Supplementary Materials for "Statehouse Democracy without the Electoral Connection: Local News and Representation in State Legislatures"

March 15, 2024

A Text Analysis Procedure

Text analysis results in Section 2.2 of the main paper use a dictionary approach. I begin by identifying a sample of newspapers that exist in the circulation dataset obtained from the Alliance for Audited Media (AAM) and my corpus of newspaper text. I identify 287 newspapers that are in both datasets.

For each newspaper, I constructed a dataset with the names of all state legislators from any state in which the newspaper sells copies, dropping non-neighboring states. Legislator names are collected from Klarner (2018) for the years 2011-2016, and a combination of LegiScan data and manual searches on Ballotpedia and state legislative websites for the remaining years. From this list of names, I produced a dictionary of search terms for each newspaper that combine the names of legislators and the name of the chamber or title of members of that chamber. I followed an identical process for members of Congress, using names from Lewis et al. (2023), and governors, using names from Kaplan (2021).

Finally, I wrote a Python script to search all 45 million newspaper articles in the corpus for stories referencing one or more legislators. I aggregate these numbers up to produce a count of the total stories in each newspaper-year for each legislator, taking care that all legislators in the search dictionary are accounted for, even if they turned up no hits in the searching process. I can then match these counts to ReaderShare_{md} from Equation (1) in the main text for newspaper-district pairs in each year to produce the results in Table 1.

B Computing Congruence

Newspaper congruence is computed using circulation within each district. Circulation data from the Alliance for Audited Media (AAM) is reported at the county level. Following Snyder and Strömberg (2010), I assume that circulation within counties is distributed according to population. This allows me to project circulation to the district level using the formula

$$\text{Circulation}_{mcd} = \text{Circulation}_{cm} \frac{\text{Population}_{cd}}{\text{Population}_{c}}, \tag{A1}$$

where Circulation_{cm} is newspaper m's circulation in county c, and $\frac{\text{Population}_{cd}}{\text{Population}_{c}}$ is the share of county c's population that lives in district d.

This forms the core building block of Congruence_d. From Circulation_{mcd}, I compute the following quantities:

$$\begin{aligned}
\text{Circulation}_{md} &= \sum_{c} \text{Circulation}_{mcd} \\
\text{Circulation}_{m} &= \sum_{d} \text{Circulation}_{md} \\
\text{Circulation}_{d} &= \sum_{m} \text{Circulation}_{md} \\
\text{ReaderShare}_{md} &= \frac{\text{Circulation}_{md}}{\text{Circulation}_{m}} \\
\text{MarketShare}_{md} &= \frac{\text{Circulation}_{md}}{\text{Circulation}_{d}} \\
\text{Congruence}_{d} &= \sum_{m} \text{ReaderShare}_{md}\text{MarketShare}_{md}
\end{aligned} \tag{A2}$$

To compute TVCongruence_d, I rely on a similar assumption that rates of TV viewership are constant within media markets. This allows me to estimate ViewerShare_{md} according to the population of districts and Designated Market Areas (DMAs), using the equation in of the main paper. I compute MarketShare_{md} for each DMA-district pair using the formula

$$\text{MarketShare}_{md} = \frac{\text{Population}_{md}}{\text{Population}_{d}}, \tag{A3}$$

which is the share of each district that exists in each media market. In almost all cases, $\text{MarketShare}_{md} = 1$ because districts are not split across markets.

For some robustness tests in Appendices F and H, I use newspaper circulation data from the Standard Rate and Data Service (SRDS) *Circulation* handbook. These data are available for 2008, 2014, and 2018, so I linearly impute county-level circulation for each newspaper. I discuss the SRDS data in more detail below.

C MRP Estimates of District-Level Opinion

Here, I provide technical details of how I estimated issue opinion at the state legislative district level. To do so, I use Multilevel Regression and Poststratification (MRP) (Park, Gelman and Bafumi 2004), which can produce reliable estimates of subnational opinion from national polls, even with sparse data at units as small as state legislative districts (Lax and Phillips 2009; Warshaw and Rodden 2012). Generally, MRP proceeds in two steps. First, a predictive model is fit—typically using hierarchical logistic regression—of individual opinion using demographic and geographic variables. This model can be used to predict average opinion among demographic subgroups in each geographic area (e.g., among Black women with a college degree aged 30-44 in Alabama). Then, these estimates are "poststratified" to the geography of interest by taking a weighted average using the known distribution of the demographic subgroups in the population as the weights.

I produce MRP estimates from the Cooperative Election Study (CES, formerly CCES), which includes approximately 60,000 respondents per survey. I begin by fitting the below predictive model using the `vglmer` package in `R` (Goplerud 2023):

$$
\begin{aligned}
Pr(\text{Opinion}_i = 1) = \text{logit}^{-1}(\beta_0 &+ \alpha^{\text{race}}_{g[i]} + \alpha^{\text{sex}}_{g[i]} + \alpha^{\text{educ}}_{g[i]} + \alpha^{\text{race}\times\text{sex}}_{g[i]} + \alpha^{\text{district}}_{d[i]} + \alpha^{\text{race}\times\text{educ}}_{g[i]} \\
&+ \alpha^{\text{sex}\times\text{educ}}_{g[i]} + \alpha^{\text{race}\times\text{sex}\times\text{educ}}_{g[i]} + \alpha^{\text{race}\times\text{district}}_{g[i]} + \alpha^{\text{sex}\times\text{district}}_{g[i]} \\
&+ \alpha^{\text{educ}\times\text{district}}_{g[i]} + \alpha^{\text{race}\times\text{sex}\times\text{district}}_{g[i]})
\end{aligned}
$$

$$
\begin{aligned}
\alpha^j_g &\sim N(0, \sigma^2_g) \text{ for all } g \text{ and } j \\
\alpha^{\text{district}}_d &\sim N(\alpha^{\text{state}}_{s[d]} + \text{s}(\text{Evang}_d) + \text{s}(\text{RepVote}_d) + \text{s}(\text{UrbanPct}_d) + \text{s}(\text{MedIncome}_d), \\
&\qquad \sigma^2_{\text{district}}) \\
\alpha^{\text{state}}_s &\sim N(\alpha^{\text{region}}_{m[s]}, \sigma^2_{\text{state}}) \\
\alpha^{\text{region}}_m &\sim N(0, \sigma^2_{\text{region}})
\end{aligned}
\tag{A4}
$$

where Opinion_i is respondent i's response to a policy question in the CES; α^j_g indexes random effects on demographic characteristics and interacted characteristics, and s($\cdot$) refers to a smoothing spline over a continuous predictor at the district level. For each question, I fit separate models for upper- and lower-chamber legislative districts in each year of the survey.

In fitting the predictive model, I must also account for uncertainty in matching respondents to districts. Appendix D describes this procedure, which results in a probabilistic matching of respondents to districts. I weight by these probabilities in the MRP model, following the weighting procedure from Ghitza and Gelman (2013). Each respondent is included in the dataset once for each district with a nonzero probability, but they are weighted such that the sum of their weights is equal to 1.

I follow the usual poststratification procedure described by Lax and Phillips (2009).

D Matching the CES to State Legislative Districts

The CES includes granular location data for respondents, including state, ZIP code, and county. However, it does not include state legislative district. I use ZIP codes and counties to match respondents probabilistically to districts. My approach is similar to that of Steelman and Curiel (2023), who use ZIP codes to match individuals to districts. However, I take advantage of more granular geography by using the overlap between ZIP codes and counties.

Technically, there is no official record of the boundaries of ZIP codes, which are created by the United States Postal Service to aid in mail delivery, and can be updated to better serve this purpose. However, the U.S. Census Bureau collects data by ZIP Code Tabulation

Areas (ZCTAs), which approximate ZIP codes. This approximation is imperfect,[1] though I use it here as the best available substitute for "true" ZIP codes.

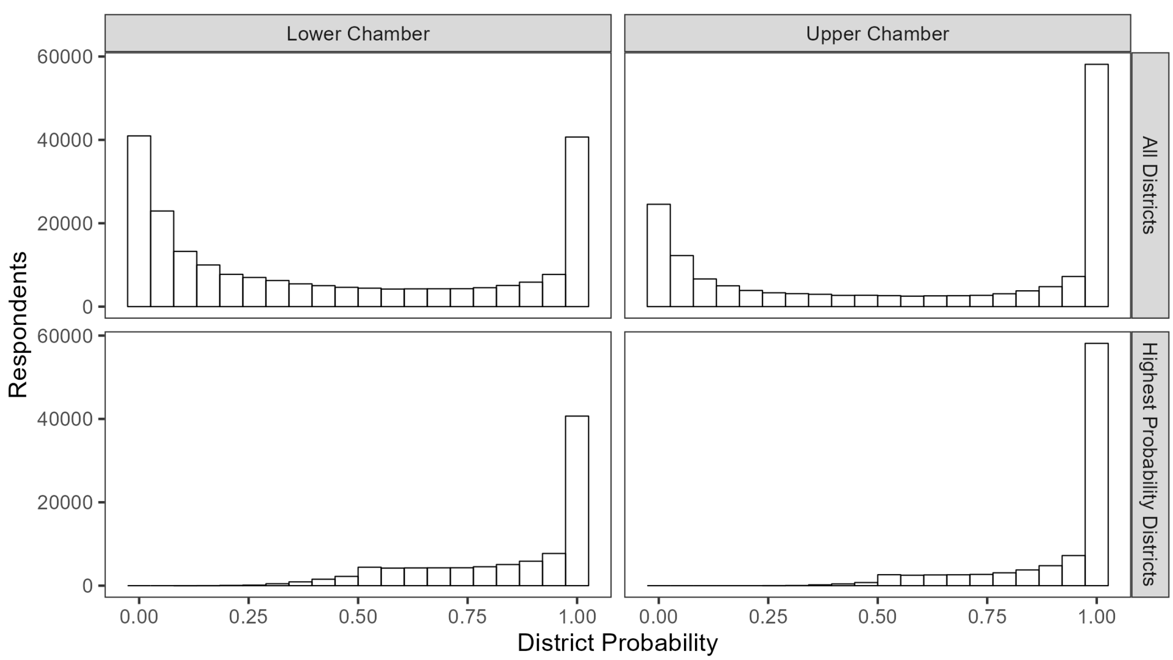
For each CES respondent, I use GIS software to find the overlap of their ZIP code (substituting ZCTA) and county. I then identify all legislative districts that intersect with this area, and find the population of each district-ZCTA-county combination. I do this by aggregating up from Census blocks, similar to areal interpolation (Goplerud 2016). I use these population distributions to estimate the probability that the respondent i lives in each district, d, conditional on their ZIP code z and county, c, using the formula

$$\Pr(\text{District}_i = d \mid \text{ZIP}_i = z, \text{County}_i = c) = \frac{\text{Population}_{dzc}}{\sum_{d=1}^{D} \text{Population}_{dzc}}. \tag{A5}$$

I generate probabilities separately for upper- and lower-chamber districts. Probabilities sum to 1 for each respondent separately across lower-chamber and across upper-chamber districts.

Figure A1 shows the distribution of the probabilities of district assignment. The top two panels show the distribution of probabilities across all districts; the bottom panels show the distribution only for each respondent's highest-probability district. I do not include ZIP code-county combinations not represented among the CES respondents from 2010-2020. The vast majority of respondents have a district with a probability of more than 95%, and nearly all respondents have districts with above 50% probability.

Figure A1: District Matching Probabilities in the CES



Note: Histograms report the distribution of district probabilities for all respondents of the CES.

[1]ZCTAs correspond to the ZIP code containing the majority of each Census block (Gill 2021).

E Dyadic Responsiveness Results

Table A1 reports baseline responsiveness results for the five policy areas in Section 4 of the main paper. The coefficients on Opinion represent the corresponding increase in the probability of a legislator voting for a policy based on a hypothetical shift in public support from 0% to 100%. Consistent with statewide studies of responsiveness, I find that legislators are more likely to vote in favor of policies if their constituents are more supportive of them.

Table A1: Baseline Responsiveness to Public Opinion

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion	2.36**	2.08**	1.90**	3.12**	5.01**	4.83**	4.37**
	(0.10)	(0.16)	(0.11)	(0.09)	(0.21)	(0.15)	(0.26)
District Ctrls.	X	X	X	X	X	X	X
Legislator Crls.	X	X	X	X	X	X	X
N	91,645	10,643	56,294	128,666	13,447	28,709	17,047
Adj. R^2	0.48	0.38	0.50	0.53	0.44	0.51	0.46

Note: Results are from OLS regressions where the dependent variable is legislator roll-call votes on the named policy area. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

F Robustness of Responsiveness Results

This appendix reports several robustness tests of my main responsiveness results.

F.1 Alternative Measures of Abortion Opinion

This section reports the results using alternative opinion measures for the abortion regressions. In the paper, the opinion measure used for abortion policymaking is support for for restricting abortion access which I construct using MRP from a battery of specific policy questions. Here, I show results with two alternative opinion measures: district-level estimates for the share of the public that supports making abortions illegal in all cases, and those who support making abortion always available as a matter of choice. I produce estimates at the district level using MRP, as described in Appendix C.

The first and third columns of Table A2 show the expected negative coefficient when opinion is measured by district-level support for making abortions available "as a matter of choice." A negative coefficient is consistent with responsiveness because the outcome is anti-abortion roll-call votes while the opinion variable is support for a liberal abortion policy. The second and fourth columns report results where the opinion measure is support for making abortions illegal in all circumstances. Here, a positive coefficient is consistent with responsiveness. Again, these results are similar to those reported in the main text.

Table A2: Congruence and Responsiveness with Alternate Abortion Opinion Measures

	Newspaper Congruence		TV Congruence	
	Abortion: Choice	Abortion: Illegal	Abortion: Choice	Abortion: Illegal
Opinion ×	-1.13**	3.70**		
Congruence	(0.24)	(0.59)		
Congruence	0.35**	-0.83**		
	(0.12)	(0.11)		
Opinion ×			-0.29	3.59**
TVCongruence			(0.31)	(0.89)
TVCongruence			-0.05	-0.81**
			(0.15)	(0.17)
Opinion	-2.80**	2.89**	-2.77**	2.74**
	(0.11)	(0.22)	(0.10)	(0.21)
District Ctrls.	X	X	X	X
Legislator Ctrls.	X	X	X	X
N	87,350	74,410	91,645	78,399
Adj. R^2	0.49	0.47	0.49	0.47

Note: Results from OLS regressions where the dependent variable is legislator roll-call votes on the named policy area. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

F.2 Models by Chamber

In Tables A3 to A6, I report results for upper and lower chambers separately. The regression models presented here are identical to those described in the main paper, except that they do not include the intercept shift for chamber. The positive coefficients on the interaction between opinion and congruence in all models (nearly all of which are statistically significant) are consistent with those in the main text; in higher-congruence districts, legislators are more responsive to public opinion.

Table A3: Newspaper Congruence and Responsiveness: Upper Chambers

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion ×	1.65**	1.53	3.94**	1.59**	2.12	3.94**	2.21
Congruence	(0.44)	(0.85)	(0.51)	(0.47)	(1.38)	(0.83)	(1.61)
Opinion	2.48**	3.01**	2.21**	3.20**	5.63**	4.26**	3.88**
	(0.18)	(0.37)	(0.22)	(0.23)	(0.57)	(0.35)	(0.56)
Congruence	-1.05**	-0.61	-1.45**	-0.92**	-0.89	-2.66**	-1.39
	(0.26)	(0.43)	(0.20)	(0.28)	(0.54)	(0.55)	(1.03)
District Ctrls.	X	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X	X
N	20,459	2,108	12,368	30,934	2,489	6,467	4,086
Adj. R^2	0.53	0.52	0.55	0.56	0.49	0.53	0.55

Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area for upper-chamber legislators. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A4: Newspaper Congruence and Responsiveness: Lower Chambers

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion ×	1.08**	1.28*	1.84**	1.53**	0.57	3.57**	3.45**
Congruence	(0.37)	(0.55)	(0.59)	(0.36)	(0.80)	(0.61)	(0.94)
Opinion	2.33**	1.98**	1.65**	3.13**	4.72**	4.74**	4.29**
	(0.13)	(0.20)	(0.13)	(0.11)	(0.25)	(0.18)	(0.30)
Congruence	-0.89**	-0.42	-0.63**	-0.77**	-0.34	-2.37**	-2.33**
	(0.23)	(0.28)	(0.21)	(0.21)	(0.33)	(0.41)	(0.58)
District Ctrls.	X	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X	X
N	66,891	8,455	41,321	92,891	10,604	20,855	12,098
Adj. R^2	0.49	0.43	0.54	0.56	0.48	0.54	0.48

Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area for lower-chamber legislators. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A5: TV Market Congruence and Responsiveness: Upper Chambers

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion ×	1.50*	1.14	3.93**	0.79	2.16	0.90	2.47
TVCongruence	(0.61)	(1.50)	(0.67)	(0.62)	(1.92)	(1.20)	(1.61)
Opinion	2.46**	3.02**	2.14**	3.26**	5.44**	4.73**	3.98**
	(0.18)	(0.38)	(0.22)	(0.22)	(0.56)	(0.37)	(0.56)
TVCongruence	-0.96**	-0.21	-1.33**	-0.34	-1.08	-0.57	-1.64
	(0.37)	(0.82)	(0.25)	(0.35)	(0.74)	(0.78)	(1.03)
District Ctrls.	X	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X	X
N	21,324	2,122	12,934	31,710	2,582	6,772	4,243
Adj. R^2	0.52	0.52	0.54	0.56	0.49	0.52	0.54

Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area for upper-chamber legislators. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^*p < 0.05$; $^{**}p < 0.01$.

Table A6: TV Market Congruence and Responsiveness: Lower Chambers

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion ×	0.89	1.61	2.85**	1.43**	0.19	2.24*	4.09**
TVCongruence	(0.51)	(0.85)	(0.84)	(0.45)	(1.36)	(0.96)	(1.13)
Opinion	2.28**	1.89**	1.63**	3.09**	4.66**	4.83**	4.13**
	(0.12)	(0.20)	(0.12)	(0.11)	(0.24)	(0.18)	(0.28)
TVCongruence	-0.79*	-0.68	-0.90**	-0.68**	-0.20	-1.56*	-2.81**
	(0.32)	(0.41)	(0.30)	(0.25)	(0.56)	(0.65)	(0.73)
District Ctrls.	X	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X	X
N	70,321	8,521	43,360	96,867	10,850	21,937	12,789
Adj. R^2	0.49	0.42	0.53	0.56	0.49	0.53	0.48

Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area for lower-chamber legislators. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^*p < 0.05$; $^{**}p < 0.01$.

F.3 Control for Party

Results in the main text do not control for legislator partisanship. This is because an important way that public opinion may translate to roll-call votes is via party. For example, a Republican voting for an abortion-rights policy in a district that supports abortion rights is just as responsive to public opinion as a Democrat would be in the same district.

Nevertheless, in this appendix, I present versions of the main results that include a control for party. I drop independents and members of third parties from this analysis. I also drop the Nebraska Unicameral because it is a nonpartisan legislature. In Table A7, I find that even when controlling for party, newspaper congruence is still associated with increased responsiveness in on most policies. I note that most coefficients are not statistically significant; however, this is likely because party is so highly correlated with opinion. The relationship with TV media market congruence in Table A8 is less clear. These results suggest that, at least on some issues, the press—especially newspapers—strengthens responsiveness on policy, even beyond the central role played by partisanship in theories of representation.

Table A7: Newspaper Congruence and Responsiveness with Party Control

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion × Congruence	0.16	0.55	1.57**	0.24	0.59	1.19**	1.89**
	(0.13)	(0.41)	(0.32)	(0.18)	(0.58)	(0.34)	(0.37)
Opinion	0.39**	0.37**	0.58**	0.40**	1.24**	-0.06	-0.10
	(0.05)	(0.14)	(0.08)	(0.07)	(0.21)	(0.12)	(0.16)
Congruence	-0.18*	-0.21	-0.60**	-0.14	-0.35	-0.84**	-1.31**
	(0.08)	(0.23)	(0.12)	(0.11)	(0.23)	(0.24)	(0.25)
Democrat	-0.65**	0.51**	0.36**	0.50**	-0.42**	0.65**	0.61**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
District Ctrls.	X	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X	X
N	87,112	10,547	53,550	123,641	13,062	27,279	16,149
Adj. R^2	0.69	0.53	0.58	0.64	0.52	0.67	0.60

Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A8: TV Market Congruence and Responsiveness with Party Control

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion ×	-0.02	0.91	1.78**	-0.22	0.90	0.13	2.06**
TVCongruence	(0.19)	(0.64)	(0.41)	(0.23)	(0.89)	(0.47)	(0.53)
Opinion	0.37**	0.28*	0.54**	0.41**	1.27**	0.00	-0.04
	(0.05)	(0.14)	(0.08)	(0.07)	(0.20)	(0.11)	(0.15)
TVCongruence	-0.03	-0.43	-0.62**	0.11	-0.54	-0.12	-1.46**
	(0.11)	(0.36)	(0.15)	(0.13)	(0.34)	(0.33)	(0.37)
Democrat	-0.65**	0.51**	0.36**	0.50**	-0.41**	0.64**	0.59**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
District Ctrls.	X	X	X	X	X	X	X
Legislator Crls.	X	X	X	X	X	X	X
N	91,407	10,627	56,152	128,404	13,402	28,666	17,000
Adj. R^2	0.69	0.53	0.57	0.64	0.52	0.66	0.58

Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

F.4 Alternative Congruence Data

The bulk of my newspaper results use a congruence variable constructed from Alliance for Audited Media (AAM) ciruclation data for 2011-2022. An alternative source for circulation data is the Standard Rate and Data Service (SRDS) *Circulation* handbook. Below, I show that my main results are generally robust to the alternative data source. SRDS data have been preferred by some other scholars (e.g, Peterson 2019) because they include more smaller newspapers that are less likely to participate in the AAM.

However, there are two limitations to the SRDS data for the purposes of this study. First, SRDS data are only digitized through 2018, which requires me to drop roll-call votes from one-third of the years in the main regressions. Second, small newspapers of the kind that may appear in SRDS but not in AAM are less likely to have the resources necessary to fund full-time coverage of the state capitol. Adding these newspapers may produce a fuller picture of news coverage in general, but not necessarily of news coverage in state capitols (this is difficult to account for after the fact as there are no national listings of state capitol bureaus after 2009).

I show in Table A9 that my results are largely robust to using the SRDS data. On most policies, I still find positive and significant coefficients on the interaction between congruence and opinion. Note that I have to drop the second set of minimum wage data. Using the alternative congruence measure, the coefficient on gun control (with the assault weapons ban opinion question) shrinks toward zero, and the Medicaid expansion coefficient falls below zero, though neither is statistically significant. These differences may be partially explained by the imperfect mapping of opinion on assault weapons bans to gun control policy more generally, and the smaller newspapers included in this sample may be less focused on the technicalities of health care policy. Still, these results are largely consistent with those reported in the main paper.

Table A9: Congruence and Responsiveness with SRDS Circulation Data

	Restrict Abortion (2011–2018)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2018)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)
Opinion ×	0.90**	0.96*	0.68**	0.05	-0.23	1.29*
SRDS Congruence	(0.25)	(0.39)	(0.21)	(0.24)	(0.49)	(0.54)
Opinion	1.89**	2.06**	1.84**	3.24**	4.95**	5.13**
	(0.14)	(0.19)	(0.11)	(0.14)	(0.23)	(0.24)
SRDS Congruence	-0.79**	-0.18	-0.09	0.10	0.05	-0.72*
	(0.16)	(0.19)	(0.07)	(0.13)	(0.20)	(0.34)
District Ctrls.	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X
N	47,830	10,643	56,279	54,000	13,444	9,789
Adj. R^2	0.43	0.39	0.50	0.51	0.45	0.51

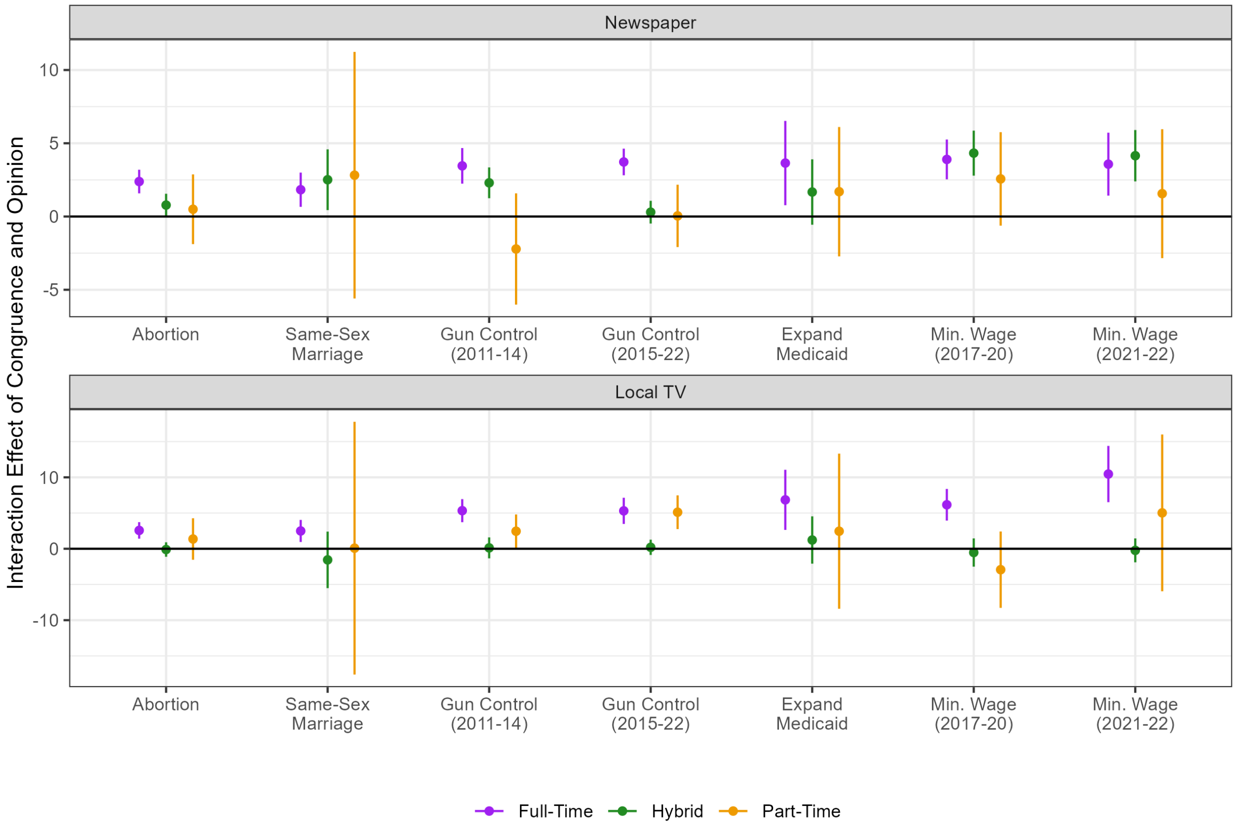
Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

G Legislative Professionalization

In Figure A2, I consider whether the media's effect on responsiveness varies with legislative professionalization. I define professionalization in three categories (full-time, hybrid, and part-time) using designations from the National Conference of State Legislatures and re-run the main models from the paper, interacting the effects of opinion, congruence, and the interaction between them with professionalization. I find consistent evidence that the media plays a role in representation in full-time legislatures, and that newspapers are important to hybrid legislatures. This is consistent with an expectation that the presence of journalists shapes politician behavior, as more professionalized legislatures tend to have more full-time news reporters.

Table A10 shows that full-time and hybrid legislatures tend to have more full-time and session reporters, compared to part-time legislatures. This difference is largely driven by newspaper reporters.

Figure A2: Responsiveness Results by Professionalization



Note: Results from OLS regressions where the dependent variable is roll-call votes on the named policy area. Models interact opinion and congruence with three-level legislative professionalization. All models include bill fixed effects. 95% confidence intervals reported from district clustered standard errors. Full results including covariates are in Appendix K.

Table A10: Professionalization and Press Corps Size

	Total Reporters (2014)	Newspaper Reporters (2014)	TV Reporters (2014)	Total Reporters (2022)
Full-Time	15.34**	5.42**	1.21	17.21**
	(4.06)	(1.88)	(0.82)	(4.39)
Hybrid	9.92**	3.51*	1.19	7.08
	(3.54)	(1.64)	(0.72)	(3.82)
Legislature Size	0.03	0.01	-0.00	0.05
	(0.03)	(0.01)	(0.01)	(0.03)
N	50	50	50	50
Adj. R^2	0.20	0.09	-0.04	0.20

Note: Results from OLS regressions where the dependent variable is the number of reporters in state capitols, according to data from Pew. All models include Census region fixed effects. $^{*}p < 0.05$; $^{**}p < 0.01$.

H Robustness of Electoral Connection Results

This appendix reports several robustness tests for the political knowledge and electoral behavior results that comprise my test of the electoral connection. Because the main paper reports null effects, I take care to show whether these results are sensitive to arbitrary modeling decisions. In general, I find that my findings are robust.

H.1 Information: Alternative Specifications

I begin by testing whether the state legislator name recognition results in Section 5 of the paper differ if I change (1) the respondent-level controls, (2) the source of circulation data used to compute congruence, or (3) the inclusion of district-level geographic controls for population density and percent urban. I report results in Table A11 and discuss them here. The model reported in the paper is bolded, in the top-left corner of the table.

First, I vary the covariates included at the CES respondent level. The paper reports results with controls for race, education, sex, age group, and number of years living in the same city (Main Respondent Controls). Results do not appear substantially different if no controls are included, or if I add controls for interest in politics, seven-level party ID, or household income (Additional Respondent Controls).

Table A11: Alternative Specifications of Political Knowledge Results

Models with District Geography Controls						
Congruence	**0.02**	0.03	0.04	0.07	0.08	0.08
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Data Source	AAM	AAM	AAM	SRDS	SRDS	SRDS
District Controls	X	X	X	X	X	X
Main Respondent Controls	X		X	X		X
Additional Resp. Controls			X			X
N	948	981	943	958	992	952
Adj. R^2	0.07	0.03	0.09	0.08	0.04	0.10
Models with no District Controls						
Congruence	0.06	0.08	0.08	0.10	0.11	0.11
	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)
Data Source	AAM	AAM	AAM	SRDS	SRDS	SRDS
District Controls						
Main Respondent Controls	X		X	X		X
Additional Resp. Controls			X			X
N	948	981	943	958	992	952
Adj. R^2	0.07	0.02	0.09	0.08	0.03	0.10

Note: Results from OLS regressions where the dependent variable is whether respondents correctly name their state representative. Results from main paper are bolded. All models include state fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K $^{*}p < 0.05$; $^{**}p < 0.01$.

Next, I vary the source of circulation data used to construct Congruence. As discussed above in Appendix F, the Standard Rate and Data Service (SRDS) is an alternative source for newspaper circulation data; in the paper, I report results using Alliance for Audited Media (AAM) data. Because SRDS includes more small newspapers, it may be the case that there is systematic error in the estimation of congruence using AAM data. However, I show in the three rightmost columns of Table A11 that results are still null if SRDS data is substituted in. Newspapers that appear in SRDS but not AAM are generally quite small, and as a result may have insufficient resources to devote to covering state politics. Thus, their informational usefulness in the specific context examined here could be limited.

Finally, I consider the case where I exclude district-level controls for logged population density, percent urban, and the quintiles of these variables to allow a nonlinear relationship. Though not statistically significant at the 5% level, these estimates are systematically larger than those that include these controls. When combined with the SRDS data (the three models on the bottom-right of the table), the estimates are larger, with smaller standard errors, and statistically significant at the 10% level.

In comparing the specifications, these district-level controls are essential, as I discuss in the main paper, and as Snyder and Strömberg (2010) note, because of the high (negative) correlation between urbanism and congruence, which is a direct result of where newspapers tend to circulate and the density of state legislative districts in urban areas. Other work has found a relationship between newspaper congruence and respondents' ability to identify their state legislator (Myers N.d.). The only specification I find that approaches the results from Myers (N.d.) requires not only using the SRDS data but also excluding the district-level covariates that are necessary to estimate the effect of congruence.

Table A12: Alternative Specifications of Political Knowledge Results for TV

	District Geog. Controls			No District Controls		
Congruence	**0.05**	0.04	0.01	0.10	0.09	0.08
	(0.09)	(0.10)	(0.10)	(0.09)	(0.10)	(0.09)
District Controls	X	X	X	X	X	X
Main Respondent Controls	X		X	X		X
Additional Resp. Controls			X			X
State FEs	X	X	X	X	X	X
N	958	992	952	958	992	952
Adj. R^2	0.08	0.03	0.10	0.07	0.03	0.09

Note: Results from OLS regressions where the dependent variable is whether respondents correctly name their state representative. Results from main paper are bolded. All models include state fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K $^{*}p < 0.05$; $^{**}p < 0.01$.

In Table A12, I show that the results on TV media market congruence and state legislative knowledge are not sensitive to the inclusion of different covariates in the regression, though coefficients increase dramatically if district controls are not included, as in the case of newspapers.

Table A13: Alternative Weighting of Political Knowledge Results

	Name State Rep.	
Congruence	0.04	
	(0.07)	
TV Congruence		0.06
		(0.10)
District Controls	X	X
Respondent Controls	X	X
State FEs	X	X
N	948	958
Adj. R^2	0.11	0.11

Note: Results from OLS regressions where the dependent variable is whether respondents correctly name their state representative, allowing them to be correct if they can name a representative for any district they could plausibly live in. All models include state fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K $^{*}p < 0.05$; $^{**}p < 0.01$.

In the paper, I manually check whether CES respondents correctly named their legislator using a probabilistic matching from respondents' ZIP code and county to districts (see Appendix D). I specifically mark which possible legislators was correctly identified and use the district-matching probabilities as weights in the regression. Here, in Table A13, I show results using an alternative approach. Similar to Rogers (2023), I consider respondents to be correct if they name *any* representative from a district where there is a nonzero probability that they live. As before, I include all districts in the regression, weighting by the probability that the respondent lives in that district. This is necessary, as congruence and the urbanism controls are at the district level; however, I code the outcome for all possible districts as 1 if the respondent can name any representative.

H.2 Electoral Behavior: Alternative Specifications

Turning to the electoral connection results, in Table A14, I check whether my null results are an artifact of model design or the choice of circulation data. I find that results reported in the paper (leftmost column, in bold) are not sensitive to the exclusion of geographic or demographic controls. I also find that when fitting models using the SRDS data for congruence instead of my preferred AAM data, results are still near-zero and not statistically significant.

Table A14: Alternative Specifications of Electoral Results

Ballot Rolloff						
Congruence	**-0.00**	0.00	-0.00	0.04	0.04	0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Data Source	AAM	AAM	AAM	SRDS	SRDS	SRDS
Geography Controls	X		X	X		X
Demographic Controls	X			X		
N	18,289	18,487	18,289	14,092	14,266	14,092
Adj. R^2	0.67	0.67	0.66	0.67	0.67	0.67
Incumbency Advantage						
Congruence× Incumbent	**-0.00**	0.00	0.00	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Congruence	-0.02	-0.03	-0.02	0.00	0.00	0.00
	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)
Incumbent (w/ Party)	0.01**	0.01**	0.01**	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Data Source	AAM	AAM	AAM	SRDS	SRDS	SRDS
Geography Controls	X		X	X		X
Demographic Controls	X			X		
N	22,318	22,356	22,318	18,197	18,235	18,197
Adj. R^2	0.88	0.88	0.88	0.90	0.90	0.90
Vote Nationalization						
Congruence	**0.01**	0.01	0.01	-0.06	-0.05	-0.06
	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)
Data Source	AAM	AAM	AAM	SRDS	SRDS	SRDS
Geography Controls	X		X	X		X
Demographic Controls	X			X		
N	14,198	14,346	14,198	9,805	9,953	9,805
Adj. R^2	0.46	0.47	0.46	0.62	0.62	0.61

Note: Results from OLS regressions. Dependent variables are rolloff in state legislative elections (top panel); two-party Democratic vote share for state legislature (middle panel), and vote nationalization (bottom panel). Results from main paper are bolded. All models include district and year fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A15 reports similar robustness tests for TV congruence and finds that the results in the paper are not sensitive to the exclusion of covariates.

Table A15: Alternative Specifications of Electoral Results for TV

	Rolloff			Incumbency			Nationalization		
TVCongruence	**-0.01**	-0.00	-0.01	-0.02	-0.04	-0.02	**-0.02**	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.05)	**(0.05)**	(0.05)	(0.05)
TVCongruence×				**-0.01**	-0.01	-0.01			
Incumbent				**(0.03)**	(0.03)	(0.03)			
Incumbent				0.01**	0.01**	0.01**			
(w/ Party)				(0.00)	(0.00)	(0.00)			
Demog. Ctrls	X			X			X		
Geog. Ctrls	X		X	X		X	X		X
N	18,187	18,235	18,187	22,221	22,221	22,221	14,136	14,136	14,136
Adj. R^2	0.67	0.66	0.67	0.88	0.88	0.88	0.46	0.46	0.46

Note: Results from OLS regressions. Dependent variables are rolloff in state legislative elections (columns 1-3); two-party Democratic vote share for state legislature (columns 4-6), and vote nationalization (columns 7-9). Results from main paper are bolded. All models include district and year fixed effects. Standard errors, in parentheses, are clustered by district. Full results including covariates are in Appendix K. $^{*}p < 0.05$; $^{**}p < 0.01$.

In the tables that follow, I show that my main results replicate if models are fit only on elections for lower- or upper-chamber seats.

Table A16: Congruence and Voting Behavior: Lower Chambers

	Rolloff		Incumbency		Nationalization	
	Newspaper	TV	Newspaper	TV	Newspaper	TV
Congruence	-0.00	0.01	-0.04	-0.02	0.00	-0.04
	(0.01)	(0.02)	(0.02)	(0.06)	(0.02)	(0.06)
Congruence× Incumbent			0.00	0.01		
			(0.02)	(0.05)		
Incumbent (w/ Party)			0.01**	0.01**		
			(0.00)	(0.00)		
District Controls	X	X	X	X	X	X
Fixed Effects	District + Year	District + Year	District + Year	District + Year	District + Year	District + Year
N	14,379	14,301	17,588	17,513	11,171	11,124
Adj. R^2	0.69	0.69	0.88	0.88	0.45	0.45

Note: Results from OLS regressions. Dependent variables are rolloff in state legislative elections (columns 1-2); two-party Democratic vote share for state legislature (columns 3-4), and vote nationalization (columns 5-6). All models include fixed effects for district, year, and quintiles of % Urban and Population Density. Standard errors, in parentheses, are clustered by district. Full results including covariates in Appendix K. $^*p < 0.05$; $^{**}p < 0.01$.

Table A17: Congruence and Voting Behavior: Upper Chambers

	Rolloff		Incumbency		Nationalization	
	Newspaper	TV	Newspaper	TV	Newspaper	TV
Congruence	-0.00	-0.06	-0.00	-0.05	0.03	0.03
	(0.02)	(0.05)	(0.04)	(0.10)	(0.04)	(0.10)
Congruence× Incumbent			-0.02	-0.07		
			(0.02)	(0.04)		
Incumbent (w/ Party)			0.02**	0.02**		
			(0.01)	(0.01)		
District Controls	X	X	X	X	X	X
Fixed Effects	District + Year	District + Year	District + Year	District + Year	District + Year	District + Year
N	3,910	3,886	4,730	4,708	3,027	3,012
Adj. R^2	0.58	0.58	0.88	0.88	0.53	0.53

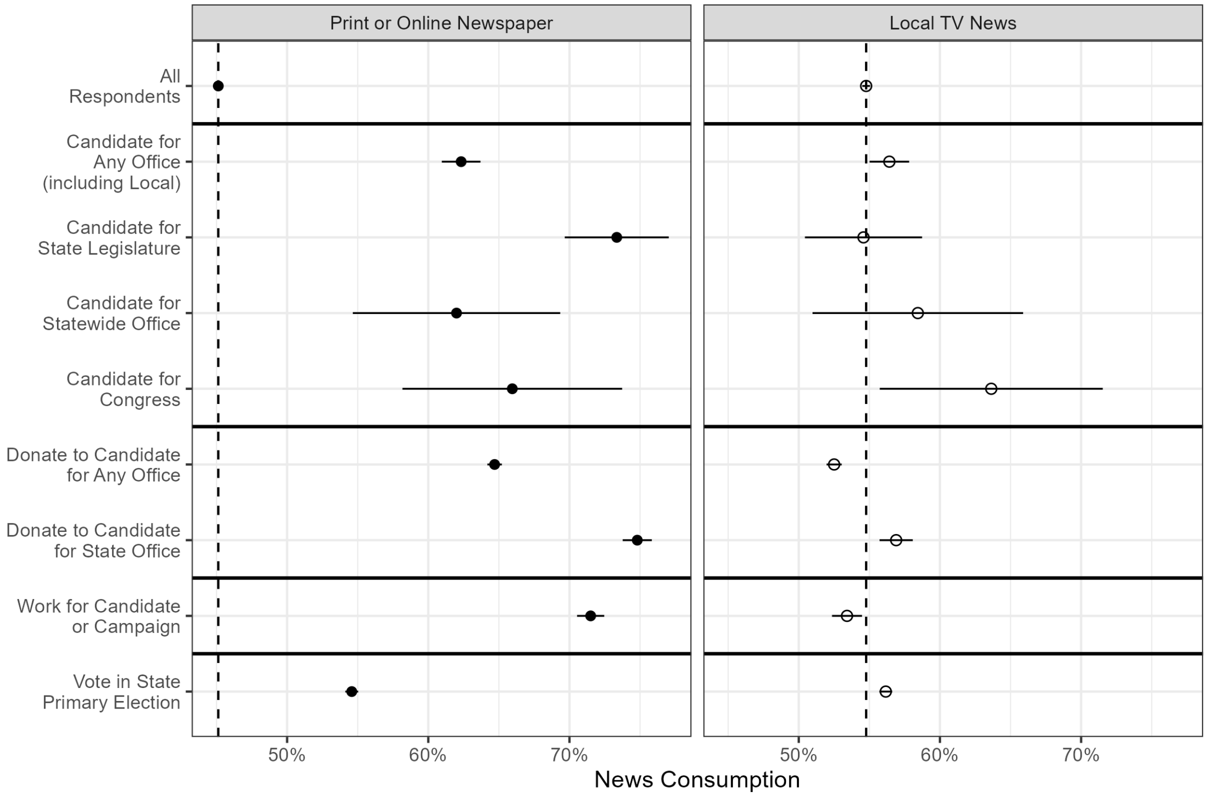
Note: Results from OLS regressions. Dependent variables are rolloff in state legislative elections (columns 1-2); two-party Democratic vote share for state legislature (columns 3-4), and vote nationalization (columns 5-6). All models include fixed effects for district, year, and quintiles of % Urban and Population Density. Standard errors, in parentheses, are clustered by district. Full results including covariates in Appendix K. $^*p < 0.05$; $^{**}p < 0.01$.

I News Readership among Elites

Figure A3 shows that these groups are more likely read print and online newspapers and (to a lesser degree) watch local TV news than is the general public, according to a pooled sample of the 2010-2020 Cooperative Election Study (CES; formerly CCES).[2]

[2] It is important to temper these interpretations with the fact that self-reported media consumption is generally overestimated (Prior 2009). Nevertheless, the reported differences are persistent, and the gaps are especially large for local newspaper readership.

Figure A3: Local Media Consumption among the Public and Political Elites



Note: Reports average newspaper readership (including print and online) and local TV news viewership among the public, as well as political candidates, campaign donors and workers, and validated voters in state primary elections. Respondents are pooled from the 2010, 2012, 2014, 2016, 2018, and 2020 CES. Error bars report 95% confidence intervals of the estimates. The dotted line shows consumption of all respondents to ease comparisons with other groups.

J Full Regression Tables

This appendix reports full regression tables for results reported in the main paper.

Table A18: ReaderShare and Coverage of State Legislators: Full Models

	Lower Chamber		Upper Chamber	
ReaderShare	33.09**	30.97**	33.27**	30.18**
	(3.13)	(3.03)	(3.25)	(3.15)
Out of State		-1.89**		-4.71**
		(0.24)		(0.58)
Log Population		-0.13		-0.16
		(0.14)		(0.87)
% High School		1.89		2.33
		(1.35)		(4.13)
% College		0.40		2.29
		(0.48)		(1.79)
Leadership		0.93**		2.32**
		(0.22)		(0.45)
Seniority: First Term		-0.36**		-0.50*
		(0.10)		(0.21)
Seniority: More than 10 Years		-0.18		0.05
		(0.11)		(0.41)
Seniority		0.01		0.05
		(0.01)		(0.03)
% 65+		-0.46		5.21
		(1.37)		(4.79)
% Black		-0.68		0.33
		(0.60)		(1.48)
% Latino		0.93		0.50
		(0.77)		(1.59)
% Other Non-white		0.69		1.23
		(0.80)		(1.74)
% Urban		0.88*		0.67
		(0.41)		(1.37)
Population Density (log)		-0.10		-0.02
		(0.07)		(0.20)
N	169,383	165,070	64,524	61,750
Adj. R^2	0.09	0.10	0.11	0.13

Note: Results are from OLS regressions where the dependent variable is the number of stories published about a legislator in a given newspaper-year. All models include state-year fixed effects. Standard errors, in parentheses, are clustered by newspaper. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A19: Newspaper Congruence and Responsiveness across Issues: Full Models

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion × Congruence	1.35**	2.07**	2.69**	1.48**	1.84*	3.47**	3.18**
	(0.29)	(0.52)	(0.42)	(0.31)	(0.75)	(0.51)	(0.85)
Opinion	2.33**	2.02**	1.74**	3.07**	4.87**	4.57**	4.17**
	(0.11)	(0.18)	(0.11)	(0.10)	(0.24)	(0.16)	(0.27)
Congruence	-0.98**	-0.87**	-0.96**	-0.80**	-0.85**	-2.36**	-2.15**
	(0.17)	(0.27)	(0.15)	(0.18)	(0.30)	(0.34)	(0.54)
% College	-0.12*	-0.02	0.09	0.00	0.07	-0.17**	-0.43**
	(0.06)	(0.09)	(0.05)	(0.04)	(0.06)	(0.06)	(0.09)
% High School	1.56**	-0.96**	-0.78**	-1.27**	0.81**	0.64**	0.56*
	(0.10)	(0.23)	(0.11)	(0.09)	(0.16)	(0.14)	(0.23)
% 65+	-0.22*	0.26	0.21	0.25**	-0.16	0.58**	0.50**
	(0.09)	(0.24)	(0.11)	(0.08)	(0.14)	(0.13)	(0.17)
% Black	-0.58**	0.37**	-0.10*	-0.10**	0.78**	-1.08**	-1.06**
	(0.03)	(0.06)	(0.05)	(0.03)	(0.07)	(0.07)	(0.12)
% Hispanic	-0.36**	0.18	-0.00	0.13**	0.11	-0.02	0.10
	(0.04)	(0.11)	(0.06)	(0.04)	(0.08)	(0.07)	(0.10)
% Other Nonwhite	-0.65**	0.40**	0.44**	0.46**	-0.12	0.20**	0.15
	(0.05)	(0.12)	(0.07)	(0.04)	(0.09)	(0.06)	(0.10)
% Urban	-0.05	-0.01	-0.10*	-0.07	-0.13*	0.04	0.11
	(0.04)	(0.10)	(0.05)	(0.04)	(0.07)	(0.06)	(0.10)
Population Density (log)	-0.03**	0.05**	0.02**	0.01*	0.03**	0.02**	0.02*
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Total Circulation (log)	0.02**	-0.02**	-0.01**	-0.01**	0.00	-0.02**	-0.01**
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Seniority: Years	-0.00	0.00*	0.00	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Seniority: First Term	0.01	-0.03*	-0.03**	0.02**	0.02*	0.04**	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Seniority: More than 10 Years	-0.04**	-0.02	0.02	0.00	-0.02	0.02	0.00
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Leadership	0.03*	-0.02	-0.01	0.01	0.01	0.01	-0.02
	(0.01)	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)	(0.03)
N	87,350	10,563	53,689	123,825	13,093	27,322	16,184
Adj. R^2	0.48	0.39	0.51	0.54	0.44	0.51	0.47

Note: Results are from OLS regressions where the dependent variable is legislator roll-call votes on the named policy area. All models include fixed effects for bills and the quintiles of % Urban and Population Density. Standard errors, in parentheses, are clustered by district. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A20: Local TV Media Market Congruence and Responsiveness across Issues: Full Models

	Restrict Abortion (2011–2022)	Same-Sex Marriage (2011–2016)	Gun Control (2011–2014)	Gun Control (2015–2022)	Expand Medicaid (2015–2016)	Min. Wage (2017–2020)	Min. Wage (2021–2022)
Opinion ×	0.99**	2.51**	3.22**	1.25**	2.47*	1.53*	4.28**
TVCongruence	(0.38)	(0.78)	(0.53)	(0.38)	(1.16)	(0.75)	(0.92)
Opinion	2.30**	1.90**	1.73**	3.00**	4.78**	4.72**	3.99**
	(0.10)	(0.18)	(0.11)	(0.10)	(0.23)	(0.16)	(0.25)
TVCongruence	-0.77**	-1.12**	-1.06**	-0.67**	-1.22**	-1.12*	-2.96**
	(0.24)	(0.40)	(0.19)	(0.21)	(0.46)	(0.50)	(0.60)
% College	-0.13*	-0.04	0.07	0.02	0.09	-0.17**	-0.39**
	(0.06)	(0.09)	(0.05)	(0.04)	(0.06)	(0.05)	(0.08)
% High School	1.64**	-0.94**	-0.84**	-1.36**	0.77**	0.49**	0.37
	(0.09)	(0.23)	(0.10)	(0.09)	(0.15)	(0.14)	(0.20)
% 65+	-0.22*	0.30	0.14	0.22**	-0.16	0.46**	0.48**
	(0.09)	(0.24)	(0.11)	(0.08)	(0.13)	(0.12)	(0.16)
% Black	-0.56**	0.36**	-0.12**	-0.10**	0.76**	-1.13**	-1.02**
	(0.03)	(0.06)	(0.05)	(0.03)	(0.07)	(0.06)	(0.11)
% Hispanic	-0.29**	0.15	-0.06	0.09**	0.12	-0.10	0.04
	(0.04)	(0.10)	(0.05)	(0.03)	(0.07)	(0.06)	(0.09)
% Other Nonwhite	-0.62**	0.42**	0.45**	0.44**	-0.15	0.14**	0.16
	(0.05)	(0.12)	(0.06)	(0.04)	(0.09)	(0.05)	(0.09)
% Urban	-0.04	-0.02	-0.10*	-0.07	-0.14*	0.01	0.12
	(0.04)	(0.10)	(0.05)	(0.04)	(0.07)	(0.06)	(0.10)
Population Density	-0.03**	0.06**	0.03**	0.01*	0.02*	0.02**	0.02
(log)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Seniority: Years	-0.00	0.00**	0.00	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Seniority: First Term	0.01	-0.03	-0.03**	0.02**	0.02*	0.04**	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Seniority: More than	-0.03**	-0.02	0.02	0.00	-0.01	0.01	-0.00
10 Years	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Leadership	0.03*	-0.02	-0.02	0.00	0.01	-0.00	-0.03
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)
N	91,645	10,643	56,294	128,577	13,432	28,709	17,032
Adj. R^2	0.48	0.38	0.50	0.53	0.44	0.51	0.46

Note: Results are from OLS regressions where the dependent variable is legislator roll-call votes on the named policy area. All models include fixed effects for bills and the quintiles of % Urban and Population Density. Standard errors, in parentheses, are clustered by district. $^*p < 0.05$; $^{**}p < 0.01$.

Table A21: Congruence and Knowledge about State Politics: Full Models

	Name State Representative		Lower Chamber Control		Upper Chamber Control	
Congruence	0.02		0.02		0.00	
	(0.07)		(0.01)		(0.01)	
TVCongruence		0.06		-0.05*		-0.02
		(0.12)		(0.02)		(0.02)
Black	-0.04	-0.04	-0.09**	-0.09**	-0.08**	-0.09**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
Latino	-0.01	-0.01	-0.08**	-0.08**	-0.06**	-0.06**
	(0.03)	(0.03)	(0.00)	(0.00)	(0.01)	(0.01)
Other Non-White	-0.00	-0.00	-0.04**	-0.04**	-0.04**	-0.04**
	(0.03)	(0.03)	(0.00)	(0.00)	(0.01)	(0.01)
Female	-0.02	-0.02	-0.16**	-0.16**	-0.15**	-0.15**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
High School	-0.11**	-0.11**	-0.26**	-0.26**	-0.25**	-0.25**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
No Diploma	-0.13**	-0.13**	-0.33**	-0.33**	-0.32**	-0.32**
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Some College	-0.07**	-0.07**	-0.13**	-0.13**	-0.12**	-0.12**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
Age: 30-44	0.00	0.00	0.03**	0.03**	0.03**	0.03**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
Age: 45-64	0.05*	0.05*	0.13**	0.12**	0.13**	0.13**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
Age: 65+	0.05*	0.05*	0.20**	0.20**	0.21**	0.21**
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
% Urban	-0.13	-0.12	0.04	0.03	0.00	-0.00
	(0.11)	(0.11)	(0.02)	(0.02)	(0.02)	(0.02)
Log Pop. Density	-0.00	-0.00	0.00	0.00	-0.00	-0.00
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
Years in Current City	0.00	0.00	0.00**	0.00**	0.00**	0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	981	981	121,605	121,605	121,990	121,990
Adj. R^2	0.07	0.07	0.15	0.15	0.18	0.17

Note: Results from OLS regressions where the dependent variable is whether respondents correctly identified their state representative (columns 1-2) or the party controlling the legislative chamber (columns 3-6). All models include fixed effects for state and the quintiles of % Urban and Population Density. Standard errors, in parentheses, are clustered by district. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A22: Congruence and Voting Behavior: Full Models

	Rolloff		Incumbency		Nationalization	
	Newspaper	TV	Newspaper	TV	Newspaper	TV
Congruence	0.00	-0.00	-0.02	-0.02	0.01	-0.02
	(0.01)	(0.02)	(0.02)	(0.05)	(0.02)	(0.05)
Congruence×			-0.00	-0.01		
Incumbent			(0.02)	(0.03)		
Incumbent			0.01**	0.01**		
(w/ Party)			(0.00)	(0.00)		
% High School	0.21**	0.22**	-0.11	-0.12	0.10	0.10
	(0.05)	(0.05)	(0.11)	(0.11)	(0.11)	(0.11)
% College	-0.08*	-0.08*	0.23*	0.22*	0.04	0.04
	(0.04)	(0.04)	(0.10)	(0.10)	(0.09)	(0.09)
% 65+	0.00	0.01	0.21	0.21	0.13	0.13
	(0.06)	(0.06)	(0.14)	(0.14)	(0.13)	(0.13)
% Black	0.01	0.01	0.05	0.05	-0.02	-0.02
	(0.06)	(0.06)	(0.10)	(0.10)	(0.13)	(0.13)
% Latino	0.19**	0.19**	0.51**	0.51**	0.04	0.04
	(0.05)	(0.05)	(0.13)	(0.13)	(0.12)	(0.12)
% Other Non-White	0.04	0.05	0.24	0.24	-0.15	-0.16
	(0.06)	(0.06)	(0.16)	(0.16)	(0.15)	(0.15)
% Urban	0.01	0.01	0.02	0.02	-0.03	-0.03
	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)
Log Pop. Density	-0.00	-0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Incumbent Running	-0.03**	-0.03**			0.03**	0.03**
	(0.00)	(0.00)			(0.00)	(0.00)
Incumbent Unopposed	0.16**	0.16**	-0.02**	-0.02**	-0.23**	-0.23**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Dem. Pres. Vote			0.53**	0.53**		
			(0.03)	(0.03)		
Lagged Dem. Pres. Vote			-0.03**	-0.03**		
			(0.01)	(0.01)		
Incumbent (w/Party)			0.24**	0.24**		
× Unopposed			(0.00)	(0.00)		
Turnout	0.18**	0.19**				
	(0.02)	(0.02)				
N	18,289	18,187	22,318	22,221	14,198	14,136
Adj. R^2	0.67	0.67	0.88	0.88	0.46	0.46

Note: Results from OLS regressions. Dependent variables are rolloff in state legislative elections (columns 1-2); two-party Democratic vote share for state legislature (columns 3-4), and vote nationalization (columns 5-6). For Incumbency analysis, the effect of congruence is Congruence×Incumbent; for all others, it is Congruence. All models include fixed effects for district, year, and the quintiles of % Urban and Population Density. Standard errors, in parentheses, are clustered by district. $^{*}p < 0.05$; $^{**}p < 0.01$.

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