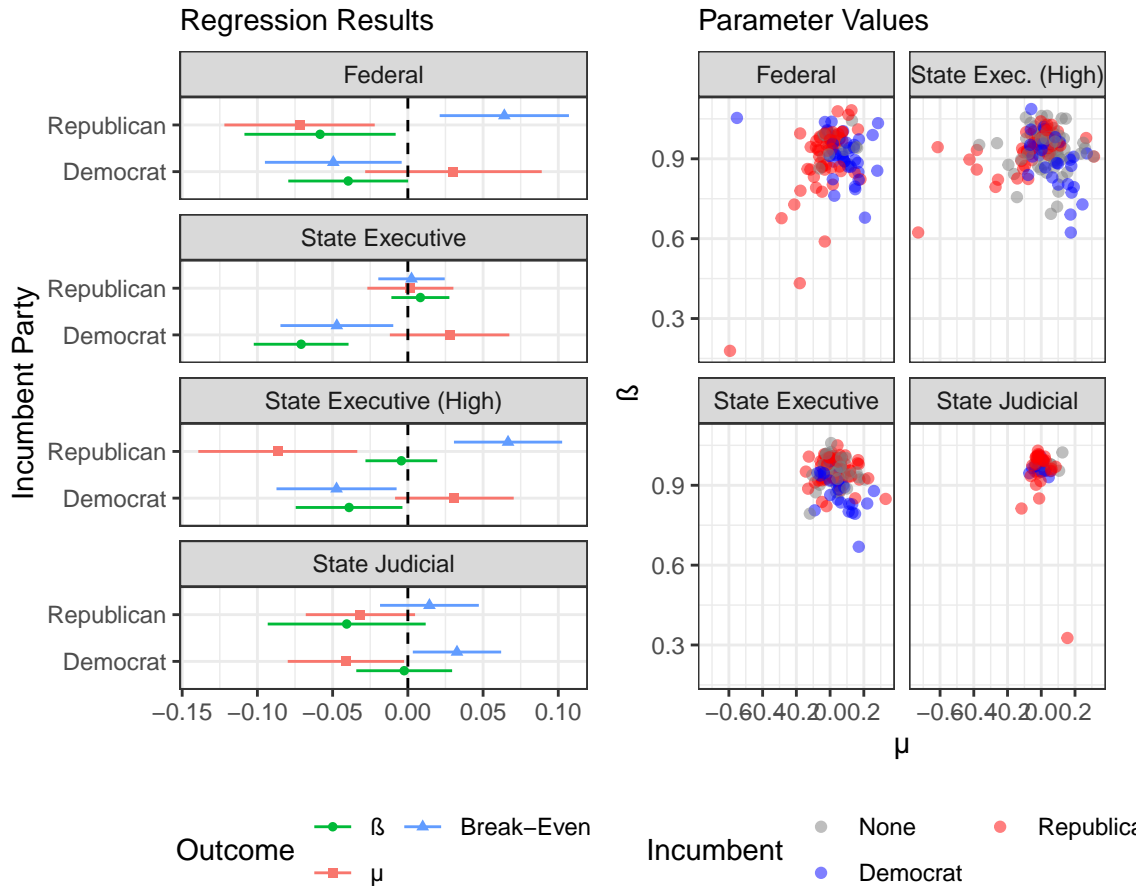


Figure 4.8: Incumbency Results by Office Type

While more imprecise, the results suggest the benefits of incumbency are realized most dramatically in higher levels of government and mostly by Democrats. Variation in the federal incumbency advantage is the greatest, with the largest average effect sizes and significant values across all outcomes except μ for Democrats. The benefits of incumbency start to dissipate as we move further down-ballot. The benefits for Democrats still exist in lower-salience state executive contests, but the effects disappear completely for Republicans. In state judicial races, there is some evidence for anti-incumbent effects for Democrats, but the results are only marginally significant and estimated on data with fairly low levels of variation (as show in the right panel). This pattern coheres with

the over-time results presented by Ansolabehere and Snyder Jr. (2002); the benefits of incumbency have tended to move in parallel across offices, but are strongest in the upper levels of government. This matches our theoretical understanding of how incumbency influences vote choice. If incumbency affords candidates access to the benefits of office and an indicator of quality, then more prestigious and powerful positions should have greater effects for incumbency.

In summary, these results show instances where partisanship's dominating effect on election outcomes wanes. Incumbency acts as an anti-nationalizing force, allowing for greater candidate valence effects and outcomes imperfectly aligned with partisan lean. However, incumbency's resistance to partisanship is limited to high salience elections; what little variation exists in down-ballot races is not explained by the presence of incumbents.

4.1.8.2 News and Mentions

Lastly, I consider how candidate-specific information and the general information and media environments condition partisanship's nationalizing effect. A worrisome trend for scholars of representation and accountability has been the decline of local media circulation and readership (Hayes and Lawless 2018). As local newspapers decline in number, so too does attention paid to local politics (Hopkins 2018). Specifically, G. J. Martin and McCrain (2019) find the acquisition of local news media stations by a national conglomerate (the Sinclair Broadcast Group) increased relative coverage of national topics at the expense of local topics and increased the rightward slant of coverage, with Levendusky (2022) finding downstream conservative effects on viewers' voting behavior. Not only has access to local news declined; so too has the ability of news to inform voters. Peterson (2021) finds the effect of newspapers on candidate-specific awareness has halved relative to previous years.

It is possible the information voters receive directly from candidates is equally nationalized. Das et al. (2022) find gubernatorial and congressional rhetoric on Twitter is remarkably similar, though mayors still seem to address different topics. Furthermore, declining access to locale-specific information has been directly tied to the nationalization of election results. Leveraging the quasi-random

geography of television media markets, Moskowitz (2021) shows residents of in-state markets (1) receive more coverage specific to their gubernatorial and senatorial candidates and (2) vote straight-ticket at a lower rate.

How should these studies shape our expectations regarding the connection between local information, partisan preference, and voting? Broadly, previous research suggests access to information specific to local candidates allows voters to make decisions informed by more than party identification. In terms of the parameters from the decomposition approach to nationalization, I would expect such information to increase the absolute effect of candidate valence. The expected partisan lean effect is unclear. With more access to information, voters may be able to better determine the relative ideological positions of candidates and make a choice better informed by their personal ideology. Alternatively, the information gained may not necessarily be ideological or partisan in nature (perhaps related to the personal qualities of the candidate), meaning decisions could be less informed by their underlying partisan lean.

I analyze these hypotheses in Figure 4.9 with multiple measures of information from three sources. First, I consider the overall newspaper circulation within a state (logged and per-capita) as a general measure of the state's information environment. These data are provided by the Alliance for Audited Media. These data also provide a measure of the proportion of total circulation given by in-state newspapers, a coarser (and non-causally identified) corollary to Moskowitz's (2021) approach. Next, I consider candidate-specific mentions in both local newspapers and national broadcast media. For newspaper mentions, I use Newspapers.com to obtain the number of pages in within-state newspapers in which candidates of the statewide election data are mentioned during the election year. For national news mentions, I use the GDELT 2.0 Television API to obtain a mentions-per-hour measures of the same candidates in national news media (CNN, MSNBC, FOX, and similar stations). All models control for logged state population with cluster-robust standard errors at the state-four-year level

Overall, most results are insignificant. There are no effects for general information environments with respect to newspapers. There is, however, a significant relationship between specific

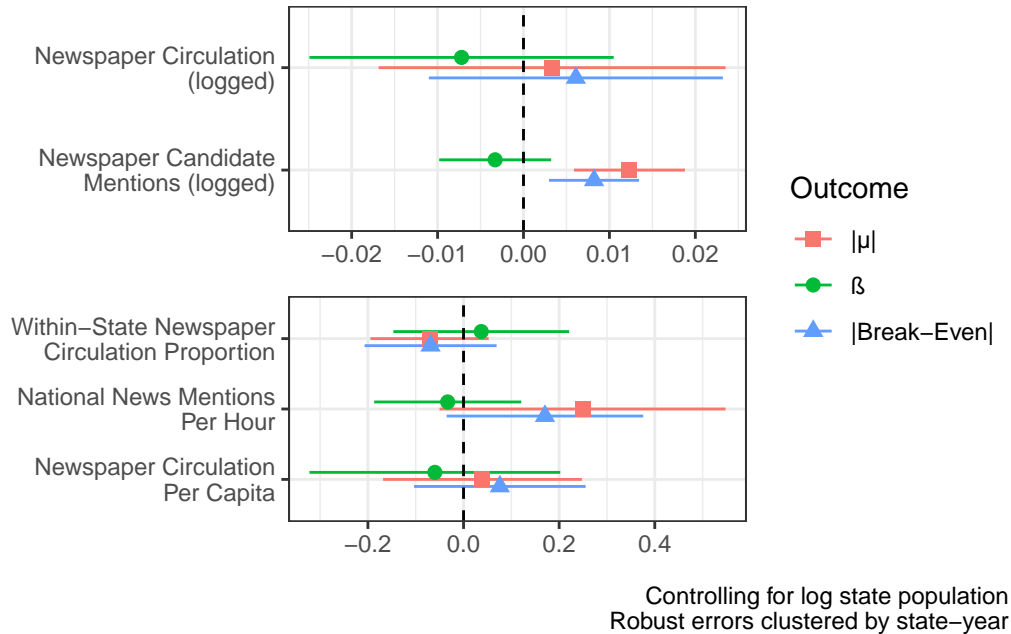


Figure 4.9: News Results

newspaper mentions and the absolute candidate valence $|\mu|$ and difference in break-even percentile relative to the president. Interpreted more intuitively, going from the minimum (0 mentions) to the maximum (17,000) is associated with an increase in the absolute candidate valence effect of 0.1. Because this effect is understood in terms of vote margin, this is a fairly large swing; it would be a change of losing 52.5% to 47.5% to winning by the same margin. Interestingly, this effect does not extend to partisan preference, suggesting the effect of media is mostly constrained to these candidate-specific effects instead of operating on how partisanship is translated into votes. Furthermore, while the effects are in the same direction, they are insignificant for national news mentions per hour. This is likely a function of data sparsity at lower levels of government. For many races (including high-profile statewide races), there are simply no national news media mentions.

Decomposing the effect of candidate-specific mentions in newspapers and national news by office category, the effects are driven by different sources. Increases in candidate valence are associated with increases in newspapers mentions in all state executive offices, and with national news mentions in high state executive offices. Significant differences in the break-even percentiles show the same association with newspaper mentions in high state executive offices. Information,

therefore, seems to be of highest leverage in more “goldilocks” situations; when office salience is high enough to attract voter attention, but not so high as to limit the potential learning on the part of voters. Overall, these results give a similar understanding of nationalization as the incumbency analysis: partisanship powerfully structures elections across all offices and is fairly resistant to other forces. Some variation is explained by increased access to local information, but mostly in high-salience statewide elections.

4.2 Appendix for Chapter 2

4.2.1 K Selection

One researcher-defined parameter in structural topic modeling is the number of topics k . There is no set rule for selecting k , but researchers should ideally balance a number of factors, including the likelihood, residuals, and semantic coherence. Increasing/decreasing k will mechanically tend to improve some factors but detract from others. Figure 4.10 shows a number of model diagnostics, including the held-out likelihood, lower bound of the marginal log-likelihood, residuals, and mean semantic coherence. Semantic coherence gives a measure of word co-occurrence; when co-occurring words are also highly likely words within a topic, semantic coherence is high.

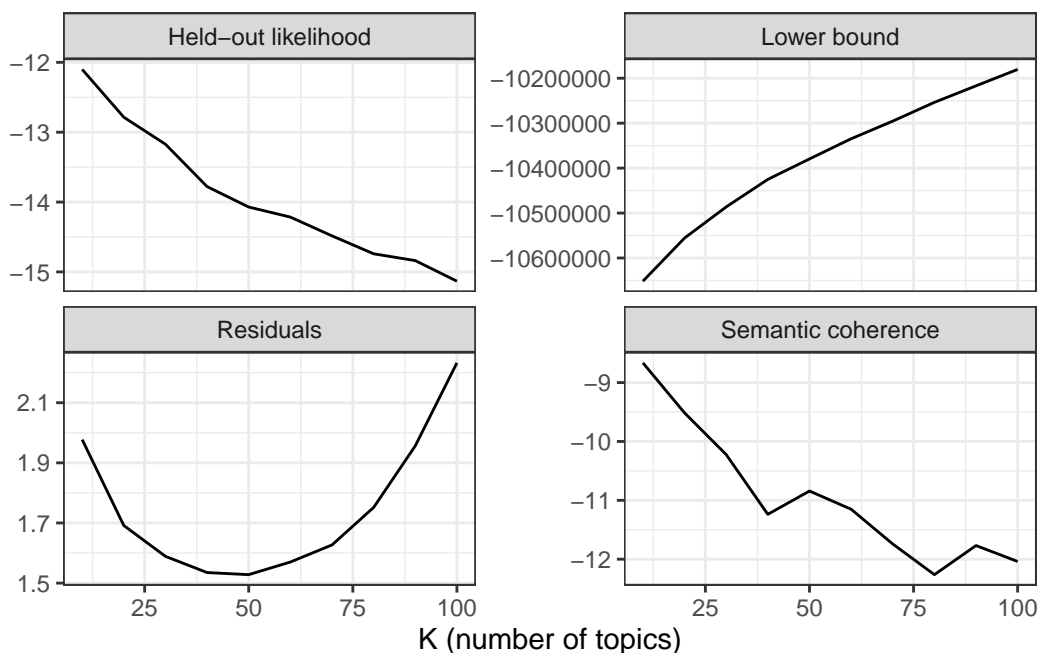


Figure 4.10: Model Diagnostics by Number of Topics

The diagnostics suggest a topic number between 40 and 50 would be a good balance of fit and coherence. I use $k = 40$ for the models presented in the paper so as to lean against potential overfitting with higher numbers of topics.

One may be concerned different topic numbers yield different results. To alleviate this concern,

I perform the same analysis presented in Table 2.1 in the tables below, varying k . The table shows the accuracy of the models in predicting the held-out training documents. Across all values of k , the results remain remarkably similar.

Table 4.3: Classification Model Performance on Heldout Documents - Varying K

K	Logistic Regression (Nonpenalized)	Naive Bayes	Penalized Logistic Regression (Lasso)	Boosted Gradient Descent (XGBoost)	Support Vector Machine
10	0.99	0.98	0.98	0.99	0.99
20	0.99	0.93	1.00	0.94	0.99
30	1.00	0.98	1.00	0.97	1.00
40	0.99	0.95	0.99	0.93	1.00
50	0.99	0.94	0.99	0.92	0.99
60	1.00	0.93	0.98	0.91	1.00
70	0.99	0.94	0.98	0.92	0.99
80	1.00	0.94	0.98	0.95	1.00
90	0.99	0.93	1.00	0.87	0.99
100	0.99	0.88	0.97	0.96	0.99

More broadly, because I do not attempt to assign post-hoc meaning to the content of certain topics (for example, interpreting one topic as the “education” topic and another as the “military” topic), my particular approach to measuring nationalization is less vulnerable to k misspecification. The topics in my methodological approach are simply collections of words that co-occur more or less frequently in state or national contexts. How those topics are sliced is less of a concern than how predictive the topics are of state or national content as a sort of dimension-reduced representation of the text itself.

4.2.2 Complete Model Results

While I use unpenalized logistic regression for the main model of the paper, other classification model approach yield fairly similar results. I present the results in the tables below. Generally, the worst-performing model is boosted gradient descent, which tends to overpredict the prevalence of state content.

Table 4.4: Classification Model Performance on C-SPAN Debates

Model	National Documents			State Documents		
	Correct	Incorrect	Accuracy	Correct	Incorrect	Accuracy
Logistic Regression	83	3	0.97	309	2	0.99
Naive Bayes	83	3	0.97	259	52	0.83
Lasso	84	2	0.98	304	7	0.98
XGBoost	56	30	0.65	311	0	1.00
Support Vector Machine	84	2	0.98	306	5	0.98

Table 4.5: Classification Model Performance on TV Ads

Model	National Documents			State Documents		
	Correct	Incorrect	Accuracy	Correct	Incorrect	Accuracy
Logistic Regression	1408	120	0.92	579	227	0.72
Naive Bayes	1196	332	0.78	631	175	0.78
Lasso	1413	115	0.92	579	227	0.72
XGBoost	803	725	0.53	786	20	0.98
Support Vector Machine	1414	114	0.93	575	231	0.71

Table 4.6: Classification Model Performance on Twitter

Model	National Documents			State Documents		
	Correct	Incorrect	Accuracy	Correct	Incorrect	Accuracy
Logistic Regression	431892	520131	0.45	74918	26588	0.74
Naive Bayes	164250	787773	0.17	93404	8102	0.92
Lasso	398934	553089	0.42	78205	23301	0.77
XGBoost	32378	919645	0.03	100478	1028	0.99
Support Vector Machine	443220	508803	0.47	73934	27572	0.73

4.2.3 Custom Stopwords

One assumption of my methodological approach is the classification model is distinguishing between meaningful state and national textual content. A concern related to this assumption would be the ability of the model to “cheat;” that is, classifying based on words that aren’t necessarily meaningful indicators of state or national topics or policies. Such words could include state names, the names of state capitols, nicknames for state residents, or common phrases spoken during speeches in particular contexts. The topic modeling approach somewhat alleviates this concern; using collections of words instead of individual words as predictors softens the impact of any one outlier word. However, I also remove the following stopwords from the entire corpus of text:

[1] “alabama, alaska, arizona, arkansas, california, colorado, connecticut, delaware, florida, georgia, hawaii, hawai, hawai’i, idaho, illinois, indiana, iowa, kansas, kentucky, louisiana, maine, maryland, massachusetts, michigan, minnesota, mississippi, missouri, montana, nebraska, nevada,

new, hampshire, jersey, mexico, york, north, carolina, dakota, ohio, oklahoma, oregon, pennsylvania, rhode, island, tennessee, texas, utah, vermont, virginia, washington, wisconsin, wyoming, house, district, representative, senate, senator, senatorial, governor, gubernatorial, governorship, question, crosstalk, applause, laughter, moderator, candidate, gentlemen, ladies, congressman, state, will, alaskan, thank, year, rebuttal, speaker, lieutenant, upstate, downstate, make, must, assemblyman, assemblywoman, assemblyperson, want, need, inaudible, commonwealth, aloha, isn, ve, didn, don, 1998, 1999, 2000, 2001, 2002, 2003, 2004, 2005, 2006, 2007, 2008, 2009, 2010, 2011, 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2019, 2020, alabamians, alaskan, arizonan, arkansan, californian, coloradan, connecticuter, delawarean, washingtonian, floridian, georgian, hawaiian, idahoan, illinoisan, indianian, iowan, kansan, kentuckian, louisianian, mainer, marylander, massachusettsan, michiganian, minnesotan, mississippian, missourian, montanan, nebraskan, nevadan, hampshirites, jerseyan, mexican, yorker, ohioan, oklahoman, oregonian, pennsylvanian, islander, carolinian, dakotan, tennessean, texan, utahn, vermonter, virginian, washingtonian, west virginian, wisconsinite, wyomingite, alabama, arizonian, arkie, californiac, coloradoan, connecticotian, muskrat, floridan, malihini, idahoer, illinoian, indianer, hawkeye, kanser, kentucker, louisianan, mainiac, marylandian, michigander, mississipper, cornhusker, nevadian, hampshireman, jerseyite, tar, heel, nodak, buckeye, oklahomians, oregoner, pennamite, rhodian, south carolinian, volunteer, texian, utahan, toner, mountaineer, cheesehead, wyomingian, bamer, inuit, arkansawyer, californio, connecticutian, kamaaina, illinoyer, hoosier, iowegian, jayhawk, kentuckyite, cajun, massachusite, michiganese, jerseyite, okie, sandlapper, big bender, tejano, badger, wyoman, aleut, nutmegger, islander, illini, michigine, haida, michiganite, inupiaq, wolverine, aleutian, inuk, alabamianss, alaskans, arizonans, arkansans, californians, coloradans, connecticuters, delawareans, washingtonians, floridians, georgians, hawaiians, idahoans, illinoisans, indianians, iowans, kansans, kentuckians, louisianians, mainers, marylanders, massachusettsans, michiganians, minnesotans, mississippians, missourians, montanans, nebraskans, nevadans, hampshirites, jerseyans, mexicans, yorkers, ohioans, oklahomans, oregonians, pennsylvanians, islanders, carolinians, dakotans, tennesseans, texans, utahns, vermonters, virginians, washingtonians, west vir-

ginians, wisconsinites, wyomingites, alabamas, arizonians, arkies, californiacs, coloradoans, connecticotians, muskrats, floridans, malihinis, idahoers, illinoisians, indianers, hawkeyes, kansers, kentuckers, louisianans, mainiacs, marylandians, michiganders, mississippers, cornhuskers, nevadians, hampshiremans, jerseyites, tars, heels, nodaks, buckeyes, oklahomianss, oregoners, pennamites, rhodians, south carolinans, volunteers, texians, utahans, toners, mountaineers, cheeseheads, wyomingians, bamers, inuits, arkansawyers, californios, connecticutians, kamaainas, illinoyers, hoosiers, iowegians, jayhawks, kentuckeyites, cajuns, massachusites, michiganeses, jerseyites, okies, sandlappers, big benders, tejanos, badgers, wyomans, aleuts, nutmeggers, islanders, illinis, michigines, haidas, michiganites, inupiaqs, wolverines, aleutians, inuks, montgomery, juneau, phoenix, little rock, sacramento, denver, hartford, district of columbia, dover, tallahassee, atlanta, honolulu, boise, springfield, indianapolis, des moines, topeka, frankfort, baton rouge, augusta, annapolis, boston, lansing, st paul, jackson, jefferson city, helena, lincoln, carson city, concord, trenton, santa fe, albany, raleigh, bismarck, columbus, oklahoma city, salem, harrisburg, san juan, providence, columbia, pierre, nashville, austin, district of columbia, salt lake city, montpelier, richmond, olympia, charleston, madison, cheyenne”

4.3 Appendix for Chapter 3

4.3.1 Policy item agreement and disagreement

4.3.1.1 By Party ID

Below we list the weighted proportion of respondents indicating they agree, disagree, or “Don’t Know” in response to being asked the affirmative setting of each of the policies which were incorporated into the conjoints. Responses are broken down by respondent party ID and wave (e.g. interaction of *partisan label condition* and *government level condition*).

Table 4.7: Policy Agreement/Disagreement by Partisan and Government Level Conditions

Policy	Party Label Shown?	Gov. Level Condition	PID	Agree	Disagree	DK
body_cameras	No	state	Democrat	0.8	0.1	0.1
charter_schools	No	state	Democrat	0.5	0.3	0.2
court_fees	No	state	Democrat	0.5	0.3	0.2
deportation	No	state	Democrat	0.3	0.6	0.1
dreamers	No	state	Democrat	0.8	0.1	0.1
highways	No	state	Democrat	0.6	0.3	0.1
israel	No	state	Democrat	0.5	0.3	0.2
medicare_for_all	No	state	Democrat	0.8	0.1	0.1
military_size	No	state	Democrat	0.3	0.5	0.1
occ_licensing	No	state	Democrat	0.4	0.3	0.3
path_to_citizen	No	state	Democrat	0.7	0.2	0.1
private_prisons	No	state	Democrat	0.7	0.1	0.2
public_option	No	state	Democrat	0.8	0.1	0.1
redistricting	No	state	Democrat	0.7	0.1	0.3
saudi_weapons	No	state	Democrat	0.7	0.1	0.1
state_pre_k	No	state	Democrat	0.7	0.2	0.2
tariffs_china	No	state	Democrat	0.5	0.3	0.3
tariffs_eu	No	state	Democrat	0.4	0.3	0.3
teacher_pay	No	state	Democrat	0.8	0.1	0.1
use_of_force	No	state	Democrat	0.6	0.2	0.2
body_cameras	No	state	Independent	0.7	0.1	0.3
charter_schools	No	state	Independent	0.4	0.3	0.3
court_fees	No	state	Independent	0.4	0.3	0.3
deportation	No	state	Independent	0.4	0.3	0.3
dreamers	No	state	Independent	0.6	0.2	0.2
highways	No	state	Independent	0.4	0.3	0.3
israel	No	state	Independent	0.3	0.3	0.4
medicare_for_all	No	state	Independent	0.4	0.3	0.3

military_size	No	state	Independent	0.2	0.5	0.3
occ_licensing	No	state	Independent	0.3	0.3	0.4
path_to_citizen	No	state	Independent	0.4	0.3	0.3
private_prisons	No	state	Independent	0.4	0.2	0.4
public_option	No	state	Independent	0.5	0.1	0.3
redistricting	No	state	Independent	0.4	0.2	0.4
saudi_weapons	No	state	Independent	0.6	0.1	0.3
state_pre_k	No	state	Independent	0.5	0.2	0.3
tariffs_china	No	state	Independent	0.4	0.2	0.4
tariffs_eu	No	state	Independent	0.3	0.2	0.5
teacher_pay	No	state	Independent	0.6	0.1	0.3
use_of_force	No	state	Independent	0.5	0.2	0.3
body_cameras	No	state	Republican	0.8	0.2	0.1
charter_schools	No	state	Republican	0.5	0.3	0.2
court_fees	No	state	Republican	0.4	0.4	0.2
deportation	No	state	Republican	0.7	0.2	0.1
dreamers	No	state	Republican	0.5	0.4	0.1
highways	No	state	Republican	0.5	0.3	0.1
israel	No	state	Republican	0.2	0.6	0.2
medicare_for_all	No	state	Republican	0.3	0.6	0.1
military_size	No	state	Republican	0.1	0.8	0.1
occ_licensing	No	state	Republican	0.4	0.3	0.3
path_to_citizen	No	state	Republican	0.3	0.6	0.1
private_prisons	No	state	Republican	0.4	0.3	0.2
public_option	No	state	Republican	0.6	0.3	0.1
redistricting	No	state	Republican	0.4	0.3	0.3
saudi_weapons	No	state	Republican	0.7	0.2	0.1
state_pre_k	No	state	Republican	0.4	0.5	0.2
tariffs_china	No	state	Republican	0.7	0.2	0.2
tariffs_eu	No	state	Republican	0.4	0.3	0.2
teacher_pay	No	state	Republican	0.5	0.4	0.1
use_of_force	No	state	Republican	0.6	0.3	0.1
body_cameras	Yes	state	Democrat	0.9	0.1	0.0
charter_schools	Yes	state	Democrat	0.6	0.3	0.1
court_fees	Yes	state	Democrat	0.5	0.2	0.2
deportation	Yes	state	Democrat	0.3	0.6	0.1
dreamers	Yes	state	Democrat	0.8	0.1	0.1
highways	Yes	state	Democrat	0.6	0.3	0.1
israel	Yes	state	Democrat	0.4	0.3	0.3
medicare_for_all	Yes	state	Democrat	0.7	0.1	0.1
military_size	Yes	state	Democrat	0.3	0.5	0.2
occ_licensing	Yes	state	Democrat	0.4	0.3	0.3
path_to_citizen	Yes	state	Democrat	0.6	0.2	0.1
private_prisons	Yes	state	Democrat	0.6	0.2	0.2
public_option	Yes	state	Democrat	0.8	0.1	0.1
redistricting	Yes	state	Democrat	0.6	0.2	0.3
saudi_weapons	Yes	state	Democrat	0.7	0.1	0.2
state_pre_k	Yes	state	Democrat	0.7	0.2	0.1
tariffs_china	Yes	state	Democrat	0.5	0.2	0.2
tariffs_eu	Yes	state	Democrat	0.4	0.3	0.3
teacher_pay	Yes	state	Democrat	0.8	0.1	0.1
use_of_force	Yes	state	Democrat	0.7	0.2	0.1
body_cameras	Yes	state	Independent	0.7	0.1	0.2
charter_schools	Yes	state	Independent	0.3	0.2	0.5

court_fees	Yes	state	Independent	0.3	0.3	0.4
deportation	Yes	state	Independent	0.3	0.4	0.3
dreamers	Yes	state	Independent	0.6	0.2	0.2
highways	Yes	state	Independent	0.4	0.3	0.3
israel	Yes	state	Independent	0.3	0.3	0.4
medicare_for_all	Yes	state	Independent	0.5	0.2	0.3
military_size	Yes	state	Independent	0.1	0.5	0.3
occ_licensing	Yes	state	Independent	0.3	0.3	0.4
path_to_citizen	Yes	state	Independent	0.4	0.3	0.3
private_prisons	Yes	state	Independent	0.4	0.2	0.4
public_option	Yes	state	Independent	0.6	0.1	0.3
redistricting	Yes	state	Independent	0.3	0.1	0.6
saudi_weapons	Yes	state	Independent	0.6	0.1	0.3
state_pre_k	Yes	state	Independent	0.5	0.2	0.3
tariffs_china	Yes	state	Independent	0.4	0.2	0.4
tariffs_eu	Yes	state	Independent	0.2	0.3	0.5
teacher_pay	Yes	state	Independent	0.6	0.2	0.2
use_of_force	Yes	state	Independent	0.5	0.2	0.4
body_cameras	Yes	state	Republican	0.8	0.1	0.1
charter_schools	Yes	state	Republican	0.5	0.3	0.2
court_fees	Yes	state	Republican	0.4	0.4	0.2
deportation	Yes	state	Republican	0.7	0.2	0.1
dreamers	Yes	state	Republican	0.5	0.4	0.1
highways	Yes	state	Republican	0.5	0.4	0.1
israel	Yes	state	Republican	0.3	0.5	0.2
medicare_for_all	Yes	state	Republican	0.4	0.5	0.1
military_size	Yes	state	Republican	0.2	0.8	0.1
occ_licensing	Yes	state	Republican	0.4	0.3	0.3
path_to_citizen	Yes	state	Republican	0.3	0.5	0.1
private_prisons	Yes	state	Republican	0.4	0.3	0.3
public_option	Yes	state	Republican	0.6	0.3	0.1
redistricting	Yes	state	Republican	0.5	0.2	0.3
saudi_weapons	Yes	state	Republican	0.7	0.1	0.2
state_pre_k	Yes	state	Republican	0.4	0.4	0.1
tariffs_china	Yes	state	Republican	0.7	0.2	0.2
tariffs_eu	Yes	state	Republican	0.4	0.3	0.3
teacher_pay	Yes	state	Republican	0.6	0.3	0.1
use_of_force	Yes	state	Republican	0.6	0.2	0.2
affordable_house	No	municipal	Democrat	0.7	0.2	0.1
business_tax_break	No	municipal	Democrat	0.6	0.2	0.2
deportation	No	municipal	Democrat	0.2	0.6	0.2
dreamers	No	municipal	Democrat	0.8	0.1	0.1
height_restriction	No	municipal	Democrat	0.5	0.3	0.2
housing_loans	No	municipal	Democrat	0.7	0.1	0.1
israel	No	municipal	Democrat	0.4	0.3	0.3
medicare_for_all	No	municipal	Democrat	0.8	0.1	0.1
military_size	No	municipal	Democrat	0.3	0.5	0.2
parking	No	municipal	Democrat	0.4	0.4	0.3
path_to_citizen	No	municipal	Democrat	0.7	0.2	0.1
population_limit	No	municipal	Democrat	0.4	0.3	0.3
public_option	No	municipal	Democrat	0.8	0.1	0.1
public_safety	No	municipal	Democrat	0.5	0.3	0.2
public_transit	No	municipal	Democrat	0.6	0.2	0.2
sanctuary	No	municipal	Democrat	0.5	0.3	0.3

saudi_weapons	No	municipal	Democrat	0.7	0.1	0.2
tariffs_china	No	municipal	Democrat	0.5	0.2	0.3
tariffs_eu	No	municipal	Democrat	0.3	0.4	0.3
affordable_house	No	municipal	Independent	0.4	0.3	0.3
business_tax_break	No	municipal	Independent	0.5	0.2	0.3
deportation	No	municipal	Independent	0.4	0.4	0.3
dreamers	No	municipal	Independent	0.6	0.2	0.2
height_restriction	No	municipal	Independent	0.3	0.3	0.4
housing_loans	No	municipal	Independent	0.5	0.3	0.2
israel	No	municipal	Independent	0.2	0.4	0.4
medicare_for_all	No	municipal	Independent	0.5	0.3	0.2
military_size	No	municipal	Independent	0.1	0.6	0.3
parking	No	municipal	Independent	0.2	0.4	0.4
path_to_citizen	No	municipal	Independent	0.4	0.3	0.3
population_limit	No	municipal	Independent	0.3	0.4	0.3
public_option	No	municipal	Independent	0.6	0.2	0.3
public_safety	No	municipal	Independent	0.3	0.4	0.3
public_transit	No	municipal	Independent	0.4	0.3	0.3
sanctuary	No	municipal	Independent	0.2	0.4	0.4
saudi_weapons	No	municipal	Independent	0.5	0.2	0.3
tariffs_china	No	municipal	Independent	0.3	0.3	0.4
tariffs_eu	No	municipal	Independent	0.2	0.3	0.5
affordable_house	No	municipal	Republican	0.5	0.4	0.1
business_tax_break	No	municipal	Republican	0.7	0.2	0.1
deportation	No	municipal	Republican	0.7	0.2	0.1
dreamers	No	municipal	Republican	0.5	0.4	0.1
height_restriction	No	municipal	Republican	0.4	0.4	0.2
housing_loans	No	municipal	Republican	0.6	0.2	0.2
israel	No	municipal	Republican	0.3	0.6	0.2
medicare_for_all	No	municipal	Republican	0.4	0.5	0.1
military_size	No	municipal	Republican	0.1	0.8	0.1
parking	No	municipal	Republican	0.3	0.4	0.3
path_to_citizen	No	municipal	Republican	0.2	0.6	0.1
population_limit	No	municipal	Republican	0.5	0.4	0.2
public_option	No	municipal	Republican	0.6	0.3	0.1
public_safety	No	municipal	Republican	0.6	0.2	0.1
public_transit	No	municipal	Republican	0.4	0.4	0.2
sanctuary	No	municipal	Republican	0.2	0.6	0.2
saudi_weapons	No	municipal	Republican	0.7	0.1	0.2
tariffs_china	No	municipal	Republican	0.7	0.2	0.2
tariffs_eu	No	municipal	Republican	0.4	0.4	0.2
affordable_house	Yes	municipal	Democrat	0.7	0.2	0.1
business_tax_break	Yes	municipal	Democrat	0.6	0.2	0.2
deportation	Yes	municipal	Democrat	0.3	0.6	0.1
dreamers	Yes	municipal	Democrat	0.8	0.2	0.1
height_restriction	Yes	municipal	Democrat	0.5	0.3	0.2
housing_loans	Yes	municipal	Democrat	0.7	0.2	0.1
israel	Yes	municipal	Democrat	0.4	0.4	0.2
medicare_for_all	Yes	municipal	Democrat	0.8	0.1	0.1
military_size	Yes	municipal	Democrat	0.3	0.5	0.2
parking	Yes	municipal	Democrat	0.4	0.3	0.2
path_to_citizen	Yes	municipal	Democrat	0.7	0.2	0.1
population_limit	Yes	municipal	Democrat	0.4	0.4	0.2
public_option	Yes	municipal	Democrat	0.8	0.1	0.1

public_safety	Yes	municipal	Democrat	0.5	0.3	0.2
public_transit	Yes	municipal	Democrat	0.6	0.2	0.1
sanctuary	Yes	municipal	Democrat	0.4	0.3	0.2
saudi_weapons	Yes	municipal	Democrat	0.6	0.2	0.2
tariffs_china	Yes	municipal	Democrat	0.6	0.2	0.2
tariffs_eu	Yes	municipal	Democrat	0.3	0.4	0.3
affordable_house	Yes	municipal	Independent	0.4	0.2	0.4
business_tax_break	Yes	municipal	Independent	0.4	0.2	0.4
deportation	Yes	municipal	Independent	0.2	0.4	0.3
dreamers	Yes	municipal	Independent	0.5	0.2	0.3
height_restriction	Yes	municipal	Independent	0.3	0.4	0.4
housing_loans	Yes	municipal	Independent	0.5	0.2	0.3
israel	Yes	municipal	Independent	0.2	0.3	0.5
medicare_for_all	Yes	municipal	Independent	0.4	0.2	0.3
military_size	Yes	municipal	Independent	0.1	0.5	0.4
parking	Yes	municipal	Independent	0.2	0.3	0.5
path_to_citizen	Yes	municipal	Independent	0.3	0.3	0.4
population_limit	Yes	municipal	Independent	0.3	0.3	0.4
public_option	Yes	municipal	Independent	0.5	0.2	0.3
public_safety	Yes	municipal	Independent	0.3	0.3	0.5
public_transit	Yes	municipal	Independent	0.5	0.2	0.3
sanctuary	Yes	municipal	Independent	0.2	0.3	0.5
saudi_weapons	Yes	municipal	Independent	0.6	0.1	0.3
tariffs_china	Yes	municipal	Independent	0.4	0.2	0.4
tariffs_eu	Yes	municipal	Independent	0.1	0.3	0.6
affordable_house	Yes	municipal	Republican	0.5	0.3	0.2
business_tax_break	Yes	municipal	Republican	0.7	0.2	0.1
deportation	Yes	municipal	Republican	0.6	0.3	0.1
dreamers	Yes	municipal	Republican	0.5	0.3	0.1
height_restriction	Yes	municipal	Republican	0.4	0.4	0.2
housing_loans	Yes	municipal	Republican	0.6	0.2	0.1
israel	Yes	municipal	Republican	0.3	0.5	0.2
medicare_for_all	Yes	municipal	Republican	0.4	0.4	0.1
military_size	Yes	municipal	Republican	0.1	0.8	0.1
parking	Yes	municipal	Republican	0.4	0.4	0.2
path_to_citizen	Yes	municipal	Republican	0.4	0.5	0.1
population_limit	Yes	municipal	Republican	0.4	0.4	0.2
public_option	Yes	municipal	Republican	0.6	0.3	0.1
public_safety	Yes	municipal	Republican	0.6	0.3	0.2
public_transit	Yes	municipal	Republican	0.5	0.4	0.2
sanctuary	Yes	municipal	Republican	0.2	0.7	0.1
saudi_weapons	Yes	municipal	Republican	0.6	0.2	0.2
tariffs_china	Yes	municipal	Republican	0.7	0.2	0.2
tariffs_eu	Yes	municipal	Republican	0.4	0.4	0.2

4.3.1.2 Pooled

Below we list the weighted proportion of respondents indicating they agree, disagree, or “Don’t Know” in response to being asked the affirmative setting of each of the policies which were incor-

porated into the conjoints. Responses are broken down by interaction of *partisan label condition* and *government level condition*, but pooled across waves of our study.

Table 4.8: Policy Agreement/Disagreement Pooled

Policy	PID	Agree	Disagree	DK
affordable_house	Democrat	0.7	0.2	0.1
body_cameras	Democrat	0.8	0.1	0.1
business_tax_break	Democrat	0.6	0.2	0.2
charter_schools	Democrat	0.5	0.3	0.2
court_fees	Democrat	0.5	0.2	0.2
deportation	Democrat	0.3	0.6	0.1
dreamers	Democrat	0.8	0.1	0.1
height_restriction	Democrat	0.5	0.3	0.2
highways	Democrat	0.6	0.3	0.1
housing_loans	Democrat	0.7	0.2	0.1
israel	Democrat	0.4	0.3	0.3
medicare_for_all	Democrat	0.8	0.1	0.1
military_size	Democrat	0.3	0.5	0.2
occ_licensing	Democrat	0.4	0.3	0.3
parking	Democrat	0.4	0.3	0.3
path_to_citizen	Democrat	0.7	0.2	0.1
population_limit	Democrat	0.4	0.4	0.2
private_prisons	Democrat	0.7	0.2	0.2
public_option	Democrat	0.8	0.1	0.1
public_safety	Democrat	0.5	0.3	0.2
public_transit	Democrat	0.6	0.2	0.1
redistricting	Democrat	0.6	0.1	0.3
sanctuary	Democrat	0.5	0.3	0.3
saudi_weapons	Democrat	0.7	0.1	0.2
state_pre_k	Democrat	0.7	0.2	0.1
tariffs_china	Democrat	0.5	0.2	0.2
tariffs_eu	Democrat	0.3	0.4	0.3
teacher_pay	Democrat	0.8	0.1	0.1
use_of_force	Democrat	0.6	0.2	0.1
affordable_house	Independent	0.4	0.3	0.3
body_cameras	Independent	0.7	0.1	0.2
business_tax_break	Independent	0.5	0.2	0.3
charter_schools	Independent	0.4	0.2	0.4
court_fees	Independent	0.4	0.3	0.3
deportation	Independent	0.3	0.4	0.3
dreamers	Independent	0.5	0.2	0.3
height_restriction	Independent	0.3	0.3	0.4
highways	Independent	0.4	0.3	0.3
housing_loans	Independent	0.5	0.2	0.3
israel	Independent	0.3	0.3	0.4
medicare_for_all	Independent	0.5	0.2	0.3
military_size	Independent	0.1	0.5	0.3

occ_licensing	Independent	0.3	0.3	0.4
parking	Independent	0.2	0.4	0.4
path_to_citizen	Independent	0.4	0.3	0.3
population_limit	Independent	0.3	0.4	0.4
private_prisons	Independent	0.4	0.2	0.4
public_option	Independent	0.6	0.2	0.3
public_safety	Independent	0.3	0.3	0.4
public_transit	Independent	0.4	0.3	0.3
redistricting	Independent	0.3	0.2	0.5
sanctuary	Independent	0.2	0.4	0.4
saudi_weapons	Independent	0.6	0.1	0.3
state_pre_k	Independent	0.5	0.2	0.3
tariffs_china	Independent	0.4	0.2	0.4
tariffs_eu	Independent	0.2	0.3	0.5
teacher_pay	Independent	0.6	0.1	0.2
use_of_force	Independent	0.5	0.2	0.3
affordable_house	Republican	0.5	0.3	0.1
body_cameras	Republican	0.8	0.2	0.1
business_tax_break	Republican	0.7	0.2	0.1
charter_schools	Republican	0.5	0.3	0.2
court_fees	Republican	0.4	0.4	0.2
deportation	Republican	0.7	0.2	0.1
dreamers	Republican	0.5	0.4	0.1
height_restriction	Republican	0.4	0.4	0.2
highways	Republican	0.5	0.4	0.1
housing_loans	Republican	0.6	0.2	0.2
israel	Republican	0.2	0.6	0.2
medicare_for_all	Republican	0.4	0.5	0.1
military_size	Republican	0.1	0.8	0.1
occ_licensing	Republican	0.4	0.3	0.3
parking	Republican	0.4	0.4	0.2
path_to_citizen	Republican	0.3	0.6	0.1
population_limit	Republican	0.4	0.4	0.2
private_prisons	Republican	0.4	0.3	0.2
public_option	Republican	0.6	0.3	0.1
public_safety	Republican	0.6	0.3	0.1
public_transit	Republican	0.4	0.4	0.2
redistricting	Republican	0.5	0.3	0.3
sanctuary	Republican	0.2	0.6	0.2
saudi_weapons	Republican	0.7	0.2	0.2
state_pre_k	Republican	0.4	0.4	0.1
tariffs_china	Republican	0.7	0.2	0.2
tariffs_eu	Republican	0.4	0.4	0.2
teacher_pay	Republican	0.5	0.3	0.1
use_of_force	Republican	0.6	0.2	0.1

4.3.2 Unconditional AMCE

Below we show the unconditional AMCE results. As we discuss in the main paper, our preferred results condition on a candidate's position according with a respondent's, rather than the raw position in the affirmative or the negative on the policy as stated. We report the results in the more classical manner (affirmative vs. negative) here, but caution that policies which high a bimodal preference profile, but are nevertheless highly salient, create cross-cutting negative and positive effects which can cancel out. Thus, we provide these unconditional effects primarily in the interest of transparency.

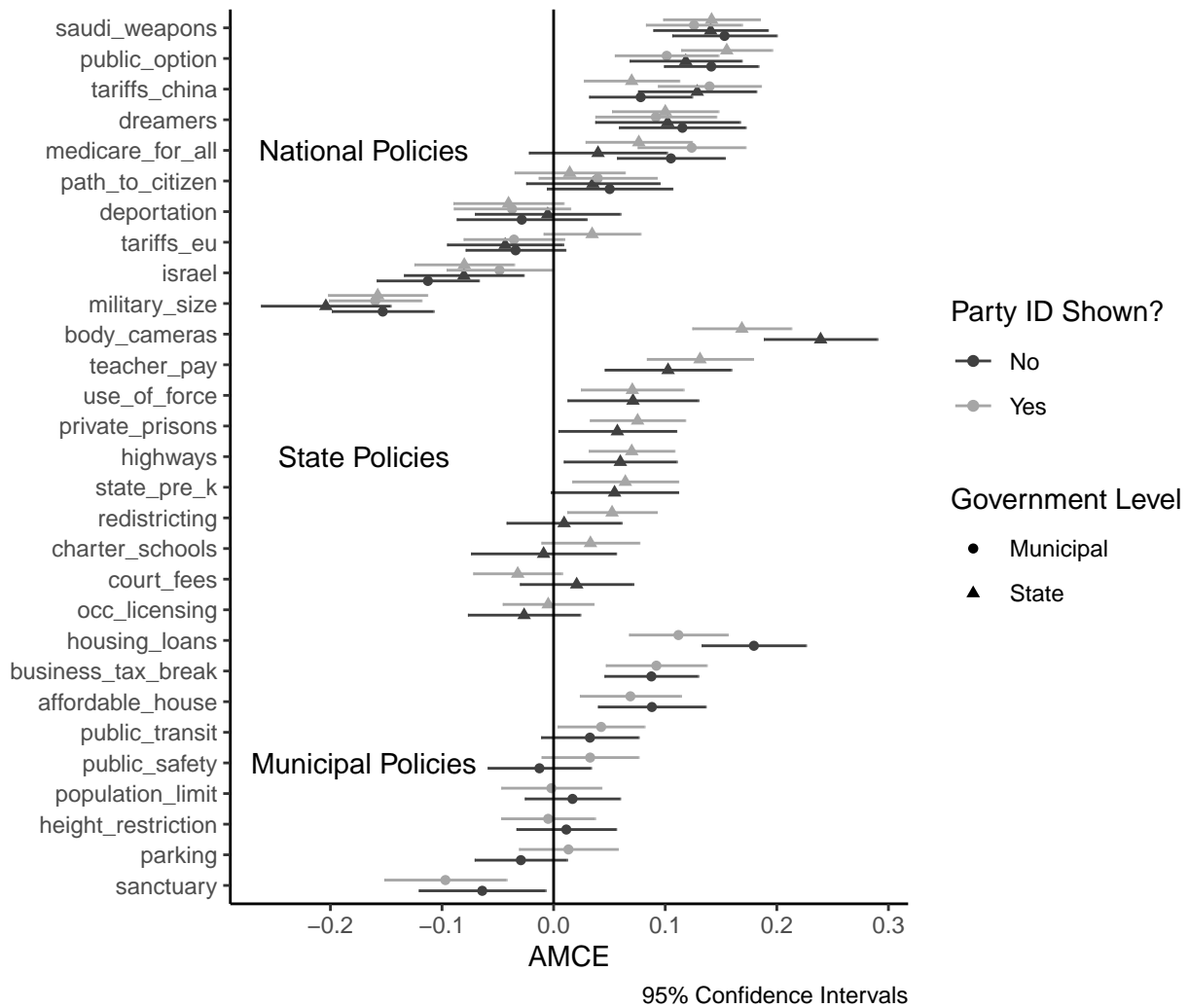
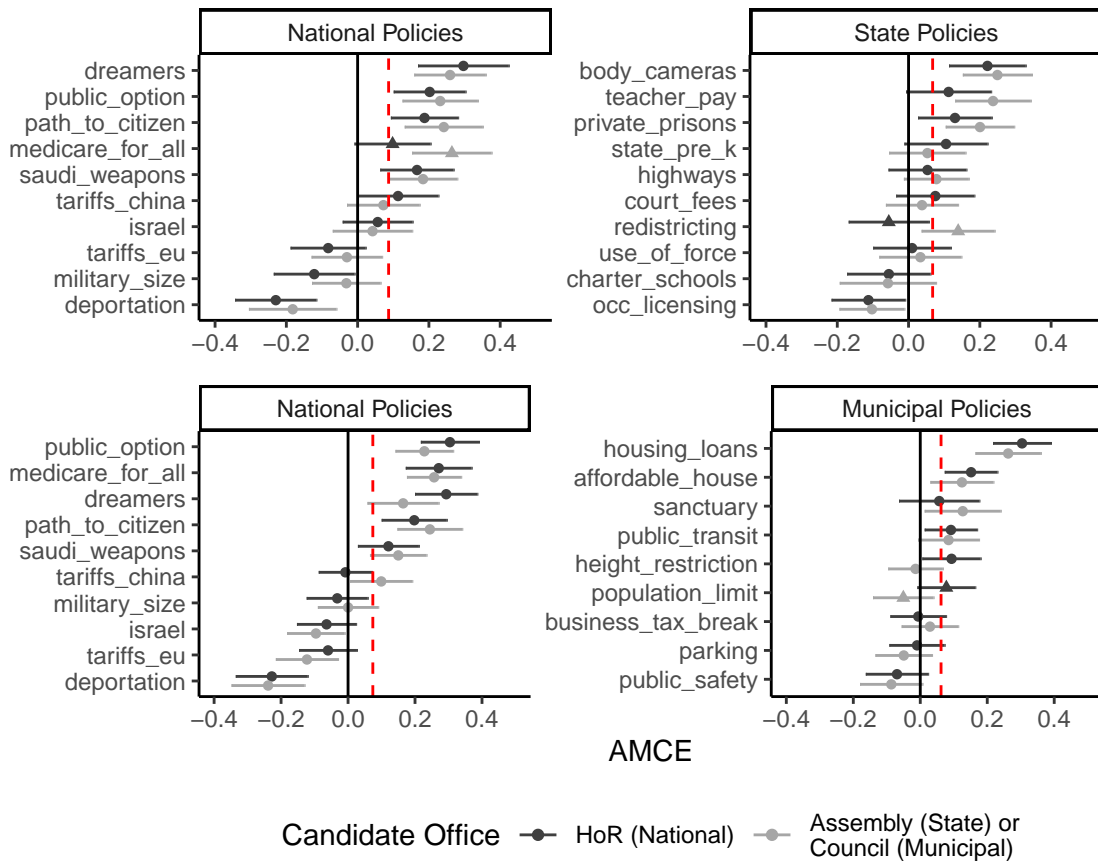


Figure 4.11: AMCE by Party Condition

4.3.3 AMCE conditional on other features

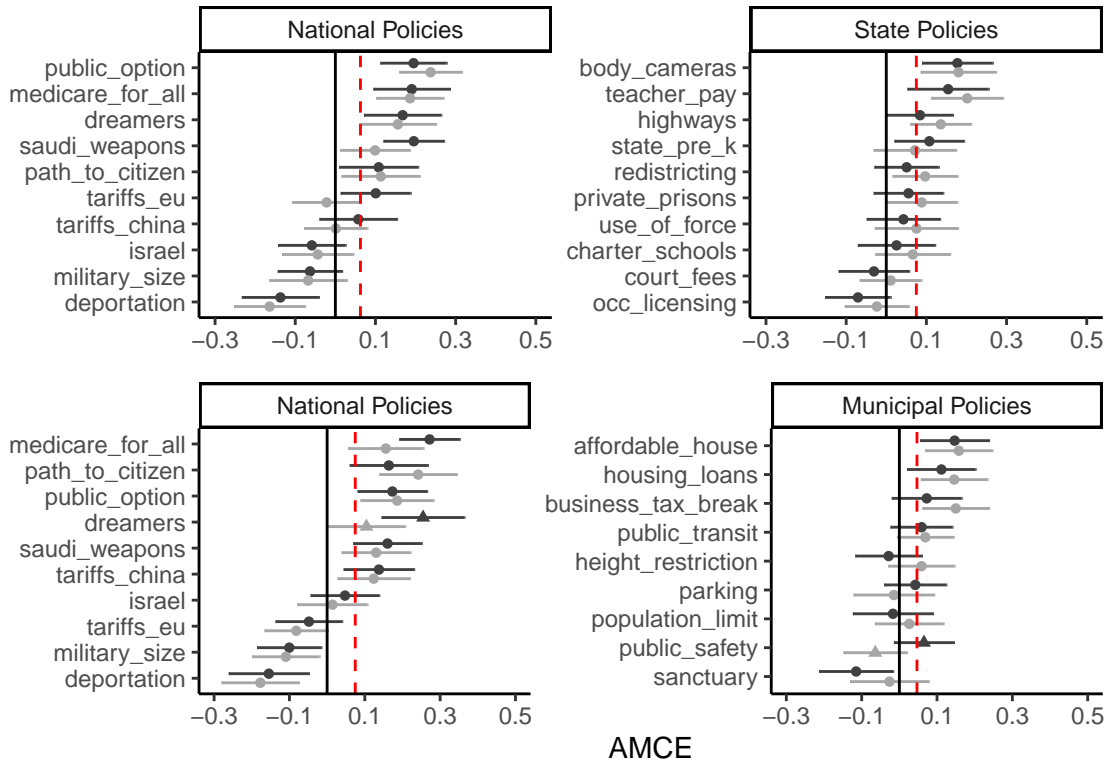
4.3.3.1 Party ID

In this section we investigate the AMCE of each policy while subsetting only to respondents with a particular party ID. In effect, this design deals with the concern raised by bimodal preference profiles by recognizing that most bimodal preference profiles emerge when the Democratic and Republican parties have opposite preferences. We present two plots for each party: in order plots for the non-partisan label and partisan label conditions for each of Democrats, Republicans, and Independents.



Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

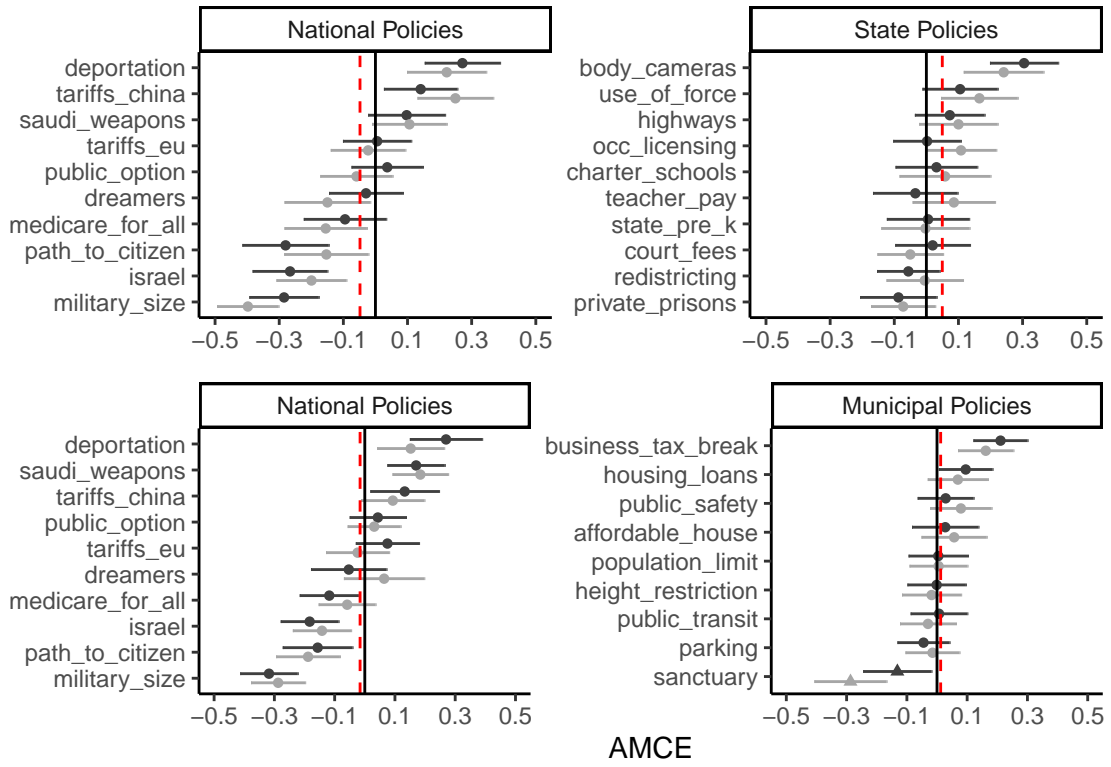
Figure 4.12: AMCE for Democratic Respondents, No Party Label



Candidate Office ● HoR (National) ● Assembly (State) or Council (Municipal)

Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

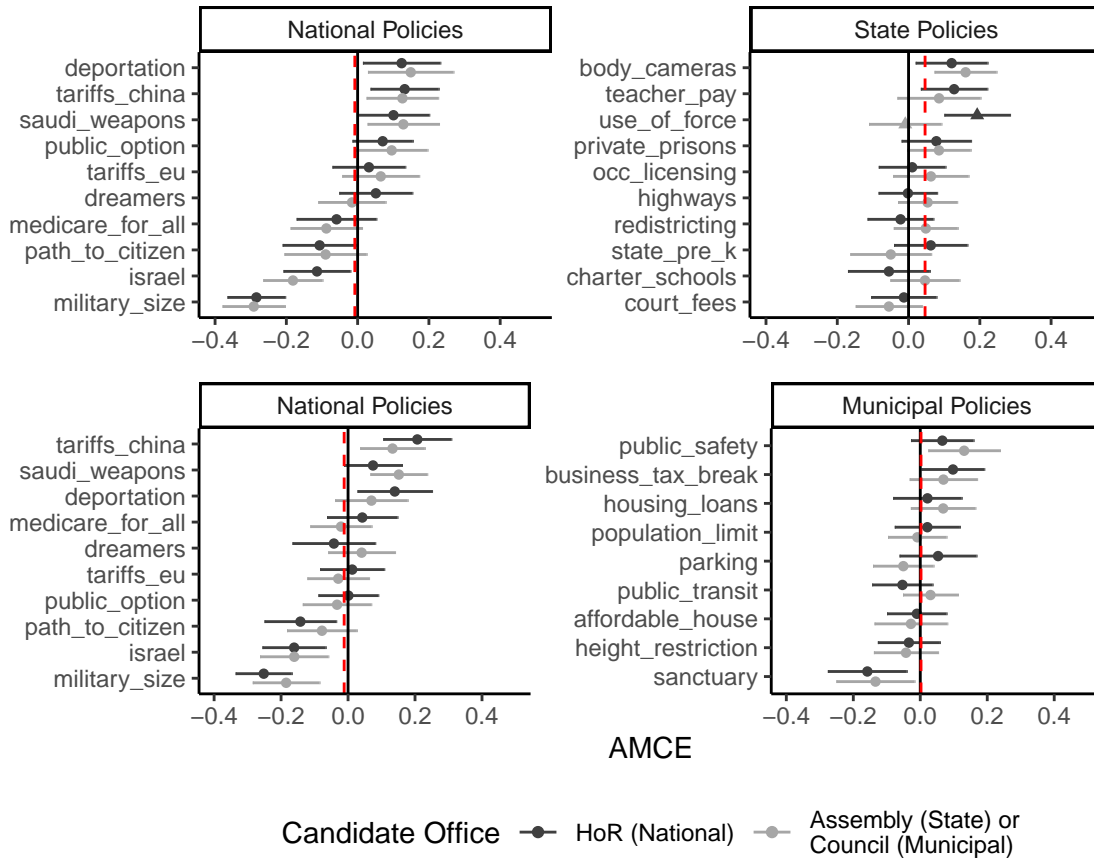
Figure 4.13: AMCE for Democratic Respondents, Party Label



Candidate Office ● HoR (National) ● Assembly (State) or Council (Municipal)

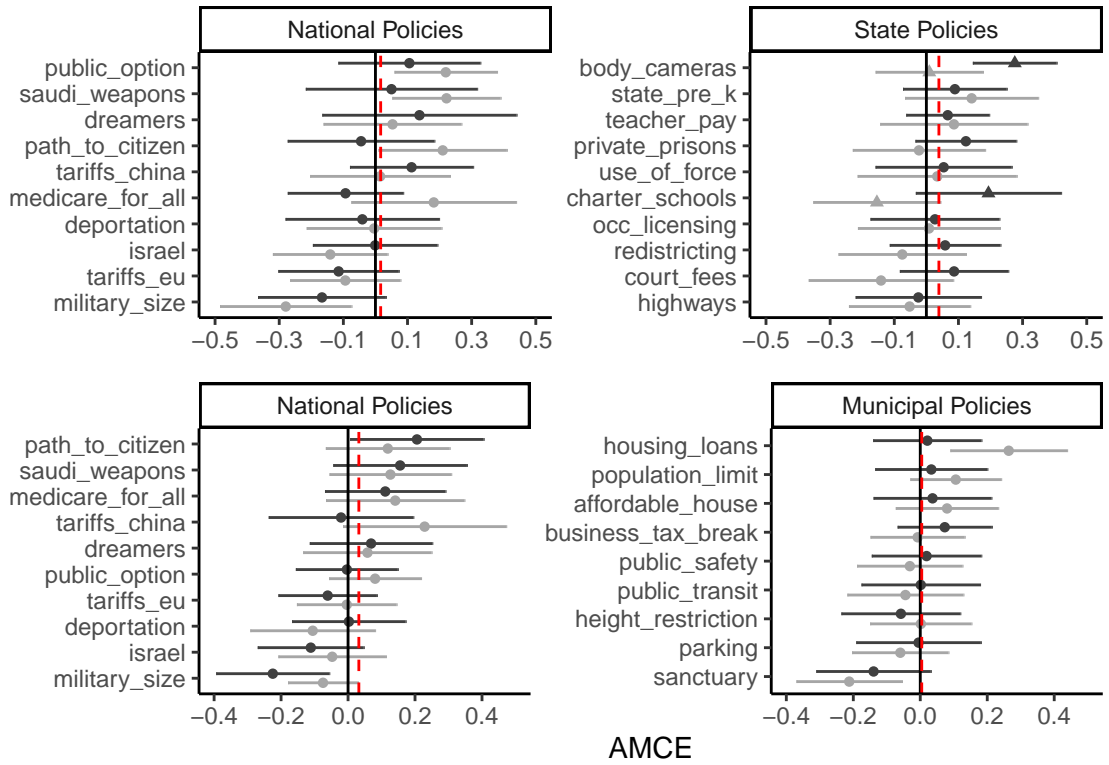
Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.14: AMCE for Republican Respondents, No Party Label



Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

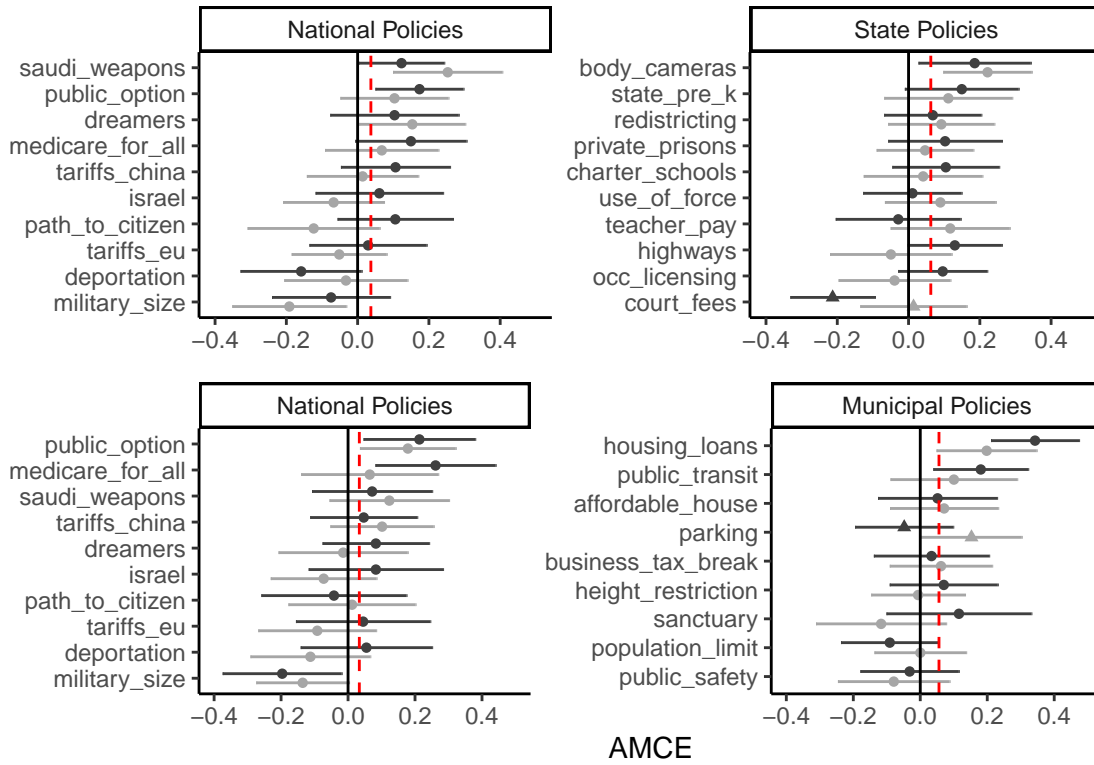
Figure 4.15: AMCE for Republican Respondents, Party Label



Candidate Office ● HoR (National) ● Assembly (State) or Council (Municipal)

Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.16: AMCE for Independent Respondents, No Party Label



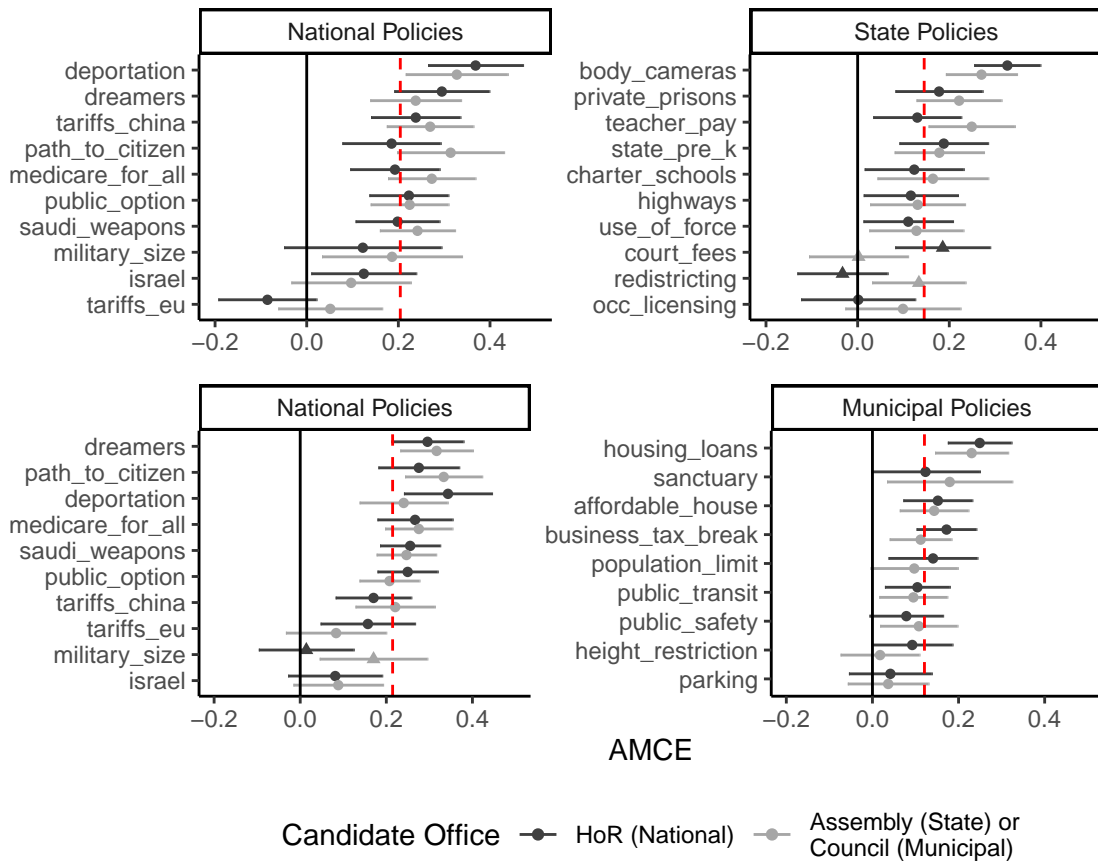
Candidate Office ● HoR (National) ● Assembly (State) or Council (Municipal)

Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.17: AMCE for Independent Respondents, Party Label

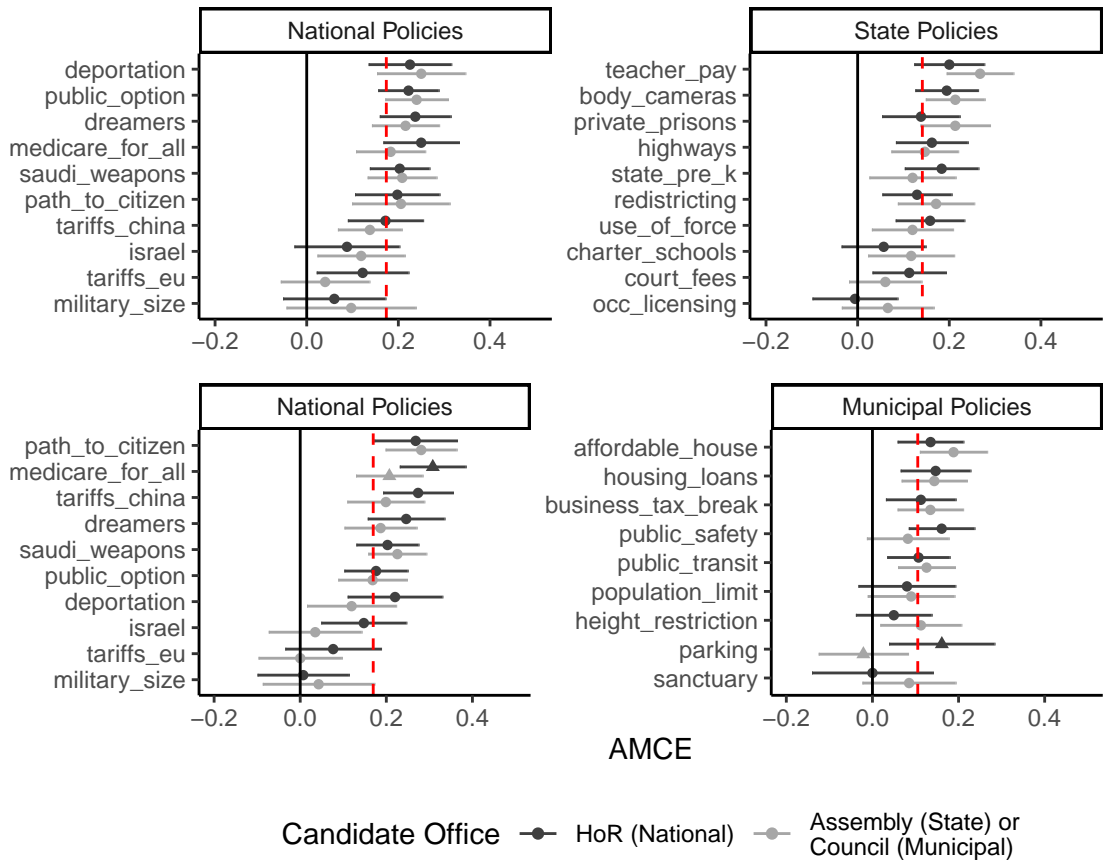
4.3.3.2 Agreement and Disagreement

Instead of conditioning AMCE based on whether respondents agree with the setting shown in the conjoint, here we show the AMCE conditional on agreeing or disagreeing with the positive setting of the policy (which can be either conservative or liberal in ideological terms).



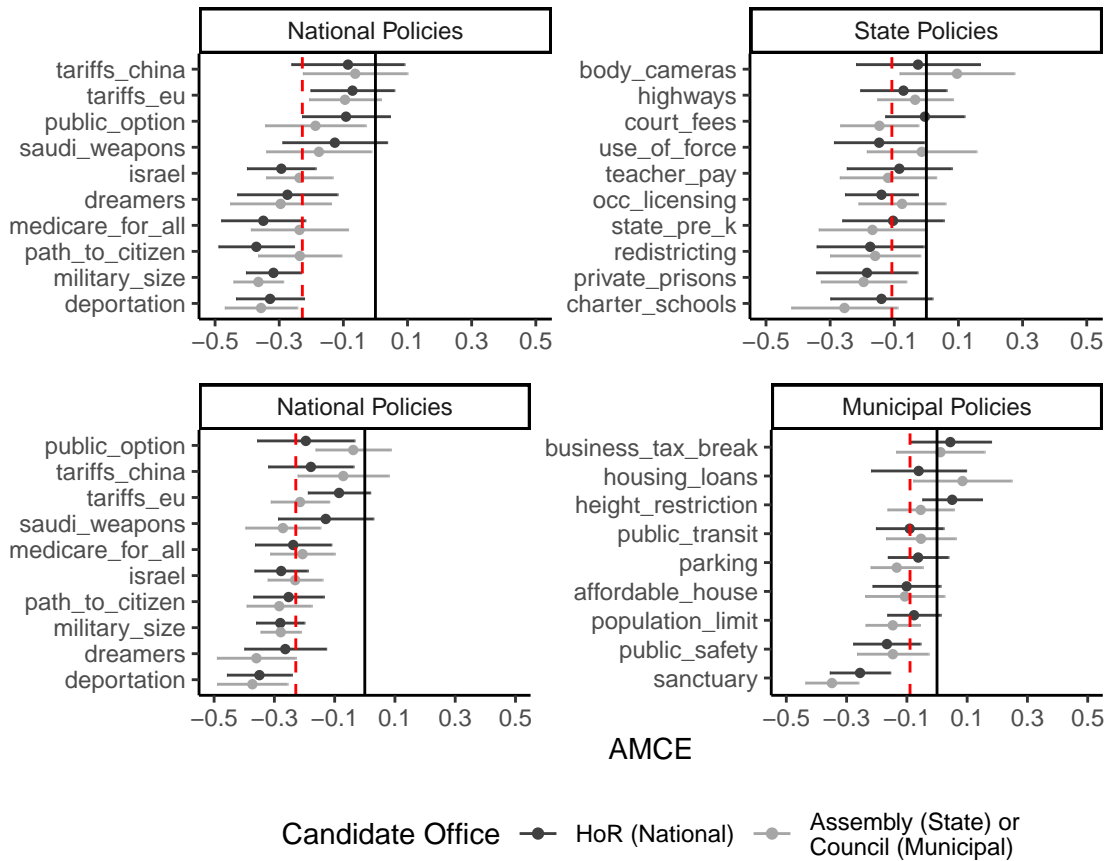
Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.18: AMCE Conditional on Policy Agreement, No Party Label



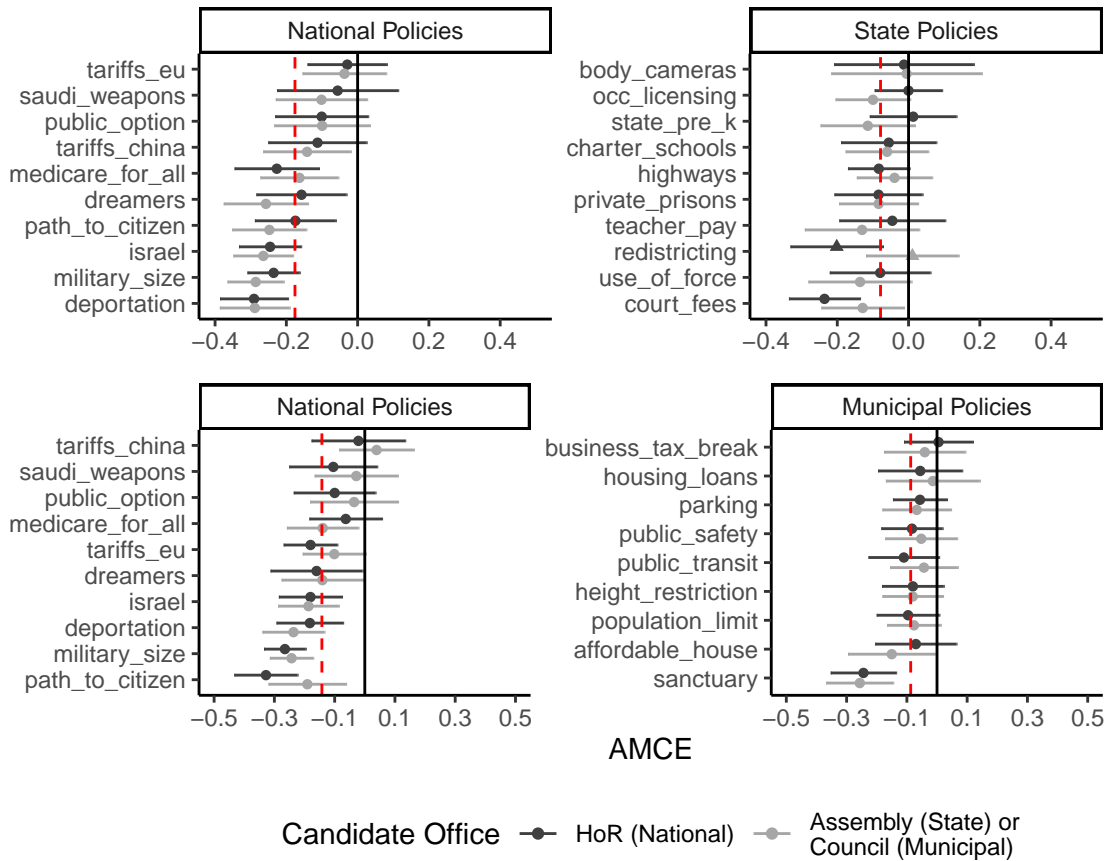
Triangles denote statistically significant (alpha = .05) difference between office conditions

Figure 4.19: AMCE Conditional on Policy Agreement, Party Label



Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.20: AMCE Conditional on Policy Disagreement, No Party Label



Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.21: AMCE Conditional on Policy Disagreement, Party Label

4.3.4 Partisan Attenuation of Policy Effects

In this section we assess how the magnitude of AMCEs change in in the partisan label conditions that reveal a party label to the respondent. In effect, if showing a party label attenuates the effect of the policy, then the policy is serving primarily as a partisan cue for the respondent rather than independent information.

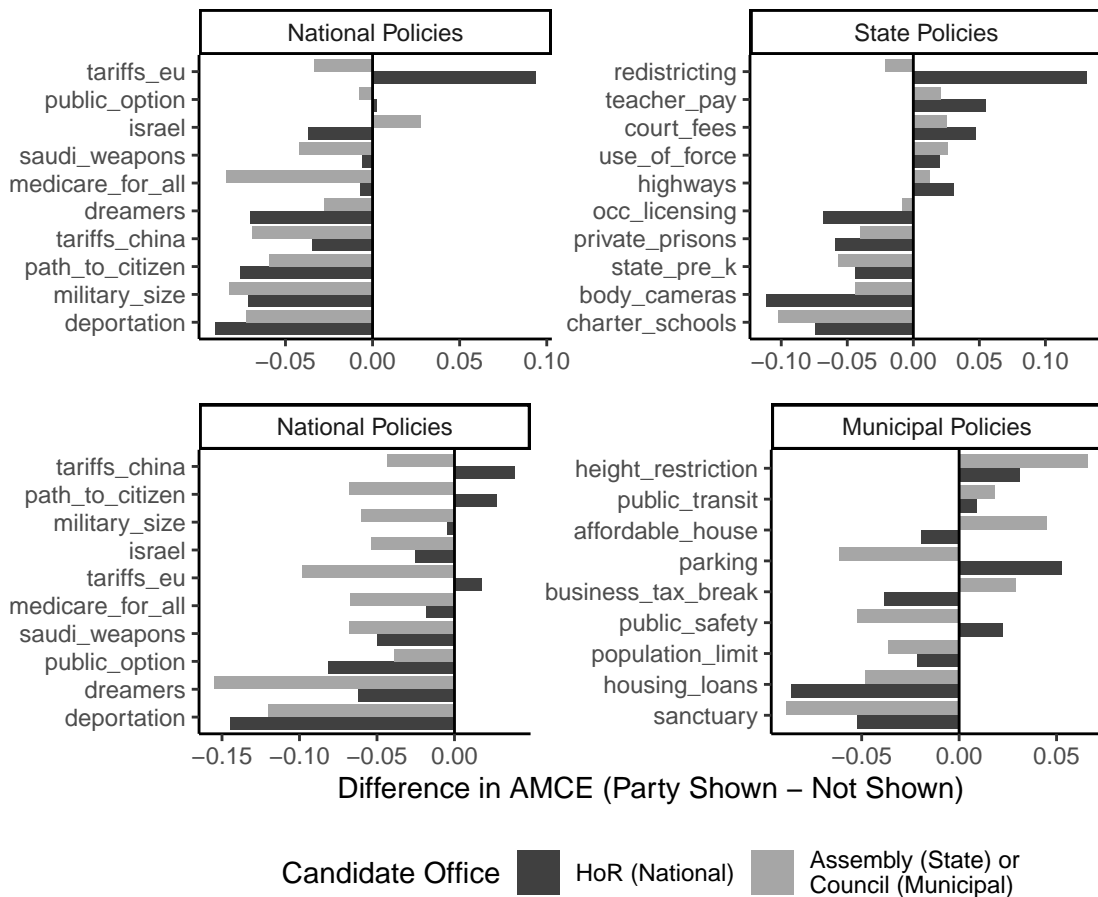


Figure 4.22: Change in AMCE with Party Label Inclusion

4.3.5 Partisan Signal Intensity

4.3.5.1 Pooled Results

Here we present a version of Figure 3.4 where the partisan signal intensity is pooled across conditions, resulting in one estimate per policy rather than one estimate per policy-condition. The results are substantively similar to our preferred specification.

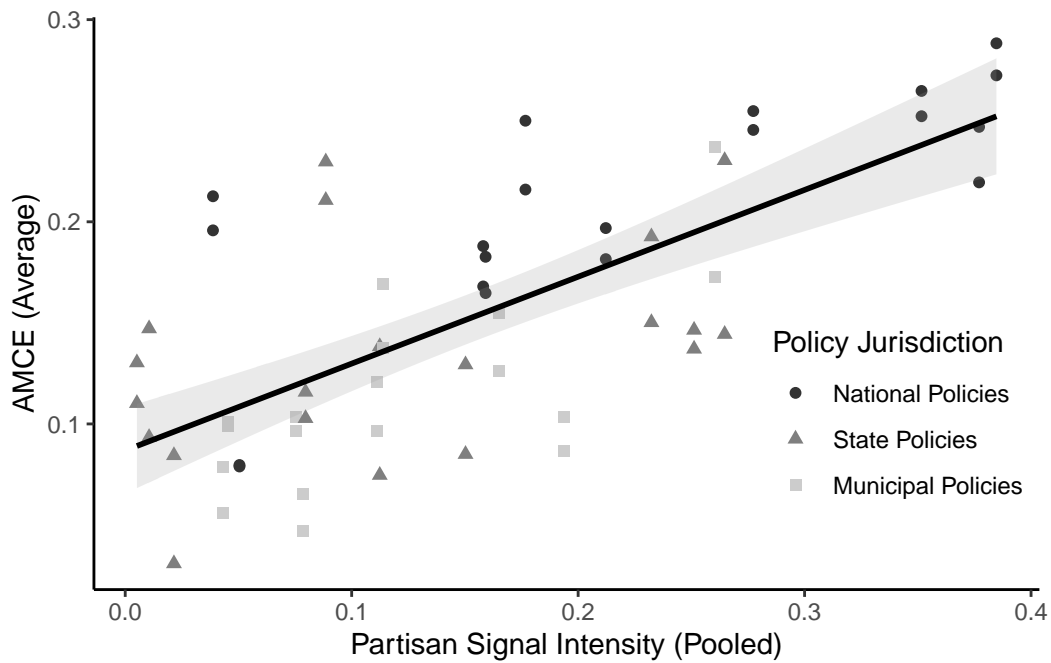


Figure 4.23: AMCE versus Partisan Signal Intensity, Pooled

4.3.5.2 Results by Level and Partisan Conditions

Table 4.9: Effect of Partisan Signal Intensity on AMCE, by Party and Level Conditions

PID Shown?	Gov. Level Condition	Policy Type	Slope Est.	Std. Error	p-val
No	state	National Policies	0.4471	0.1172	0.0013
No	state	State Policies	0.1314	0.1377	0.3524
No	municipal	National Policies	0.3394	0.0720	0.0002
No	municipal	Municipal Policies	0.5263	0.1488	0.0027
Yes	state	National Policies	0.3008	0.0739	0.0007
Yes	state	State Policies	0.3407	0.1266	0.0149
Yes	municipal	National Policies	0.2651	0.0865	0.0067
Yes	municipal	Municipal Policies	0.3732	0.1132	0.0046

4.3.6 Weighting

4.3.6.1 Population Target Weights

LUCID Theorem, our sample provider, uses quota sampling (a non-random sampling procedure) to construct a sample whose marginal attributes match a target population. We further weight respondents to approximate a nationally representative sample (both to eliminate chance imbalances in LUCID’s quota filling and to incorporate targets that cannot be supplied as a quota via LUCID’s interface). We weight respondents using iterative proportional fitting (raking) (Rudkin 2022). Our population target is the one used in the UCLA + Democracy Fund Nationscape survey, which is based on 2017 ACS 5-year data. These targets in combination with LUCID’s sample have been shown to perform comparably to Pew and other national surveys (Holliday et al. 2021). The survey targets are reproduced below, along with a histogram of actual assigned weights.

The choice to adopt such a robust set of weight targets presents a bias-variance tradeoff. In general, including fewer population targets will increase bias (because the resulting sample will differ systematically from the population of interest) while decreasing variance (because weights will be less extreme). The design effect (degree to which variance is inflated) of our the chosen weighting scheme is 2.44 (Kish 1995). A visual diagnostic of extreme weights can be found in Figure 4.24. We offer versions of our main result with unweighted respondents in Figure 4.25 and note that our findings are unchanged.

Table 4.10: Respondent Weight Assignment Targets

variable	level	proportion
gender	Male	0.48
gender	Female	0.52
region	Midwest	0.21
region	Northeast	0.18
region	South	0.38
region	West	0.24
hispanic	Not Hispanic	0.84
hispanic	Mexican	0.10
hispanic	Other Hispanic	0.06
race	White	0.74
race	Black	0.12
race	AAPI	0.07
race	Other race	0.07
household_income	\$19,999 or less	0.11
household_income	\$20,000-\$34,999	0.12
household_income	\$35,000-\$49,999	0.12
household_income	\$50,000-\$64,999	0.11
household_income	\$65,000-\$79,999	0.10
household_income	\$80,000-\$99,999	0.11
household_income	\$100,000-\$124,999	0.10
household_income	\$125,000-\$199,999	0.15
household_income	\$200,000 and above	0.09
education	No high school diploma	0.12
education	High school diploma	0.27
education	Some college	0.22
education	Associate's degree	0.08
education	Bachelor's degree	0.19
education	Graduate degree	0.11
age	18-23	0.10
age	24-29	0.11
age	30-39	0.17
age	40-49	0.16
age	50-59	0.17
age	60-69	0.15
age	70+	0.13
education_x_gender	Associate's degree x Female	0.05
education_x_gender	Associate's degree x Male	0.04
education_x_gender	Bachelor's degree x Female	0.10
education_x_gender	Bachelor's degree x Male	0.09
education_x_gender	Graduate degree x Female	0.06
education_x_gender	Graduate degree x Male	0.05
education_x_gender	High school diploma x Female	0.14
education_x_gender	High school diploma x Male	0.14
education_x_gender	No high school diploma x Female	0.06
education_x_gender	No high school diploma x Male	0.06
education_x_gender	Some college x Female	0.12
education_x_gender	Some college x Male	0.11

gender_x_race	Female x AAPI	0.04
gender_x_race	Female x Black	0.07
gender_x_race	Female x Other race	0.03
gender_x_race	Female x White	0.38
gender_x_race	Male x AAPI	0.03
gender_x_race	Male x Black	0.05
gender_x_race	Male x Other race	0.03
gender_x_race	Male x White	0.36
race_x_hispanic	AAPI x Mexican	0.00
race_x_hispanic	AAPI x Not Hispanic	0.07
race_x_hispanic	AAPI x Other Hispanic	0.00
race_x_hispanic	Black x Mexican	0.00
race_x_hispanic	Black x Not Hispanic	0.12
race_x_hispanic	Black x Other Hispanic	0.00
race_x_hispanic	Other race x Mexican	0.03
race_x_hispanic	Other race x Not Hispanic	0.02
race_x_hispanic	Other race x Other Hispanic	0.02
race_x_hispanic	White x Mexican	0.06
race_x_hispanic	White x Not Hispanic	0.64
race_x_hispanic	White x Other Hispanic	0.04
race_x_education	AAPI x Associate's degree	0.00
race_x_education	AAPI x Bachelor's degree	0.02
race_x_education	AAPI x Graduate degree	0.01
race_x_education	AAPI x High school diploma	0.01
race_x_education	AAPI x No high school diploma	0.01
race_x_education	AAPI x Some college	0.01
race_x_education	Black x Associate's degree	0.01
race_x_education	Black x Bachelor's degree	0.02
race_x_education	Black x Graduate degree	0.01
race_x_education	Black x High school diploma	0.04
race_x_education	Black x No high school diploma	0.02
race_x_education	Black x Some college	0.03
race_x_education	Other race x Associate's degree	0.00
race_x_education	Other race x Bachelor's degree	0.01
race_x_education	Other race x Graduate degree	0.00
race_x_education	Other race x High school diploma	0.02
race_x_education	Other race x No high school diploma	0.02
race_x_education	Other race x Some college	0.02
race_x_education	White x Associate's degree	0.06
race_x_education	White x Bachelor's degree	0.15
race_x_education	White x Graduate degree	0.09
race_x_education	White x High school diploma	0.20
race_x_education	White x No high school diploma	0.08
race_x_education	White x Some college	0.16
hispanic_x_education	Mexican x Associate's degree	0.01
hispanic_x_education	Mexican x Bachelor's degree	0.01
hispanic_x_education	Mexican x Graduate degree	0.00
hispanic_x_education	Mexican x High school diploma	0.03
hispanic_x_education	Mexican x No high school diploma	0.03
hispanic_x_education	Mexican x Some college	0.02
hispanic_x_education	Not Hispanic x Associate's degree	0.07
hispanic_x_education	Not Hispanic x Bachelor's degree	0.17
hispanic_x_education	Not Hispanic x Graduate degree	0.10
hispanic_x_education	Not Hispanic x High school diploma	0.23

hispanic_x_education	Not Hispanic x No high school diploma	0.07
hispanic_x_education	Not Hispanic x Some college	0.19
hispanic_x_education	Other Hispanic x Associate's degree	0.00
hispanic_x_education	Other Hispanic x Bachelor's degree	0.01
hispanic_x_education	Other Hispanic x Graduate degree	0.00
hispanic_x_education	Other Hispanic x High school diploma	0.02
hispanic_x_education	Other Hispanic x No high school diploma	0.01
hispanic_x_education	Other Hispanic x Some college	0.01

4.3.6.2 Distribution of Respondent Weights

Our raked weights are constrained such that the average weight is 1 and the maximum respondent weight is 5. As a result, respondents whose initial inclusion probability is highly divergent from population targets can be forced to high (near-or-at 5) or low (< 0.01) weights.

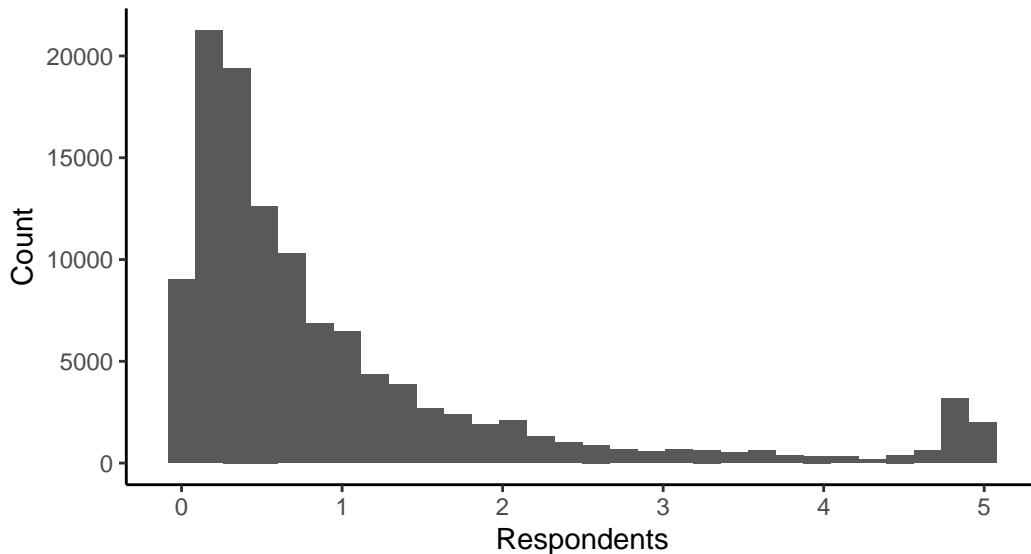


Figure 4.24: Respondent Weight Assignments

4.3.6.3 Inferential Impact of Weighting Decisions

To what extent, if any, do our results depend on the population targets and methodological choices described above? Hardly at all. Our primary results describe the AMCE of agreeing with *a candidate's randomly assigned position* on vote choice, not on having a particular position. We do not require that both sides of an issue be precisely or accurately measured, only that the error in mea-

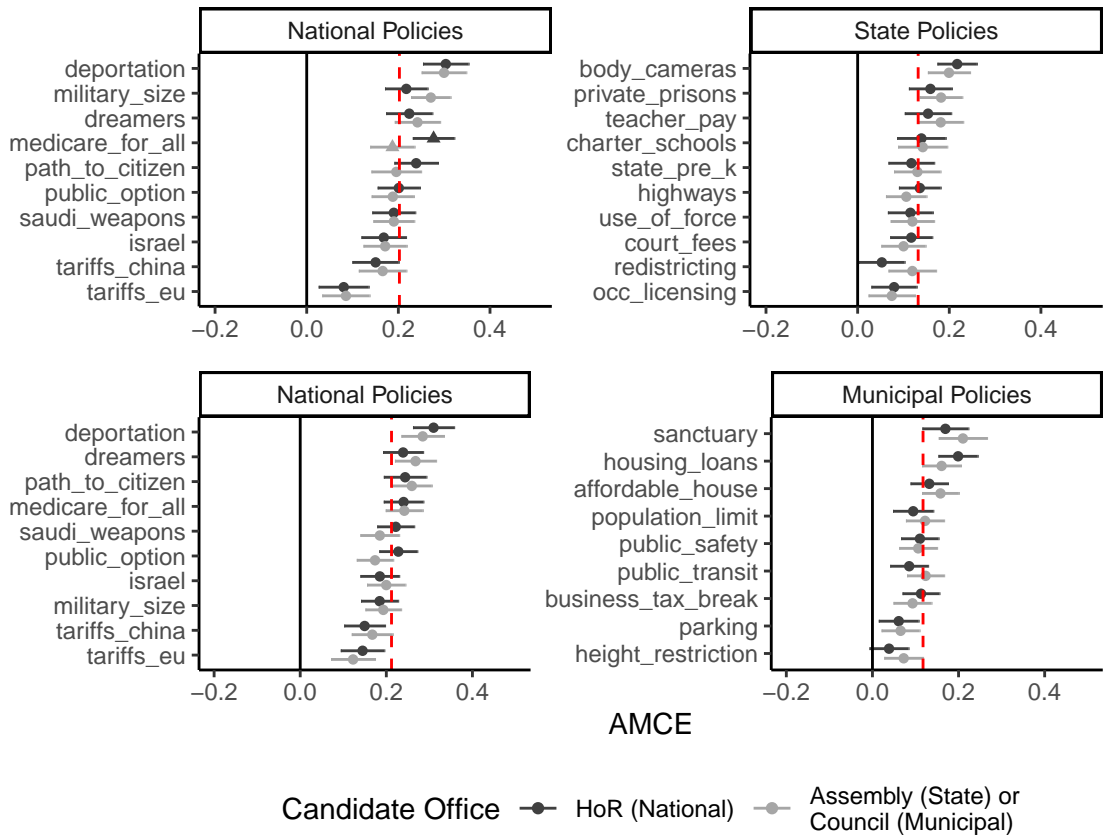
asuring raw support does not correlate with a respondent's tendency toward nationalization. Confounding would exist if, say, opponents of a pathway to citizenship for undocumented immigrants nationalized the issue more than proponents and their weights were mis-estimated in a way that correlates with their opposition or support. Doubtless there are such imbalances across our sample and the population at large, but we have no *a priori* belief about which direction the asymmetry cuts. Even then, our findings do not rely on any one assigned issue: we would only be vulnerable if national, state, or municipal issues were systematically mis-estimated in this manner. Were that so, an overestimation of national issue salience would exaggerate nationalization hypotheses, and an overestimation of state or local issue salience would attenuate nationalization hypotheses.

Our results which rely on partisan signal intensity, likewise, do not depend on accurate weighting across political parties, only within: to the extent Democrats are weighted too highly in the overall sample but intra-Democratic weights are correct relative to one another, the estimate of the gap between parties will remain accurate because we depend only on relative divisiveness of issues between parties.

Finally, our results place no significance on the exact size of particular AMCEs. As we note in the main text, the exact effect sizes are a function of our design because AMCEs are constrained by the joint distribution of all other AMCEs and the number of attributes varied in the conjoint, since exactly half of all candidates are selected and exactly half are rejected. Rather, our results depend primarily on ordinal comparisons between types of policies.

4.3.6.4 Key Result, Unweighted

In this section we provide versions of our key results estimated without survey weights. Because our sample is still a product of a quota-based sampling process, sample characteristics remain close to the target population. We observe that the substantive and statistical significance of the results are unchanged; the results also hold using a reduced set of weight targets which exclude interaction terms (omitted here for brevity).



Candidate Office ● HoR (National) ● Assembly (State) or Council (Municipal)

Triangles denote statistically significant ($\alpha = .05$) difference between office conditions
 Red lines represent mean AMCE

Figure 4.25: Unweighted version of main AMCE result

4.3.7 Conjoint Believability

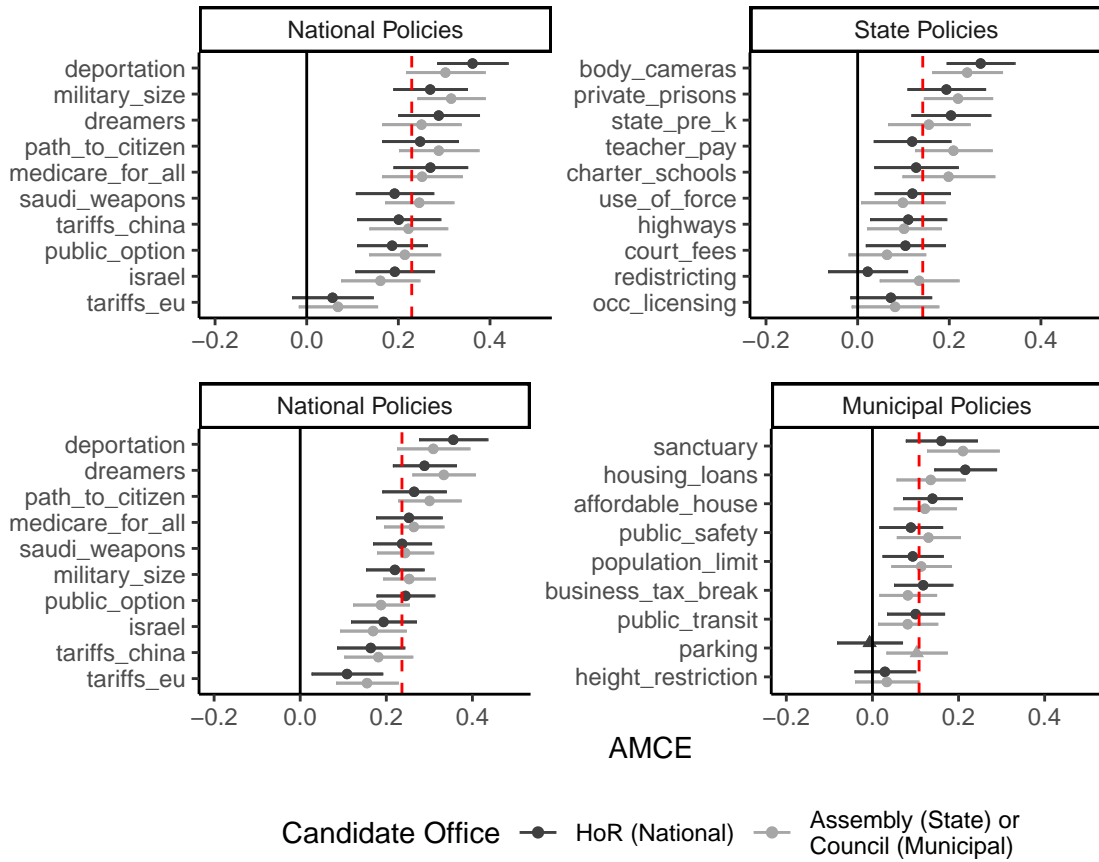
One concern with conjoint designs is the randomization of attributes creates improbable choice profiles for respondents, which are weighted equally with more externally valid choice profiles (de la Cuesta, Egami, and Imai 2022). While we mitigate the threat of *impossible* combination of policies by restricting randomization such that only one policy per policy area is shown in a choice profile, the improbability of certain attribute combinations poses a threat to our external validity.

We assuage external validity concerns in two ways. First, in the non-partisan condition, we eliminate conjoints where policy settings are preferred by opposite partisan majorities (e.g. where a candidate has one policy position supported by Republicans and not Democrats, and another policy position supported by Democrats and not Republicans) and where those policies are a point of disagreement between candidates. We call this “policy coherence” and explore it in Figure 4.26. We measure the partisan leaning of a policy using the reported policy preferences of our full sample.

Second, in waves where the partisan label condition is assigned to show the party ID of the candidate, we eliminate conjoints where the partisan identification of the candidate conflicts with one of the candidate’s preferred policy positions; in effect, demanding that all presented candidates have policy positions that reflect their real-world party’s preferences. We call this partisan coherence and explore it in Figure 4.27.

The results remain substantively identical to the main results presented in the paper, though the sharply reduced sample size reduces our statistical power considerably.

4.3.7.1 Policy Coherence



Triangles denote statistically significant ($\alpha = .05$) difference between office conditions

Figure 4.26: Conditional AMCE, No Policy Mismatch, No Party Label

4.3.7.2 Partisan Coherence

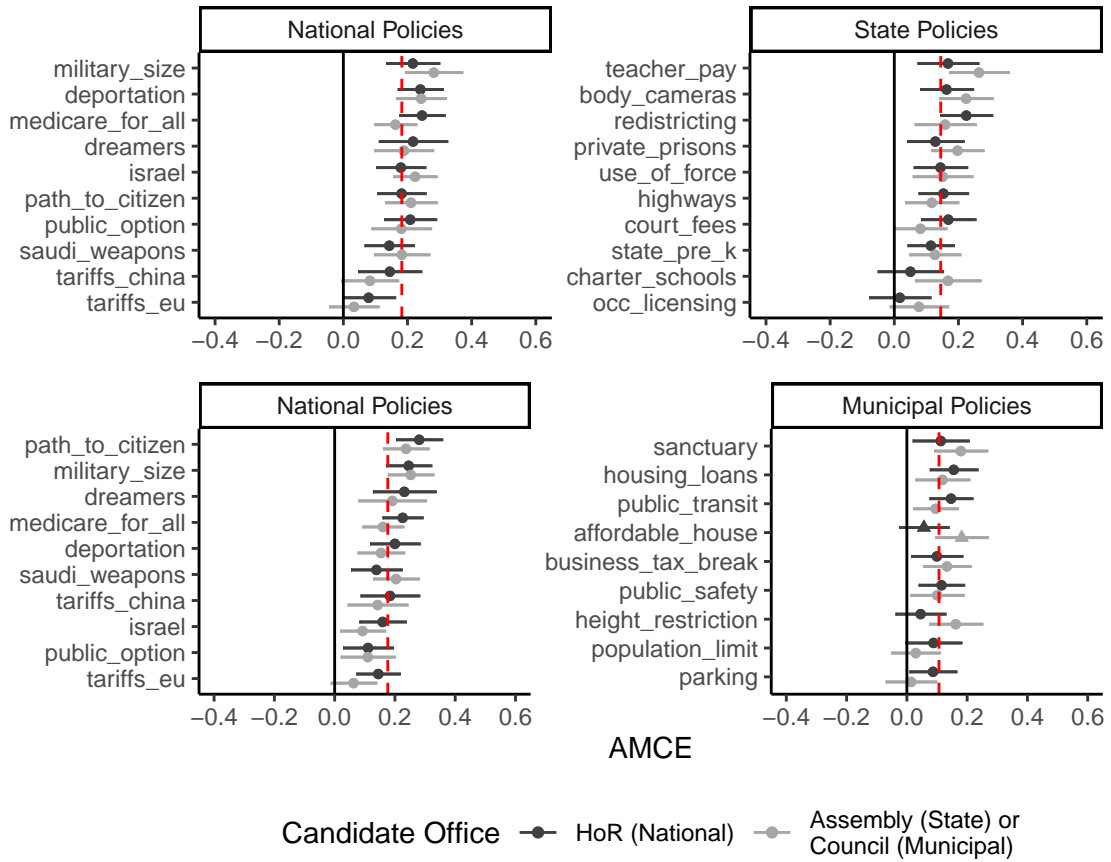


Figure 4.27: Conditional AMCE, No Party Mismatch, Party Label

4.3.8 Equivalence Testing

In this section, we conduct equivalence testing. As discussed in the main paper and in (Hartman and Hidalgo 2018), equivalence testing “inverts” the direction of a traditional null hypothesis test. While a traditional hypothesis test assumes no difference between office conditions, equivalence testing assumes there *is* a difference. Given that assumption, we can determine how large the difference could be given our data. The equivalence ranges shown below give the range of possible difference by office condition for each policy given our results. For example, while our original formulation of the results shows no significant difference between the state and national office conditions for use-of-force training policy, the equivalence testing formulation supports a maximum difference in AMCE of about 0.04. We plot the actual observed differences inside the ranges.³

³Note that some policies (highways, medicare for all, affordable housing, and height restriction) demonstrated such close equivalence that our equivalence testing was unable to estimate any upper bound for an effect. These policies were omitted from the plot.

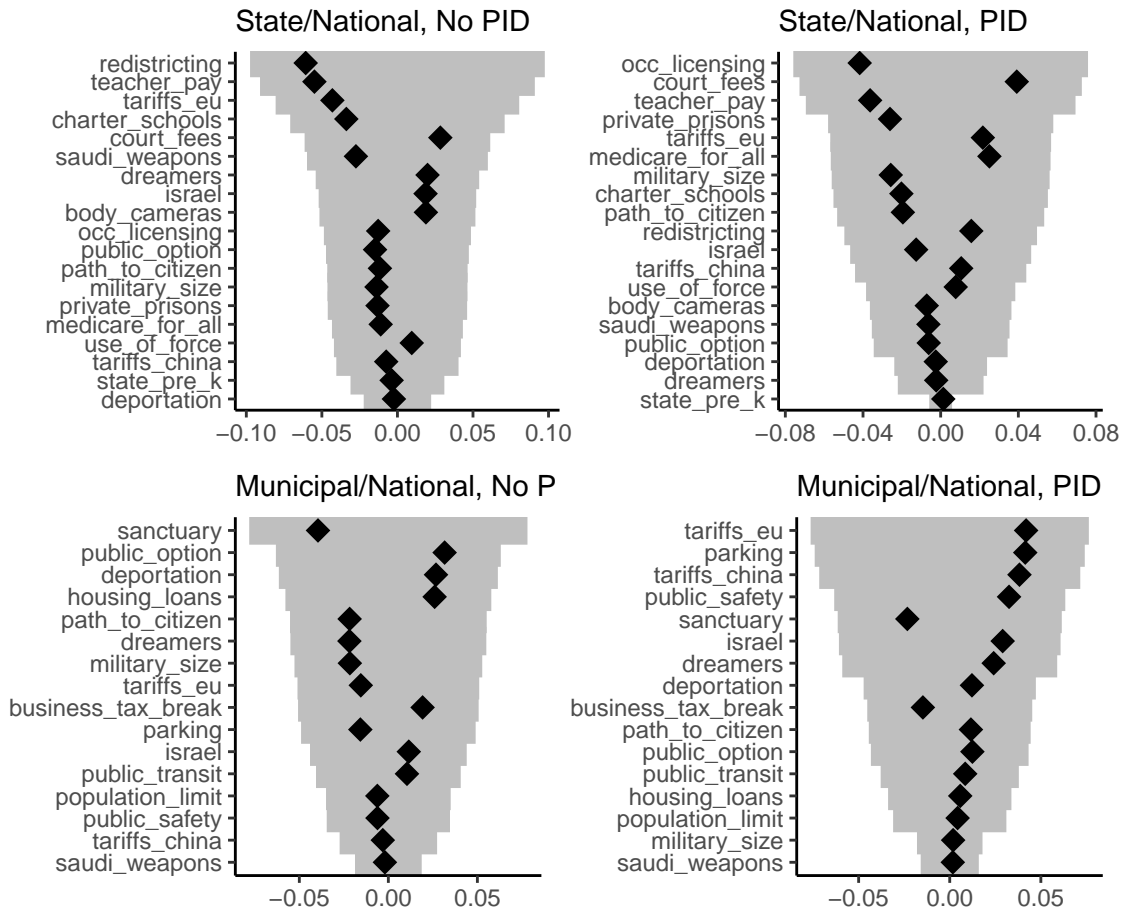


Figure 4.28: Equivalence Tests of AMCE by Office Condition

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