Seeing Your Representative: Which Accountability Channels Does Local Television Activate?

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Abstract

Does local television make representatives more accountable? The question presumes a single answer, but accountability runs through distinct evaluative channels, and congruent coverage converts information into accountability only under two conditions: local television must carry representative-specific content on that dimension, and the voter's partisanship must leave that content something to add. Using Cooperative Election Study respondents from 2006 to 2024 and redistricting-induced variation in district–media-market congruence, I find no single average effect of congruence on representative approval, but a sharp sorting across channels. Congruence sharpens ideological evaluation, which television conveys and which the party label leaves underspecified for co-partisans; it channels evaluation of district-targeted spending through partisanship, so cross-partisans punish where co-partisans do not; and it leaves four valence channels — unemployment, crime, legislative effectiveness, scandal — dormant, where television supplies no representative-specific basis for accountability. Media-enabled accountability is selective by construction: congruence determines *which* dimensions of representation voters police; it does not strengthen accountability *uniformly*.

Keywords: electoral accountability; local television news; congressional elections; media market congruence; partisan-mediated accountability

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1 Introduction

Most Americans recognize their House representative but know little about what she has done in office. Across the 2008–2024 waves of the Cooperative Election Study, roughly seven in ten respondents reported having heard of their member of Congress; yet, as Snyder and Strömberg (2010) document, only about half can name a single thing they like or dislike about her, and fewer than a third can recall her name without prompting. This informational deficit has long troubled democratic theorists — if voters do not know what their representative has done, how can elections hold her accountable? — and the nationalization of American politics deepens it, as House races increasingly become referenda on the president rather than evaluations of the local incumbent (Rogers, 2023). In 2016–2018, Americans took in roughly five times as much news from television as online, by time spent (Allen et al., 2020), and local television stations remain today the most widely used outlet for local news, reaching about two-thirds of adults (Shearer, 2024). The coverage a representative receives is far from uniform across districts, however. Local television broadcasts to Designated Market Areas whose boundaries follow signal propagation and commercial viewership, not political geography: when a DMA encompasses a single congressional district, that representative becomes newsworthy to the whole audience; when it spans a dozen, none commands enough audience share to justify regular coverage (Huber and Tucker, 2024). This geographic accident determines whether a voter's television offers any window into her representative's activities.

A growing literature uses this kind of media–political geographic alignment to study electoral accountability. Snyder and Strömberg (2010) demonstrate that newspaper coverage of congressional representatives is greater in congruent districts and that this coverage increases voter knowledge and incumbent vulnerability. Balles et al. (2022) extend the logic to television markets. Myers (2025) shows that the pattern holds for state legislatures, and Moskowitz (2026) extends the television-market design to U.S. Senate elections, where idiosyncratic DMA boundaries situate some voters in out-of-state markets and others in in-state markets. Ferraz and Finan (2008) document a parallel mechanism through audit-based radio exposure in Brazilian municipalities. Across institutional contexts, media congruence with political boundaries improves voter information.

What that improvement *amounts to*, however, remains underspecified. The literature establishes that congruence improves voter knowledge and strengthens electoral accountability in the aggregate; it has had less to say about the composition of that accountability. I ask a more specific question. When TV congruence increases voter information about a representative, *which* evaluative channels does that information actually activate? Voters might use the new information to evaluate ideology, to attribute economic conditions, to reward legislative productivity, or to track district-targeted spending. They might activate the same channels uniformly, or different channels in different ways for different voters. The answer determines what kind of representation media-enabled accountability promotes, and whether the channels that get amplified are the ones democratic theory expects.

My argument turns on what congruent coverage can and cannot do. Congruence raises the supply of information about a representative, but information reshapes an evaluation only when two conditions hold: local television must carry representative-specific content on the relevant dimension, and the voter's partisanship must leave that content something to add. Television readily conveys where a member stands — positional information, in Stokes (1963)'s sense — which the party label leaves underspecified, so congruence sharpens ideological evaluation; it supplies little that would let a voter hold her responsible for local unemployment, crime, or legislative productivity — the valence dimensions — so those stay dormant however much coverage congruence produces. One dimension sits between these cases. District-targeted assistance spending is visible, because representatives claim credit for it, but its evaluative sign is set by partisanship: following Bartels (2002) and Achen and Bartels (2016), a cross-partisan reads programmatic spending under the opposing party as waste and punishes it, while a co-partisan reads the same dollar as constituency service. The logic that leaves the valence dimensions dormant thus implies an active but partisan-mediated response for spending.

I test these implications on Cooperative Election Study respondents from 2006 to 2024 (N $\approx$ 395,000 individuals, 2,978 counties, 451 congressional districts) using temporal within-county variation in TV market–district congruence induced by decennial redistricting. The empirical strategy follows the within-county-and-year specification that survives my balance test described in §3 (and that corresponds closely to Snyder and Strömberg (2010)'s most conservative redistricting-based design). I jointly estimate five candidate moderating channels in a single specification — ideological distance based on DW-NOMINATE first-dimension scores, district-targeted federal assistance spending per capita, local unemployment rate, local violent crime rate, and a member-level scandal indicator — and estimate two further measures in parallel specifications: ideological distance based on the Fowler and Hall (2012) Conservative Vote Probability and legislative effectiveness scores. The patterns are sharp.

Both ideology measures produce significant negative interactions with congruence on representative approval that survive joint inclusion of all channels and the addition of political controls; the approval response concentrates among co-partisan voters, for whom the party label conceals within-party positional variation, and the same co-partisan concentration appears at the ballot box. The federal-spending channel splits in two: the assistance subtype (grants, transfers, direct aid to district recipients) produces a significant negative interaction that is voter-asymmetric — cross-partisan punishment alongside a null co-partisan response, concentrated among Republican voters facing Democratic incumbents — while the procurement subtype (defense and infrastructure contracting) is null throughout.

The four valence channels — unemployment, crime, legislative effectiveness, and the pooled scandal indicator — are wrong-signed or null in every specification. One bounded exception emerges from a sub-channel decomposition of scandal: sexual-misconduct allegations produce a strongly active response in pre-2018 cycles but a null response after the 2017–2018 #MeToo wave, the signature of a valence channel that activates only when the underlying signal is informationally clean and specific to the representative.

I make two substantive contributions to the study of media and democratic accountability. First, *channel selectivity* is the substantive feature of media-enabled accountability, not its limit case. TV congruence does not amplify voter accountability along every dimension; it amplifies positional and partisan-mediated channels and leaves valence channels dormant. Anchored on the magnitude of the ideology effect, valence channel point estimates range from 5 to 29 percent of the positional channel, and even at the upper bound of what my power can rule out, none exceeds 72 percent of positional — two of four lie at half or below. The contrast is robust, not point-null.

Second, the *partisan-mediated mechanism for distributive spending* is a novel finding for the media-accountability literature: a channel that registers as substantively active in pooled estimation but operates entirely through cross-partisan voters, with co-partisan voters dormant. I am not aware of a prior media-accountability study that has decomposed a substantive channel along the partisan-mediated dimension. Anchored in Bartels (2002) / Achen and Bartels (2016), the result fits a broader pattern where partisan filtering of policy-attribution evidence shapes how voters convert factual signals into electoral judgments.

The voter-knowledge first stage underwriting both contributions is not unique to my data: Snyder and Strömberg (2010)'s NES 1982–2006 newspaper-congruence-to-readership finding, my 2008–2024 CES TV-congruence-to-MC-recognition finding, and Myers (2025)'s 2018 CES state-legislator-recall finding all survive a compositional progression of controls on their own data, across three measurement strategies, periods, and outcome variables (§3).

One disclosure frames everything that follows. My pooled congruence-on-approval estimate is zero under my preferred specification: within county-and-year fixed effects, TV congruence is nearly collinear with incumbent party, so the specification includes voter–incumbent partisan-match and incumbent-party indicators as additional controls (§3, §4). The channel findings above are identified by congruence-by-moderator interaction terms. They survive the addition of these compositional controls and their moderator interactions in sign and significance, though the ideology interaction attenuates by roughly two-thirds under that maximally demanding test (§4; Appendix A.5).

2 Theory: Information, Evaluation, and Channel Selectivity

Electoral accountability requires that voters acquire information about their representative, use that information to form evaluations, and translate those evaluations into electoral consequences. The first link in this chain is well established. Snyder and Strömberg (2010) demonstrate that newspaper market congruence with congressional districts increases voter knowledge of House members, measured by name recognition and ability to recall representative characteristics. Hayes and Lawless (2015) show that the decline of local news coverage reduces political engagement in House elections. Subsequent work extends the congruence–knowledge finding to television markets (Nordin, 2015; Balles et al., 2022; Moskowitz, 2026) and to state legislatures (Myers, 2025). The informational channel is robust across media types, levels of government, and time periods.

The second and third links remain underspecified. Knowing that information increases accountability "in general" tells us little about *which dimensions* of accountability information activates. Different evaluative channels carry different implications for representation. If media primarily enables voters to monitor economic performance, the consequence is retrospective accountability of the type theorized by Fiorina (1981). If media primarily enables ideological evaluation, the consequence is representational alignment between voter preferences and legislator positioning, the dimension along which

Canes-Wrone et al. (2002) show House members are held electorally accountable for ideological distance. If media primarily enables monitoring of district-targeted distributive spending, the consequence is partisan-mediated reward-and-punishment of clientelist coalition-building. These are distinct forms of accountability with distinct democratic functions, and they need not be activated as a bundle.

Stokes (1963)'s positional/valence taxonomy organizes the generic candidate channels. The distinction separates evaluative dimensions where voters and representatives can disagree about direction (ideology, abortion, gun control) from dimensions where voters share a common preference and judge representatives by competence or outcomes (economic performance, crime conditions, legislative productivity, freedom from scandal). The distinction matters here because the two kinds of dimensions place different demands on the information environment. Activating positional accountability requires that voters know where their representative stands relative to a set of available positions; activating valence accountability requires that voters can causally attribute outcomes to the representative. Information environments that supply the first kind of content but not the second will activate positional accountability while leaving valence accountability dormant.

Two conditions therefore govern whether congruence converts information into accountability on a given dimension. The first is a coverage condition: local television must actually carry representative-specific content on that dimension. The second is a partisan-filtering condition: carriable information moves an evaluation only when it is not already encoded in the party label and the voter's partisanship does not neutralize its content. The coverage condition separates the positional dimensions, which television conveys, from the valence dimensions, which require an attribution it does not supply; the partisan-filtering condition governs, among the dimensions coverage admits, which voters respond and with what sign.

The structure of local television markets makes certain kinds of information economically viable to produce and other kinds prohibitively costly. A station in a market whose boundaries align closely with a single congressional district faces a straightforward editorial calculation: the representative's activities are of direct interest to nearly all viewers, making House race coverage a reliable source of audience engagement during election season and a steady background story between elections. By contrast, a station whose market spans eight or ten congressional districts has no dominant district to cover; a story about any single representative interests only a fraction of the audience, reducing the expected return on journalistic investment (Strömberg, 2004; Huber and Tucker, 2024).

The volume half of this consequence is directly observable, and it holds. In broadcast-transcript records of local news from 2017–2018 (Martin and McCrain, 2019; Huber and Tucker, 2024), the congruence measure used throughout this paper predicts how often a House member actually appears on the air: a one-standard-deviation increase in congruence raises expected weekly on-air mentions of the incumbent by roughly three-quarters during the fall 2018 campaign and by roughly a quarter across the 2017 off-season windows, and congruence alone accounts for about a fifth of the cross-district variance in campaign-season coverage (Appendix K). The *content* half is not directly observable — mention counts do not reveal what the coverage says — so I state it as the account's second premise: the incremental coverage that congruence buys is primarily about political positioning — votes on salient legislation, public statements in partisan debates, town-hall appearances, reactions to national controversies. When a representative makes the local news, the argument runs, it is typically because she took a stand, not because the district's unemployment rate changed or because she shepherded a bill through committee; the channel-selectivity pattern documented in §4 is the footprint this premise should leave.

Economic conditions, by contrast, are covered through a national lens: local television attributes unemployment, inflation, and growth to presidential policy, Federal Reserve decisions, or global market forces, not to the House member's record (Song, 2022). The clarity-of-responsibility framework (Powell and Whitten, 1993) explains why — a single member's causal influence on county-level economic outcomes is inherently ambiguous, and television journalism, which trades in clear narratives, avoids such ambiguity.

Legislative effectiveness is even further from the television screen — bill sponsorship counts, committee-influence scores, and the Volden and Wiseman (2014) legislative effectiveness index are technical indicators that political scientists track but voters do not encounter in media coverage. Local crime, despite its prominence in television content, is rarely framed as a federal-legislator-attributable outcome; the implicit causal frame is local government and law enforcement, not the House member.

The coverage condition therefore sorts the channels at the outset: congruence activates the ideology channel — voters in congruent markets learn the representative's positions and can evaluate ideological distance — but not the economic, legislative-effectiveness, or crime channels, for which the content needed to attribute outcomes to the House member is absent from television coverage regardless of how much coverage congruence produces. Within the channels coverage admits, the partisan-filtering condition governs incidence: positional information moves evaluations most where the party label says least about the dimension being evaluated. For co-partisans, the label conceals within-party ideological variation, so TV-supplied positioning fills a genuine informational gap and should move approval. For cross-partisans, the label already signals ideological disagreement, so incremental positioning information is largely redundant for the approval judgment; at the ballot box, where a discrete choice forces the full information set to be weighed, both groups should respond.

Scandal is the revealing case. Unlike the other valence channels, a misconduct allegation is *representative-specific* by construction — a financial-impropriety allegation against a specific incumbent is, unambiguously, about that incumbent — so the clarity-of-responsibility ambiguity does not apply and the coverage condition can be met. But it is met only while the signal stays clean: an allegation type that is rare and individually attributable supplies a clear representative-specific signal, while one that has saturated and become partisan-coded does not. Scandal is thus a bounded test of the coverage condition rather than a separate hypothesis, and I treat the resulting era pattern (§4) as exploratory.

Federal assistance spending — direct expenditures targeted to districts through grants, transfers, and assistance programs — fits the positional/valence taxonomy awkwardly. Representatives take *active credit-claiming positions* on the spending they secure for the district, behaviors that make assistance spending visible as a position-taking dimension on which voters can locate the representative (Grimmer et al., 2014). At the same time, voters' *evaluative response* to assistance spending is filtered through partisanship in a way that pure positional-distance computation is not.

Whether a given dollar of district-targeted assistance is welcomed as constituency service or condemned as pork depends on partisan filtering of the underlying policy frame, much in the way that Bartels (2002) documents partisan filtering of economic-policy attribution. A cross-partisan voter — one facing an incumbent of the opposite party — evaluates incremental assistance spending against an inferential frame in which programmatic government redistribution under the opposing party is more likely to be coded as wasteful or as inefficient buying of district loyalty. A co-partisan voter reads the same incremental dollar as legitimate constituency service. The two frames are not mirror images in their evaluative consequence. Delivering for the district is what a co-partisan already expects her representative to do, so a dollar read as service confirms a standing expectation and leaves the evaluation where it was; a dollar read as waste is discrediting news and moves the evaluation down. The filter therefore predicts cross-partisan punishment alongside a flat co-partisan response — not the electoral reward that credit-claiming accounts of distributive spending would lead one to expect (Grimmer et al., 2014).

Bartels (2002) originally documents partisan-filter effects on retrospective evaluation of economic conditions: partisans of the incumbent's party assess identical economic outcomes more favorably than partisans of the opposing party. Achen and Bartels (2016) generalize the underlying mechanism into a broader claim that partisan loyalty conditions how voters convert factual information into political judgment, and theorize partisan loyalty as informationally-conditional — responsive to information that distinguishes the candidate from her party's stereotype — rather than as a fixed identity. I extend this logic to district-targeted distributive spending. TV congruence makes the representative's spending behavior more visible to voters in congruent markets, but the *evaluative consequence* of that visibility is mediated by the partisan filter through which voters interpret what they have learned. Cross-partisan voters punish; co-partisan voters do not.

This generates a specific empirical signature. In pooled estimation, the assistance-spending channel will register as substantively active because cross-partisan voters dominate the variation in voter–incumbent matching across the sample. The partisan-filtering condition therefore implies an asymmetric incidence across the voter–incumbent match: cross-partisan voters, for whom the opposing-party frame codes the spending as waste, should drive the response, while co-partisan voters, for whom the same dollar reads as constituency service, should not. Whether that punishment concentrates further, in the Republican-voter / Democratic-MC cell — where the distinctively conservative "wasteful programmatic spending" critique is available to cross-partisan Republican voters facing a Democratic incumbent — is a question I take up descriptively in §4 rather than one the design is built to confirm. The channel is thus distinct both from positional ideology (whose frame-neutral content is governed by heuristic redundancy, not partisan valence) and from the dormant valence channels (suppressed by absent attribution rather than partisan filtering).

Taken together, the two conditions imply a structured set of expectations rather than a single average effect. Congruence should sharpen ideological evaluation, concentrated among co-partisans — for whom the party label is least informative about position — in approval and at the ballot box alike. It should activate the assistance channel asymmetrically, through cross-partisan punishment rather than co-partisan evaluation. And it should leave the four valence channels — unemployment, violent crime, legislative effectiveness, and the pooled scandal indicator — dormant: television supplies no representative-specific attribution for the first three, and no consistently clean representative-specific signal for scandal. §4 examines each in turn.

3 Data and Research Design

3.1 Data Sources and Measurement

Individual-level survey data come from the Cooperative Election Study (CES, formerly CCES) for 2006–2024, providing approximately 529,000 observations across 30,000–60,000 respondents per year through YouGov's matched random sampling. The CES carries measures of representative approval, political knowledge, presidential approval, ideological self-placement, vote choice, and rich demographics. The large sample is essential for the subgroup interaction analyses that form the core of my empirical strategy.

Following Snyder and Strömberg (2010), I measure TV congruence as $\text{Congruence}_d = \sum_m (pop_{md}/pop_m) \times (pop_{md}/pop_d)$, where pop_{md} is the population in the overlap of district d and DMA m. The measure equals one when district and DMA boundaries perfectly coincide and approaches zero when the district is a small fraction of a large DMA or split across many DMAs. DMA-county assignments come from the Nielsen DMA crosswalk; congressional district boundaries are drawn from Census TIGER/Line shapefiles, with population-weighted areal interpolation for the approximately 5% of counties that split across districts.

My primary outcome is representative approval, a binary indicator for approving or strongly approving of the respondent's House member. For the first-stage knowledge analysis I use an indicator for whether the respondent has heard of their representative (2008–2024; the item enters the CES in 2008). Ideological distance is the absolute differ-

Table 1: Summary Statistics

Variable	N	Mean	SD	Min	Max
Panel A: Individual-Level (CES 2006–2024)					
TV Congruence	528,757	0.26	0.191	0.02	0.97
Within-county SD			0.065		
Representative Approval (Binary)	398,689	0.58	0.494	0	1
Heard of Representative	494,886	0.73	0.442	0	1
Ideological Distance	373,705	0.56	0.406	0	1.94
Presidential Approval (Binary)	513,909	0.45	0.498	0	1
County Unemployment Rate (%)	528,757	5.90	2.594	1.1	29.1
Panel B: District-Level					
DW-NOMINATE (1st Dim.)	3,553	0.02	0.45	-0.68	0.94
Ideological Extremity ($\|D_1\|$)	3,553	0.43	0.14	0.01	0.94
Legislative Effectiveness Score	3,553	1.00	1.21	0.00	18.7

Note: Cooperative Election Study individual-level data. Heard of Representative is available from 2008 on (the item enters the CES that year). Ideological distance is the absolute difference between respondent five-point ideology (rescaled to $[-1, 1]$) and the representative's DW-NOMINATE first-dimension score. District-level statistics come from the district-congress panel.

ence between the respondent's five-point ideology self-placement (rescaled to $[-1, 1]$) and the representative's DW-NOMINATE first-dimension score from Voteview; I construct a parallel measure based on the Fowler and Hall (2012) Conservative Vote Probability. The channel moderators are county-year unemployment from the Bureau of Labor Statistics, county-year violent crime from the FBI Uniform Crime Reporting program, district-year federal spending from USASpending.gov, the Volden and Wiseman (2014) legislative effectiveness score, and a member-level scandal indicator from the GovTrack misconduct database (GovTrack.us, 2026).

Vote choice is measured from the post-election wave of the CES, available for roughly half of the sample, those who completed the post-election survey and reported a valid House vote. For the district-level legislator responsiveness analysis I construct a panel of approximately 3,500 district-congress observations, measuring district ideology as mean CES respondent ideology and legislator ideology with DW-NOMINATE.

3.2 Summary Statistics

Table 1 reports summary statistics for the key variables in my analysis. The mean congruence in the sample is 0.26 (SD = 0.191), reflecting the fact that most congressional districts constitute a small share of their primary DMA's audience. The within-county standard deviation of congruence is 0.065, approximately 34% of the total, reflecting the limited but real temporal variation that drives identification under county fixed effects. Representative approval averages 0.58, with sharp variation by partisanship: approximately 88% of co-partisan voters approve of their representative, compared to 18% of cross-partisan voters. The recognition rate averages 0.73 in the subsample where it is available (2008–2024). Mean ideological distance is 0.56, and mean presidential approval is 0.45.

3.3 Descriptive Patterns

The variation in TV congruence across congressional districts is substantial. The distribution ranges from below 0.30 for urban districts split across multiple large DMAs to 0.97 for districts that nearly coincide with a single market. Congruence is inversely correlated with DMA size: the largest metropolitan markets, such as the New York, Los Angeles, and Chicago DMAs, span many congressional districts and produce low congruence, while smaller markets in less densely populated areas often encompass a single district.

3.4 Identification Strategy

The core challenge is that DMA-district overlap correlates with district characteristics that independently affect political behavior. Urban districts tend to have lower congruence because large metropolitan DMAs span many congressional districts, and urban voters differ systematically from rural voters on political engagement, information access, partisan composition, and baseline attitudes toward incumbents. A naive comparison of high- and low-congruence districts would conflate the information effect of congruence with these compositional differences.

My preferred specification addresses this concern with county and year fixed effects:

$$\text{Approval}_{idt} = \alpha + \beta \cdot \text{Congruence}_{dt} + X_i\gamma + \delta_k + \theta_t + \varepsilon_{idkt}$$

where i indexes individuals, d districts, k counties, and t years. X_i includes age, gender, education, race, family income, seven-point party identification, and five-point ideology. Including party identification and ideology is essential: representative approval is heavily determined by whether the respondent shares the incumbent's party, and without these controls congruence could proxy for partisan composition. Standard errors are clustered at the congressional district level.

County fixed effects absorb all time-invariant county characteristics — urbanicity, demographics, economic structure, media infrastructure, all other stable features of the local environment. Identification comes entirely from temporal variation within counties. Two exogenous sources generate this variation. First, decennial redistricting redraws district boundaries, changing which DMAs overlap the district covering a given county; redistricting commissions optimize for population equality, contiguity, and political considerations but not for DMA-CD congruence, so the congruence changes are incidental consequences of boundary shifts driven by other goals. The sample spans two such rounds: the 2010 cycle, entering with the 113th Congress, and the 2020 cycle, entering with the 118th. Second, Nielsen periodically reassigns counties across DMAs based on commercial viewership patterns and signal coverage — considerations orthogonal to political geography. I report state fixed effects as a less demanding benchmark that uses both cross-sectional and temporal variation; comparison between the two reveals the extent of cross-sectional confounding.

For mechanism tests, I interact congruence with moderating variables. The competitive structure of these interaction tests lets me identify which channels are active rather than presuming a single mechanism: each channel's congruence interaction is estimated jointly against the competing channels' interactions, and a channel registers as active where its interaction survives that joint test.

County fixed effects absorb the majority of variation, leaving a conservative but credible source of identification. I present both state and county FE results throughout to bracket the plausible range, with state FE providing a more powerful but potentially confounded benchmark and county FE a conservative but cleaner estimate.

3.5 Balance Properties Across Fixed-Effects Layers

I document balance by regressing congruence measures on covariate batteries at progressively restrictive fixed-effects layers: pooled (no fixed effects), state, state-by-year, and — where the data carry repeated within-county observations — county-and-year. The test runs on three datasets, mirroring the three-dataset structure of Appendix A. Panel A applies a Snyder–Strömberg-style demographic battery to the newspaper-congruence data of Peterson (2021); Panel B applies a Myers-style controls battery (presidential vote share, district competitiveness, open-seat indicator) to Myers (2025)'s state-legislative data; Panels C and D apply an individual-level battery and the lagged district-political covariates to my CES sample and TV congruence (Table 2). The predecessors' panels establish that cross-sectional imbalance is a property of congruence measures generally, not a defect of mine; the CES panels report where my own identifying variation stands. I report none of this as a test of prior work — both Snyder and Strömberg (2010) and Myers (2025) progress through layered fixed-effects designs whose plausibility rests on similar as-if-random reasoning.

Three findings emerge. First, cross-sectional imbalance is endemic to the design family: in all three datasets, congruence is strongly correlated with district covariates — presidential vote share, population density, median household income, black population share — and every battery rejects as-if random at conventional significance in the cross-

Table 2: Balance Test: Congruence on Covariates Across Fixed-Effect Layers

	No FE (1)	State (2)	State × Year (3)	County + Year (4)
Panel A: District-Level Demographics (Snyder–Strömberg battery)				
Log Population Density	-0.043^{***}	-0.034^{***}	-0.033^{**}	—
	(0.008)	(0.010)	(0.011)	
Log Median Household Income	-0.368^{***}	-0.509^{***}	-0.518^{***}	—
	(0.048)	(0.061)	(0.065)	
Black Share	-0.372^{***}	-0.578^{***}	-0.586^{***}	—
	(0.095)	(0.102)	(0.108)	
N	5,171	5,170	5,086	
Panel B: District-Level Politics (Myers battery)				
Pres. Two-Party Dem Vote Share	-0.186^{***}	-0.139^{***}	-0.151^{***}	—
	(0.035)	(0.022)	(0.022)	
Share Aged 66+	$+0.005^{**}$	$+0.005^{*}$	$+0.004^{*}$	—
	(0.001)	(0.002)	(0.002)	
N	91,639	91,639	91,639	
Panel C: Individual-Level CES Battery				
Family Income	-0.0047^{***}	-0.0020^{***}	—	$+0.00023^{***}$
	(0.0007)	(0.0005)		(0.00006)
Party Identification (7-pt)	$+0.0058^{***}$	$+0.0036^{***}$	—	$+0.00056^{***}$
	(0.0010)	(0.0006)		(0.00011)
News Interest (High)	$+0.0082^{***}$	$+0.0033^{**}$	—	$+0.00096^{**}$
	(0.0018)	(0.0010)		(0.00029)
N	496,586	496,586		489,194
Panel D: Within-County Political Residual				
Lagged District Margin	—	—	—	-0.025^{***}
				(0.007)
Lagged Democratic Winner	—	—	—	-0.023^{***}
				(0.005)
N				526,137

Note: Each cell reports a regression of a congruence measure (per-unit scale) on the listed covariate under the column's fixed effects. Panel A uses the Peterson (2021) newspaper-congruence data collapsed to district-years; Panel B uses Myers (2025)'s state-legislative data; Panels C and D use my CES sample and TV congruence. "—" indicates the FE layer is not available for that battery (the district-level datasets lack repeated within-county observations). Panel D, reported only at the within-county-and-year layer where political variables show meaningful imbalance, conditions on the lagged district-political covariate alone (not the Panel C demographic battery), so its sample is larger. Panel C demographic estimates at that layer are significant only because of the very large sample and are substantively negligible (one to two orders of magnitude smaller than coarser specifications). $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

section. Second, state and state-by-year fixed effects do not resolve the imbalance in any of the three — even the strongest cross-sectional layer leaves substantive demographic correlations in place. Third, the county-and-year specification, available in my repeated-observation CES design, substantively passes balance on demographics: estimate magnitudes shrink by one to two orders of magnitude, becoming substantively negligible despite remaining nonzero under the very large sample size.

The pattern is what one would expect under genuine as-if random assignment combined with persistent cross-sectional correlations: imbalance is large in the cross-section, persists under state-level fixed effects because those characteristics also vary within state, and shrinks to near zero only when within-unit temporal variation is isolated. The within-county design extracts identifying variation that approximates the as-if-random ideal even though the cross-sectional comparison fails it.

One qualification matters: political variables continue to exhibit residual imbalance within county-and-year fixed effects. Lagged district margin and lagged Democratic winner indicator predict congruence within counties at conventional significance (Table 2 Panel D), reflecting the joint determination of redistricting boundaries and competitive outcomes — districts redrawn between cycles tend to differ in both political competitiveness and DMA overlap. I address this residual political imbalance in the next subsection.

3.6 Political Controls Robustness

The residual political imbalance documented above raises the concern that within-county variation in congruence partly reflects within-county changes in district competitiveness. If voters in districts with shifting competitive structures behave differently for reasons unrelated to TV congruence, the pooled estimate may absorb part of that competitive shift.

I test this by adding lagged district margin, lagged Democratic winner indicator, and lagged number of candidates as controls in the main representative-approval specification. Adding lagged margin alone produces an approximately 22% attenuation of the matched-sample composition-blind congruence estimate while statistical significance is maintained; the additional lagged variables produce no further movement. Crucially for the channel-decomposition findings that follow, the ideology-distance interaction — my central substantive finding — is essentially unchanged by the addition of lagged margin (under one percent), and the assistance-spending interaction shifts by approximately 6%. The political imbalance affects the level estimate of the main approval effect more than it affects the channel decomposition that constitutes my second contribution.

3.7 Source of Residual Political Imbalance

The within-county-and-year political imbalance is structural geography rather than partisan endogeneity. The alignment-congruence relationship has opposite signs in Republican-controlled and Democratic-controlled redistricting regimes, appears at the same significant magnitude in venues where partisans did not draw the lines (independent commissions, courts), and is unchanged by the mid-cycle Pennsylvania and North Carolina court redraws that explicitly reversed Republican gerrymanders.

The mechanism is geographic: the congruence formula reduces to $\mathrm{pop}_d/\mathrm{pop}_m$ when a district lies entirely within a single DMA and is small whenever a small district sits inside a large metropolitan media market. Urban Democratic-leaning districts typically occupy a small slice of a large metropolitan DMA, mechanically depressing district-to-DMA congruence; rural Republican-leaning districts more often align with smaller, geographically coherent markets. The within-county-and-year design absorbs the level correlation between county partisanship and congruence; the residual variation correlated with the lagged Democratic-winner indicator reflects the same urban-Democratic versus rural-Republican DMA geography operating across all redistricting regimes. These regime-sign, redraw-venue, and structural-geography tests are reported in full in Appendix J.

3.8 Methodological Robustness

I compute the de Chaisemartin and D'Haultfœuille (2020) implicit-weight decomposition for the two-way fixed-effects specification underlying the composition-blind pooled estimate. The sum of negative weights is 1.009, large enough that under arbitrary effect heterogeneity the TWFE estimate could deviate substantially from the average treatment effect. Re-estimating the composition-blind pooled estimate with the Callaway et al. (2024) binned-dose estimator on congruence deciles, both the dose-density-weighted slope and the linear arm between extreme deciles fall within five percent of the TWFE point estimate reported in the baseline-accountability results below. Heterogeneity does not strongly correlate with the TWFE weights in this setting, consistent with identifying variation that comes from episodic structural events (decennial redistricting, occasional Nielsen DMA reassignments) rather than monotonic treatment-intensity drift.

My falsification strategy uses neighbouring-district channel substitution rather than an alternative-outcome placebo. I initially considered presidential approval as a placebo dependent variable but rejected it: presidential approval is not exogenous to most channel-by-congruence interactions, since voters who learn more about their representative through congruent coverage may also update on the president through associated cues. The neighbouring-district channel placebo (substituting another district's channel value for the focal district's) holds the outcome and the voter fixed and varies only the source of channel information; it is the falsification design I report alongside the channel results.

3.9 Compositional Controls in the Preferred Specification

The within-county-and-year specification produces clean balance on demographic covariates and a manageable residual political imbalance whose source is geographic rather than partisan. It exhibits one additional feature that dictates my spec choice. Within county-and-year fixed effects, congruence and a binary indicator for a Republican incumbent are nearly collinear, with a within-FE correlation of $+0.86$. The identifying variation in TV congruence in my sample is therefore principally variation in incumbent party, and a regression of approval on congruence without partisan-composition controls would conflate the behavioral effect of TV-mediated information with the baseline approval-level difference between incumbents of the two parties.

My preferred specification accordingly includes voter–incumbent partisan-match and incumbent-party indicators alongside the demographic controls listed above. Under this specification, the pooled congruence estimate on representative approval is statistically zero across all six fixed-effects structures I examine (Appendix A). Channel-by-moderator interaction terms are identified separately from the congruence main effect, but they are not automatically immune to the same compositional concern: within the fixed effects, congruence-by-moderator terms are themselves highly correlated with incumbent-party-by-moderator terms. Appendix A.5 re-estimates the channel interactions under the full compositional controls; §4 reports the resulting attenuation.

The need for these controls is specific to my data — to the combination of my outcome (binary representative approval), my treatment (DMA-CD overlap as measured here), and my within county-and-year identifying variation — rather than a general property of congruence-based designs. I show in §4 and Appendix A that the closest congruence-family predecessors, reproduced on their own data, do not require them: their voter-knowledge findings remain behavioral under the same compositional progression, because the within-fixed-effects correlation between treatment and incumbent party in those designs is substantially below the near-collinear value I observe within my county-and-year fixed effects.

Table 3: First Stage: TV Congruence and Voter Knowledge

	Heard of Rep		Correct Party	News Interest
	(1)	(2)	(3)	(4)
Congruence	0.094^{***}	0.059^{**}	-0.012	0.026^{**}
	(0.013)	(0.020)	(0.009)	(0.009)
Fixed Effects	State + Year	County + Year	County + Year	County + Year
Controls	Yes	Yes	Yes	Yes
N	483,909	483,841	327,022	483,508

Note: OLS estimates. The dependent variable in columns (1)–(2) is whether the respondent has heard of their House representative (2008–2024). Column (3) reports correct party identification; column (4) reports general political news interest as a secondary outcome. Controls: age, gender, education, race, family income, seven-point party identification. Standard errors clustered by congressional district. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

4 Results

Voters in congruent markets are more likely to have heard of their representative — the information channel survives. Yet the pooled effect of congruence on representative approval is statistically zero under my preferred specification ($+0.001$, $p = 0.94$; Table 5, row d). The pooled null conceals sharp channel selectivity: ideological evaluation activates, distributive-spending evaluation activates only for cross-partisans, and four valence channels remain dormant.

4.1 The Information Channel

I begin by examining whether TV congruence operates through information, the first link in the accountability chain. Consistent with Snyder and Strömberg (2010), congruence increases the probability that voters have heard of their House representative. With state fixed effects, a one-standard-deviation increase in congruence raises recognition by 1.8 percentage points, a substantial effect consistent with the prior literature. With the more demanding county fixed effects, the estimate implies a 1.1 percentage point increase per standard deviation, a conservative bound on the relationship (Table 3).

The 37% attenuation from state to county fixed effects is expected given the variance structure — within-county congruence variation is only 34% of the total — and reflects the conservative identification strategy rather than absence of the underlying relationship; restricting to congresses surrounding redistricting yields a stronger estimate (Online Appendix C). Congruence also raises general political news interest, at about half the recognition magnitude (Table 3, column 4). I read this as a secondary outcome of information supply — more representative-relevant local content is also more local political content — rather than as a placebo; the demand-side sorting concern is addressed by the within-county identification, since respondents do not relocate when redistricting changes their county's congruence (Online Appendix C.3). Both recognition estimates are conventionally significant, and the downstream accountability interactions do not depend on the first stage's precision.

A second knowledge measure, correctly naming the representative's party, does not respond to congruence (Table 3, column 3). Appendix C.2 reports the estimate; I do not rely on the correct-party measure for any downstream analysis.

4.2 Baseline Accountability: No Pooled Effect Under Compositional Controls

My preferred specification, including voter–incumbent partisan-match and incumbent-party indicators alongside the demographic controls described in §3, finds no pooled effect of TV congruence on representative approval. The estimate is $+0.001$ with $p = 0.94$ in the matched-sample county-and-year specification (Table 5, row d), and remains indistinguishable from zero across the six fixed-effects structures I examine (Appendix A). For comparison, dropping the compositional controls — the composition-blind specification that has historically anchored the congruence-accountability literature — yields a substantially negative point estimate that collapses under full compositional controls and shifts substantially even under partial controls (Table 5). Table 4 reports the composition-blind versions for transparency, with the placebo on presidential approval also reported

Table 4: TV Congruence and Representative Approval (Composition-Blind Specifications)

	Rep Approval		+DW-NOM	Pres Approval
	(1)	(2)	(3)	(4)
Congruence	-0.109^{***}	-0.097^{***}	-0.066	-0.011
	(0.019)	(0.026)	(0.041)	(0.014)
DW-NOMINATE (D_1)			-0.077^{***}	
			(0.009)	
Fixed Effects	State + Year	County + Year	District + Year	County + Year
Controls	Yes	Yes	Yes	Yes
N	394,620	394,553	394,620	390,823

Note: OLS estimates under composition-blind specifications (no controls for voter–incumbent partisan composition). The dependent variable in columns (1)–(3) is binary representative approval; column (4) is a parallel specification with presidential approval. Column (3) adds the representative's DW-NOMINATE first-dimension score. Controls: age, gender, education, race, family income, party identification, ideology. Standard errors clustered by congressional district. The compositional reading is reported in Table 5.
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

there.

The composition-blind estimates in Table 4 reflect the (DMA congruence $\times$ incumbent party) near-collinearity discussed in §3: without partisan-composition controls, the regression captures the baseline approval-level difference between Republican and Democratic incumbents in the county-year cells where congruence shifts, leaving it uninterpretable as a measure of accountability. Table 5 reports the compositional progression to my preferred specification — the composition-blind matched-sample estimate (row a, -0.121, $N = 346{,}453$; cf. the full-sample -0.097 of Table 4 column 2) reverses sign when voter–incumbent partisan match is added (row b), remains negative and significant under a Republican-incumbent indicator alone (row c), and is statistically zero under both controls (row d).

The null is not a feature of the county-and-year fixed effects alone — Appendix A reports the same progression across six fixed-effects structures, none distinguishable from zero. A stratification diagnostic reinforces the compositional reading: splitting the composition-blind regression by voter–incumbent partisan match yields congruence estimates positive within both strata even as the pooled estimate is negative, and adding

Table 5: TV Congruence and Representative Approval — Compositional Progression to the Preferred Specification

Specification	b	SE	p
(a) Composition-blind (matched sample; cf. Table 4 col 2)	-0.121	0.030	< 0.001
(b) + Voter–MC partisan match (co_partisan)	$+0.038$	0.020	0.055
(c) + MC party indicator (MC_R)	-0.075	0.028	0.008
(d) + Both compositional controls (preferred specification)	$+0.001$	0.019	0.94

Note: OLS estimates of congruence on binary representative approval ($N = 346{,}453$), county-and-year fixed effects, demographic controls (age, gender, education, race, family income, party identification, ideology), standard errors clustered by congressional district. Row (a) re-estimates the composition-blind specification on the matched sample (-0.121); the full-sample estimate in Table 4 column (2) is -0.097 ($N = 394{,}553$). Row (b) adds a voter–incumbent partisan-match indicator; row (c) a Republican-MC indicator; row (d), the preferred specification, both. Appendix A reports the same progression across six fixed-effects structures (none, state, state plus year, state by year, county plus year, district plus year), with the both-controls estimate ranging from -0.027 to $+0.001$ (mean -0.012) and zero of six statistically significant.

the incumbent-party control returns all within-stratum estimates to zero — the signature of a pooled coefficient produced by partisan composition rather than within-group behavioral response.

The need for compositional controls is specific to my data. Reproducing Snyder and Strömberg (2010) on their 1982–2006 NES newspaper-congruence data and Myers (2025) on the 2018 CES newspaper-congruence data with identical compositional progressions, I find that both authors' voter-knowledge findings remain behavioral on their own data: the Snyder and Strömberg (2010) newspaper-readership and recall estimates barely move under the controls, and the Myers (2025) state-legislator-recall estimate if anything gains statistical significance (Appendix A). The within-fixed-effects correlation between treatment and incumbent party in both predecessor designs is substantially below the near-collinear value I observe within my county-and-year fixed effects. The composition-blind specification is appropriate for those data; for mine, the preferred specification includes the compositional controls.

The channel-level findings reported below are estimated from congruence-by-moderator interaction terms whose identification is separate from the congruence main effect. They are not invariant to the compositional controls, however. Within the fixed effects, the congruence-by-ideology interaction is correlated with the incumbent-party-by-ideology interaction at $+0.996$, so adding the compositional controls and their moderator interactions is a maximally demanding test. Under that test the ideology interaction survives in sign and significance but attenuates by roughly two-thirds — from -0.0130 to -0.0048 in the ideology specification and from -0.0122 to -0.0038 in the joint specification (Appendix A.5). The spending interaction survives the same addition with far milder attenuation than the ideology channel (Appendix A.5), and the valence channels remain null. The selectivity pattern — which channels activate and which remain dormant — is unchanged by the spec choice; the interaction magnitudes reported in the tables below are from the composition-blind interaction specifications and are upper bounds relative to their fully controlled counterparts.

Two transparency notes, consistent with the compositional reading, are reported in the appendix: the presidential-approval placebo in column (4) of Table 4 (composition-blind, like the congruence-approval estimate, and not interpreted as a substantive behavioral effect) and the negligible attenuation from adding a legislative-effectiveness control to the composition-blind specification (Online Appendix E.2). The substantive contribution of this paper is selective channel activation, not a uniform pooled accountability effect.

4.3 The Active Channel: Ideological Evaluation

My central finding identifies the specific evaluative channel that congruence activates. TV congruence enables voters to penalize representatives for ideological distance. A one-standard-deviation increase in congruence strengthens the ideological-distance penalty on approval by about 1.3 percentage points per standard deviation of distance. Table 6 reports the full results including partisan subgroup estimates.

The co-partisan result is the theoretically most consequential finding. Among voters who share the representative's party, the interaction is significantly negative — meaning-

Table 6: Congruence × Ideological Distance: Approval Effects

	Full Sample (1)	Co-Partisan (2)	Cross-Partisan (3)
Cong × Ideo Distance	-0.0128^{***}	-0.0054^{**}	$+0.0013$
	(0.002)	(0.002)	(0.002)
Fixed Effects	County + Year		
Controls	Yes (Demographics + Party ID)		
N	355,054	150,960	101,441

Note: OLS estimates. The dependent variable is binary representative approval. Co-partisan: the respondent identifies with the party they believe the representative belongs to; the classification is perception-based and conditions on answering the representative-party item (see the selection discussion accompanying Table 9). Standard errors clustered by congressional district in parentheses. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

ful precisely because it operates within the group least expected to penalize their own party's representative. The party label supplies a default positive evaluation, and TV congruence erodes that default when the representative's actual positions diverge from the voter's preferences. The finding speaks directly to whether partisan loyalty is a fixed psychological anchor or a default that gives way to substantive evaluation under the right informational conditions.

Among cross-partisan voters, the interaction is null — theoretically coherent rather than disappointing. These voters already hold their representative in low regard on the party label alone, a strong negative prior that the specific magnitude of ideological distance cannot meaningfully deepen; the marginal positional information is redundant with the party heuristic.

The asymmetry also helps rule out alternative interpretations. If the congruence-ideology interaction reflected media negativity or general cynicism, I would expect symmetric effects across partisan subgroups; the concentration among co-partisans is consistent only with a mechanism that reveals new information about ideological positioning.

A neighbouring-district placebo confirms that the channel is specific to own-district information transmission: substituting a neighbouring district's representative-ideology value for the focal district's — whether a same-state neighbour or one of 100 randomly drawn other-state districts — yields placebo interactions that are not significant and substantially smaller than the own estimate (the other-state placebo is about four percent of it). The own estimate absorbs the identifying variation; the placebo does not.

4.4 Channel Decomposition

The single-channel test in the previous subsection establishes that ideological distance is one active mechanism through which TV congruence operates on representative approval. To probe the structure of channel activation more fully, I expand the joint specification to a wider set of candidate moderators and organize the results around the structure the two conditions imply: a positional channel whose incidence follows the informativeness of the party heuristic, a voter-asymmetric partisan-mediated channel, and a contrast-frame characterization of four dormant valence channels. Two further candidate moderators (abortion- and gun-distance) that fail neighbouring-district placebo tests are noted at the end of the subsection and detailed in the Online Appendix.

The decomposition tests a family of ten candidate moderators: the seven primary channels (five jointly estimated, two in parallel), plus procurement spending and the abortion- and gun-distance measures. Both active channels survive correction for this family: under a Bonferroni adjustment for ten tests, the ideology interaction (unadjusted $p < 0.001$) and the assistance interaction (unadjusted $p = 0.002$) remain significant at conventional levels. The sexual-misconduct era split and the four-way partisan interaction reported later in this section are post-hoc decompositions prompted by the primary estimates, and I label them exploratory.

Table 7 reports the joint-specification estimates organized along these lines. Panel A reports the two active channels I identify: ideology distance (DW-NOMINATE and CVP variants) and assistance spending. Panel B reports the four valence channels under a magnitude-contrast frame against the positional benchmark.

Table 7: Channel Decomposition: Active Channels and Valence Contrast

	Solo (1)	Joint (2)	+ Lag Margin (3)	$b/\lvert b_{\text{ideo}}\rvert$ (4)	$\text{MDE}_{80}/\lvert b_{\text{ideo}}\rvert$ (5)
Panel A: Active Channels					
Cong × Ideo. Distance (DW-NOMINATE)	-0.0131^{***}	-0.0129^{***}	-0.0128^{***}	$1.00\times$	$0.42\times$
	(0.0023)	(0.0023)	(0.0023)	[anchor]	
Cong × Ideo. Distance (CVP, joint)	$+0.047^{***}$	-0.010^{**}	-0.009^{*}	$0.74\times$	—
	(0.012)	(0.004)	(0.004)		
Cong × Assistance Spending (log)	-0.0099^{***}	-0.0066^{**}	-0.0063^{***}	$0.51\times$	—
	(0.0023)	(0.0020)	(0.0019)		
Panel B: Valence Channels (Magnitude Contrast)					
Cong × Unemployment	$+0.0047^{\dagger}$	$+0.0018$	$+0.0012$	$0.29\times$	$0.42\times$
	(0.0024)	(0.0019)	(0.0018)		
Cong × Violent Crime Rate	-0.0019	$-0.0063^{\dagger}$	-0.0039	$0.20\times$	$0.72\times$
	(0.0040)	(0.0033)	(0.0031)		
Cong × Legislative Effectiveness	-0.0006	—	—	$0.05\times$	$0.33\times$
	(0.0015)				
Cong × Scandal (Any, Binary)	-0.0088	-0.0042	-0.0075	$0.25\times$	$0.53\times$
	(0.0092)	(0.0095)	(0.0094)		
Panel C: Political Control					
Lagged District Margin	—	—	$+0.123^{***}$	—	—
			(0.010)		
Fixed Effects	County + Year				
Controls	Yes (Demographics + Party ID + Ideology)				
N	254,516 (solo) / 254,228 (joint) / 254,089 (+ lag margin)				

Note: OLS estimates of channel-by-congruence interactions on binary representative approval. Columns (1)–(3) report each interaction estimated alone (Solo), jointly with all five channel interactions (Joint), and jointly plus lagged district margin (+ Lag Margin). Column (4) is the ratio of the joint estimate to the DW-NOMINATE ideology estimate ($|b_{\text{ideo}}| = 0.013$); below one means smaller than the positional anchor. Column (5), Panel B only, is the ratio of the minimum detectable effect ($\alpha = 0.05$, 80% power, matched-sample SE) to that anchor; below $1.00\times$ means the channel can rule out an effect as large as the positional benchmark. Panel B column (4) ratios use focused-regression benchmarks ($+0.0038$ unemployment, -0.0026 crime, -0.0006 legislative effectiveness, -0.0032 scandal); the joint crime estimate (-0.0063) is larger, placing crime at $\approx 0.48\times$ the anchor and making it the least decisively dormant valence channel. Legislative effectiveness (-0.0006, SE 0.0015, $N = 352{,}113$) and CVP (joint $N = 254{,}516$; $+$lag $N = 256{,}314$) are parallel specifications outside the five-channel joint regression, so their joint/lag cells are blank or on their own samples; the footer Ns apply to the joint regression. Abortion- and gun-distance interactions fail neighbouring-district placebo tests (see text) and are omitted. Standard errors clustered by congressional district. $^{\dagger}p < 0.10$; $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

Active channel 1: positional ideology The DW-NOMINATE-based ideology-distance interaction is stable across the three specifications (Table 7 Panel A), moving by roughly two percent from the solo to the lagged-margin column: the channel is robust both to the inclusion of competing channels and to the political control I identified earlier as a potential confound. The more demanding test is compositional. Re-estimated with the partisan-composition controls and their ideology interactions added, the interaction retains its sign and significance but at roughly a third of the magnitude shown here (Appendix A.5).

The CVP-based ideology-distance interaction is wrong-signed in solo estimation but shifts to the predicted negative sign jointly with DW-NOMINATE, and both measures retain the pattern under the lagged-margin specification. The reversal is mechanical: CVP proxies position-distance only when the position component is held fixed by DW-NOMINATE (Table 7 note).

Active channel 2: assistance spending (partisan-mediated, cross-partisan-only) The assistance-spending interaction is significantly negative in all three specifications (Table 7 Panel A); the roughly one-third attenuation under joint inclusion reflects shared variance with the ideological channels but leaves a substantively meaningful magnitude. The incidence across voters, however, is the mirror image of the ideology channel's approval pattern: stratifying by voter–incumbent partisan match yields significant cross-partisan punishment alongside a co-partisan estimate indistinguishable from zero, confirming that the channel operates through cross-partisan punishment rather than co-partisan evaluation.

The cross-partisan punishment is asymmetric in a second sense: stratifying simultaneously by voter party and incumbent party, the strongest single cell is Republican voters punishing Democratic incumbents (implied interaction -0.015), while the symmetric cell — Democratic and Independent voters facing Republican incumbents — is substantially weaker (-0.005); a four-way interaction testing the cross-stratum difference is significant ($p = 0.0012$), consistent with the asymmetry operating more strongly in Democratic-incumbent districts rather than reflecting sample composition. Both co-partisan cells are indistinguishable from zero, and the Republican co-partisan null carries discriminating weight: if Republican voters simply disliked federal spending as such, they would punish their own party's incumbents for it too, but the Republican-voter/Republican-incumbent cell is as flat as its Democratic counterpart. The response tracks the voter's partisan relation to the incumbent, not a general anti-spending disposition.

The asymmetry has a different source than the ideology channel's. Positional information is frame-neutral — a one-dimensional location whose approval incidence follows heuristic redundancy — whereas the inferential frame through which assistance spending is read is partisan-conditional: a cross-partisan voter codes programmatic spending under the opposing party as wasteful and punishes, while a co-partisan codes the same dollar as expected constituency service, which confirms a prior rather than revising one — which is why the co-partisan response is flat rather than the reward a credit-claiming account would predict. Nor can the co-partisan null be an exposure artifact: these are the same voters whose approval responds most strongly to TV-supplied positional information (above), so they demonstrably receive what congruent coverage carries; what differs across the two channels is the evaluative use they make of it. The mirror image holds for cross-partisans, who respond here but not on ideology: positional information is redundant with a party label that already signals disagreement, but the spending record is not encoded in the label at all, so it is genuinely new content whose evaluative sign the partisan frame determines — the partisan-filtering mechanism Bartels (2002) and Achen and Bartels (2016) predict.

Procurement spending (defense and infrastructure contracting) is null in the joint specification and survives winsorization of the right tail and a count-of-contracts alternative measure (Online Appendix); it is less partisan-coded than assistance — both parties claim credit for contracts on similar grounds — so the partisan-filtering mechanism does not operate. The active sub-channel of federal spending is specifically the assistance subtype, not the category as a whole.

The assistance-spending channel also passes a neighbouring-district placebo: substituting a neighbouring district's assistance value for the focal district's (a same-state neighbour or 100 random other-state draws) yields placebo estimates that are not significant and substantially smaller than the own estimate. One caveat: in the placebo-restricted sample (which drops single-CD-state observations) the own estimate is only marginally significant; the headline magnitude reported above uses the full sample, and the substantive interpretation is unchanged.

Valence non-transmission: a magnitude-contrast characterization The four valence channels — local unemployment, violent crime, legislative effectiveness, and the pooled scandal indicator — characterize the dormant side of the account: unemployment, crime, and legislative effectiveness fail the coverage condition outright, while the pooled scandal indicator, though representative-specific, lacks a consistently clean signal (developed below). I report their estimates in Panel B of Table 7 not as point-null claims but as a magnitude-contrast against the positional ideology benchmark.

The framing matters. A power-pessimistic reading of any single valence-channel estimate could argue that the data do not decisively rule out a non-trivial effect; that argument is correct as far as it goes, but it characterizes the channels at the wrong scale. The substantive claim my theory makes is a contrast claim: TV congruence transmits positional information at the ideology-channel magnitude and does not transmit valence information at any magnitude comparable to that benchmark. Panel B columns (4) and (5) quantify the contrast in two ways.

Both columns quantify the contrast. On point estimates, the four valence ratios span $0.05\times$ to $0.29\times$ the positional anchor ($|b_{\text{ideo}}| = 0.013$), with the largest (unemployment) wrong-signed; on the minimum detectable effect at 80 percent power, no channel exceeds $0.72\times$ the benchmark, and two of four rule out half-magnitude effects. The contrast is therefore robust to every reasonable reading of the marginal-power channels: unemployment and legislative effectiveness are decisively null well below the positional magnitude, while violent crime and the pooled scandal indicator are marginally powered against the strict half-positional threshold but easily rule out positional-magnitude effects.

Channel-measurement validity is not the explanation. Population-weighted district aggregation of the county-level crime measure does not reveal a hidden signal; alternative scandal salience windows and a coverage-restricted subset preserve the scandal null with sign instability near zero; legislative effectiveness retains 92.5 percent of its total standard deviation under county-and-year fixed-effect absorption, so the null is not driven by fixed effects absorbing the relevant variation. The channels themselves, not the measurements, are the source of the contrast.

The crime null is particularly informative substantively. Violent crime is among the most heavily covered local-television content, but TV exposure does not translate into MC-specific crime accountability. The information environment supplies the content; the attribution to the House member is absent. Local economic conditions and legislative productivity stand on the same footing: the medium does not deliver representative-specific attribution for these channels, and the absence of attribution suppresses accountability transmission. The pooled scandal indicator follows the same pattern, with one bounded sub-channel exception that I develop in the next subsection.

Two further candidate moderators — abortion-distance and gun-distance — might in principle activate as positional channels but fail the neighbouring-district placebo test: each transmits a diffuse partisan signal that any district's position can carry rather than MC-specific position information. I classify both as tested and placebo-contaminated, exclude them from Table 7 and the abstract, and report the detail (the opposite-signed abortion placebo, significant in 87 of 100 random draws, and the larger-than-own gun placebo) in the Online Appendix. The methodological lesson generalizes: channel-decomposition stories are vulnerable to placebo contamination when the channel variable is partisanloaded, and I recommend the neighbouring-district placebo as a baseline diagnostic for future work in this design family.

4.5 Sexual Misconduct as a Bounded Exception

The pooled scandal indicator falls under the valence-non-transmission contrast reported in Panel B of Table 7: small magnitude relative to the positional benchmark, marginally powered, no within-aggregate signal. Sub-category decomposition reveals that one sub-channel — sexual-misconduct allegations — is exceptional in the full sample, with a significantly negative interaction across 1,221 positive observations (Table 8). It is the only valence sub-channel with a significant negative interaction, and the clean-signal condition developed in §2 implies a specific decomposition: if scandal accountability requires an informationally clean, representative-specific signal, the response should track shifts in the informational environment surrounding the allegation type.

The sample is heavily concentrated in 2018, when the post-#MeToo wave produced 1.5% of observations against 0.2–0.4% in earlier cycles; splitting at 2018 (Table 8) yields a large, highly significant pre-2018 interaction and a null one after, and the triple interaction confirms that the within-era effect inverts after the wave.

The pattern runs opposite to a naive visibility reading — under which broader #MeToo attention should amplify accountability — and is instead what the clean-signal condition implies: the response collapses when the signal saturates. When allegations were rare (pre-2018) they functioned as clear individual signals about the specific representative implicated; once common and partisan-coded (post-2018), voters in high-congruence markets processed them through a diffuse partisan-conflict lens that did not discriminate between representatives' records. This sharpens rather than undermines the selective-activation story: valence accountability operates only where the underlying signal is informationally clean and specific to the representative, a condition pre-2018 allegations met and the other valence channels (unemployment, crime, legislative effectiveness, the

Table 8: Sexual Misconduct Sub-Channel: Pre-/Post-#MeToo Decomposition

	Estimate	SE	p-value	N (n_+)
Full Sample (Pooled)	-0.1000^{*}	0.049	0.042	268,892 (1,221)
Panel A: By Era				
Pre-2018 (2006–2016)	-0.3584^{***}	0.043	< 0.001	171,432 (371)
2018+ (#MeToo Era)	-0.0299	0.057	0.60	97,299 (850)
Panel B: Triple Interaction (Cong × Sexual × Post-2018)				
Cong × Sexual (pre-2018 base)	-0.3550^{***}	0.053	< 0.001	268,892
Cong × Sexual × Post-2018	$+0.2946^{***}$	0.082	< 0.001	268,892
Implied Post-2018 Effect	-0.0604	—	ns	—

Note: OLS estimates of the congruence-by-sexual-misconduct interaction on binary representative approval, county and year fixed effects, full controls. The pre-2018 cell uses 2006–2016 ($n_+ = 371$ positive observations); the 2018+ cell uses 2018, 2020, and 2022 ($n_+ = 850$, of which 526 are 2018). Panel B estimates a single specification with the interaction and its product with a post-2018 indicator; the implied post-2018 effect is $-0.3550 + 0.2946 = -0.0604$, not distinguishable from zero. Standard errors clustered by congressional district. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

pooled scandal indicator) meet in no period in my sample. I treat the sub-channel as a bounded exception that maps the boundary conditions of valence accountability, not a contradiction of the framework.

4.6 Robustness

Replacing DW-NOMINATE with the Fowler and Hall (2012) Conservative Vote Probability as the basis for ideological distance leaves the headline unchanged: both measures produce significant negative interactions jointly and survive the lagged-margin specification.

Adding lagged district margin, lagged Democratic winner, and lagged number of candidates to all main specifications absorbs the residual political imbalance documented in §3; consistent with that account, the substantive interactions (ideology, assistance) are essentially unchanged, confirming that the channels I identify as active are not artifacts of the within-county political variation the lagged-margin variable captures. The four-way assistance asymmetry (Republican-voter / Democratic-MC) is likewise unchanged under the full lagged-control set — its interaction coefficient shifts by under two percent — so the partisan-asymmetric spending result is not an artifact of the urban-Democratic versus rural-Republican geography behind the residual imbalance.

The within-county variation in congruence is not concentrated in extreme cases: approximately 64% of counties exhibit a within-county congruence range exceeding 0.05, and dropping the top decile of counties by within-county variation does not weaken any main result; the composition-blind pooled estimate strengthens by roughly half in that subsample, indicating that the extreme-variation counties attenuate rather than drive it (Appendix I.3). The identification is supported by broad within-county variation rather than by a small number of extreme-variation counties.

Full robustness details, including additional channel constructions (abortion- and gun-distance proxies that fail their own falsification tests), year-by-year estimates, and the full battery of placebo specifications, appear in the Online Appendix.

5 Downstream Consequences

5.1 Vote Choice

If ideological evaluation is genuine rather than merely attitudinal, it should carry through to voting behavior. It does. The congruence-by-ideological-distance interaction for vote choice is significantly negative and close in magnitude to the corresponding (composition-blind) approval interaction (Table 9). A one-standard-deviation increase in congruence strengthens the ideological-distance penalty on party voting by about 1.5 percentage points per standard deviation of distance.

The partisan split at the ballot box mirrors the approval split. The co-partisan interaction is negative and significant: voters who believe they share the representative's party redirect their vote when positioning information reveals ideological distance, just as they withdraw approval. The cross-partisan interaction is negative but not distinguishable

Table 9: Downstream Consequences: Vote Choice and Legislator Responsiveness

	Full Sample (1)	Co-Partisan (2)	Cross-Partisan (3)
Panel A: Vote Choice × Ideology Distance (Individual-Level, County FE)			
Cong × Ideo Distance	-0.0145^{***}	-0.0029^{*}	-0.0020
	(0.0024)	(0.0014)	(0.0016)
N	227,851	123,919	80,225
	District FE (4)	State FE (5)	
Panel B: Legislator Responsiveness (Cong × District Ideology → DW-NOMINATE)			
Cong × District Ideology	-0.033^{*}	-0.029^{*}	
	(0.014)	(0.013)	
N	3,548	3,553	

Note: Panel A reports OLS estimates of the congruence-by-ideological-distance interaction on voting for the party the respondent believes the representative belongs to (post-election wave, county FE; controls: age, gender, education, race, family income, party identification). Co-partisan: the respondent identifies with that perceived party; the split conditions on answering the representative-party item (see the selection discussion in the text). Panel B reports the congruence-by-district-ideology interaction predicting legislator DW-NOMINATE first-dimension scores, estimated on the district-congress panel with the indicated fixed effects (district-and-congress or state-and-congress) and no individual controls. Standard errors clustered by congressional district. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

from zero — as in approval, positioning information matters least for voters whose party label already encodes disagreement. Heuristic redundancy governs where positioning information changes behavior, in approval and vote choice alike.

Two features of the split columns require explicit statement. First, the co-/cross-partisan classification uses the party the respondent believes the representative belongs to, and the vote-choice outcome is voting for that perceived party. Both mechanisms the paper develops — the erosion of the party label's default evaluation, and partisan filtering of policy content — concern the party the voter attributes to the representative, so the classification follows that belief; the same perception-based classification is used throughout the paper. Classifying instead by the representative's actual (Voteview) party leaves the full-sample interactions unchanged and preserves the sign pattern of the splits with wider uncertainty; the ideology co-partisan cell, reclassified on identical rows, is -0.0032 ($p = 0.056$) against the -0.0054 of Table 6. Second, answering the representative-party item is itself weakly predicted by congruence ($+0.0087$, $p = 0.049$), though answering it correctly is not (-0.0036, $p = 0.23$); the split columns therefore condition on a response indicator that congruence may move, a selection concern that does not apply to the full-sample column. The paper's claims rest on the full-sample estimates, not on the splits.

The baseline congruence effect on vote choice provides interpretive context. The direct effect of congruence on voting for the perceived party of the representative is negative, consistent in sign with the composition-blind congruence main effect on approval (§4) — and, like that main effect, it is not the behaviorally identified quantity, and I do not rely on it. The identified term is the interaction with ideological distance: information rewards ideological proximity and penalizes distance rather than shifting votes uniformly.

Turnout is unaffected. The direct effect of congruence on validated voter turnout is null. Information changes who voters support, not whether they participate. This null is important for the interpretation of the vote choice results. If congruence affected both turnout and vote choice, the vote choice finding could reflect compositional changes in the electorate rather than changed evaluative criteria among the same voters. The null on turnout confirms that the mechanism operates through the content of evaluation, not through differential mobilization.

5.2 Legislator Responsiveness

The accountability loop closes on the supply side. If voters in congruent markets evaluate and vote based on ideological distance, representatives facing these electorates have incentives to reduce that distance by aligning with constituent preferences. I find evidence consistent with this expectation. The interaction between congruence and district ideology predicting legislator DW-NOMINATE scores is significantly negative under both district and state fixed effects (Table 9 Panel B), indicating that representatives in high-congruence districts track their constituents' ideological center more closely.

The interaction implies a symmetric responsiveness pattern. In a conservative district, higher congruence is associated with a more conservative DW-NOMINATE score; in a liberal district, a more liberal one. This symmetry is expected from an accountability mechanism that penalizes ideological distance rather than pushing in a particular direction.

Congruence is also associated with lower ideological extremity: the cross-sectional estimate is robust, the within-district estimate is marginal ($p = 0.054$), and party-specific estimates point in consistent directions (Democrats rightward, Republicans leftward) though neither reaches conventional significance given the modest sample of roughly 3,500 district-congress observations (Appendix G, H.4). Together, the responsiveness interaction and the extremity reduction describe representatives in congruent districts tracking their constituents' center more closely — an equilibrium in which incumbents facing more informed electorates align with constituent preferences rather than with party or personal ideology. I present these supply-side results as complementary evidence rather than a central contribution: I cannot fully separate selection from adaptation, since congruent districts may attract moderate candidates rather than inducing incumbents to moderate, and the within-district identification reduces but cannot eliminate that concern at this sample size. The findings are best understood as closing the loop rather than standing alone.

6 Conclusion

Channel selectivity, not uniform accountability, is what media congruence produces. Different media transmit different content (Gentzkow, 2006); different content activates different evaluative dimensions; and the democratic consequences of any given media environment depend on which dimensions are activated and which are left dormant. The accountability infrastructure of local television, in its current form, transmits positioning information and activates one substantive partisan-mediated channel where partisan-filtered policy attribution is the operative mechanism.

The two active channels work differently. Positional information moves evaluations where the party heuristic is least informative — among co-partisans, in approval and at the ballot box — while partisan-mediated assistance spending produces opposite-signed responses depending on the voter's partisan match with the incumbent, in the manner Bartels (2002) and Achen and Bartels (2016) predict, concentrated in the Republican-voter / Democratic-MC cell; to my knowledge this is the first decomposition of a substantive channel along the partisan-mediated dimension in the media-accountability literature. The bounded sexual-misconduct exception locates the boundary precisely: a valence signal activates accountability when it is rare and individual-specific and goes dormant once it becomes common and partisan-coded. The operative distinction is not between positional and valence dimensions per se but between information that maintains representative-specific clarity at the moment of receipt and information that does not.

The voter-knowledge first stage that underwrites these findings triangulates across three independent datasets — Snyder and Strömberg (2010)'s newspaper-congruence-to-readership, my TV-congruence-to-recognition, and Myers (2025)'s newspaper-congruence-to-state-legislator-recall estimates all remain behavioral on their own data under the compositional progression of §3 — so the channel selectivity I document operates at the second stage, where information becomes evaluation.

The co-partisan ideological finding bears on the limits of partisan loyalty. Voters who share their representative's party nonetheless penalize ideological distance when local television supplies positioning information, in both approval and vote choice — partisan loyalty is here partly an informational equilibrium rather than a fixed psychological constraint, with voters updating on independently available positioning when it contradicts the label.

Several limitations bear on the interpretation. The need for partisan-composition controls in my preferred specification is itself data-specific: within my county-and-year fixed effects, TV congruence is nearly collinear with incumbent party, a near-collinearity not present in the canonical congruence-family predecessors whose voter-knowledge findings remain behavioral on their own data under the same controls (Appendix A). The channel-level findings are identified by congruence-by-moderator interactions estimated separately from the main effect; they survive the addition of the compositional controls and their moderator interactions in sign and significance, though the ideology interaction attenuates by roughly two-thirds under that maximally demanding test (Appendix A.5). Two of four valence channels (violent crime and the pooled scandal indicator) are marginally powered against the strict half-positional threshold, though no channel exceeds seventy-two percent of the positional benchmark even at the eighty-percent-power upper bound. Residual within-county political imbalance and a modest district-level supply-side sample carry over from the empirical strategy. The exposure link itself is anchored in measured coverage — the congruence index predicts on-air mentions of the incumbent in broadcast-transcript data, in campaign and off-season windows alike (Appendix K) — but coverage *content* remains unmeasured: mention counts establish that congruent-market voters see more of their representative, not what the coverage says, and the compositional premise of §2 rests on the pattern of results rather than on direct content measurement (Martin and McCrain, 2019; Huber and Tucker, 2024).

The selective-activation pattern documented here also bears on how to read recent Senate-level evidence. Moskowitz (2026) finds that DMA in-state share produces an interactive electoral effect with senator roll-call extremity — moderates gaining and extremists losing — on incumbent vote share. That finding is consistent with the positional ideology channel I identify in Table 7 Panel A; my contribution is to show that this channel does not extend to the four valence dimensions and operates partisan-asymmetrically for distributive spending, within the same family of media-geography designs.

The research agenda this opens is to map media characteristics to accountability dimensions across institutional settings (newspapers, radio, television, digital) and levels of government, and to ask whether the selective-activation pattern I document in the U.S. House holds elsewhere. The partisan-mediated distributive-spending mechanism in particular invites comparative work: whether it generalizes to other partisan-coded policy dimensions — immigration, taxation, social-welfare programs — or is specific to the credit-claiming structure of district-targeted assistance spending is a question the present data cannot settle. What the present data do settle is what representative-specific coverage changes. Where local television supplies that coverage, voters police the representative's positions and, through a partisan filter, her district spending; where it does not, those two channels are what is missing, while the valence dimensions are dormant either way.

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A Boundary Characterization

This appendix documents the within-fixed-effects compositional boundary that bears on the pooled congruence-on-approval coefficient reported in §4 (Table 5) and disclosed in §3 (Compositional Controls in the Preferred Specification). The boundary is characterized along four dimensions: (i) the compositional progression of the pooled coefficient on three independent datasets (my CES sample on the 2006–2022 spending-matched frame of Table 5, Snyder and Strömberg (2010) NES 1982–2006, and Myers (2025) CES 2018); (ii) the within-fixed-effects correlation gradient between treatment and incumbent party across these datasets; (iii) the fixed-effects-invariance of the compositional collapse on my data; and (iv) the resulting identification specificity of the artifact.

A.1 Compositional Progression on Three Datasets

I apply a four-spec compositional progression to each dataset on its original authors' own data: (a) baseline composition-blind specification reproducing the published headline, (b) baseline + voter-incumbent partisan-match indicator, (c) baseline + incumbent-party indicator, and (d) baseline + both compositional controls jointly. Table A1 summarizes the (a) $\rightarrow$ (d) progression for each dataset.

A.1.1 My CCES 2006–2022 — congruence on representative approval. The headline congruence-on-approval coefficient ($N = 346{,}453$, county-and-year fixed effects) collapses from -0.121 (a, $p < 0.001$) to $+0.001$ (d, $p = 0.94$) under the compositional progression. Adding partisan-match alone reverses the sign ($+0.038$, $p = 0.055$); adding incumbent-party alone moves the coefficient toward zero but leaves it negative and significant (-0.074, $p = 0.009$). The two controls together exhaust the pooled-coefficient magnitude.

A.1.2 Snyder and Strömberg (2010) NES 1982–2006 — newspaper congruence on incumbent readership. I first reproduce Snyder and Strömberg (2010) Table 4 ($b = +0.278$,

Table A1: Compositional Progression on Three Datasets

Dataset / outcome	(a) baseline	(d) + both controls	Collapse ratio	Verdict
Mine: CCES, approval	-0.121^{***}	$+0.001$	0.01	**COMPOSITIONAL**
S&S: NES, readership	$+0.173^{***}$	$+0.176^{***}$	1.02	Behavioral
S&S: NES, incumbent recall	$+0.162^{***}$	$+0.161^{***}$	0.99	Behavioral
Myers: CES, legislator recall	$+0.156^{*}$	$+0.186^{*}$	1.19	Behavioral

Note: Column (a) reports the composition-blind baseline regression. Column (d) adds both the voter-incumbent partisan-match indicator and the incumbent-party indicator. The collapse ratio is column (d) divided by column (a); ratios near 1.0 indicate behavioral coefficients (unaffected by compositional controls), ratios near zero indicate compositional artifacts. Fixed-effects structures: mine, county-and-year (with demographic controls clustered at the congressional-district level, $N = 346{,}453$); S&S, state-and-year (NES 1982–2006); Myers, state (CES 2018, compositional-controls subsample $N = 765$). $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

$p = 0.0003$) on their own NES data, then apply the four-spec progression (state-and-year fixed effects, member fixed effects stripped), which reports column (a) $+0.173$, column (d) $+0.176$, a collapse ratio of 1.02. The newspaper-congruence coefficient is essentially unchanged under the compositional progression on the original authors' own data.

A.1.3 Snyder and Strömberg (2010) NES 1982–2006 — newspaper congruence on incumbent recall. I reproduce the parallel incumbent-recall finding ($b = +0.243$, $p < 0.0001$); the four-spec progression reports column (a) $+0.162$, column (d) $+0.161$, a collapse ratio of 0.99. Behavioral.

A.1.4 Myers (2025) CES 2018 — newspaper congruence on state-legislator recall. I reproduce Myers (2025) Table 2 column 1 ($b = +0.149$, $p = 0.030$) on his own CES data, then apply the four-spec progression (state fixed effects, on the compositional-controls subsample $N = 765$), which reports column (a) $+0.156$, column (d) $+0.186$, a collapse ratio of 1.19. The coefficient grows under compositional controls rather than collapsing, and statistical significance gains rather than loses (p drops to 0.026). Behavioral. The parallel progression on correct state-legislator identification is inconclusive because I did not match Myers (2025)'s Table 3 controls; future work could repeat it with those controls.

Table A2: Within-Fixed-Effects Correlation between Treatment and Incumbent Party

Dataset / outcome	FE structure	within-FE cor(treat, MC_R)	Compositional verdict
Mine: CCES, congruence $\rightarrow$ approval	county + year	$\mathbf{+0.86}$	**COMPOSITIONAL**
Myers: CES, congruence $\rightarrow$ legislator recall	state	$+0.344$	Behavioral
S&S: NES, congruence $\rightarrow$ readership / recall	state + year	$+0.105$	Behavioral
Mine: CCES, congruence $\rightarrow$ voter knowledge	county + year	—	Behavioral†

Note: Within-fixed-effects correlation is computed after partialing out the indicated fixed-effects structure from both the treatment and the Republican-MC indicator. †The within-county-and-year specification with the voter-knowledge (heard-of-representative) outcome yields a behavioral coefficient ($b = +0.117$, collapse ratio 1.23) under the same compositional progression that collapses the approval coefficient, indicating that the compositional artifact is outcome-specific rather than a generic property of within-county-and-year fixed effects applied to congruence treatments.

A.2 Within-Fixed-Effects Correlation Gradient

The mechanical source of the compositional artifact is the within-fixed-effects correlation between the treatment (TV or newspaper congruence) and an indicator for a Republican incumbent. Table A2 reports this correlation across the four datasets in §A.1, together with the parallel within-county-and-year correlation when my treatment is paired with the voter-knowledge outcome rather than approval.

The compositional artifact in my pooled approval coefficient is driven by near-collinearity between TV congruence and incumbent party within county-and-year fixed effects ($+0.86$). In Myers' state-level structure the correlation is non-trivial but substantially lower ($+0.344$), and in the Snyder and Strömberg (2010) state-and-year structure it is small ($+0.105$). My parallel within-county-and-year design with the voter-knowledge outcome is also behavioral, indicating that the compositional artifact is outcome-specific rather than a generic property of within-county-and-year fixed effects applied to congruence treatments.

A.3 Fixed-Effects-Invariance of the Compositional Collapse

The collapse documented in §4 (Table 5) for my county-and-year specification is not a feature of that one fixed-effects choice. Table A3 reports the (a) and (d) columns of the

Table A3: Compositional Progression Across Six Fixed-Effects Structures (My Data)

Fixed-effects structure	(a) baseline	(d) + both controls	Significant at $p < 0.05$?
No fixed effects	-0.100^{***}	-0.008	No
State	-0.107^{***}	-0.014	No
State + year	-0.106^{***}	-0.012	No
State $\times$ year	-0.109^{***}	-0.014	No
County + year (preferred)	-0.121^{***}	$+0.001$	No
District + year	-0.103^{*}	-0.027	No
Range	—	$[-0.027, +0.001]$	0/6 sig
Mean	-0.108	-0.012	

Note: Each row reports column (a) baseline composition-blind specification and column (d) baseline + voter-incumbent partisan-match indicator + incumbent-party indicator, applied at the indicated fixed-effects layer. Column (a) is the composition-blind baseline at each of the six fixed-effects layers. Column (d) collapses uniformly to a range of -0.027 to $+0.001$ (mean -0.012), with zero of six specifications statistically distinguishable from zero. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

compositional progression across six alternative fixed-effects structures, applied to the same congruence-on-approval specification on my CCES data.

The compositional collapse is fixed-effects-invariant on my data. The composition-blind specifications across all six fixed-effects layers reported in §3 collapse uniformly when the two compositional controls of column (d) are added, with zero of six statistically distinguishable from zero. The within-fixed-effects correlation between TV congruence and incumbent party remains in the near-collinear region at every layer, consistent with the FE-invariance of the column-(d) collapse.

A.4 Identification Specificity of the Compositional Artifact

The compositional artifact requires near-collinearity between TV congruence and an incumbent-party indicator within the operative fixed-effects layer. This property is specific to certain (treatment $\times$ outcome $\times$ fixed-effects) combinations and is not a general property of media-congruence research designs. The two predecessors closest to my setting in the congruence-family literature — Snyder and Strömberg (2010) (newspaper congruence on voter knowledge, state-and-year fixed effects) and Myers (2025) (newspaper congruence on state-legislator recall, state fixed effects) — do not exhibit the artifact on their own data because the within-fixed-effects correlation between treatment and incumbent party in their data structures is well below the near-collinear threshold ($+0.105$ and $+0.344$, respectively, versus my $+0.86$). My own within-county-and-year specification paired with the voter-knowledge (heard-of-representative) outcome is also behavioral, indicating that the artifact is specific to the combination of my outcome (binary representative approval), my treatment (DMA-CD overlap as measured here), and my identifying variation (within county-and-year fixed effects). The boundary documented here applies to this specific combination and should not be read as a methodological critique of the broader literature.

A.5 Channel Interactions under Compositional Controls

The channel-by-moderator interactions reported in §4 (Tables 6 and 7) are identified by interaction terms rather than by the congruence main effect, but they are not automatically immune to the compositional issue documented in §A.1–§A.4: within county-and-year fixed effects on the channel estimation sample, the correlation between congruence and the incumbent-party indicator is $+0.87$, and the congruence-by-ideology interaction is correlated with the incumbent-party-by-ideology interaction at $+0.996$. Adding the compositional controls and their moderator interactions is therefore a maximally demanding test of the channel findings. Table A4 reports the progression for the ideology interaction in the ideology-focused (Table 6) specification and the joint five-channel (Table 7) specification: rung (a) is the published baseline; rung (b) adds the partisan-match and incumbent-party main effects; rung (c) additionally adds incumbent-party-by-moderator and partisan-match-by-moderator interactions for every channel moderator.

Under the most demanding rung the ideology interaction retains its sign and statistical significance in both samples but attenuates by roughly two-thirds relative to baseline. The attenuation is the expected price of near-collinearity: the incumbent-party-by-ideology

Table A4: Ideology Interaction under Compositional Controls

Specification	Rung	b (cong $\times$ ideo)	SE	p
Ideology (Table 6)	(a) baseline	-0.0130	0.0019	< 0.001
	(b) + compositional main effects	-0.0068	0.0012	< 0.001
	(c) + compositional $\times$ moderator	-0.0048	0.0012	0.0001
Joint (Table 7)	(a) baseline	-0.0122	0.0023	< 0.001
	(b) + compositional main effects	-0.0062	0.0015	< 0.001
	(c) + compositional $\times$ moderator	-0.0038	0.0015	0.011

Note: OLS estimates of the congruence-by-ideological-distance interaction on binary representative approval, county-and-year fixed effects, demographic controls, standard errors clustered by congressional district. Ideology specification: $N = 348{,}118$; joint specification: $N = 249{,}912$ (both restricted to observations with non-missing compositional-control inputs, which is why the rung-(a) baselines differ marginally from the published full-sample estimates). Rung (b) adds the voter–incumbent partisan-match and incumbent-party indicators; rung (c) additionally interacts both with every channel moderator. In the joint rung-(c) specification the spending interaction survives (-0.0054, $p = 0.005$), the unemployment, crime, and scandal interactions remain null, and the congruence main effect is statistically zero. $^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$.

term is itself significant, and the two terms split variance that the composition-blind specification attributes wholly to congruence. The channel selectivity pattern — ideology and spending active, valence dormant, pooled main effect zero — is unchanged.

B Variable Definitions and Data Construction

B.1 TV Congruence Measure

Following Snyder and Strömberg (2010), TV congruence is measured as the sum of audience-share products across all DMAs overlapping a congressional district. Formally, for district d:

$$\text{Congruence}_d = \sum_m \frac{\text{pop}_{md}}{\text{pop}_m} \times \frac{\text{pop}_{md}}{\text{pop}_d}$$

where pop_{md} is the population in the overlap of district d and DMA m, pop_m is the total DMA population, and pop_d is the total district population. The first ratio is the DMA audience share residing in the district (the media market's incentive to cover that district's representative); the second is the district population share served by that DMA. The measure equals 1 when district and DMA boundaries perfectly coincide, and approaches 0 when the district is a small fraction of a large DMA or is split across many DMAs. DMA-county assignments come from the Nielsen DMA crosswalk (Peterson), updated periodically. Congressional district boundaries are drawn from Census TIGER/Line shapefiles, with county-to-district assignments based on population-weighted areal interpolation for split counties.

B.2 Ideological Distance

Ideological distance is the absolute difference between the respondent's five-point ideology self-placement (rescaled to [-1, 1]) and the representative's DW-NOMINATE first-dimension score (which ranges approximately from -1 to 1). The rescaling maps the CES ideology measure to the DW-NOMINATE scale to ensure comparability.

B.3 Legislative Effectiveness Score (LES)

I use the Volden and Wiseman (2014) Legislative Effectiveness Score, which captures a member's ability to advance bills through the legislative process. The score is standardized within each congress to have mean 1.

B.4 Sample Construction

The analysis sample begins with all CES respondents from 2006 to 2024 who can be assigned to a congressional district (N 537,000). I exclude respondents with missing congressional district assignments or missing values on the key outcome (representative approval). The first-stage knowledge analysis is restricted to 2008–2024; the recognition item enters the CES in 2008. Vote choice analysis is restricted to respondents who completed the post-election wave and reported a valid House vote (roughly half of the full sample). The district-level analysis of legislator responsiveness uses a panel of approximately 3,500 district-congress observations.

District identifiers. At-large seats are coded `-00` in the Ferrara DMA-CD crosswalk and `-01` in the CES, Volden–Wiseman LES, and Voteview files; the two forms are harmonised to `-01`, so Delaware, Montana, North Dakota, South Dakota, Vermont, and Wyoming enter every wave. MIT Election Lab returns are harmonised on the same rule (`0` to `01`).

Alaska and Hawaii. The Ferrara crosswalk carries no population weights for Alaska or Hawaii before the 118th Congress; both states are therefore also dropped at the 118th, so the frame covers the same 48 states in every wave.

118th-Congress crosswalk. Missouri has no rows in the Geocorr county-to-CD118 file, and Connecticut appears there only as 2022 planning regions. Both are rebuilt from the Census 2020 block assignment file crossed with PL 94-171 block populations and spliced in, Connecticut on its eight pre-2022 counties.

DMA name splits. Market names carried in two forms across sources are recoded to one canonical market: Harrisonburg and Charlottesville (ICPSR 22720 `dmaindex`, Peterson, SVW), and the bare strings `Springfield'' and`Lafayette’’, which otherwise pool Springfield (MO) with Springfield (MA)–Holyoke and Lafayette (LA) with Lafayette (IN).

C First Stage — Additional Results

C.1 State FE vs. County FE Comparison

The first-stage estimates (congruence on the heard-of-representative measure) across specifications are reported below.

Specification	Estimate	SE	p-value	N
State + Year FE	+0.094	0.013	<0.001	483,909
County + Year FE	+0.059	0.020	0.003	483,841
State x Year FE	+0.096	0.012	<0.001	483,909

All three rows are estimated on the same sample and control set and report the estimate on the raw per-unit congruence scale; multiplying by the congruence standard deviation (0.191) gives the per-standard-deviation effect ($+0.0180$, $+0.0113$, and $+0.0182$, respectively).

The 37% attenuation from state to county FE is consistent with the limited within-county variation in congruence. The within-county standard deviation of congruence is 0.065, approximately 34% of the total standard deviation (0.191). County FE absorb the cross-sectional variation that drives the larger state FE estimate.

C.2 Correct Party Identification

As a secondary knowledge measure, I test whether congruence increases the probability of correctly identifying the representative's party. With county FE, the estimate is -0.012 (SE = 0.009, p = 0.22), a null: congruence improves recognition of the representative (C.1) without improving recall of the representative's party label. I do not rely on this measure for subsequent analysis.

C.3 News Interest

Congruence raises general political news interest: +0.026 (SE = 0.009, p = 0.003) with county FE, about half the recognition effect. The ratio of the two estimates is similar in the 2008–2018 and 2020–2024 sub-windows, so the cell is not an artifact of the added waves. I read it as a secondary outcome of information supply — more representative-relevant local content is also more local political content — rather than as a placebo, since a null was never the value a supply account predicts. The demand-side sorting concern that a placebo framing would address is handled by the within-county identification: respondents do not relocate when redistricting changes their county's congruence.

C.4 Redistricting Window

Restricting the sample to congresses immediately surrounding the 2010 redistricting cycle (110th–113th Congresses) yields a stronger first-stage estimate: +0.117 (SE = 0.039, p = 0.003). This result is consistent with redistricting as the primary source of within-county congruence variation and provides additional evidence for the informational channel.

D Baseline Accountability — Additional Specifications

The analyses in this appendix operate on the composition-blind specification (Table 4). Per §3–§4, the pooled congruence-on-approval estimate is compositional rather than behavioral under the preferred specification; the results below characterize the composition-blind benchmark and are reported as diagnostics, not as estimates of a behavioral accountability effect.

D.1 DW-NOMINATE Control

Adding the representative's DW-NOMINATE first-dimension score as a control yields a congruence estimate of -0.066 (SE = 0.041, p = 0.11; Table 4 column 3, District + Year fixed effects). Relative to the County + Year baseline of -0.097 (Table 4 column 2) this is a roughly one-third smaller magnitude and no longer significant, though the comparison spans two fixed-effects structures and so bounds rather than isolates the DW-NOMINATE adjustment. The attenuation is consistent with ideology being a channel through which congruence affects approval rather than a confound; per §3–§4, the composition-blind level estimate is in any case a diagnostic rather than a behavioral quantity.

D.2 Presidential Approval Placebo

The congruence effect on presidential approval is -0.011 (SE = 0.014, p = 0.46) with county FE: a null, against a representative-approval benchmark of -0.097. Congruent-market voters do not simply disapprove of all politicians; whatever the composition-blind representative-approval estimate captures is specific to the House incumbent.

D.3 News Interest Interaction

The interaction between congruence and high political news interest is -0.065 (SE = 0.019, p < 0.001). The baseline accountability effect is concentrated among politically attentive voters, inconsistent with a pure media negativity story (which would predict uniform effects) and consistent with an information-processing mechanism.

E Competing Channels — Detailed Results

E.1 Congruence x Unemployment

Subsample	Estimate	SE	p-value
Full	+0.0039	0.002	0.046
Co-partisan	+0.0041	0.001	0.005
Cross-partisan	+0.0048	0.002	0.012

All estimates in this appendix section are on the standardized congruence $\times$ standardized unemployment scale (the scale of the Table 7 Panel B benchmark).

The positive sign is the opposite of what an economic accountability story predicts. If congruence enabled voters to punish representatives for poor economic conditions, the interaction should be negative. The positive coefficient, if anything, suggests that informed voters in areas with high unemployment are slightly more likely to approve of their representative, perhaps because TV coverage provides context that separates the representative's actions from macroeconomic conditions.

E.2 Congruence x Legislative Effectiveness Score

Subsample	Estimate	SE	p-value
Full	-0.0011	0.001	0.445
Co-partisan	-0.0003	0.001	0.838
Cross-partisan	-0.0015	0.002	0.352

The null results are precisely estimated across all subsamples. Voters do not use local TV information to evaluate representatives on legislative productivity. This is consistent with the theoretical expectation that LES-type information rarely appears in television coverage.

E.3 Idiosyncratic Unemployment Decomposition

To distinguish local economic conditions from the national business cycle, I decompose county-level unemployment into a national component and an idiosyncratic residual. The idiosyncratic component is the residual from regressing county unemployment on national unemployment, capturing county-specific deviations from the aggregate economic trend. The correlation between county and national unemployment is 0.77, indicating substantial shared variation.

The congruence interaction with the idiosyncratic unemployment residual is +0.001 ($p = 0.889$). This null strengthens the case that congruence does not activate a local economic attribution channel. When the national business cycle component is removed, the local-specific economic signal produces no differential evaluation in congruent versus in-congruent markets. The mechanism, to the extent it exists in the pooled estimate, operates through aggregate economic conditions rather than county-specific deviations.

E.4 National Unemployment Placebo

As a placebo test, I interact congruence with national unemployment. If the economic interaction in E.1 reflects local economic attribution, congruence should not interact with national unemployment (which is absorbed by year fixed effects in its main effect). The interaction is +0.0033 ($p = 0.038$), similar in magnitude to the county-level interaction (+0.0035, $p = 0.085$).

This result must be interpreted in light of the high correlation between county and national unemployment ($r = 0.77$). Because the main test and the placebo share substantial variation, a similar interaction coefficient does not imply that congruence specifically enables national economic attribution. Rather, it confirms that the marginal county-level interaction (E.1) is driven primarily by the shared national component of unemployment. The critical result is E.3: the idiosyncratic unemployment residual—which is orthogonal to the national business cycle—produces a null interaction ($p = 0.889$). This confirms that congruence does not enable locally specific economic attribution, consistent with the theoretical prediction that local TV conveys political positioning information rather than economic attribution information.

E.5 Partisan Heterogeneity in Economic Channel

The pooled congruence-unemployment interaction (+0.0035, $p = 0.085$) masks opposing partisan effects:

Partisan Group	Estimate	SE	p-value
Democrats	+0.032	0.012	<0.01
Republicans	-0.015	0.009	0.098

Democrats in congruent markets with high unemployment increase approval of their representative, while Republicans show a weak tendency toward economic punishment. This pattern suggests that congruence may activate partisan-differentiated economic narratives rather than a uniform economic accountability channel. One interpretation is that Democratic voters, when exposed to more information about their representative, attribute poor economic conditions to national (presidential or opposition-party) rather than local causes, reinforcing support for their representative. Republican voters may engage in the opposite attribution pattern.

This heterogeneity complicates the clean "null economic channel" interpretation but remains consistent with the selective accountability framework. The dominant evaluative channel activated by congruence is ideological, as demonstrated by the strong and consistent ideology distance interaction across partisan groups. The economic channel, to the extent it operates, does so in partisan-specific ways that cancel in the pooled estimate.

E.6 Unemployment Residual (Presidential Approval Partialed Out)

After partialing out presidential approval from unemployment, the congruence x unemployment-residual interaction is +0.0006 ($p = 0.818$). The economic channel is null even after removing the presidential approval component of unemployment effects.

F Vote Choice — Additional Results

F.1 Direct Congruence Effect on Vote Choice

The main effect of congruence on voting for the party the respondent believes the representative belongs to is negative:

Subsample	Estimate	SE	p-value	N
Full	-0.0201	0.0058	0.0005	261,194
Co-partisan	-0.0025	0.0014	0.069	142,879
Cross-partisan	-0.0003	0.0039	0.949	91,603

The direct effect is negative in the full sample, marginal among co-partisans, and null among cross-partisans. Like the composition-blind congruence main effect on approval (§4), the level estimate conflates information effects with the partisan composition of who receives congruent coverage, so it is not the behaviorally identified quantity, and no result in the paper relies on it. The identified quantity is the interaction with ideological distance (F.3): information rewards ideological proximity and penalizes distance.

F.2 Turnout

Congruence has no effect on validated voter turnout: -0.001 (SE = 0.003, p = 0.770, N = 524,389). Information changes the direction of the vote, not whether voters participate.

F.3 Vote Choice x Ideology Distance by Partisan Group

Subsample	Cong $\times$ ideology distance	SE	p-value	N
Full	-0.0145	0.0024	<0.001	227,851
Co-partisan	-0.0029	0.0014	0.030	123,919
Cross-partisan	-0.0020	0.0016	0.20	80,225

The split mirrors the approval results: the co-partisan interaction is negative and significant, and the cross-partisan interaction is negative but not distinguishable from zero. Positioning information moves approval and vote choice through the same group — voters for whom a shared party label is least informative about position.

G District-Level Outcomes

G.1 District FE + Congress FE

Outcome	Estimate	SE	p-value
Extremity (|D1|)	-0.099	0.051	0.054
Margin	-0.016	0.045	0.731
Incumbent share	-0.041	0.041	0.326
Log votes (turnout)	+0.392	0.328	0.233
LES	-0.049	0.359	0.892

With the demanding district-and-congress fixed effects, no outcome clears conventional significance; extremity comes closest ($p = 0.054$). Together with the cross-sectional comparison in G.2, the pattern supports the characterization used in the main text: an extremity effect that is precise in the cross-section and marginal within districts.

G.2 State FE (Cross-Sectional Comparison)

Outcome	Estimate	SE	p-value
Extremity	-0.089	0.029	0.002
Margin	-0.110	0.034	0.001
Log votes	+0.264	0.108	0.015

With state FE, multiple outcomes are significant. The discrepancy between state and district FE estimates suggests that cross-sectional variation in congruence correlates with district characteristics that independently affect electoral outcomes. The extremity result — precise here, marginal under the within-district specification (G.1) — is the most credible district-level effect, on the strength of its cross-sectional robustness.

H Legislator Responsiveness — Additional Results

H.1 Congruence x District Ideology → DW-NOMINATE

Specification	Estimate	SE	p-value	N
District FE	-0.033	0.014	0.019	3,548
State FE	-0.029	0.013	0.022	3,553

The responsiveness interaction is consistent across both specifications, supporting the interpretation that representatives in congruent districts track constituent preferences more closely.

H.2 Party-Specific DW-NOMINATE Shifts

Party	Estimate	SE	p-value
Democrats	+0.046	0.031	0.14
Republicans	-0.032	0.023	0.16

Both parties shift toward the center in congruent districts, though neither estimate is individually significant. The consistent direction supports the moderation interpretation.

H.3 Ideology Gap (|Legislator - District|)

Specification	Estimate	SE	p-value
District FE	-0.016	0.010	0.117
State FE	-0.003	0.006	0.598

The direct effect of congruence on the ideology gap is null under both specifications. The interaction specification (H.1) provides the evidence for responsiveness; the direct gap measure does not.

H.4 Extremity Reduction

Congruence is associated with lower ideological extremity (absolute DW-NOMINATE):

Specification	Estimate	SE	p-value
District + Congress FE	-0.0195	0.0101	0.054

This is the same specification as the extremity row of G.1, reported there per raw congruence unit (-0.099) and here per standard deviation of congruence; the two entries differ only in scaling. The estimate is marginal within districts and precise in the cross-section (G.2). Read jointly, representatives in congruent districts are less extreme, with the within-district evidence suggestive rather than conclusive.

I Additional Robustness Analyses

Analyses in this appendix that involve the pooled congruence-on-approval estimate (the approval row of I.2, and I.3) operate on the composition-blind specification; per §3–§4 those results characterize the composition-blind benchmark rather than a behavioral accountability effect. The first-stage and channel-interaction analyses are unaffected.

I.1 Power Analysis for County FE First Stage

Within-county variance accounts for 11.2% of total congruence variance, and the resulting county FE standard error is 1.6 times the state FE standard error. The minimum detectable effect at 80% power is 0.055; the observed county FE estimate (0.059, p = 0.003) exceeds it and sits just below the 90%-power threshold (0.064). If the true effect equals the state FE estimate (0.094), the county FE specification detects it with power 0.998.

The power curve is as follows:

True Effect Size	Power
0.03	0.33
0.04	0.53
0.05	0.72
0.06	0.86
0.07	0.95
0.08	0.98
0.10	1.00

The county FE design trades precision for internal validity by absorbing cross-sectional confounds; at the observed effect size, the design is adequately powered rather than marginal.

I.2 Leave-One-State-Out Jackknife

To assess whether any single state drives the main results, I re-estimate each specification dropping one state at a time (48 states).

Analysis	Range	Sign Consistent	Most Influential State
First stage: heard-of-rep (county FE)	[0.051, 0.072]	All positive (48/48)	CA (+0.071)
Approval (county FE, composition-blind)	[-0.110, -0.086]	All negative (48/48)	CA (-0.110)
Ideology interaction (county FE)	[-0.0136, -0.0120]	All negative (48/48)	IL (-0.0120)

All three estimates maintain their sign across all 48 leave-one-out samples. No single state drives the results. The most influential state for the approval estimate is California (dropping CA moves it from -0.097 to -0.110, a 13% increase), consistent with California containing the largest and most heterogeneous media markets, which attenuate the pooled estimate.

I.3 Within-County Congruence Variation

The county FE identification relies on temporal variation in congruence within counties, driven primarily by redistricting. Of 3,000 counties in the sample, 63.8% experience a congruence range exceeding 0.05, and 46.5% exceed 0.10. The median within-county standard deviation is 0.028. Redistricting produces meaningful congruence changes: 559 counties experience absolute changes exceeding 0.10 between pre- and post-2010 redistricting periods.

As a sensitivity check, I drop the top decile of counties by within-county congruence variation (300 counties, 7.7% of observations). The composition-blind approval estimate strengthens from -0.097 (SE = 0.026, $p < 0.001$) to -0.149 (SE = 0.039, $p < 0.001$). The results are not driven by outlier counties with extreme congruence changes; if anything, these counties attenuate the estimate.

I.4 Campaign Spending Control

To distinguish news coverage from campaign advertising as the information source, I add log total campaign disbursements (from the DIME database, Bonica (2014)) as a control in the ideology interaction specification (matched sample $N = 348{,}952$). Campaign spending is significant (coefficient = -0.023, SE = 0.003, $p < 0.001$), indicating that higher-spending races are associated with lower approval, perhaps reflecting increased competitiveness. However, the congruence-by-ideological-distance interaction is unchanged: the attenuation is 0.2% (from -0.0131 to -0.0131). The ideological evaluation channel operates through news coverage, not campaign advertising.

I.5 Newspaper Data Limitation

A natural extension is to control for newspaper congruence to distinguish TV from print media channels. Historical newspaper circulation data (ICPSR 30261, US Newspaper Panel 1869–2004) is available but does not extend to the study period (2006–2024). Constructing a newspaper congruence measure analogous to the TV congruence measure would require contemporary circulation-by-county data that is not publicly available.

J Residual Political Imbalance: Structural-Geography Tests

The within-county-and-year specification leaves a small but significant residual correlation between congruence and lagged district partisanship (Table 2 Panel D). This appendix documents that the residual is structural geography rather than partisan redistricting endogeneity, supporting the identification claim in §3.

J.1 Partisan Alignment Balance Test

If the residual imbalance reflected partisan gerrymandering — line-drawers shaping districts in a way correlated with DMA structure — the alignment between the line-drawing party and a district's partisan lean should predict congruence in the same direction under both Republican and Democratic control. It does not. I classify each post-2010 state-cycle by redistricting authority (Republican-controlled, Democratic-controlled, independent commission, court, split, single-district) and regress congruence on an indicator for whether the district's lagged winner matches the line-drawing party, within county and year fixed effects.

Sample	Alignment effect	SE
Pooled (R + D controlled)	+0.026***	0.008
Republican-controlled only	+0.031***	0.008
Democratic-controlled only	-0.031**	0.011

The sign reverses between Republican- and Democratic-controlled regimes. A partisan-gerrymandering mechanism would produce the same sign in both; the reversal rejects it.

Two further checks confirm the pattern is not partisan. First, the same imbalance appears in venues where partisans did not draw the lines:

Venue	Pseudo-alignment effect
Independent commissions	+0.028**
Court-drawn maps	+0.035*
Split-control states	-0.007 (null)

Second, the mid-cycle Pennsylvania and North Carolina court redraws that explicitly reversed Republican gerrymanders did not change the alignment-congruence relationship: a difference-in-differences estimate of the alignment effect before versus after the court intervention is -0.007 ($p = 0.77$, null).

J.2 Structural-Geography Decomposition

The residual imbalance is instead a mechanical consequence of district-to-media-market size geography. For a district lying within a single DMA, congruence reduces to $\text{pop}_d/\text{pop}_m$: urban Democratic-leaning districts occupy a small slice of a large metropolitan DMA (low congruence), while rural Republican-leaning districts nearly fill a smaller market (high congruence).

A natural alternative explanation is DMA fragmentation — Democratic districts split across more markets. The data reject it. Within county and year fixed effects, Democratic districts are associated with fewer, not more, DMAs (-0.30, $p < 0.001$), and controlling for the number of DMAs does not attenuate the lagged-partisanship imbalance:

Specification	Lagged-Democratic coefficient	% of baseline
Baseline	-0.0267***	100%
+ Number of DMAs (factor)	-0.0323***	121%
+ Log number of DMAs	-0.0314***	117%

The imbalance survives even among single-DMA districts, where fragmentation is zero by construction: the lagged-Democratic coefficient is -0.023 ($p < 0.001$) in the single-DMA stratum, and the interaction of lagged partisanship with DMA count is null (+0.0003, $p = 0.97$). Fragmentation is not the channel; district-to-DMA relative size is. The residual imbalance reflects the same urban-Democratic-versus-rural-Republican geography across every redistricting regime, consistent with the placebo pattern in J.1.

K Direct First-Stage Validation: Congruence and Measured Broadcast Coverage

K.1 Data and Construction

The coverage condition of §2 has two parts: congruence must raise the volume of representative-specific coverage, and the incremental coverage must carry positional content. This appendix tests the first part directly against measured broadcast coverage. The data are the replication files of Huber and Tucker (2024) (Harvard Dataverse doi:10.7910/DVN/0TCU7L), built from the Martin and McCrain (2019) TVEyes archive of local television news transcripts: for each of 701 local stations, counts of news segments mentioning each House incumbent, with campaign-advertisement segments filtered out, over six windows spanning March 2017 through October 2018 — three 2017 windows and two early-2018 windows (jointly, "off-season") and the September 7–October 1, 2018 general-election window ("campaign").

I collapse the station-by-district file to one observation per district. For district d and media market m, let mentions$_m$ be the mean per-week ad-filtered mention count across market-m stations, and let w_{md} be the share of district d's population residing in market m — the same weights that enter the congruence index of §3. Expected per-resident weekly exposure is $E_d = \sum_m w_{md}\,\text{mentions}_m / \sum_m w_{md}$; the observed markets account for at least 96 percent of district population in every district (median 100 percent), so the normalization is essentially inert. The regressor is the paper's own congruence index at its 115th-Congress values, standardized. Three districts with multiple incumbents during the archive window (OH-12, TX-27, WA-5 special-election turnover), Alaska (missing market-share data), and Hawaii (absent from the congruence panel) are excluded, leaving 428 districts. Because Pennsylvania's districts were redrawn mid-cycle by court order in February 2018, specifications marked "no PA" exclude Pennsylvania. Coverage is scarce in levels, consistent with Huber and Tucker (2024): the median district's incumbent draws 1.7 expected mentions per week in the campaign window and 0.2 per week off-season.

K.2 Results

	(1) Campaign	(2) Campaign	(3) Campaign, no PA	(4) Off-season 2017	(5) Off-season 2018-pre, no PA
Congruence (std.)	+0.562***	+0.567***	+0.563***	+0.240***	+0.375***
	(0.051)	(0.051)	(0.052)	(0.017)	(0.025)
Controls		Yes	Yes	Yes	Yes
Districts	428	428	411	428	411
Adj. R^2	0.215	0.313	0.331	0.390	0.473

Notes: OLS at the district level; the dependent variable is $\log(1 + E_d)$ for the window indicated. Heteroskedasticity-robust standard errors in parentheses. Controls: absolute Cook PVI, log district population, log station count, and archive-coverage share. *** $p < 0.001$.

A one-standard-deviation increase in congruence (0.20 on the index, against a sample mean of 0.25) raises expected weekly on-air mentions of the incumbent by roughly 76 percent in the campaign window and by 27 to 46 percent in the off-season windows; congruence alone accounts for 21.5 percent of the cross-district variance in campaign-season coverage (column 1). The estimates are insensitive to the mid-cycle Pennsylvania redraw and robust to construction choices: summing rather than averaging station mentions within markets yields +0.69 (SE 0.06), and an unlogged levels specification yields +4.98 expected weekly mentions per standard deviation ($p < 0.001$).

Two readings of this table bear on the design. First, it is the supply-side counterpart of the voter-side first stage in Table 3: together the two trace congruence $\rightarrow$ coverage $\rightarrow$ recognition, with the middle link measured directly rather than assumed. The estimate is also consistent with Huber and Tucker (2024)'s station-level finding that audience-share overlap predicts coverage; the specification here aggregates that relationship to the district level using the paper's own treatment variable. Second, the off-season columns matter independently of the campaign column: coverage levels outside campaigns are low, but the congruence *gradient* persists at high precision, so congruent-market voters receive more representative-specific information year-round, not only in the weeks before an election. What the counts cannot supply is content — these data record that an incumbent was mentioned, not what the segment said — which is why the compositional premise of §2 rests on the pattern of results rather than on direct content measurement.