

DC On My Mind: Voter Evaluations of Nationalized Policy Positions in State and Local Elections

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Abstract

Recent research demonstrates that national, state, and local election outcomes are becoming closely correlated, raising concerns that political accountability may be undermined by voters making electoral decisions using information irrelevant to subnational governance. The observational and aggregate data used to make these conclusions, however, are consistent with an alternative: the dimensions of national and subnational politics are correlated, and voters make informed decisions with imperfect but useful information. We distinguish between these possibilities using conjoint experiments varying the contested office level and candidate policy positions across national, state, and local politics. We find national issues more strongly affect candidate selection, but do not eliminate the effects of state and local issues. Additionally, national, state, and local issues affect candidate selection for all levels of office. Our results suggest U.S. politics is nationalized, but not fundamentally broken, with meaningful opportunities for the enforcement of political accountability in low-information settings.

Keywords: nationalization, state and local politics, political behavior, survey experiment

Supplementary information for this article is available in the online edition. Replication files are available in the JOP Data Archive on Dataverse (<https://dataverse.harvard.edu/dataverse/jop>). The empirical analysis has been successfully replicated by the JOP replication analyst. This research involves human subjects and has been approved by Institutional Review Boards of University of California–Los Angeles (21-001360) and for followup work discussed in Supplementary Appendix 10, Stanford University (IRB-80657). This publication was made possible by grants from the Rapoport Family Foundation, the UCLA Political Psychology Fellowship, and Trinity College Dublin Faculty of Arts, Humanities, and Social Sciences Research Fellows Award. The statements made and views expressed are solely the responsibility of the authors.

How do voters make electoral decisions in low-information elections? This fundamental question of electoral behavior has received renewed attention as scholars of American politics document the “nationalization” of U.S. elections.¹ That is, national political actors and issues increasingly influence state and local political activity, evidenced by an increasing correlation between presidential and down-ballot election results since the 1970s (Hopkins 2018; Jacobson 2015; Sievert and McKee 2019; Weinschenk et al. 2020; Zingher and Richman 2019). Insofar as candidates for Governor, state legislature, or even city council share party labels with national political figures, and voters lack alternative information cues to make decisions in rapidly deteriorating local media environments, where residents of less than half of U.S. counties have access to reliable local news (Abernathy 2023), scholars contend voters in state and local contests treat state and local elections simply as extensions of national elections. Implicit in this claim are dire consequences for political representation: federal institutions are designed to divide power and responsibility between the national, state, and local governments, so if voters lacking relevant policy information punish or reward state and local officials for the unrelated actions of the President, their ability to hold such officials accountable for actions pertinent to subnational government may be strained.

We contend this diagnosis is premature. Observational measurement strategies of aggregate voting outcomes, like those often used to show increasing “nationalized” correlations, lack insight into different mechanisms underlying individual-level behavior.² 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 representation. In this case, aggregate measures of nationalization reflect the genuine state- and local-level preferences of informed voters, rather than being the byproduct of national-level preferences of uninformed voters. To the extent national, state, and local political dimensions are cor-

1. The studies in this paper are pre-registered, as described in Supplementary Appendix 11 (page 31).

2. See Kuriwaki (2024), however, for an example of ballot-level data on nationalization in state and local elections.

related, national-level issue positions can provide voters with useful signals for determining ideological similarity to candidates.

It is therefore necessary to document the presence of “nationalized” behavior at the individual level by manipulating the information available for respondents to use when making decisions, allowing us to unpack the mechanisms underpinning nationalized behavior. While certain research has utilized individual-level survey data (Abramowitz and Webster 2016; Davis and Mason 2016; Jacobson 2019), we use an experimental approach to measure directly how voters use information 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. For example, a candidate for municipal office might oppose creating a path to citizenship for undocumented immigrants. While irrelevant to the duties of the candidate’s office, such a position may indicate qualities of the candidate (e.g., conservativeness) that *are* relevant to voters.

We find the office the candidate seeks has almost no effect on how voters use policy information to choose a candidate; regardless of whether candidates take issue positions on policies inside or outside their jurisdiction, voters in (dis)agreement with their policy stance are more (less) likely to select them as their preferred candidate. The size of a policy’s effect on candidate selection does not vary by the office of the candidate. However, we find national-level policies have larger effects on candidate selection in both state/municipal and national-level contests. This effect persists whether voters are given access to party labels for the candidates—regardless of office—indicating that policy stances convey signals to voters beyond partisan labeling, though we also find that when policies have clearer associations with different parties, such policies have greater effects on candidate selection.

Our findings introduce some of the first individual-level causal effects in the nationalization literature, provide more detail on the potential mechanisms behind nationalized political behavior in the U.S., and suggest a less dire representational picture in state and local politics and in low-information environments more generally. While national issue positions certainly sway voters in state and local elections, they do not do so at the complete expense of state and local issue positions. Our results are consistent with a homogenization of individual preferences over issue dimensions across levels of office (Schaffner, Rhodes, and La Raja 2024). Specifically, because voters see issue positions across jurisdictions as connected, every issue position taken by a candidate acts as an informative signal of candidate type. As a result, the limited information received by voters, especially in areas of declining news access, becomes significantly more valuable in decision-making.

U.S. Political Behavior has Nationalized

Researchers studying the nationalization of elections have found an increasing correlation between presidential and state/local partisan vote shares in elections. 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 (2015) 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 local and, especially, state political elites with their national counterparts. For example, state party platforms have become increasingly

homogeneous across state boundaries (Hopkins 2018). State legislative agendas also display signs of homogenization (Burke 2021; Butler and Sutherland 2023). 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 (social media posts) 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; Martin 2019; Rojas and Alford 2019). Such nationalized appraisals extend to gubernatorial races in Washington (2016), West Virginia (2011), and Texas (2011) (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.”³ 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, like when Joe Arpaio (former Sheriff of Maricopa County, Arizona), would appear 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’s 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

3. See <https://web.archive.org/web/20220904200204/https://stoptherepublicanrecall.com/about/>

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, state officials are not merely defending their federally designated roles, they are actively weighing in on inherently national issues.

We do not dispute the increasingly nationalized landscape of U.S. politics; national, state, and local politics are more connected than ever. We depart from existing literature in interpreting the *content* of that connection and assessing the *consequences* for state and local representation, accountability, and democracy.

Nationalized Politics is not (Necessarily) Broken Politics

The foundation of connections between national and state/local political behavior rests in how voters make decisions between candidates using limited information. To our knowledge, however, there is no published research that directly observes individual-level behavior in a manner that allows for researchers to determine how information is being used to make electoral decisions. Such research relies either on aggregate-level voting data (Amlani and Algara 2021; Hopkins 2018; Jacobson 2019; Sievert and McKee 2019) or descriptive survey data without causal effect estimation (Abramowitz and Webster 2016; Davis and Mason 2016; Jacobson 2019). Moskowitz (2021) employs an observational design allowing for causal effect attribution, but the “treatment” occurs at the media-market level of aggregation.

This lack of appropriate data poses a challenge for understanding the mechanisms behind nationalization, as identical electoral outcomes can emerge through the aggregation of votes from voters whose behavioral pathways substantially differ. In turn, understanding these behavioral pathways is critical to diagnosing if, and how, nationalized politics poses a threat to democratic functioning. Fundamentally, the nationalization literature is concerned with the *content* of signals received by voters and how such signals are *utilized* in decision-making, which we detail below.

Nationalized Signal Content

The primary representational concern from the nationalization literature (and research on partisanship more broadly) is that the informational signals received by voters are orthogonal to the information necessary to hold elected officials accountable. As Hopkins (2018, page 118) describes:

“When political behavior is nationalized... voters’ default is to support the same political party at all levels of government... As state and local politicians adapt to nationalized political behavior, they may well see less reason to cater to their own constituents, as those constituents are unlikely to vote on the basis of their performance in office.”

In this reckoning, the partisan signals a candidate sends are divorced from the policy performance of elected officials, leading to poorer representation. This threat is felt acutely at the subnational level, where routine governance is distinct from national policy (Anzia 2021; Oliver, Ha, and Callen 2012; Peterson 1981). Additionally, a significant body of research contends partisanship is, rather than primarily ideological, an affective, expressive identity, so we should expect voters to vote according to their party identification (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). This identity-based connection to partisanship further strains voters’ ability to correctly infer any connection between policy, performance, and party. Moreover, research is inconclusive on voters’ ability to assign functional responsibility for policy to the appropriate level of government (Arceneaux 2005, 2006; Brown 2010), so even those voters not operating purely based on partisan signals may struggle. All this coheres into a picture where the needs of state and local constituencies are not addressed by elected representatives.

But there is another, much more benign, story that can be told about nationalized voting results in aggregate data: one in which nationalized partisan and policy signals convey real, not just illusory, information about state and local contexts, and voters are acting as rational agents making decisions in low-information environments. For example, knowing a

candidate is in favor of deporting undocumented immigrants may encourage voters to update their priors on whether the same candidate is in favor of increasing funding for local policing. Recent research demonstrates that party platforms, which once had significant heterogeneity by state, have homogenized: state and national parties are now seen as more singular than separate (Butler and Sutherland 2023; Caughey, Dunham, and Warshaw 2018). As elite-driven supply of policy homogenizes, the dimensions of state and local politics begin to mirror the left-right contours of national politics (Caughey and Warshaw 2016; Lee, Landgrave, and Bansak 2023; Shor and McCarty 2011). To be certain, the authors that represent this perspective do not claim that all variance in state and local politics is subsumed by national politics (Bucchianeri et al. 2021; Jensen et al. 2021), but if the correlation is strong enough, which some research suggests is likely the case (Schaffner, Rhodes, and La Raja 2024), voters can vote effectively, connecting candidates to policy and performance more readily. In that case, political accountability under nationalization may be bent, but not completely broken.

Nationalized Decision-Making

Which meaningful political signals voters are exposed to is one component of nationalized behavior. How voters *utilize* such signals in decision-making is equally, if not more, important. Research shows that voters operate in an environment of limited information (Abernathy 2018; Hayes and Lawless 2018). Given a lack of meaningful information about state and local political contests (or the costliness of obtaining information that exists), voters must rely on national information cues instead (Martin and McCrain 2019). This increased reliance on national information as a shortcut perhaps results in voters defaulting to the candidate of their preferred party (Moskowitz 2021). Although voters are capable of inferring ideological information over the course of a campaign even in non-partisan electoral contests (Holman and Lay 2021; Sances 2018), and make use of other heuristics like candidate demographics (Kirkland and Coppock 2018), these avenues are costly and often crowded out by more explicitly partisan signals (Boudreau, Elmendorf, and MacKenzie 2015; Crowder-

Meyer, Gadarian, and Trounstein 2020). When partisan labels are available, regardless of jurisdiction, voter reliance on partisan cues is greater (Conevska et al. 2025).

As we first described above, the degree to which this lack of germane state- and local-level information is concerning for representation depends on the fitness of nationalized information to fill the gaps. It is, then, more concerning if some voters have access to state and local information but *willfully* reject it in favor of national information. McCrain and Peterson (2025), for example, show no increase in local televised news and newspaper consumption in the lead-up to local elections, even when these sources are freely available and interest should be at its peak. When voters are offered free subscriptions to local newspapers, few actually subscribe (Hopkins and Gorton 2024), and even those who do subscribe do not show any change in levels of political knowledge, attitudes, engagement, or voting behavior (Trexler 2025). American voters appear to be drawn toward the abstract, moralizing, and negative rhetoric endemic in national politics and on social media (Dillion et al. 2024; Robertson et al. 2023).

Fortunately, even in an environment where voters privilege these stimulating but (arguably) vacuous political signals, there is still some evidence that voters have policy preferences beyond simple party identification. 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. Voters get important information about a candidate’s ideology from party labels, but they allow other signals to influence their behavior as well.

Observable Implications from Individual Behavior

We assess the presence (and nature) of nationalization at an individual voter level. Given the theories of nationalization discussed above and an ideal controlled environment, we expect that voters would display some mix of three possible archetypal patterns of voting behavior.

We emphasize these archetypes are caricatures meant to anchor the potential poles of voter behavior; our goal is to determine where voters exist in relation to such poles.

First, we could find that *all politics is national*: only national policy positions and/or partisanship affect candidate selection (or else completely dominate state and local policies). Second, we could find that voters are *responsible federalists*: that voters use national information to decide national races and state/municipal information to decide state/municipal races. With access to all the information they need to decide, voters prioritize the “sure thing” of jurisdictionally-germane policy positions. Third, voters may be *jurisdictionally agnostic*, and make use of national and state/municipal information to vote on all races, in effect taking all information as a signal of candidate type (or, alternatively, if voters insist that legislators can or should ignore jurisdictional limitations and advocate for preferred policies no matter what).⁴

To determine the relative prevalence of such archetypes, we implement a design (described below) wherein voters are asked to choose between candidates contesting office at various levels of government given an array of (possibly relevant or irrelevant) policy and party information. Our findings are clear: In our results section, we show support for a mix of the *all politics is national* archetype and the *jurisdictionally agnostic* archetype. In particular, we observe four implications for the behavior of American voters. **(1) National policy signals are stronger:** All policy signals are useful to voters, but some (typically national signals) are more useful than others. **(2) American voters are not empty-headed partisans:** They use policy information, not just party label, when deciding how to vote. **(3) American voters are *jurisdictionally agnostic*:** If they approve of a policy, they will use that approval to decide how they vote, irrespective of the office they are evaluating a candidate for. **(4) Partisan homogenization is occurring:** American voters display

4. Other archetypes are theoretically possible: perhaps no policy positions have any effect on candidate choice at all – voters could make decisions based on party ID alone – 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 consideration in our text.

correlated preferences in whom they want representing them across all levels of government. Policy signals that reflect the deepest divides also convey the most signal to voters.

These results are neither a total relief nor a total crisis for the quality of representation. Consistent with skeptical theories, voters do appear to use imperfect and/or irrelevant political signals. On the other hand, not all is lost: voters are not so selectively attentive that they reject signals that *are* useful. We are not so optimistic (or naive) as to assume voters skillfully triangulate ideological ideal points from noisy signals, but neither can we rule out the possibility they are genuinely attempting to do so.

Design

We conduct a series of conjoint survey experiments consisting of nationally representative and weighted samples.⁵ In a conjoint experiment, a respondent is given a set of choices to make: in our case, respondents choose between two hypothetical candidates for office whose attributes have been varied randomly. Our respondents are each given ten “prompts” (pairs of candidates to choose between).

Our design directly reflects our goals: we must (a) establish whether respondents care about national issues, state/local issues, both, or neither; (b) whether issue positions convey any information beyond simple party identification of candidates; and (c) whether the issues they care about depend on the office they are voting for. This suggests a design where respondents select candidates who have policy positions on issues at varying levels of responsibility, including or excluding an explicit party label, and running for various offices.

5. The survey was fielded online in two waves. The pilot wave, consisting of the state level, no partisan label 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 Supplementary Appendix 6.1 (page 14); in Supplementary Appendix 6.2 (page 15) we plot a distribution of assigned respondent weights. We discuss theoretical concerns about the inferential impact of our weighting scheme in Supplementary Appendix 6.3 (page 15). Our core result is robust to alternate weighting schemes or to dropping weights entirely (results presented in Supplementary Appendix 6.4, page 16). The 2022 survey wave, which comprises most of our data, is pre-registered. We observe no major deviations from our pre-registration. The 2021 survey wave, which served as a pilot, was not pre-registered.

We describe how our design satisfies these goals below.

In each prompt, respondents select between two candidates who express positions on issues at various levels. For every respondent, some of these positions will be on issues that are national responsibilities. The remainder will be on issues that are either state or local (municipal) responsibilities. The choice between municipal and state as the comparison level is called the *level condition*. Every respondent is additionally assigned to a *partisan label condition*, where the prompts either reveal or hide the party label associated with the candidates. This design yields four distinct analysis groups, shown in Figure 1.

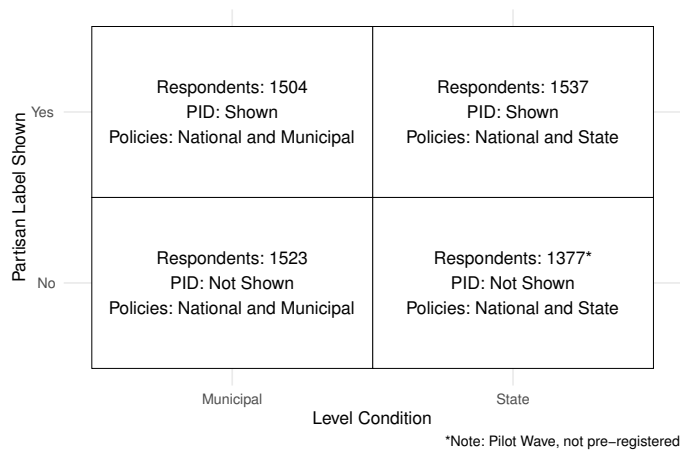


Figure 1: Details of survey design

ALT TEXT FOR FIGURE 1: A two-by-two table indicating the analysis groups. In order, the groups are: National vs. Municipal Policies (party ID shown), n=1,504; National vs. State Policies (party ID shown), n=1,537; National vs. Municipal Policies (party id not shown), n=1,523; National vs. State Policies (party ID not shown, n=1,377).

Third and finally, we must vary the office for which the candidates are running. Each respondent is given five prompts where candidates are running for the federal House of Representatives. They are additionally given five prompts where candidates are running for the lower office corresponding to the level condition above. We call this the *candidate office condition*. Thus, each respondent has prompts assigned to two of the three candidate office conditions (national, plus either state or local).⁶

6. The order in which respondents receive these sets of prompts is experimentally varied (*order condition*). Our reported results ignore this condition, but we observe no statistically or substantively significant differences between the two conditions and do not discuss this feature further.

Before selecting their preferred candidates, respondents are given the following preamble to consider, which reflects the level condition they are assigned to and the candidate office condition that applies to the group of prompts:

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.

We turn now to the final major design attribute: the hypothetical candidates themselves. We randomly assign both candidates in a given prompt to four policy issues, which each candidate will take a position on. Policy issues are chosen from a set of 29 distinct issues: 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 policy issues chosen depend on the level condition, but not on the candidate office condition. Thus, a respondent assigned to the state level condition will choose between candidates who express positions on both national and state issues, irrespective of whether the candidates are running for national or state office. A candidate can take one of two stances on each policy issue (which we call "settings"). One setting is affirmative: "Mandate the use of body cameras for state police." The other is negative: "Do not mandate the use of body cameras for state police."

In some cases, both candidates will have the same position on a given policy (though we restrict randomization so that each pair disagrees on at least one policy). 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. This simulates the kinds of comparisons real voters make between candidates.

We aimed to select policy issues which seemed facially salient, covered a broad array of topics, and could be plausibly associated with the unique jurisdictional powers of national, state, and local governments, noting that under the U.S. system of federalism almost no policy domain is wholly the jurisdiction of one level of government. We include issues for which the affirmative setting is the liberal position, the conservative position, or not obviously ideological. A full list of policy issues and settings is given in Table 1.⁷ Moreover, we note that the “national” policy domains include issues where candidates for state and local office have indeed taken public stances. For example, Zohran Mamdani has strongly criticized U.S. support for Israel during the 2025 New York City Mayoral election (Fandos 2025) while other local government leaders have denounced tariffs implemented under the second Trump administration (Durr 2025).⁸

Prior to conducting the conjoint experiment, we measure each respondent’s baseline policy issue positions. We discuss our reasoning in the Analysis and Results section; briefly, it is because our quantity of interest is not the popularity of the liberal (conservative) position (which we report in Supplementary Appendix 1.1, page 2), but rather the effect of the candidate agreeing with the respondent’s own position on respondent vote choice.⁹ 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 1: Conjoint Policies

Policy	Category	Level	Positive (Negative) Setting
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7. The 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). The potential universe of policies that could be included is nearly limitless; we use previously published survey items in order to avoid the appearance of cherry-picking policies with respect to outcomes, but we discuss limitations common to any set of policies in our Discussion section below.

8. We document additional examples of state and local politicians taking positions on ostensibly national policy issues in Supplementary Appendix 1.3.

9. We additionally fit an item-response model to the response data and discuss the spatial structure of the data in Supplementary Appendix 1.2, page 2, similar to the analysis conducted by Schaffner, Rhodes, and La Raja (2024). In brief, the responses are well-characterized by a two-dimensional model and factor loadings are not influenced materially by the level of the issue, such that municipal and state issues are spatially distributed along the same lines as national issues.

Military size	Military	National	(Not) Substantially reduce the size of the U.S. military
Israel support	Israel	National	(Not) Withdraw military support from the state of Israel
Path to citizenship	Immigration	National	(Not) Create a path to citizenship for all undocumented immigrants
DREAMers	Immigration	National	(Not) Create a path to citizenship for undocumented immigrants brought here as children
Deportation	Immigration	National	(Not) Deport all undocumented immigrants
China tariffs	Trade	National	(Not) Substantially increase tariffs on imports from China
EU tariffs	Trade	National	(Not) Substantially increase tariffs on imports from the European Union
Saudi Arabia weapons	Weapons	National	(Not) Stop the sale of weapons to Saudi Arabia
Medicare for all	Healthcare	National	(Not) Provide government-run health insurance to all Americans
Public option	Healthcare	National	(Not) Provide the option to purchase government-run health insurance to all Americans
Teacher pay	Education	State	(Not) Mandate a substantial pay raise for state public school teachers
State pre-k	Education	State	(Not) Create a state-run pre-kindergarten program
Charter schools	Education	State	(Not) Substantially increase state funding of public charter schools
Private prisons	Corrections	State	(Not) Ban the use of privately operated prisons
Court fees	Courts	State	Eliminate (Keep) state court fees for defendants
Body Cameras	Police	State	(Not) Mandate the use of body cameras for state police
Use of Force	Police	State	(Not) Substantially increase funding for use-of-force trainings for state police
Highways	Transportation	State	(Not) Fund major state highway improvements with additional toll revenue
Redistricting	Elections	State	(Not) Create a non-partisan state redistricting commission for the drawing of electoral boundaries
Occupational licensing	Licensing	State	(Not) Substantially reduce state occupational licensing requirements for non-medical occupations
Affordable housing	Housing	Municipal	(Not) Substantially increase spending on affordable housing
Public transit	Transit	Municipal	(Not) Substantially increase spending on public transportation projects
Public safety	Police	Municipal	(Not) Substantially increase spending on policing
Business tax breaks	Development	Municipal	(Not) Use tax breaks and subsidies to attract new businesses
Housing loans	Housing	Municipal	(Not) Make grants or loans available to buy, build, or renovate multi-family housing in the area
Height restriction	Height	Municipal	(Not) Implement a height restriction on new residential and commercial development in the area
Population limit	Population	Municipal	(Not) Establish a population ceiling to maintain neighborhood character
Parking minimums	Parking	Municipal	(Not) Eliminate minimum parking space requirements for new businesses
Sanctuary cities	Immigration	Municipal	(Do not) Enact a ‘Sanctuary City’ policy forbidding local authorities from cooperating with federal agents on immigration issues

Results from conjoint experiments are often presented as average marginal component effects (AMCEs) (Hainmueller, Hopkins, and Yamamoto 2014). AMCEs are interpreted as the average causal effect in percentage points of including a policy (or party label) in a candidate profile on a respondent’s choice of which candidate to select (Hainmueller, Hopkins, and Yamamoto 2014). In other words, *ceteris paribus*, if a candidate takes a respondent’s position on an issue (say, Medicare for All), how many percentage points more likely is the re-

spondent to choose the candidate? Because each conjoint prompt has multiple simultaneous randomized treatments, AMCEs are an efficient way to estimate their effects, and because real elections also feature candidates presenting voters with baskets of policies and a forced choice between candidates, conjoint experiments have high external validity (Hainmueller, Hangartner, and Yamamoto 2015).

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. For example, respondents could never receive two profiles with identical federal policy positions but different local policy positions. These constraints ensure that every respondent contributes at least some information towards our estimation.¹⁰ 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.¹¹

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

10. In 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.

11. We 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 Supplementary Appendix 7 (page 19) 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.

choices. On one hand, the absence of partisan labels limits the real-world generalizability of our results to the typical two-party contest setting seen in many U.S. state and national elections. But this design is important analytically and still has clear real-world analogs. 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 (Barber and Pope 2019)? If respondents are simply empty-headed partisans, we would expect policy positions to have almost no effect when included alongside unambiguous partisan indicators.

Because we explicitly indicate the level of government (national, state, or municipal) responsible for each policy item to the respondent, we “stack the deck” squarely against nationalized behavior. We are unambiguously telling respondents which policy items should be prioritized or discounted if they wish to act as a “responsible federalist,” evaluating candidates purely with regard to issues over which they have functional responsibility. If policy signals from areas outside candidates' jurisdictions affect behavior despite this choice, we claim it is not because of ambiguity in functional responsibility, but because the issue position sends a useful signal about candidate desirability nonetheless.¹² Real-world candidates can deceive voters when they are ambiguous about levels of delegated responsibility for policies they have no influence over; by “helping” our respondents, we attenuate a source of nationalization in the real world.

After excluding respondents who failed a simple attention check, completed the entire survey module in less than 30 seconds, or for whom demographic information was incomplete

12. Of course, respondents may bring their own preconceptions about jurisdictional ambiguity and ignore or disagree with our characterization of the level of government responsible for an issue. Nevertheless, we expect that our strategy of explicitly labeling jurisdictional responsibility provides a clearer and less ambiguous information environment than most real-world elections. We re-field our experiment without these labels and discuss results in Supplementary Appendix 10 (page 28), but find that our results are not sensitive to the inclusion of these labels.

or insufficient to weight to our preferred population targets ($n = 1,341$, 18.4% of our total sample), our survey yields 58,750 completed conjoint responses (117,500 choice sets).¹³

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.

Analysis and Results

We estimate AMCEs, pooled over and split by office condition. In a traditional conjoint experiment, the effect measures the impact of the chosen setting (affirmative or negative) on respondent choice. We are not interested in whether the liberal or conservative position on a given issue is more or less popular. Instead, we are interested in, conditional on a candidate agreeing (disagreeing) with the respondent’s position, how pivotal is the issue to the respondent’s vote choice. Thus, we first re-code all candidates to reflect whether they agree or disagree with the respondent on the issue. AMCEs are estimated using ordinary least squares regression with standard errors clustered at the respondent level.

An AMCE represents the average change in probability (in percentage points) of selecting a candidate when that candidate holds that policy position. Positive (negative) coefficients indicate that respondents are more (less) likely to select candidates. For this reason, AMCEs are mathematically bounded between -1 and 1.¹⁴ If respondents randomly chose between the

13. Because inattentive survey respondents provide lower-quality responses and bias the resulting distribution of political attitudes (Alvarez et al. 2019), we ask the following question prior to our conjoint prompts: “Please select your favorite color. Paying attention and reading the instructions carefully is critical for our survey. If you are paying attention to the survey, please choose Silver below.” Respondents are given a list of 5 colors, including silver. Those who do not select silver are dropped from our analysis.

14. These are the absolute theoretical limits of the AMCE: the effect of including a policy can fall anywhere from guaranteeing a respondent does not select a candidate they otherwise would have, to guaranteeing that they do select a candidate they otherwise never would have. In practice, however, the limits are more tightly

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., where 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.

As we previewed in discussion of our findings above, our AMCEs are conditioned on policy agreement (whether the candidate’s position agrees with the respondent’s baseline position) because without doing so, the effects are difficult to interpret. Suppose our sample is evenly divided between people who believe all undocumented immigrants should be deported and those who believe undocumented immigrants should not be deported. Then, without conditioning on agreement, the coefficient of this policy issue’s affirmative setting is mechanically 0: both camps cancel each other out. This would appear indistinguishable from an issue that is simply irrelevant to vote choice. As a result, we condition on agreement.¹⁵ This design is well-documented in the literature (Hanretty, Lauderdale, and Vivyan 2020; Tausanovitch and Holliday 2025). We prefer this approach to conditioning on party identification (because preference distributions may not be perfectly partisan), but we also provide additional analyses doing so in Supplementary Appendix 3.1 (page 7).

Almost all policies affect behavior, but national issues have the strongest effects

We begin by analyzing the AMCEs pooled over office condition to understand the relative influence of various policy issue positions. The results are shown in Figure 2, where the results are split row-wise by party condition (non-partisan on the top row, partisan on the bottom

bound to zero because of (a) cases where both choice sets contain the same setting for the same policy: in these cases, the selected candidate and the rejected candidate both contribute in opposite directions, attenuating the effect, (b) the cumulative total of the effects of other simultaneously included policy issues, and (c) the baseline probability of selecting a candidate (in our case, 0.50 because each prompt offers a choice between exactly two candidates). Thus, the true bounding of an AMCE coefficient is design specific, but narrower than the above-mentioned -1 to 1.

15. We provide pooled AMCE estimates in Supplementary Appendix 2 (page 6). We also present results in Supplementary Appendix 3.2 (page 10) which condition on (dis)agreeing with the affirmative setting of the policy (rather than accord between candidate and respondent).

row) and column-wise by jurisdiction level (state versus national on the left, municipal versus national on the right). 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 18–26 percentage points more likely to select that candidate (depending on the party label condition). Put differently, candidates who share a respondent’s position on body cameras are selected about 59.1–62.9% of the time, whereas candidates who do not are selected roughly 40.9–37.1% of the time.¹⁶ The exact magnitude of this coefficient is of secondary importance to our research, because it is an artifact of our 4-attribute 2-setting design.¹⁷ Instead, our emphasis here is on the statistical and substantive distinction from no effect, the relative rank of policies against each other, the relative group differences between policy levels, and the relative interaction of policy level and level condition.

While some state and municipal policies (e.g., body cameras and sanctuary cities) have larger effects than some national policies (e.g., EU or Chinese tariffs), national policies on average have much larger effects on candidate selection.¹⁸ Mean national policy AMCEs were 0.086 (95% CI [0.058, 0.114]) – 8.6 percentage points – and 0.062 (95% CI [0.039, 0.086]) larger in the non-partisan and partisan conditions, respectively, or almost double the

16. This is the “marginal mean” interpretation of AMCE, which is possible to use in this conjoint design because each attribute (policy) has only two settings (agree/disagree), so the reference category being used in the regression for each policy position is the setting of the policy that disagrees with the respondent’s preferred setting (Leeper, Hobolt, and Tilley 2020).

17. Moreover, any candidate for office foolish enough to triangulate their policy proposals on the basis of our evidence should expect an attenuated effect, first because taking an issue position involves alienating opposing voters in a way that our design intentionally ignores as described above; and second because although our design is a good conceptual proxy for real-world elections, there is no reason to believe it is a good numerical proxy for real-world elections, which feature more than four policy positions and often more than two candidates.

18. Note 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; none of our results are sensitive to the inclusion of this issue.

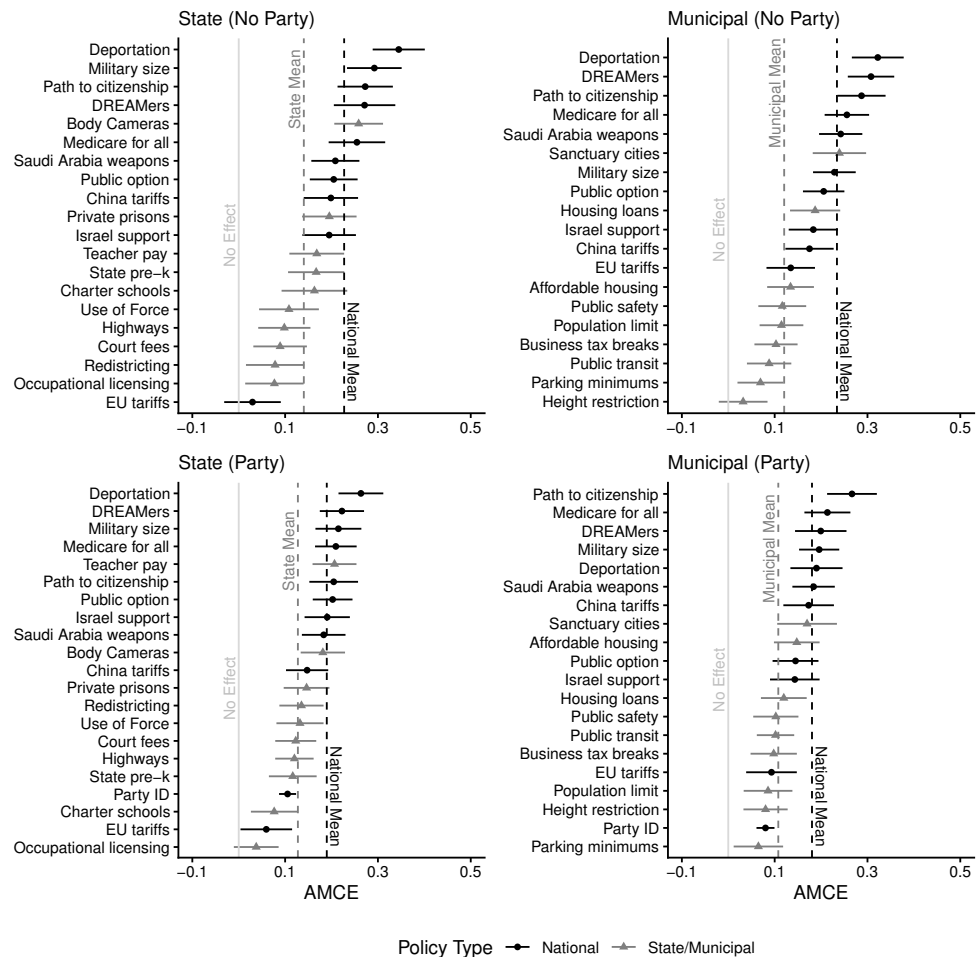


Figure 2: AMCE (Effect Size) by policy item with 95 percent confidence intervals. Each facet of the figure presents the AMCE sizes for a given office and partisan label condition. AMCEs are bounded between -1 and 1, and are interpreted as the percentage point increase (decrease) in the likelihood of selecting a candidate when they express the respondent’s preferred position on the issue. Larger effect sizes indicate more policy issues that are more important to determining respondent vote choice. The top two facets present the no partisan label condition (where respondents are not given a party label for the candidates they choose between) while the bottom two present the partisan label condition. In each facet, the national policies show larger effect sizes than the state or local policies, although all policies have positive and in almost all cases significant effects.

ALT TEXT FOR FIGURE 2: A two-by-two faceted plot of AMCE (Effect Size) estimates with 95 percent confidence intervals. The facets are: national vs. state policies (no party label shown); national vs. municipal policies (no party label shown); national vs. state policies (party label shown); national vs. municipal (party label shown). In each facet, the national policies show larger effect sizes than the state or local policies, although all policies have positive and in almost all cases significant effects.

magnitude. The gap for municipal policies is slightly more pronounced, with mean AMCE differences of 0.114 (95% CI [0.090, 0.138]) and 0.072 (95% CI [0.047, 0.098]) for the non-partisan and partisan conditions, respectively.¹⁹ Substantively, these results suggest national issues dominate state and local issues, but not to the point of their complete exclusion. This pattern is most consistent with the *all politics is national* archetype described above.

Another potential explanation for the difference in AMCE is simply that national policy issues are more important, salient, familiar, or exciting for voters. While we attempt to select from the most important policies in the exclusive domains of state and national policymaking, we are unable to evaluate if we successfully did so without an exhaustive inclusion of all potential policies.²⁰

Does the inclusion of candidate party labels significantly alter 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*.

The effect of party identification itself is small in comparison to national policies, but

19. Confidence intervals are estimated with 2,500 bootstrap replicates performed at the respondent level. In 100% of all bootstrap iterates, national policies had larger AMCEs than state and local policies in both party label conditions.

20. We 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. Supplementary Appendix 9 contains discussion of several subsample-policy interactions of interest (none of which our results are sensitive to). 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.

roughly similar to the state and municipal policy effects.²¹ Across all policies, we see an average decline in AMCE of just 0.03 between the non-partisan and partisan conditions, indicating the majority of the effect of policies remains *net* of partisanship. 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 0.013, reflecting lower overlap between our state/local policies and partisan flashpoints, and also visible in the baseline policy preference ideological loadings discussed in Supplementary Appendix 1.2).²² 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, as the policy effects are both larger than and robust to the inclusion of candidate party identification (similar to a recent set of results from Orr, Fowler, and Huber (2023) in the affective polarization literature linking out-party affect more directly to policy considerations than to group identity). Such a pattern is still most consistent with the *all politics is national* archetype, but not with its most deleterious, empty-headed form.

Respondents are jurisdictionally agnostic when evaluating policies

We now consider the difference in the policy AMCE conditional on office condition: do respondents use issue positions differently depending on the contested office? The differences between the national (federal House of Representatives) and state/municipal (state assembly/city council) office condition for each policy are plotted in Figure 3. If respondents prioritize policy information germane to the candidates' office, we should observe large positive values for the national policy issues (in black) and large negative values for the state and municipal policy issues (in gray). However, in all but one case (creating a non-partisan redistricting commission, in the non-party label condition), there is no significant difference in the policy AMCE by candidate office. Substantively speaking, it does not matter whether

21. Note 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.

22. All differences are reported in full in Supplementary Appendix 4 (page 12).

the policy position taken by the candidate is under that candidate’s potential jurisdiction. Instead, respondents seem to treat *all* policy information as useful when making decisions between candidates. While this supports findings from previous research indicating the reach of national politics into state and local contexts, 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, consistent with the *jurisdictionally agnostic* voter archetype.

The absence of difference between office conditions is a result of substantive interest, but it is important to note our statistical tests (like any hypothesis tests) are statistically “conservative” in the sense they are weighted toward finding null results. While our unweighted results (presented in full in Supplementary Appendix 6.4, page 16) increase the precision of this null effect, to more directly test the equivalence of the survey-weighted 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 office 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.²³

The full results of the equivalence test are given in the Supplementary Appendix 8 (page 22). Most of our results lay within an equivalence range of -0.05 and 0.05, meaning our data are statistically consistent with the absolute differences in policy AMCE between national and state/local office conditions being within 5 percentage points of each other. The maximum equivalence range is for redistricting (an effect where we did observe a significant difference), being just between -0.1 and 0.1. While these ranges dampen claims of exact equivalence between estimates, we reiterate that the relative magnitude of these ranges to

23. 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 $\alpha = 0.05$.

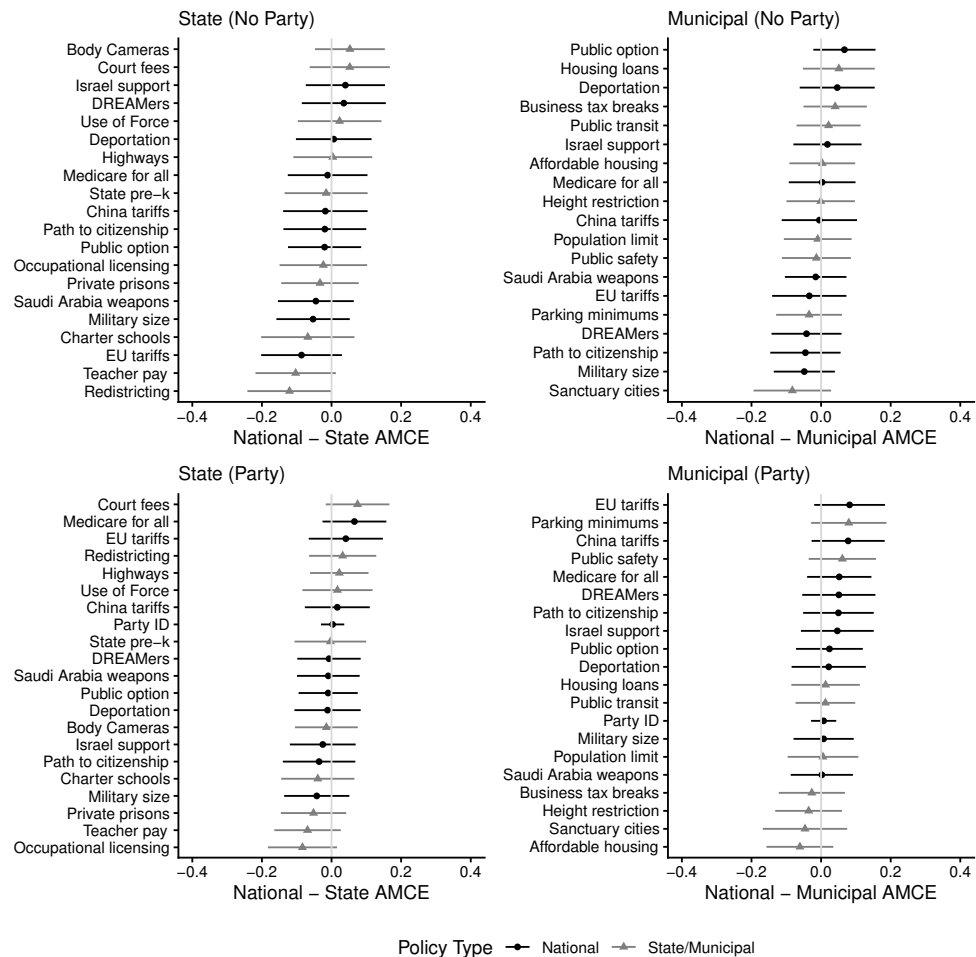


Figure 3: Difference in office condition AMCEs (Effect Sizes) by policy with 95 percent confidence intervals. The x-axis in each facet reports the difference between policy AMCEs applied to candidates for national office and policy AMCEs applied to candidates for state/local office: thus, how the effect associated with a policy changes depending on whether it is expressed by a candidate for the relevant office or an irrelevant office. The gray line represents no difference. Because almost all policies overlap the gray vertical line at zero difference, we observe that the office of the candidate does not change the effect of issue positions, even when the policies are outside the jurisdiction of that office.

ALT TEXT FOR FIGURE 3: A two-by-two facet figure showing the difference in office condition AMCEs with confidence intervals. The facets are: national vs. state policies (no party label shown); national vs. municipal policies (no party label shown); national vs. state policies (party label shown); national vs. municipal (party label shown). In each facet, the effect shown represents the difference between the effect of a policy expressed by a candidate for the relevant office and the same policy expressed by a candidate for an irrelevant office. Because almost all policies overlap the gray vertical line at zero difference, we observe that the office of the candidate does not change the effect of issue positions.

overall effect size is a more useful comparison. Almost all AMCE, regardless of office condition, were greater than 0.1, including half of the national policy effects being greater than 0.2. Even if the differences in office condition AMCEs *were* at the limits of the equivalence ranges we find, the AMCE for each office condition would still be significant, meaning respondents make use of jurisdictionally irrelevant policy information in making candidate selections.

More partisan issues have larger effects

Our results thus far are consistent with some mix of the *all politics is national* and *jurisdictionally agnostic* archetypes; almost any policy signal (but especially a national 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. This should be true *even when* we give information to respondents on which jurisdiction each policy “belongs” in (as we do in our design).

We investigate this conjecture in Figure 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 who agree with the policy’s affirmative setting.²⁴ 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

24. We 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.

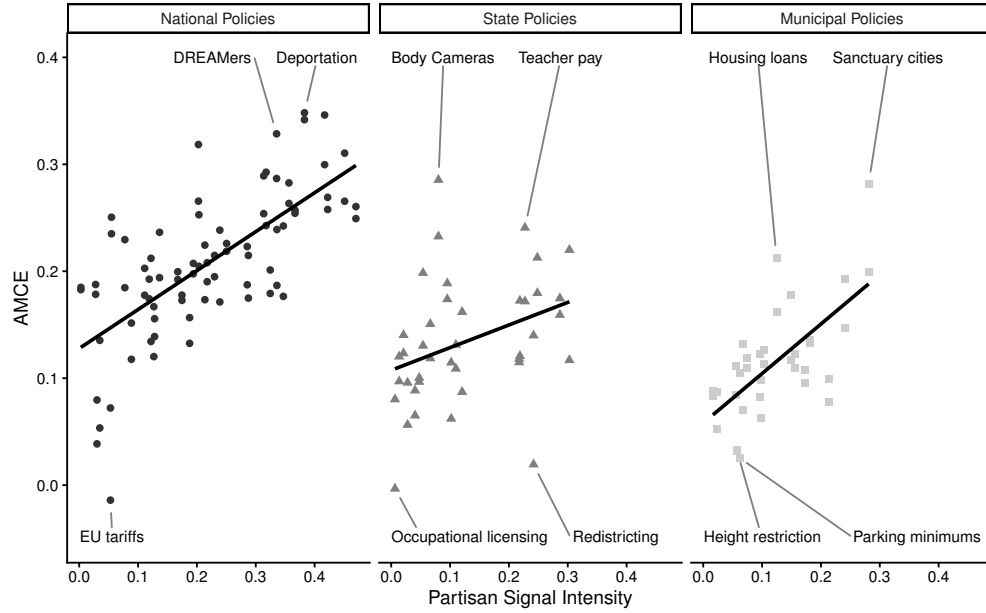


Figure 4: Policy AMCEs (Effect Sizes) and partisan signal intensity, measured as the absolute difference in partisan support, by policy jurisdiction, with 95 percent confidence intervals. The x-axis represents the extent to which Democrats and Republicans differ on an issue. The y-axis represents the AMCE of that policy issue (how many percentage points more likely a respondent is to select a candidate who agrees with them on the issue). Larger AMCEs indicate more pivotal policies for respondent vote choice. The steeper the slope of the line within a given facet, the stronger the relationship between AMCE and partisan signal intensity among that set of issues. The domain of the x-axis is held constant across jurisdictions, even though national policy items have a wider range of partisan signal intensities than state/local policies.

ALT TEXT FOR FIGURE 4: The figure depicts the relationship between partisan signal intensity (the extent to which Democrats and Republicans differ on an issue) and AMCE (effect size), faceted across policy jurisdiction. The facets are national policies, state policies, and municipal policies. In each case the slope is positive and steep.

spectrum. We then plot the partisan signal intensity against the AMCE from Figures 2 and 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 AMCEs).²⁵

The results of our analysis in Figure 4 suggest that the stronger the partisan signal, the greater effect the policy has on candidate selection (across all policy types), supporting the

25. In Supplementary Appendix 5.1 (page 13), 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.

hypothesis that strong signals of type are particularly useful to voters. This pattern persists even when splitting the data by *office condition* and *policy type*; regardless of whether candidates are contesting national, state, or municipal office or if the policies are (sub)national in nature, partisan signal intensity is positively associated with candidate selection.²⁶

We also note the range of partisan signal intensity is much greater for our selection of national issues than for state and municipal issues. This mirrors our finding from the AMCEs that the national left-right dimension may dominate state and local issues; not in the sense that state and local issues do not matter to voters, but that the dimension along which many such issues are contested is subsumed by national political cleavages. The results of these analyses also broadly concord with what we might expect from the distribution of baseline policy positions explored in Supplementary Appendix 1.2 (page 2): issues with high partisan signal intensity also load highly along the primary baseline ideological dimension; national issues load more strongly on average; but state and local issues do load on the same dimension and still matter.

Does this mean state and local policy issues are unimportant? Certainly not. Issues fundamental to quality of life concerns (e.g., infrastructure development) are undoubtedly important and engender productive policy debates. Our evidence suggest such issues are simply *inefficient* as signals of national policy positions. In this way, the salience of an individual issue for the voter behavior we observe is not directly tied to its objective importance, but to the *total* importance voters appear to place on *related* issues.

Discussion

In combination, our results suggest a different picture of nationalized behavior than has been articulated by existing literature, made possible by our ability to analyze individual-level preferences through an experimental design. Certainly, national politics plays a significant

26. If 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 Supplementary Appendix 5.2 (page 13).

role in subnational politics, as evidenced by the relative strength of the effects national policies have on behavior. However, almost all policy signals, regardless of functional relevance to the office being contested, are useful to voters, suggesting voters are *jurisdictionally agnostic*. Our results suggest voters leverage whatever information they have to better triangulate the type of the candidates they are evaluating. This casts doubt on the existence of *responsibly federalist* voters, who see distinctions between offices insofar as distinguishing information is available to them, but also on the most extreme manifestation of the *all politics is national* archetype, as national policy signals only partially crowd out other informational cues.

On average, national policy positions do 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 in the eyes of voters. This conclusion is supported by the positive relationship between policy effect and the strength of the partisan signal attached to the policy, as well as policy effects holding net of partisanship, suggesting this desired “type” is partially, but not entirely partisan. Voters, therefore, 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.

Importantly, we emphasize that we are telling a story about how voters respond to nationalized information cues. In this paper, we do not comment with certainty on the extent to which candidates do or do not provide those cues themselves. While we offer motivating examples of such behavior occurring (both in the article and Supplementary Appendix 1.3 [page 4]), a representative accounting of this type of elite behavior is beyond the scope of this paper, and we encourage further research on the subject. At minimum, we believe our results highlight the strategic incentives for candidates to use such nationalized cues either to align themselves with the majority partisan constituency or to highlight partisan features of their opponent that are perhaps unpopular with the majority constituency. Indeed, the latter behavior was conspicuously apparent in the off-cycle 2025 gubernatorial contests in New Jersey and Virginia, where the Democratic victors linked their opponents to Trump’s

presidency (Ax, Tsvetkova, and Reid 2025).

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 chosen policy areas (which we attempt to mitigate through the explicit labeling of policies’ jurisdictions, even if one could argue otherwise): 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 – we assess a version of this criticism in Supplementary Appendix 7 (page 19) and find little evidence that it drives our results (Cuesta, Egami, and Imai 2022). Though our experiment includes a wide variety of policies, we are sensitive to criticisms that our choice of policies may drive results. For example, if we had selected national issues that were clearly more important or salient in response to priming events than the state and local policies, or if we selected state and local policies systematically less pivotal than counterfactual issues at the same level. We endeavored to choose policies used by other survey research and that facially seem to capture the major debates at each level of functional responsibility (Jensen et al. 2021), and subsequent research purporting to measure national and local dimensions of public opinion use an almost identical set of issues with no notable outliers (Schaffner, Rhodes, and La Raja 2024). To the extent local issues are being “missed,” they are being missed by almost all published research. Future research could involve measuring a baseline of policy importance or salience (through, for instance, elite surveys or observational patterns in press coverage or elite communications) to ensure a fair comparison.

We additionally note that individual issues may be less applicable to various subsamples of our respondents (for example, respondents who live in a state with a non-partisan districting commission might systematically differ from those who live in a state without such a commission). Although we cannot assess all possible unintended interactions between issues and respondents, we examine six in Supplementary Appendix 9 (page 24: sensitivity

of municipal issues to non-urban respondents; sensitivity to redistricting policy to state of residence; and four others; and find our results are robust to these concerns).

Finally, we did not use a “pure” control condition (where respondents considered only one office, or only issues associated with one office) to see whether revealed preferences differ systematically in a control setting. We expect that the robustness of our findings across subsamples, and across the order in which office conditions were displayed to respondents, provide some evidence that the results are not sensitive to this design.

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. Insofar as nationalization elevates divisions of national importance while homogenizing preferences across levels of government, it threatens a *direct* avenue of accountability while opening an *indirect* path. In this sense, accountability can be loosely or incidentally achieved on the set of policy issues ideologically correlated across levels of government. 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).

However, there remain significant opportunities for accountability failures in a nationalized setting. For dimensions of state and local politics uncorrelated with national left-right cleavages, representational slippages are more likely to occur. In the aggregate, it is even possible that the *majority* of sub-national governance occurs on such orthogonal dimensions (Anzia 2021), leading to only limited opportunities for incidental accountability. Lastly, nationalized signals may give voters insights into candidate *preferences*, but less so their *capabilities*; nescient voters may be unable to distinguish between two ideologically proximal candidates who differ drastically in their governing efficiency and ability to deliver upon campaign promises. So, while efficient indicators of a candidate’s preferences may be a *neces-*

sary condition for accountability in a nationalized context, they are potentially *insufficient*. Further work is needed to more deeply understand the nature of the accountability structure and what pressures nationalization puts on the future of representation.

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Supplementary Appendix for DC On My Mind: Voter Evaluations of Nationalized Policy Positions in State and Local Elections

March 10, 2026

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1 Baseline Policy Items

1.1 Policy item agreement and disagreement

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 conjoint experiments, by PID and pooled across waves.

Table A.1: **Policy Agreement/Disagreement by Partisanship**

Democrats				Republicans			
Policy	Agree	Disagree	DK	Policy	Agree	Disagree	DK
Affordable housing	0.69	0.19	0.12	Affordable housing	0.52	0.35	0.13
Body Cameras	0.85	0.09	0.06	Body Cameras	0.76	0.16	0.08
Business tax breaks	0.62	0.18	0.20	Business tax breaks	0.69	0.19	0.12
Charter schools	0.52	0.32	0.16	Charter schools	0.53	0.28	0.19
Court fees	0.51	0.25	0.24	Court fees	0.40	0.39	0.21
Deportation	0.27	0.59	0.14	Deportation	0.66	0.21	0.13
DREAMers	0.80	0.12	0.08	DREAMers	0.52	0.38	0.11
Height restriction	0.49	0.29	0.22	Height restriction	0.41	0.36	0.22
Highways	0.58	0.27	0.15	Highways	0.50	0.36	0.13
Housing loans	0.73	0.15	0.12	Housing loans	0.62	0.23	0.15
Israel support	0.41	0.31	0.28	Israel support	0.25	0.56	0.19
Medicare for all	0.76	0.12	0.12	Medicare for all	0.39	0.49	0.13
Military size	0.31	0.52	0.17	Military size	0.14	0.77	0.09
Occupational licensing	0.39	0.32	0.29	Occupational licensing	0.42	0.31	0.27
Parking minimums	0.40	0.35	0.26	Parking minimums	0.35	0.40	0.24
Path to citizenship	0.67	0.20	0.13	Path to citizenship	0.32	0.57	0.11
Population limit	0.39	0.38	0.23	Population limit	0.43	0.38	0.19
Private prisons	0.65	0.16	0.19	Private prisons	0.42	0.34	0.25
Public option	0.78	0.12	0.11	Public option	0.56	0.30	0.14
Public safety	0.49	0.32	0.19	Public safety	0.60	0.26	0.14
Public transit	0.62	0.23	0.15	Public transit	0.43	0.39	0.18
Redistricting	0.60	0.13	0.27	Redistricting	0.45	0.27	0.28
Sanctuary cities	0.45	0.29	0.26	Sanctuary cities	0.19	0.65	0.16
Saudi Arabia weapons	0.70	0.14	0.16	Saudi Arabia weapons	0.66	0.16	0.18
State pre-k	0.67	0.18	0.15	State pre-k	0.42	0.44	0.14
China tariffs	0.51	0.24	0.25	China tariffs	0.67	0.17	0.16
EU tariffs	0.34	0.36	0.30	EU tariffs	0.39	0.37	0.24
Teacher pay	0.79	0.10	0.10	Teacher pay	0.53	0.33	0.14
Use of Force	0.64	0.21	0.15	Use of Force	0.63	0.23	0.14

1.2 Baseline Position Scaling

Here, we examine the structure of respondent baseline positions. Because each respondent receives only 2/3rds of the issues (all national issues, and then either state or municipal by

level condition), we use an item-response model to reduce the dimensionality of the data. We fit a marginal maximum-likelihood 2-parameter logistic item-response model (2PL) for 1–4 latent dimensions. We conduct a post-hoc orthonormal rotation (i.e., principal components analysis) of the item-loading matrix. Using the resulting loadings, we are interested in addressing three questions:

First, how many dimensions characterize the resulting space? In Figure A.1, we show a plot analogous to a scree plot (using the sum of squared rotated factor loadings across items). The resulting space is well characterized in a single dimension, and certainly in two dimensions. Second, do the state and local items load on a same dimension as national items? Generally, yes. Items from each of the three levels load on each of the spatial dimensions (though there is proportionately more residual variation on the state and local issues loading on the second dimension).

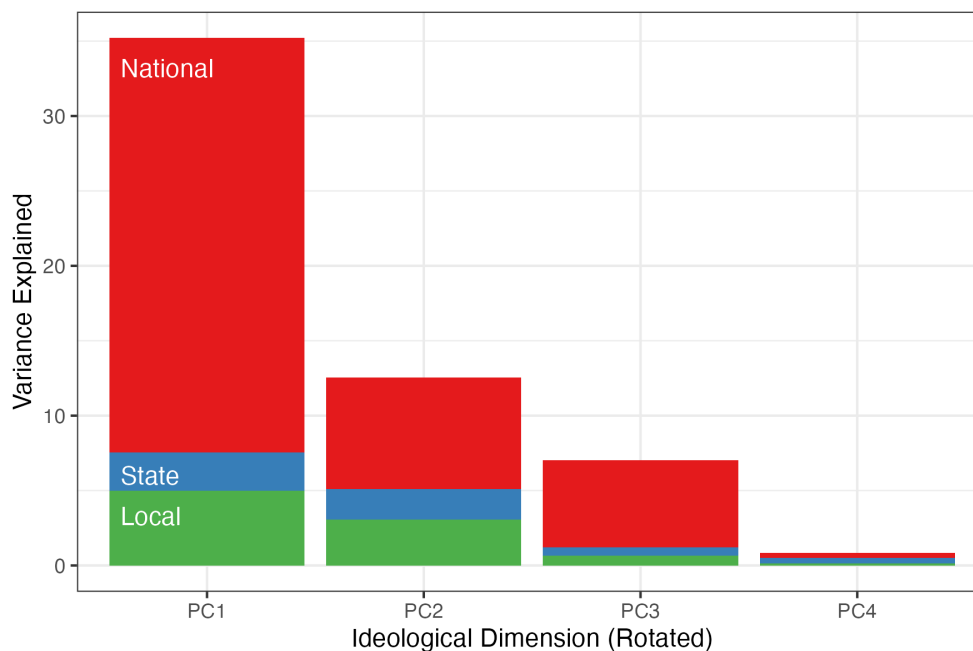


Figure A.1: **“Scree Plot” of Item Loadings:** This figure shows the squared sum of item loadings from a 2PL item-response model, rotated using PCA to create an orthonormal basis. The plot shows that most variation is captured in one dimension, and certainly in two. State and local issues load on the same dimensions as national issues.

Third, what is the “content” of the main ideological dimensions? We show the answer in Figure A.2. The resulting first rotated dimension appears to clearly depict something resembling left-right ideology. The ideological poles of the first dimension are “Deport all undocumented immigrants” and, conversely, “Path to citizenship for all undocumented immigrants”. Other items loading negatively on the first dimension fit with the ideological left, and other items loading positively on the first dimension fit with the ideological right. National issues load slightly more strongly than state and local issues, but all issues are dispersed throughout.

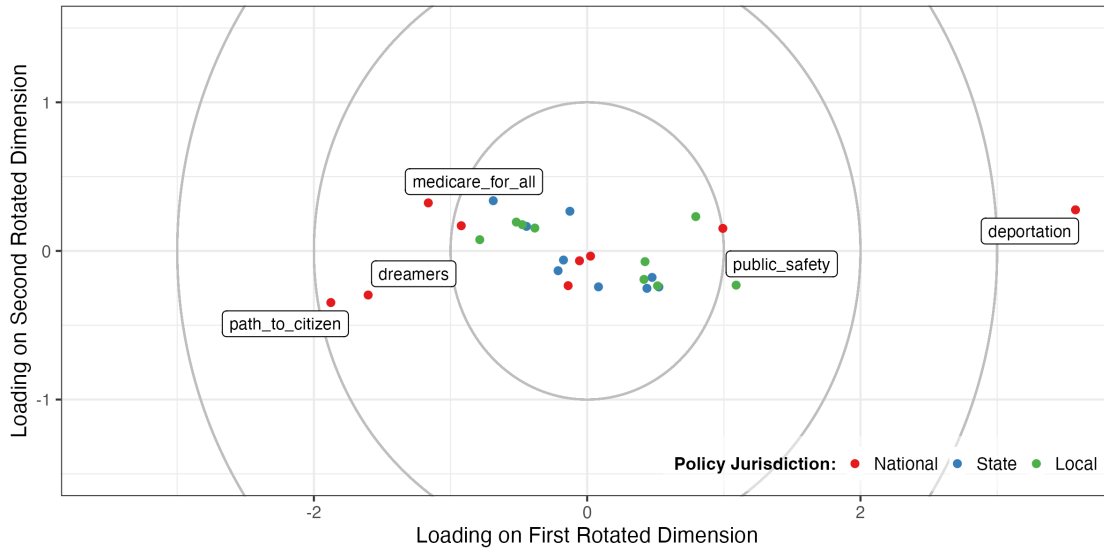


Figure A.2: **Baseline Position Scaling:** We plot the item loadings from a 2PL item-response model, rotated using PCA to create an orthonormal basis. The primary dimension depicts an ideological space similar to the traditional left-right issue space, with immigration issues anchoring the left and right poles. National policies have somewhat more extreme factor loadings, but state and local issues load along the same dimensions and across the distribution.

1.3 External Validity of Selected Policy Items

In this section, we consider the “external validity” of our set of federal policies, with respect to the propensity of the policies to be contested by state and local issues. That is, do candidates (and officeholders) of sub-national offices actually provide voters with nationalized policy

signals?

As a theoretical point, we note that most first-term members of Congress have state and local political experience (Porter and Treul 2025). To some extent, climbing the ladder in one’s political career *requires* engaging in political debates that are outside of one’s current jurisdiction. Similarly, those who run for office are, by definition, extreme outliers in their level of political engagement and policy demands (Hall 2019). To the extent such candidates are highly and publicly interested in state or local dimensions of politics, it is plausible (and perhaps likely) that they would have similarly displayed interest in national politics, especially if we believe these dimensions are correlated.

We complement this theoretical point with a set of 20 examples of state and local officials engaging in national debates specific to the federal policy areas we randomize in our design. We caveat this exercise by stipulating that this is not an exhaustive or representative set of cases, merely a minimum to establish that the policies chosen are among those where real-world officials engage in nationalized rhetoric. To the extent our paper establishes the “demand-side” of nationalization, this Appendix substantiates the “supply-side” of nationalization.

Table A.2: **Examples of Nationalized Policy Positions from State and Local Officials and Candidates**

Policy Area	Example Article(s) (Hyperlinked)
Military	1. Kansas Governor opposes federalization of National Guard without state consent. 2. Maine state legislators oppose deployment of military forces without Congressional approval.
Israel	1. New Mexico State Senator meets with Benjamin Netanyahu. 2. NYC mayoral candidate condemns U.S. support for Israel. 3. Somerville City Council calls for Israel-Gaza ceasefire.
Saudi Arabia (Weapons)	1. West Virginia Governor comments on US-Saudi Arabia alliance. 2. Chicago City Council supports expelling Saudi Ambassador after Khashoggi murder.
Immigration	1. El Cajon Mayor comments on aiding Trump deportation plan. 2. Utah Governor supports Trump’s mass deportations. 3. NYC mayoral candidate supports work requirement path-to-citizenship policy. 4. City of Santa Ana contributes to legal defense of undocumented immigrants facing deportation. 5. Bipartisan Group of American Mayors Urges Congressional Leaders to Act on DACA
Trade/Tariffs	1. Iowa Governor praises tariff policy. 2. NYC council members criticize tariff increases. 3. Mayors from U.S. and Canada stand together against Trump tariffs.
Healthcare	1. Ohio Governor lobbies federal government to introduce Medicaid work requirements. 2. Long Beach Mayor launches Medicare-for-All advocacy group. 3. New Orleans Mayor criticizes attempted repeal of Obamacare. 4. The MinnesotaCare buy-in, explained.

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. In the interest of transparency, we report the results in the more classical manner (affirmative vs. negative) here, but caution that policies which have a bimodal preference profile, but are nevertheless highly salient, create cross-cutting negative and positive effects which cancel out.

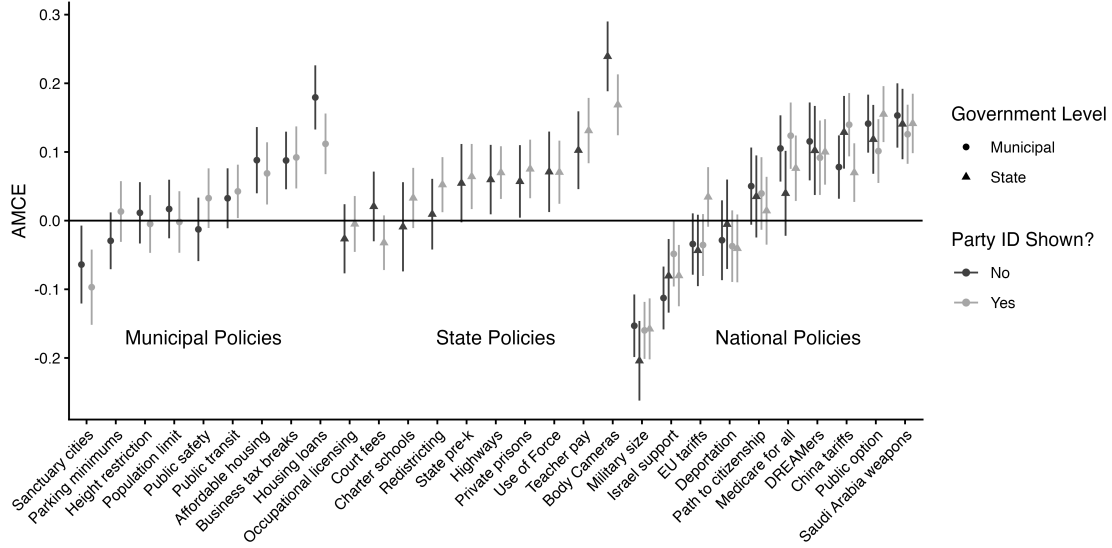


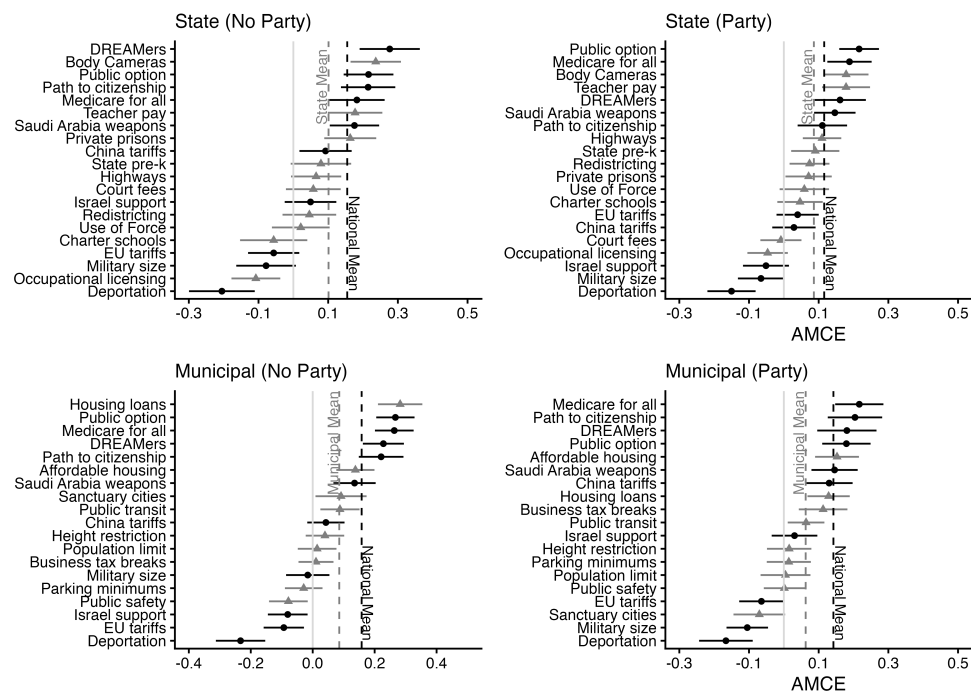
Figure A.3: **Unconditional AMCE by Condition:** This figure shows the impact of conditioning on the affirmative setting of the policy, rather than policy agreement with the respondent. Contentious or evenly divided policies have their AMCE attenuate towards zero, while policies on which there is wide agreement retain large positive or negative coefficients.

3 AMCE conditional on other features

3.1 Party ID

Here, we show the AMCE of each policy subset by respondents with a particular party ID. 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, each containing separate estimates for both parties: the AMCE pooled across office conditions, and the difference in office condition AMCEs. When plotting the mean effect lines, we present the mean *absolute* effect, as we observe some negative AMCEs when conditioning on factors other than agreement.

Democrats



Republicans

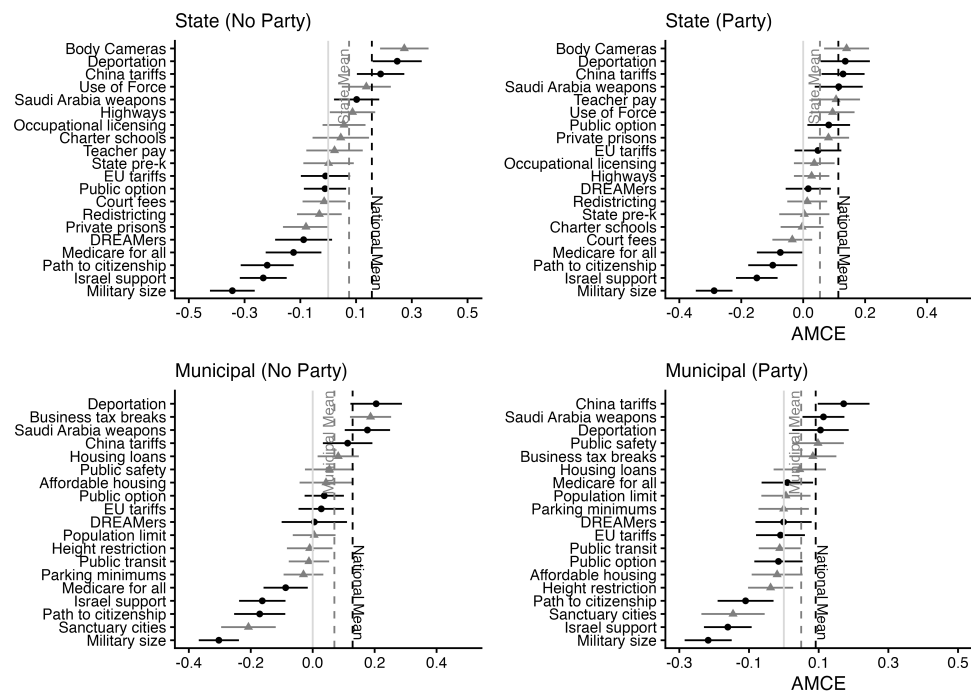
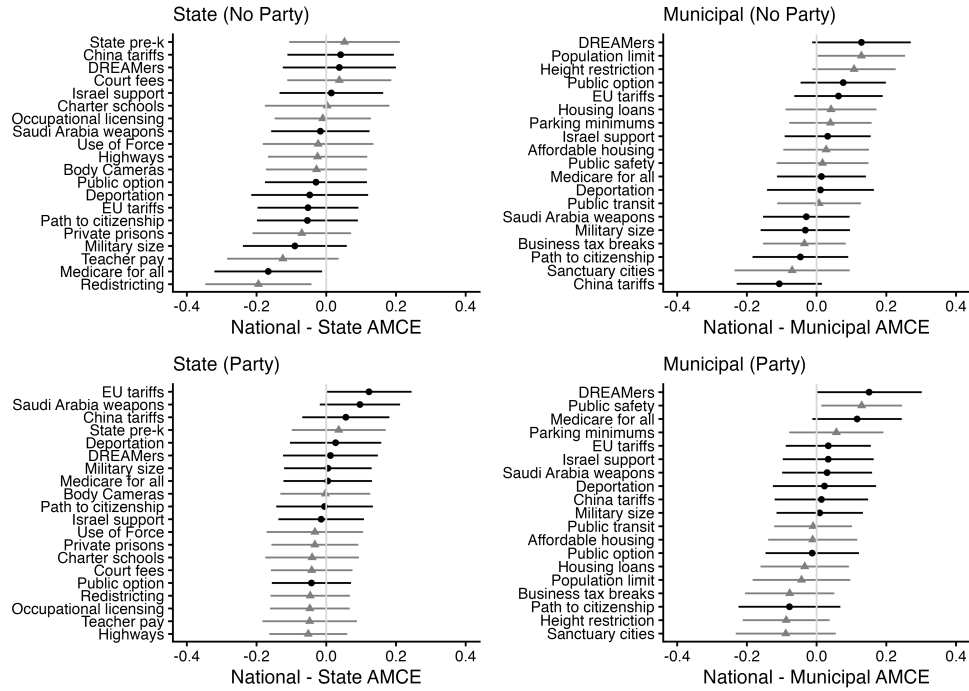


Figure A.4: **AMCE for Democratic Respondents:** This plot depicts the AMCEs for respondents who identified as Democratic/Republican, and conditioning on the positive setting of the policy. The rank ordering of effects reflects generally understood policy views by partisans.

Democrats



Republicans

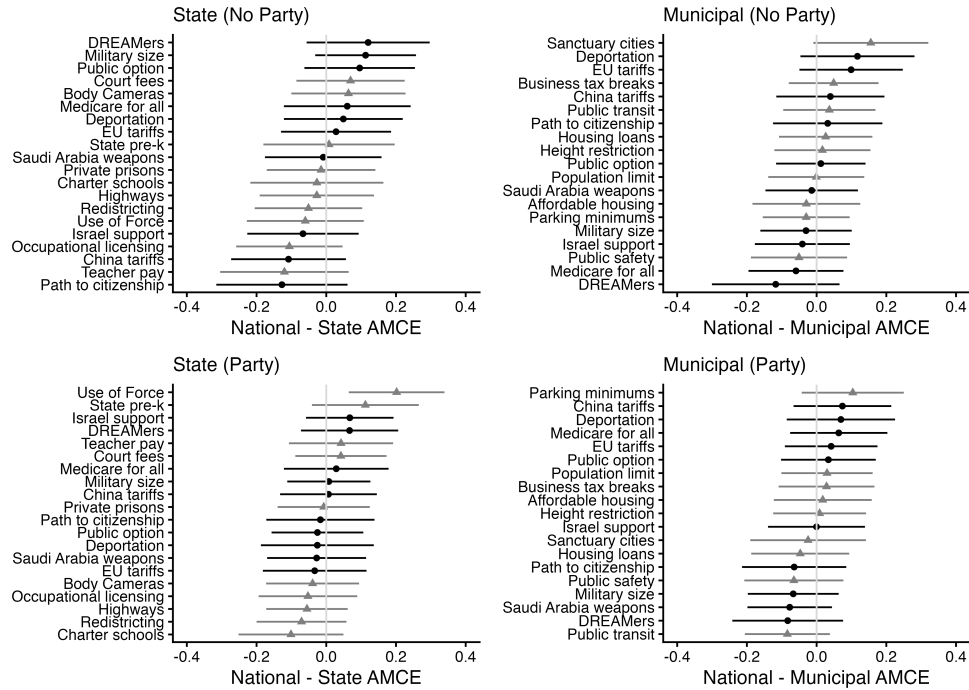


Figure A.5: **Difference in Office Condition AMCE for Partisan Respondents:** Subsetting to partisans and conditioning on the affirmative setting of the policy rather than policy agreement, we still observe almost no policies whose effects differ depending on the office condition presented to the respondents.

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 (dis)agreeing with the positive setting of the policy (which can be either conservative or liberal in ideological terms).

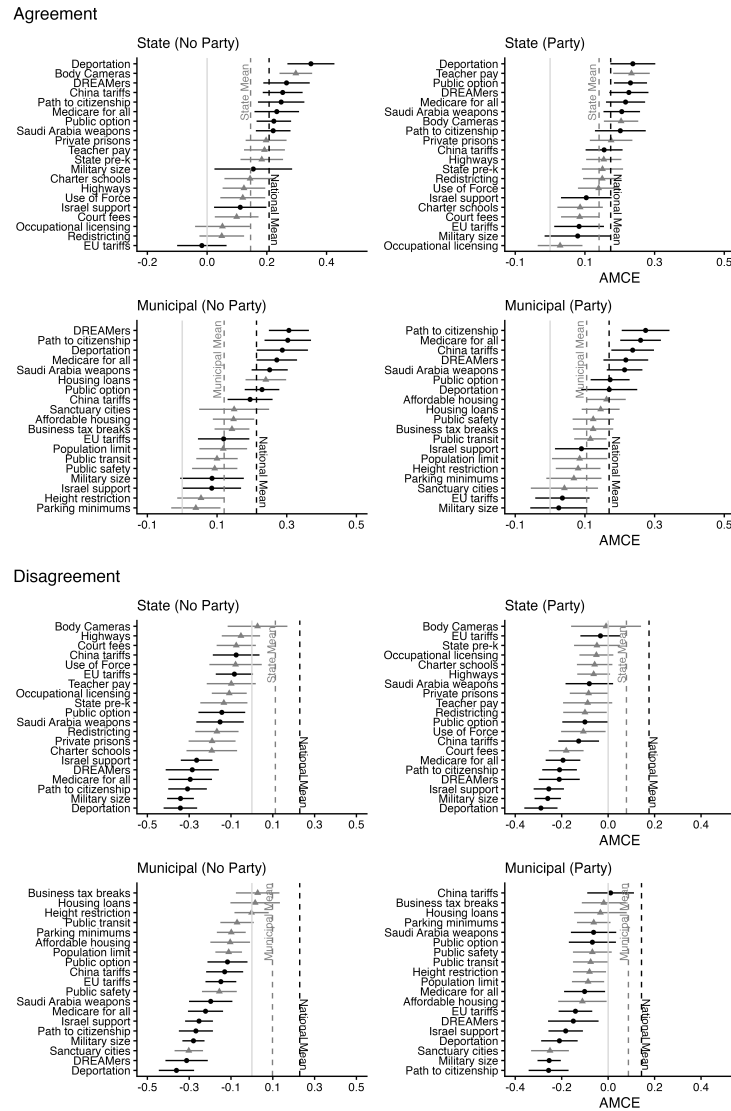
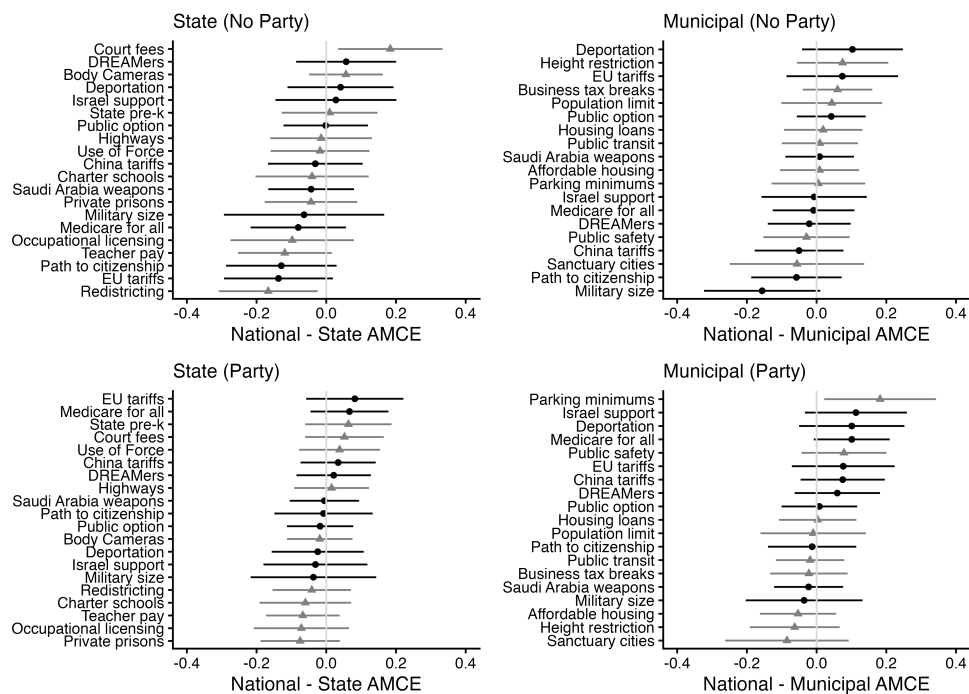


Figure A.6: **AMCE Conditional on Policy (Dis)Agreement:** In our preferred specification, we condition calculations on the respondent agreeing (disagreeing) with the setting chosen for a given policy. Here, we show the results of conditioning on the respondent (dis)agreeing with the affirmative setting of the policy (which can be either conservative or liberal). This figure depicts comparable results to our main figures.

Agreement



Disagreement

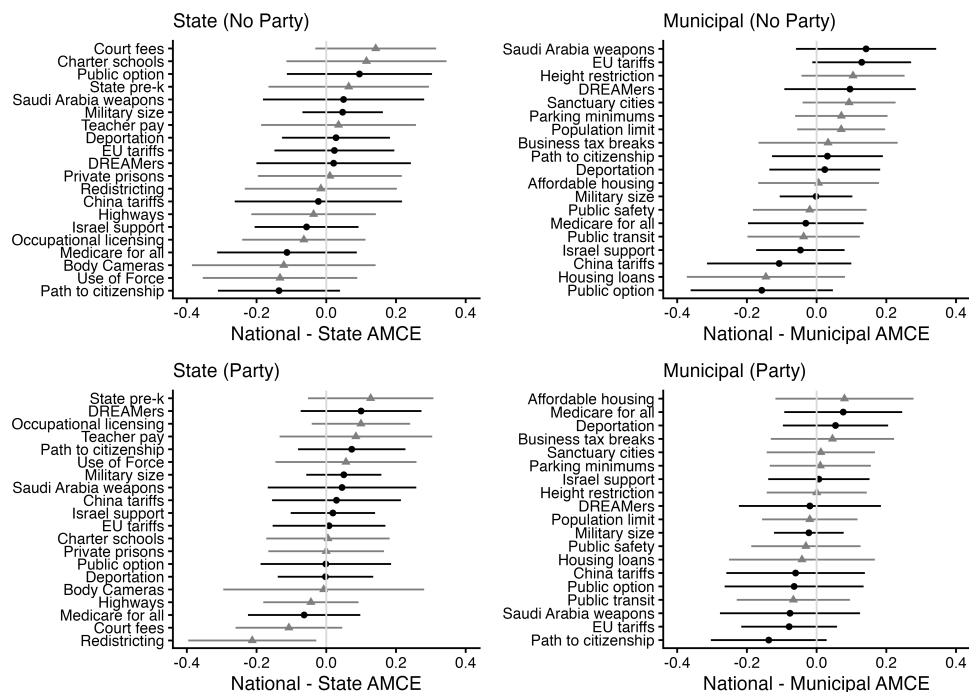


Figure A.7: **Difference in Office Condition AMCE Conditional on Policy (Dis)Agreement:** In our preferred specification, we condition calculations on the respondent agreeing (disagreeing) with the setting chosen for a given policy. Here, we show the results of conditioning on the respondent (dis)agreeing with the affirmative setting of the policy (which can be either conservative or liberal). This figure depicts comparable results to our main figures.

4 Partisan Attenuation of Policy Effects

In this section, we assess how the magnitude of AMCEs change 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.

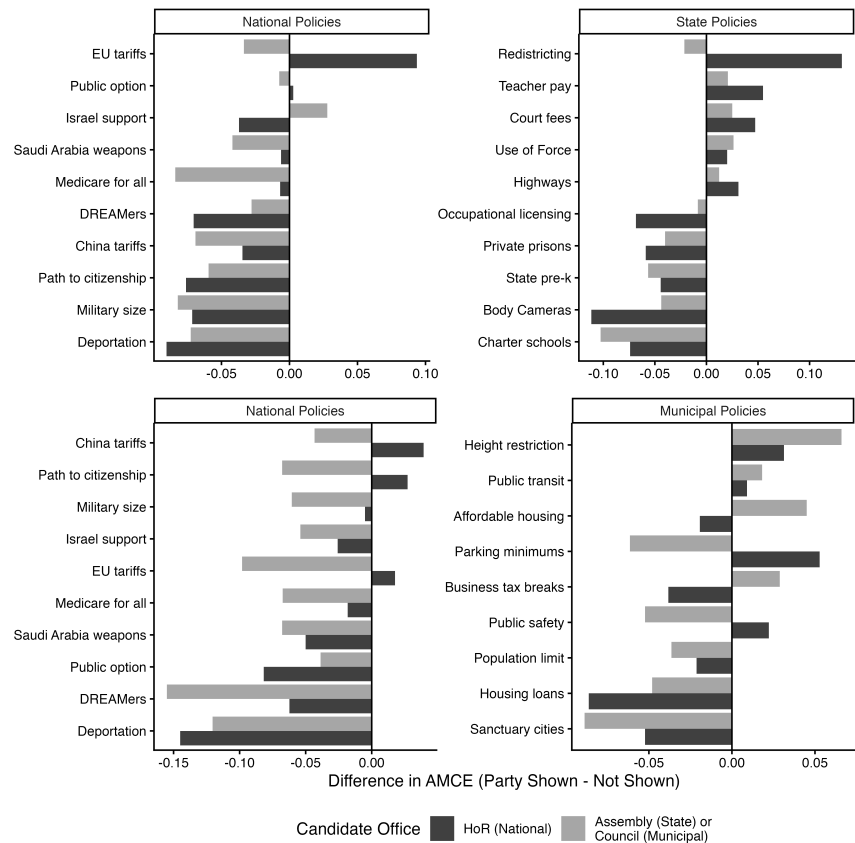


Figure A.8: **Change in AMCE with Party Label Inclusion:** This figure depicts the difference in magnitude of AMCEs by policy across party label conditions; in effect, the extent to which the significance of a policy increases (decreases) in the presence (absence) of party labels. Policies generally convey somewhat less information in the presence of party labels, but patterns vary by policy. Support for deportation of undocumented immigrants varies most sharply.

5 Partisan Signal Intensity

5.1 Pooled Results

Here, we present a version of Figure 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.

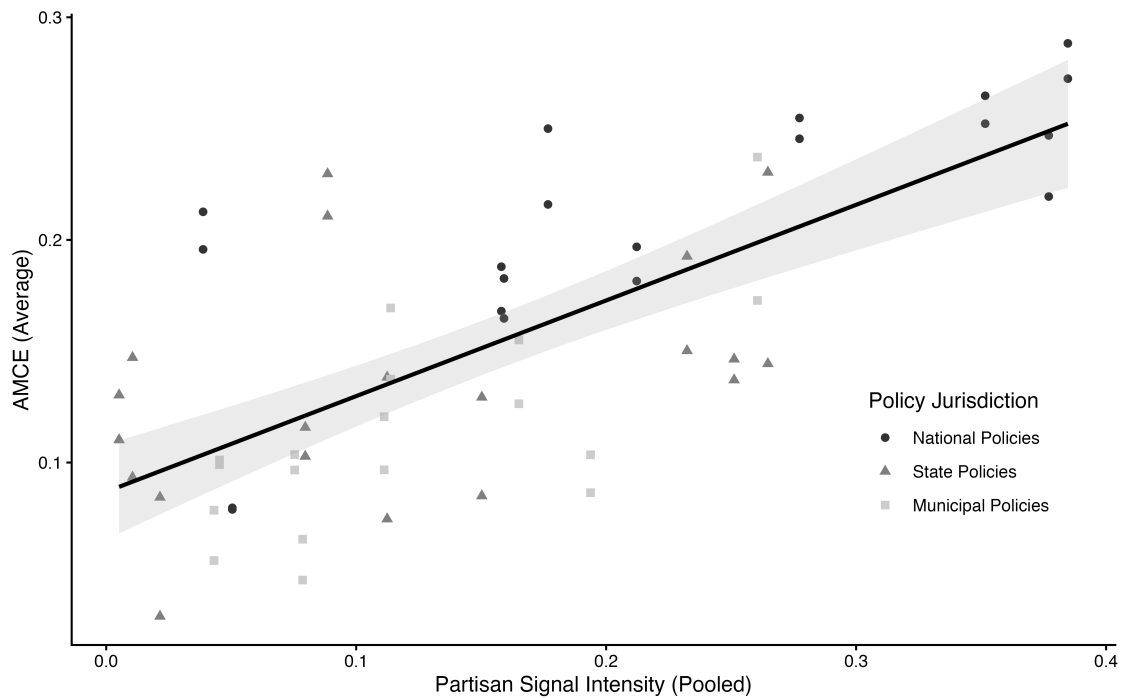


Figure A.9: **AMCE versus Partisan Signal Intensity, Pooled:** In our primary results we report the relationship between AMCE (effect size) and Partisan Signal Intensity (a measure of the magnitude of the gap in support for the affirmative version of a policy between respondents of both parties) faceted by policy jurisdiction. Here we report the results pooled; the slope and sign of the relationship are comparable.

5.2 Results by Office and Partisan Conditions

Here, we show the disaggregated hypothesis tests making up the content of Figure 4. Save one instance (non-party, state), all conditions demonstrate a significant and positive relationship between partisan signal intensity and AMCE.

Table A.3: Effect of Partisan Signal Intensity on AMCE, by Party and Office Conditions

Party ID Shown?	Office Condition	Policy Type	Slope Estimate	Standard Error	p-value
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

6 Weighting

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 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 Appendix Figure A.10. We offer versions of our main result with unweighted respondents in Appendix Figures A.11 and A.12, noting that our findings are unchanged.

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.

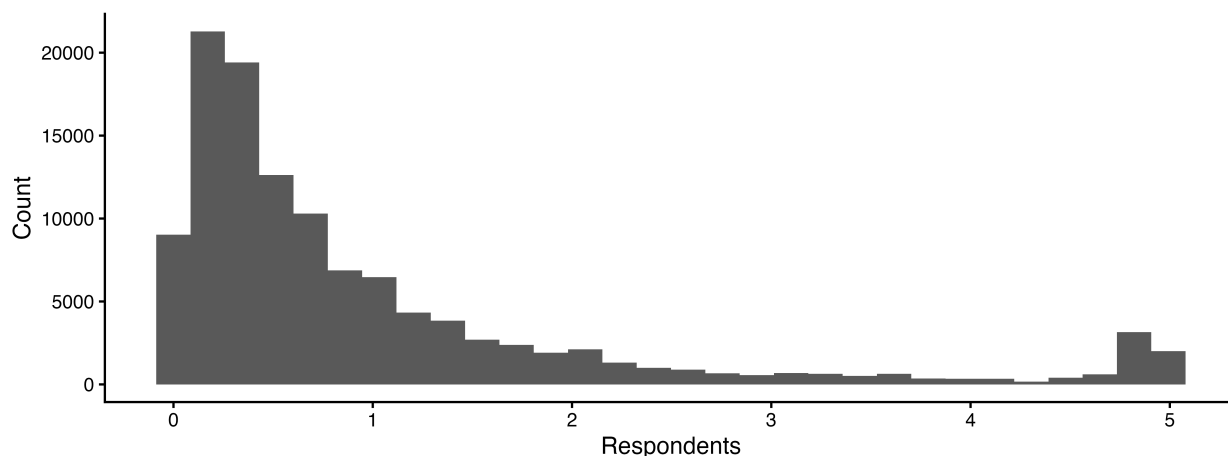


Figure A.10: **Respondent Weight Assignments:** We assign respondent weights through raking, restricting respondents to a maximum weight of 5. Because the maximum weight is 5 and weights are scaled to be mean 1, respondents can receive arbitrarily low near-zero weights. This figure depicts a histogram of weight distributions, showing that although some respondents were pushed to extreme weights, they make up relatively little of our sample.

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 measuring 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 over-estimation 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.

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).

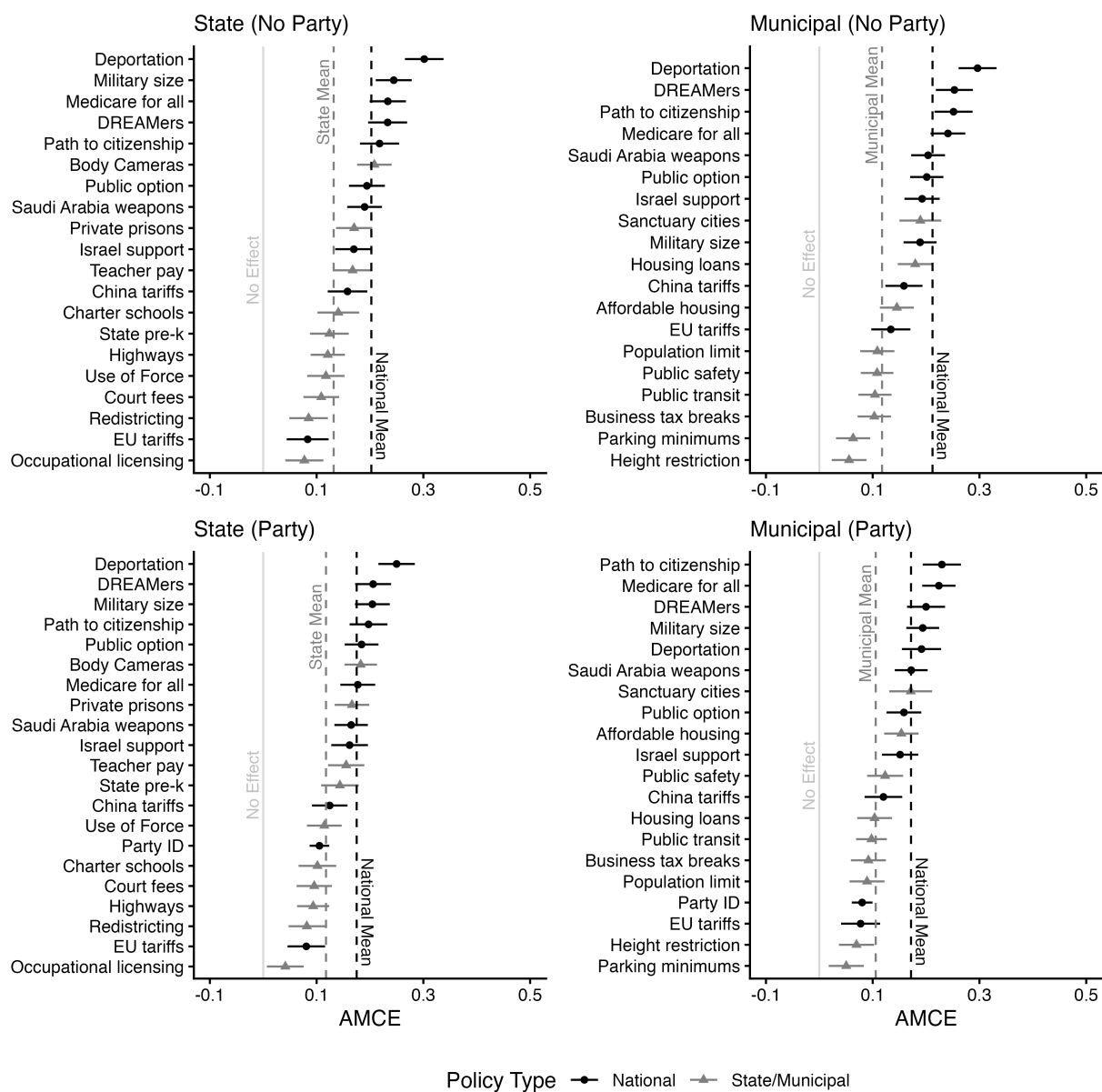


Figure A.11: **Unweighted version of main AMCE results:** Our survey consists of a sample drawn via quota sampling from a nationally representative pool. In our primary results, we use weighting to achieve sample representativeness on a wider variety of demographic features. In this plot, we repeat our main results without weighting observations, and demonstrate that our results are not driven by our weight targets.

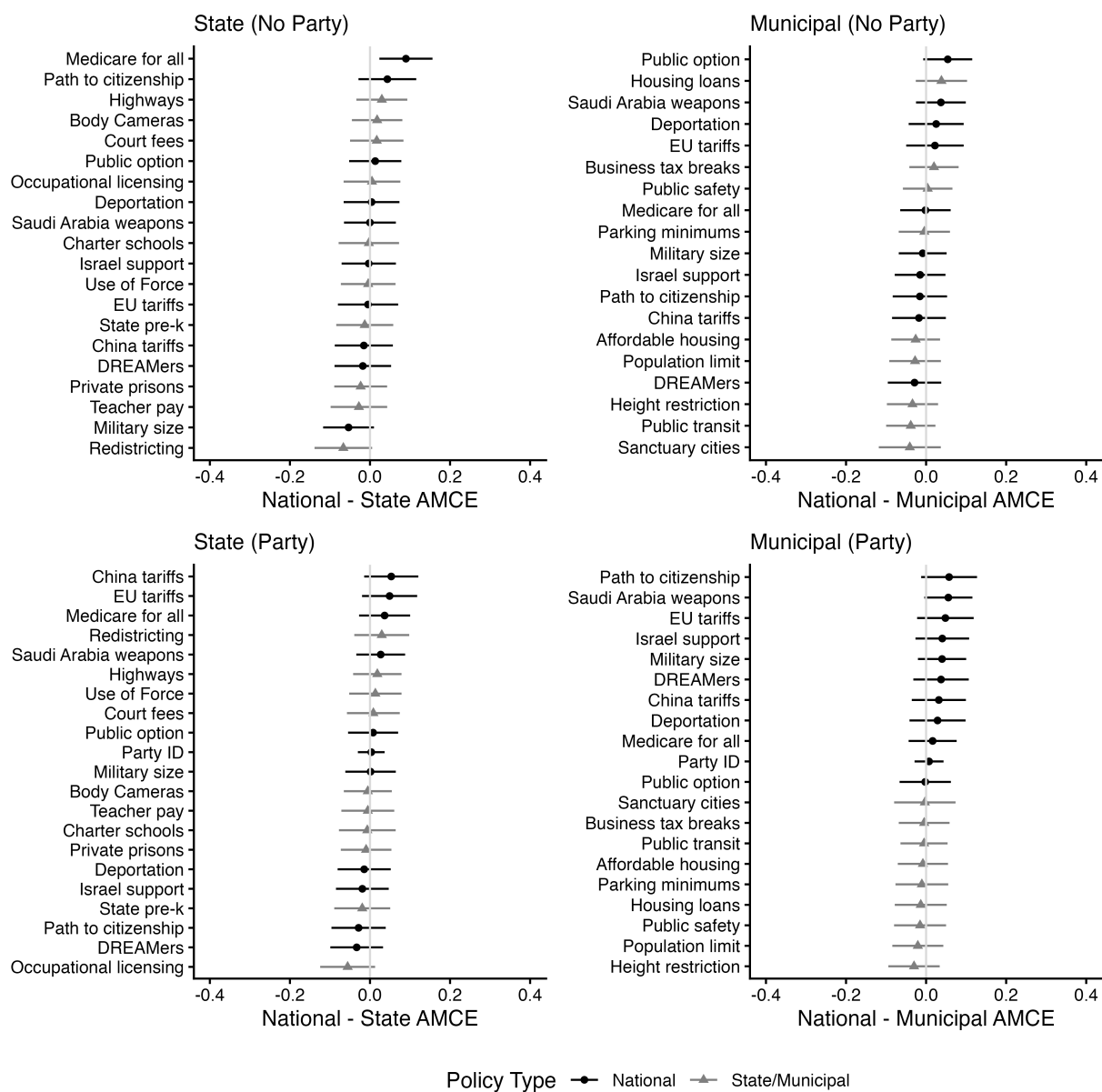


Figure A.12: **Unweighted version of main AMCE difference results:** Our survey consists of a sample drawn via quota sampling from a nationally representative pool. In our primary results, we use weighting to achieve sample representativeness on a wider variety of demographic features. In this plot, we repeat our main results without weighting observations, and demonstrate that our results are not driven by our weight targets.

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 (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 individual conjoint experiments 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 the top rows of the figures below. 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 individual conjoint experiments 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 the bottom rows of the same figures.

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

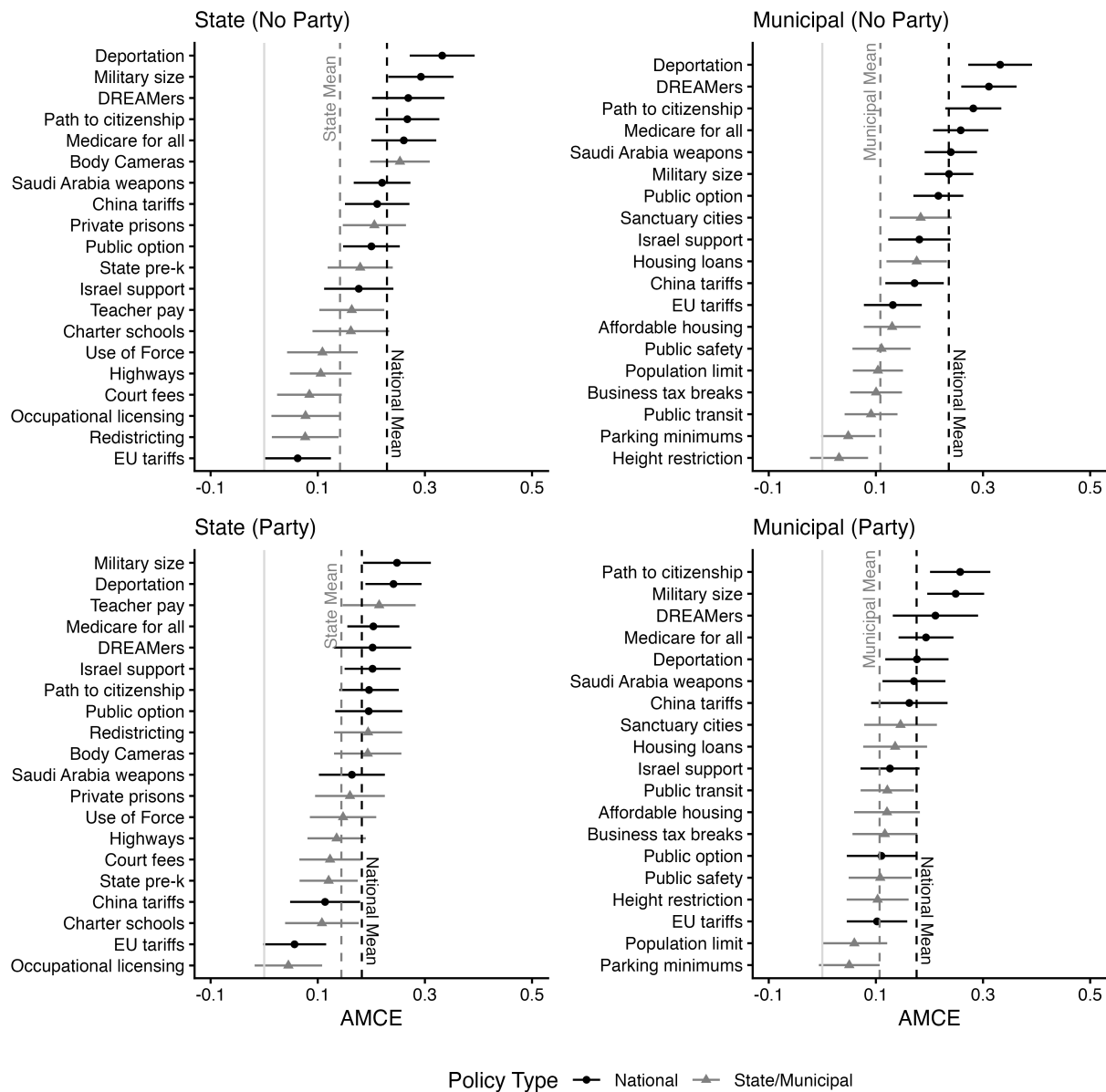


Figure A.13: **AMCE, No Policy or Partisan Mismatch:** To guard against concerns that some conjoint prompts would contain improbable policy combinations or clashes between the randomly assigned party labels and preferred policies. In this figure, we eliminate such cases, leaving only cases that display partisan coherence. The results do not substantively differ from our main findings, though confidence intervals are wider owing to dramatically reduced sample sizes.

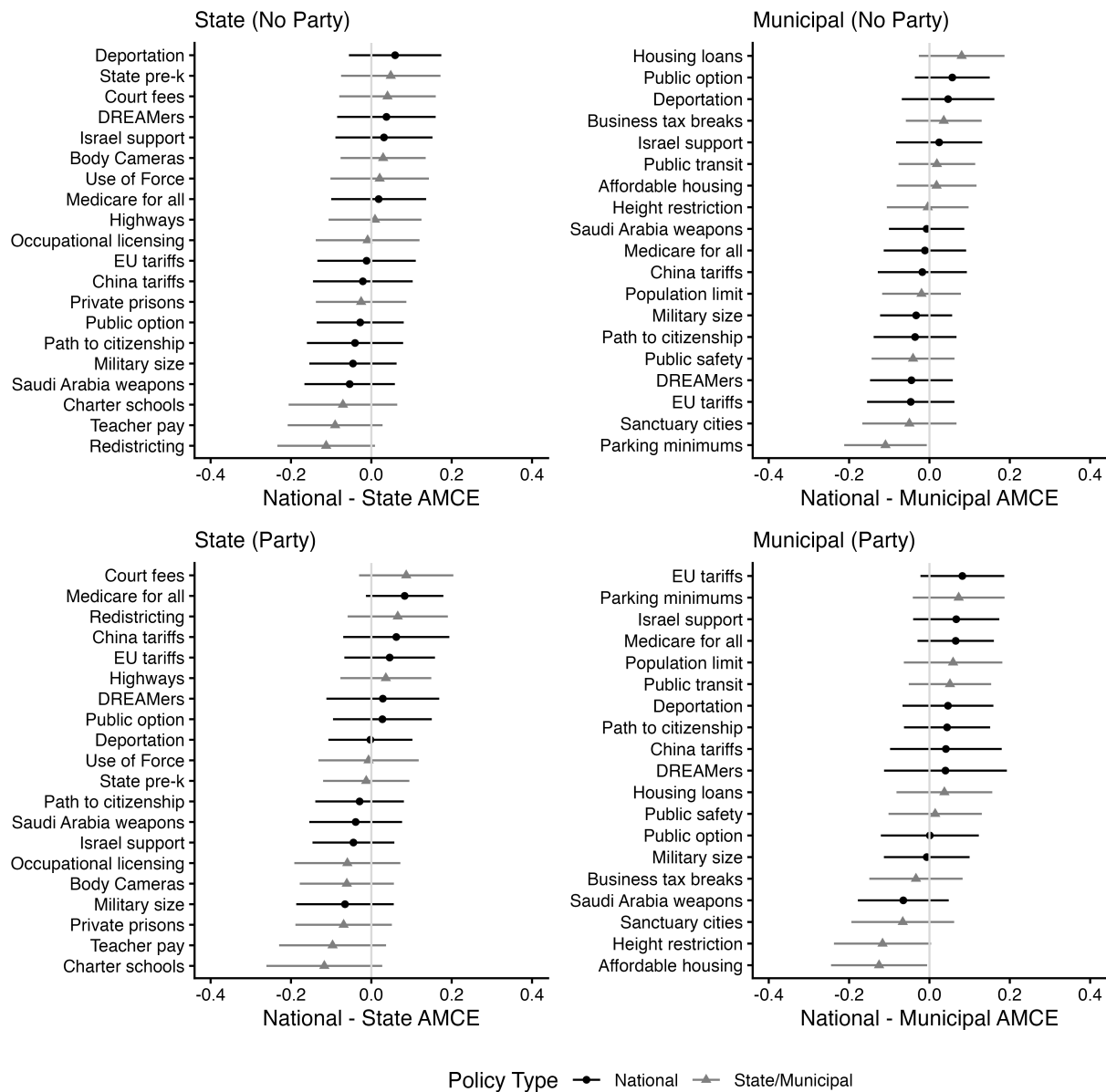


Figure A.14: **Office Condition AMCE Differences, No Policy or Partisan Mismatch:** To guard against concerns that some conjoint prompts would contain improbable policy combinations or clashes between the randomly assigned party labels and preferred policies. In this figure, we eliminate such cases, leaving only cases that display partisan coherence. The results do not substantively differ from our main findings, though confidence intervals are wider owing to dramatically reduced sample sizes.

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.¹

1. Note 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.

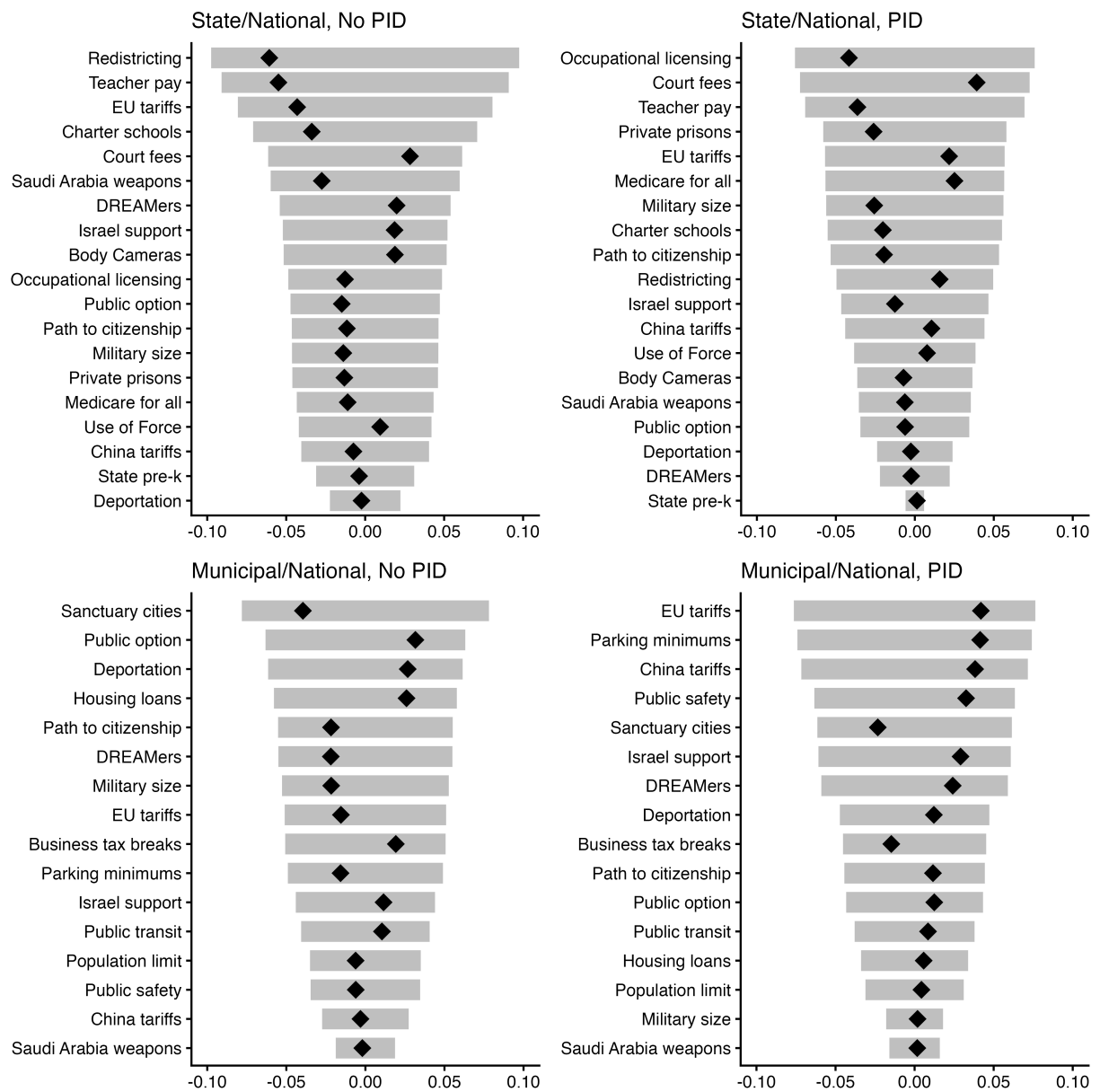


Figure A.15: **Equivalence Tests of AMCE by Office Condition:** Equivalence tests are an alternative hypothesis test that invert the assumptions of traditional hypothesis testing in order to create bounds for plausible effect sizes, in this case differences in AMCE by office condition. The depicted equivalence ranges reflect our observed results – in general, there is no substantive difference between office conditions.

9 Sensitivity to Subsample-Policy Fit Concerns

In Appendix Section 7 above, we discuss the concern that combinations of issue profiles may be implausible and undermine the believability of the conjoint. Here, by contrast, we address the concern that *particular issues* may be systematically invalid or irrelevant for particular subsets of respondents. There are infinitely many possible subsample-policy interactions; for brevity, we explore two such concerns here.²

9.1 Urban Respondents

Our chosen set of municipal policies reflect some of the major policy debates in urban areas. Respondents who live outside urban areas may not find these policies salient. We explore whether our findings on municipal policies are driven by non-urban voters, by subsetting our respondents to those reporting urban ZIP codes according to NCES ZCTA Locale Assignments and re-report our results (unweighted). We observe that our findings are statistically and substantively unchanged.

9.2 Redistricting Commission

One of our state issues, “*Create a non-partisan redistricting commission for the drawing of electoral boundaries*”, may be impacted by the fact that several states (Arizona, California, Colorado, and Michigan) already have such redistricting commissions. We re-analyze our data, subsetting respondents to only those who reside in states without non-partisan redistricting commissions at the time of our survey. Again, we observe that our findings are statistically and substantively unchanged.

A wider set of states may present concerns for this policy – several states have at-large

2. We received additional feedback that policing issues may more accurately be considered to be local issues; and that the functional responsibility for teacher pay varies by location. We ran analyses omitting all conjoint prompts with policing issues as one of the policies; omitting all conjoint prompts with teacher pay issues as one of the prompt; and partitioning the sample into the South/non-South respondents. Our results are substantively unchanged, and we omit figures for these analyses for brevity.

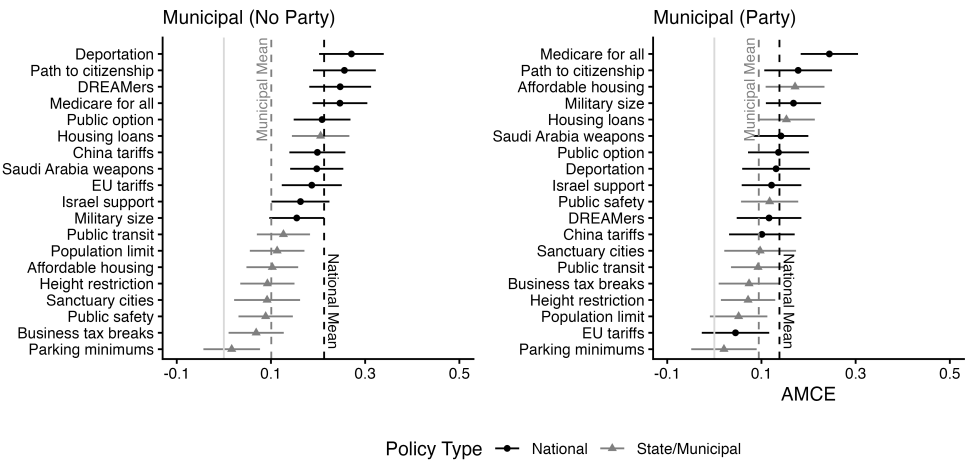


Figure A.16: **AMCE (conditional on policy agreement) for respondents living in urban areas only:** Because respondents living in non-urban areas may not feel that our choice of municipal policies are relevant to their experience, we re-analyze our results dropping all respondents living outside urban areas. The results are substantively and statistically unchanged: national issues still dominate municipal issues.

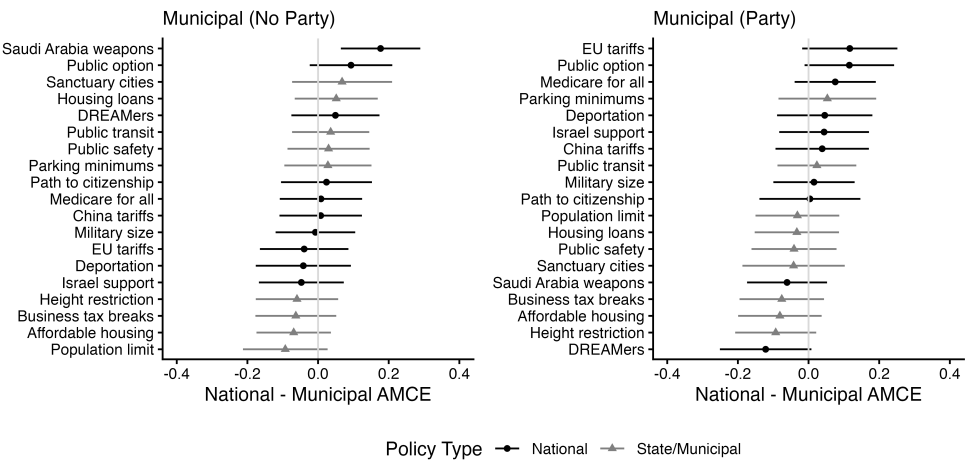


Figure A.17: **Difference in office condition AMCE for respondents living in urban areas only:** Because respondents living in non-urban areas may not feel that our choice of municipal policies are relevant to their experience, we re-analyze our results dropping all respondents living outside urban areas. The results are substantively and statistically unchanged: national issues still dominate municipal issues.

districts; several more have court-supervised redistricting; several more have redistricting commissions that are independent of the legislature but also partisan. Again, we re-analyze our data excluding respondents in this wider category (retaining only those respondents in

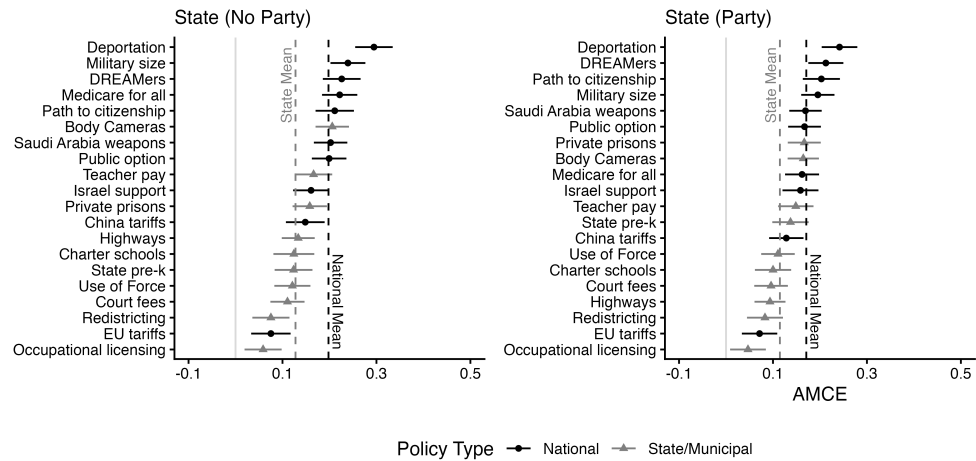


Figure A.18: **AMCE (conditional on policy agreement) for respondents living in states without non-partisan redistricting commissions:** In this figure we discard all respondents who live in four states that had already adopted non-partisan redistricting commissions in order to verify that an interaction between the redistricting policy and subsamples of our respondents is not driving substantive findings. Neither the AMCE for redistricting nor the broader results show any change.

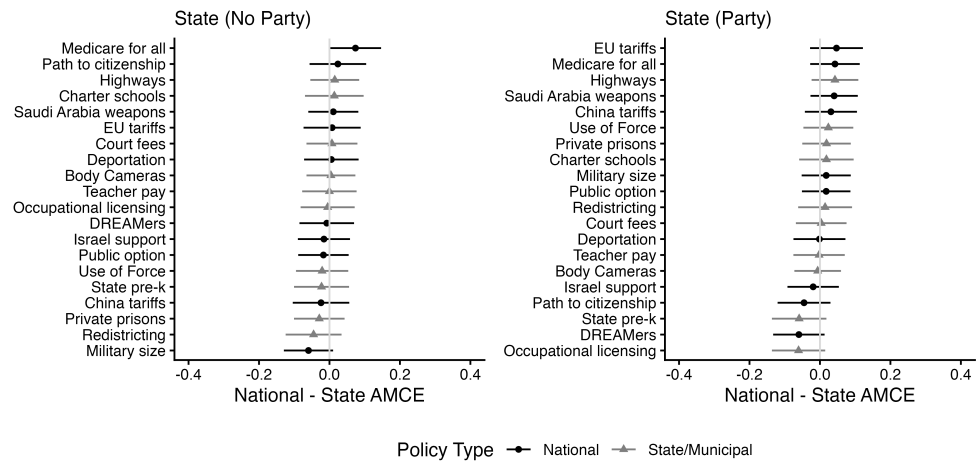


Figure A.19: **Difference in office condition AMCE for respondents living in states without non-partisan redistricting commissions:** In this figure we discard all respondents who live in four states that had already adopted non-partisan redistricting commissions in order to verify that an interaction between the redistricting policy and subsamples of our respondents is not driving substantive findings. Our findings do not change. Although the relative rank of the redistricting policy changes across party label conditions, both differences overlap 0 (indicating that redistricting, like our other policies, is not considered more germane in voting for federal office than state office).

states with traditional legislature-driven redistricting) and find the results unchanged.

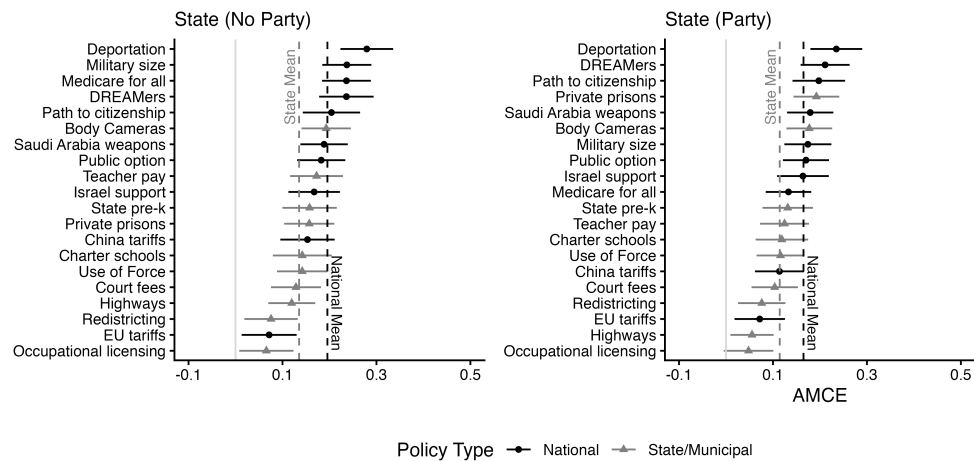


Figure A.20: **AMCE (conditional on policy agreement) for respondents living in states with explicitly partisan redistricting setups:** In this figure we discard all respondents who live in states lacking explicitly partisan redistricting setups to verify that an interaction between the redistricting policy and subsamples of our respondents is not driving substantive findings. Our results are unchanged.

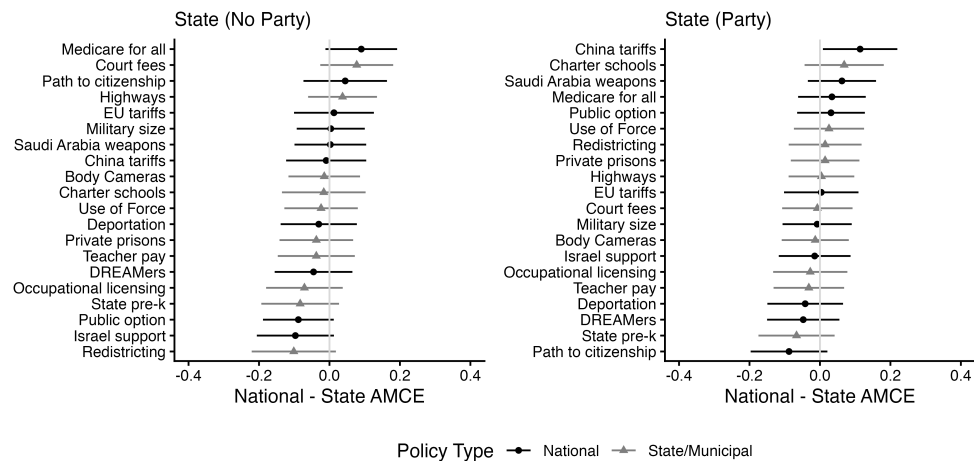


Figure A.21: **Difference in office condition AMCE for respondents living in states with explicitly partisan redistricting setups:** In this figure we discard all respondents who live in states lacking explicitly partisan redistricting setups in order to verify that an interaction between the redistricting policy and subsamples of our respondents is not driving substantive findings. Our findings do not change. The relative rank of the redistricting policy changes across partisan label conditions, but both differences overlap 0.

10 Randomizing Assistance in Identifying Jurisdictional Levels

On May 23, 2025, we fielded an additional wave of our survey to assess whether providing respondents access to the jurisdiction level labels – e.g., specifically telling respondents that immigration policy is the purview of the federal government – may artificially influence how respondents evaluated each policy. In effect, in an effort to create a “hard test” for nationalization and “stack the deck squarely against nationalized behavior,” we may have made it too easy for respondents to appear to be responsible federalists. In real life, candidates engage in evasion and obfuscation, claiming responsibility over issue portfolios they have little control over (or vice versa), and policy domains are not always cleanly divided between levels of government. In this appendix, we report the impact of experimentally varying access to this information.

In specific, we deploy a lean version of our survey to 1,477 respondents, quota-sampled to match national demographic targets, using the survey provider CloudResearch Connect.³ All respondents were given a simple attention check.

Owing to resource constraints, we restrict this survey wave in several ways that differ from our main results: in this wave, we focus solely on the comparison between national and municipal policies (on the assumption that these are the most disjoint from one another); we reduce the set of policies used in the conjoint to 6 national and 6 municipal policies; we reduce the number of tasks per respondent to 8 (4 national office races, 4 municipal office races). We suppress party ID (but see above generally for our findings that the inclusion of party ID labels does not materially affect our results). We made these changes to maximize our power to investigate the above claim. All other features, including restrictions on randomization,

3. This survey provider and these quotas differ from the original survey waves due to changes in the provider landscape in the intervening period. Without engaging in a more detailed post-mortem of every possible survey difference, we caution readers to avoid formally pooling these results with our main research and instead suggest they simply be employed for the purpose they were conducted: to assess the hypothesis experimentally varied.

mirror the survey.

In this wave, respondents are randomly assigned to one of two **assistance conditions**. In the control condition, no jurisdictional information is shown next to each of the policies. In the treatment condition, such jurisdictional information is shown, replicating our original design. In sum, we observe no substantive or statistical difference between respondents who received the treatment assistance condition.

Table A.4: Effect Differences by Assistance Condition

	<i>Dependent variable:</i>		
	Profile Selected		
	Treatment	Control	Pooled
Policy Agreement	0.229*** (0.008)	0.245*** (0.008)	0.245*** (0.008)
Correct Jurisdiction	−0.021*** (0.006)	−0.013** (0.006)	−0.013** (0.006)
Assistance			0.008 (0.006)
Agreement × Correct Jur.	0.043*** (0.012)	0.029** (0.012)	0.029** (0.012)
Agreement × Assistance			−0.016 (0.012)
Correct Jur. × Assistance			−0.008 (0.009)
Agreement × Correct Jur. × Assistance			0.015 (0.017)
Observations	38,638	40,794	79,432
R ²	0.063	0.067	0.065
Adjusted R ²	0.063	0.067	0.065
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		

We first formally test whether being assisted with jurisdictional information has any effect on respondent behavior across all policies. To do so, we format our data such that each observation is a respondent–conjoint–profile–setting. We regress the dependent variable (whether the respondent selected this profile) on a binary indicator for policy agreement; whether the policy correctly matches the office condition; and a binary indicator for the

assistance condition, interacting each. As shown in Table A.4, the assistance condition has no effect on any behavior. While we do observe a significant effect for the interaction between agreement and correct jurisdiction, the magnitude remains roughly 1/10th that of policy agreement, confirming our prior findings that policy preferences trump jurisdictional considerations regardless of office.

We then consider whether the assistance condition affects behavior with regard to any single policy item. These results are shown in Figures A.22 and A.23. Again, we observe no significant differences by assistance condition for any individual policy. The original findings that (1) national policies have greater effects than municipal policies and (2) there is no difference in AMCE between office conditions persist in this wave as well.

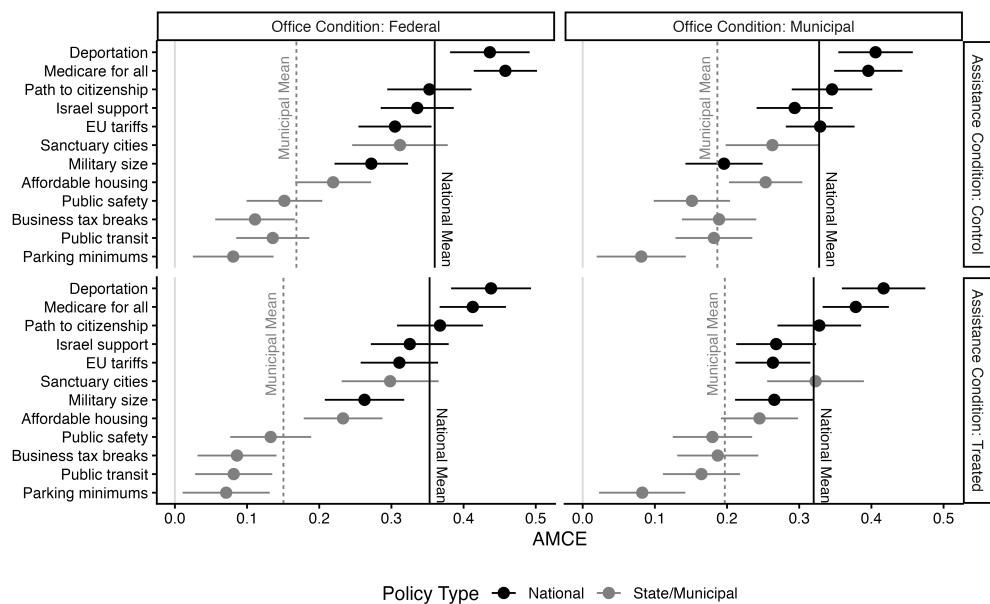


Figure A.22: **AMCE (conditional on policy agreement) for additional wave comparing assistance conditions:** National policies show broadly higher AMCEs than municipal policies; there is no difference in AMCE by office condition; and AMCEs do not differ between assistance conditions

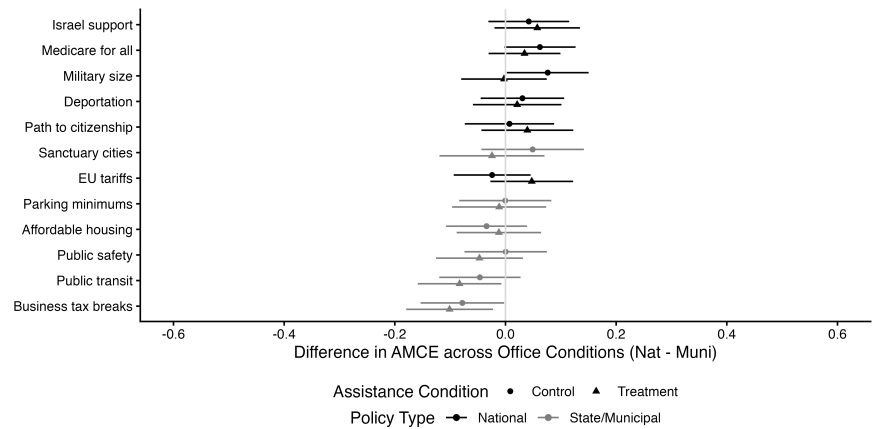


Figure A.23: **Difference in office condition AMCE for additional wave comparing assistance conditions:** This figure conveys two key findings – AMCEs do not differ across office conditions (respondents place equal emphasis on policies germane to the position they are voting for as they do to non-germane policies); and this pattern is observed across both assistance conditions, indicating it is not driven by our choice to show respondents the “correct” office for the policy.

11 Pre-Registration

The 2022 survey wave is pre-registered. We observe no major deviations from our pre-registration. The 2021 survey wave, which served as a pilot, was not pre-registered. The additional survey wave, described in Supplementary Appendix 10, although consistent with the main survey design, was not pre-registered and should be evaluated as exploratory.

Anonymized pre-registration materials can be found at:
https://osf.io/asvmn/?view_only=ba0dfdccf32e4b1f81902517c6e12992.

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