Supplementary Materials for "Local News and Policy Responsiveness in the States"

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Contents

A Media Text Corpus Collection and Content Analysis

The text analysis results in the main text rely on data collected from two sources. First, I collected full newspaper texts from ProQuest covering English-language, local newspapers in the United States over the period 2012-2021. I also collected additional newspaper texts via NewsBank. The full corpus comprises 290 newspapers.

These are not a complete sample of all newspapers in the country, nor are they randomly drawn. In building the NewsBank sample, I took special care to fill gaps in the ProQuest data, where possible—especially by collecting text from the largest newspapers and those in larger markets.

A.1 Automated Text Analysis Procedure

Text analysis results in Sections 2 and 3 of the main paper and Appendix F.2 use a dictionary method. I first constructed a list of state legislators' names from any state in which a newspaper sells at least 1,000 copies. Names of legislators are from Klarner (2018) for 2012-2016, and a combination of LegiScan and manual searches for 2017-2021. From this list, I produced a dictionary of search terms for each outlet and year that combine the names of legislators and the name of the chamber or office. I followed an identical process for members of Congress, using names from Lewis et al. (2023), and governors, using names from Kaplan

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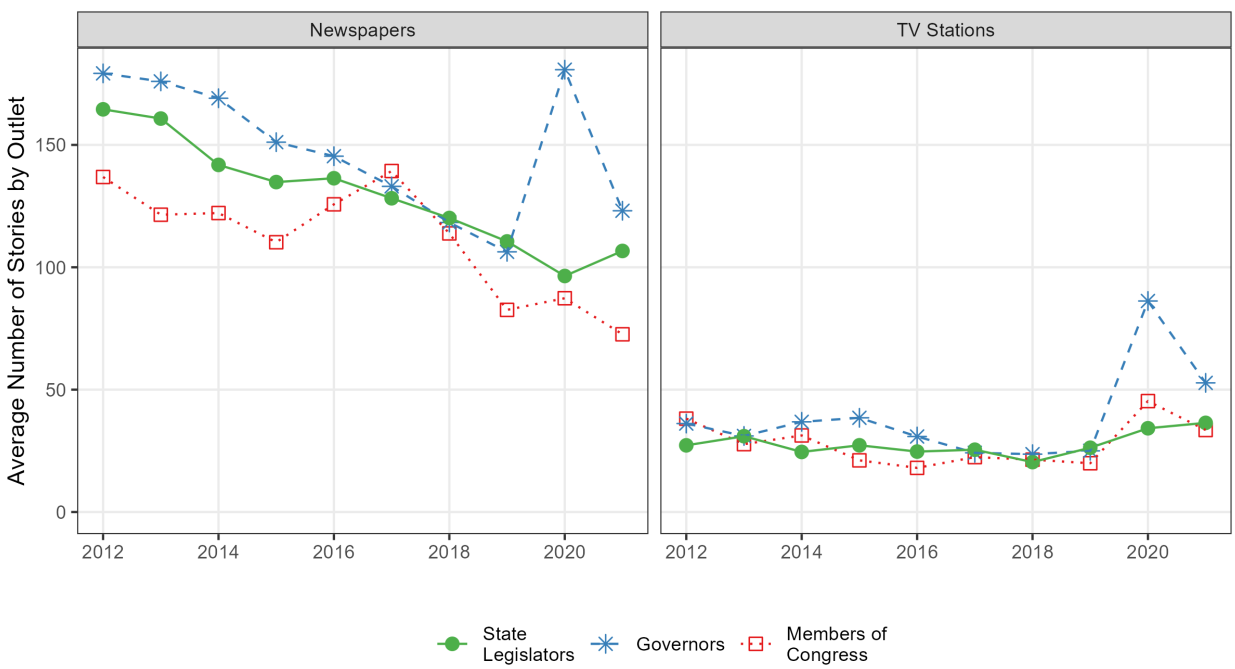
(2021). Finally, I conducted an automated search of all articles in the corpus for stories referencing one or more legislators.

A.2 Comparing Newspaper Coverage with TV

My results focus on newspapers, rather than television, because newspapers engage in considerably more coverage of public affairs and state politics than do local TV news broadcasts. In 2022, newspaper journalists made up 33% of full-time state government reporters from mainstream news outlets (i.e., excluding government insider publications, expressly ideological outlets, and the trade press), compared to 15% for television.

This variation reveals itself in the text of newspapers and TV news broadcasts. I extended the automated text analysis procedure described above to TV, using an archive of news broadcast transcripts and online articles from 239 TV stations. Figure A1 reports the average number of stories mentioning state legislators, governors, and members of Congress by name in the local newspapers and TV stations in my sample. Although the coverage on TV has been more stable over time than newspaper coverage, politics is much less frequently covered on broadcast news, as compared to newspapers.

Figure A1: State Government Coverage in Newspapers and Local TV



Note: Number of articles that mention individual politicians by name, in newspapers and TV coverage.

A.3 Newspaper Content

In order to understand the substance of local newspaper coverage of state legislatures, I worked with a team of research assistants to read and hand-code information about a random sample of newspaper stories identified by the automated procedure as mentioning state legislators by name. The sample contains 939 articles, which include news coverage as well as opinion content. Each article was hand-coded by at least two coders for a variety of variables. We find considerable evidence of substantive, policy-focused coverage of state legislatures.

First, we considered whether news coverage is focused on policy, elections, or other topics of political coverage. We find that nearly 65% of stories focus primarily on policy, either taking a broad look at an area of state policymaking or focusing on particular pieces of legislation. Each of these categories garners considerably more press attention than elections (16%), the personal lives of legislators (10%), or process stories (9%).

Table A1: Story Focus from Content Analysis

Story Focus	Share of Stories
Policy, General	33.92%
Specific Legislation	33.04%
Elections	14.39%
Personal Lives	10.30%
Procedure	8.35%

In light of the large quantity of stories published relating to policy, we next identified the topics these stories focus on. Stories were coded according to a list of potential policy topics, and each could be assigned to multiple topics if applicable. Table A2 reports the share of stories focused on each topic.

Table A2: Story Topics: Full List

Topic	Pct.	Topic	Pct.	Topic	Pct.
Education	11.5%	Transportation	4.5%	Other	1.7%
Elections (General)	10.6%	District Community Event	4.3%	Civil & Family Law	1.6%
Health Care	9.1%	Social Welfare	4.3%	Immigration	1.3%
Crime & Criminal Justice	9.0%	Multiple Policies	3.6%	State-Local Gov. Relations	1.3%
Government Operations	7.9%	Personal Lives	3.6%	Campaign Finance	0.9%
Budget	7.8%	Ethics	3.0%	Agriculture	0.8%
Civil Rights & Liberties	7.3%	Commemorative	2.9%	State-Federal Relations	0.6%
Business & Econ. Dev.	6.5%	Process (General)	2.5%	Rules and Procedure	0.6%
Environment & Energy	6.4%	Gambling	2.2%	Foreign Affairs	0.5%
Taxes	5.9%	Election Policy	2.1%	Interstate Relations	0.3%
Labor & Employment	4.9%	Elections (Results)	1.8%		

Note: Frequencies do not sum to 100% as some stories contain multiple topics.

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:

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

For some robustness tests in Appendix F, 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 Bill and Roll-Call Data Collection

Bill roll-call data are obtained from LegiScan. The sources of bills in each policy area are below:

Table A3: Sources of Relevant Legislation

Policy Domain	**Source**	**Search Term or URL**
Restrict Abortion	LegiScan	`abortion+OR+(pregnancy+NEAR+termination)`
Same-Sex Marriage	LegiScan	`(marriage+NEAR+man+NEAR+woman)+OR+(marriage +NEAR+same+NEAR+sex)+OR+(marriage+NEAR +equality)+OR+(marriage+NEAR+sexual+NEAR +orientation)`
Stricter Gun Laws	LegiScan	`firearm+OR+handgun+OR+rifle`
Police Body Cameras	LegiScan	`(police+NEAR+body+NEAR+camera)+OR+(law+NEAR +enforcement+NEAR+camera)`
Minimum Wage	NCSL	`https://www.ncsl.org/labor-and-employment/ minimum-wage-legislation-database`

C.1 Bill Ideological Classification

To analyze all bills on a given policy domain in a single regression, I code the ideological direction of bills. Specifically, I fit the logistic regression model

$$\Pr(\text{Vote}_i = 1) = \beta_0 + \beta_1 \text{Dem}_i + \varepsilon_i \tag{A3}$$

for each bill, where Vote_i is whether a legislator i voted in favor of a bill, and Dem_i is a binary variable indicating whether they are a Democrat. Where $\beta_1 > 0$, I code bills as taking the liberal position on an issue, and where $\beta_1 < 0$, I code them as conservative.

I validated these automated codings by hand using a sample of 352 bills—a random sample of 100 bills on abortion and gun laws, plus all bills in the sample about LGBTQ rights, police body cameras, and the minimum wage. Table A4 reports results of this effort. I find that 17.7% of bills are either nonideological or are included in the sample incidentally (e.g., because surrounding sections of law mention abortion and are reprinted in the text of the bill, though the bill focuses on something else). Of the remaining 82.3% of bills, the regression classification approach correctly identifies the ideological direction of 90.6% of bills, with some variation across policy domains.

Table A4: Validation of Bill Ideology Classification

Policy Domain	Accuracy	Correct (Raw)	Incorrect (Raw)	Incidental/ Neutral
Abortion	98.2%	82.5%	1.5%	16.0%
LGBTQ Rights	96.4%	81.5%	3.1%	15.4%
Gun Control	87.2%	68.0%	10.0%	22.0%
Minimum Wage	94.1%	94.1%	5.9%	0.0%
Police Body Cameras	77.1%	50.0%	14.8%	35.2%
Total	**90.6%**	**75.2%**	**7.1%**	**17.7%**

D MRP Estimates of District Opinion

Here, I provide technical details of district opinion estimation using 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*b*; Warshaw and Rodden 2012).

D.1 Opinion Data

To estimate opinion, I rely on responses to the Cooperative Election Study (CES, formerly CCES), which is conducted every two years and includes approximately 60,000 respondents per survey. I construct the main opinion measures from the following questions on the CES; my results are robust to several alternative questions (see Appendix F.4)

Restrict Abortion: (2010-2012) "Which one of the opinions on this page agrees with your view on abortion?" Coded as *restrict* if respondents say that abortion should be allowed "never," "only in case of rape, incest, or when the woman's life is in danger," or "only after the need for the abortion has been established." (2014-2020) "Do you support or oppose the following proposals?" Coded as *restrict* if respondents support abortions "always...as a matter of choice," "only in cases of rape, incest, or when the woman's life is in danger," or only before "the 20th week of pregnancy."

Same-Sex Marriage: (2010) "Do you support a constitutional amendment banning gay marriage?" Opposition to the amendment is coded as *support* for same-sex marriage. (2012-2016) "Do you favor or oppose allowing gays and lesbians to marry legally?"

Stricter Gun Laws: (2010-2012) "In general, do you feel that the laws covering the sale of guns should be..." Coded as *restrict* if respondents say laws should be "more strict." (2014-2022) "On the issue of gun regulation, are you for or against each of the following..." Coded as *restrict* if respondents support either of the following policies: "background checks" or "ban assault rifles." Because these questions differ dramatically (see the distributions of opinion below), district opinion estimates are rescaled to mean-zero, unit variance within question type before being combined.

Police Body Cameras: (2016, 2020-2022) "Do you support or oppose each of the following proposals? Require police officers to wear body cameras that record all of their activities while on duty."

Minimum Wage: (2016-2018) "If your state put the following questions for a vote on the ballot, would you vote for or against? Raise the state minimum wage to $12 an hour." (2020) "Do you support each of the following proposals? Raise the minimum wage to $15 an hour." Because these questions differ dramatically, district opinion estimates are rescaled to mean-zero, unit variance within question type before being combined.

D.2 Modeling Opinion

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 CES. I begin by fitting the below predictive model using the `vglmer` package in `R` (Goplerud 2023):

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

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

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 $\text{s}(\cdot)$ refers to a flexible spline over a continuous predictor at the district level. In the cases of same-sex marriage and abortion opinion, I include a spline $\text{s}(\text{Evangelical}_d)$ for the percent of the district that is evangelical, following Lax and Phillips (2009*a*). By a similar logic, I include I include a spline for $\text{s}(\text{PctUnion}_d)$ for the share of a district that is a member of a labor union when estimating support for minimum wage increases. For each question, I fit separate models for upper- and lower-chamber legislative districts in each year of the survey.

A final wrinkle to my opinion estimation approach is that MRP models typically include a random effect for the target geography—in this case, state legislative districts, which are not included in the CES. To address this problem, I use geographic information included in the survey—ZIP code and county—to determine the probability that each respondent lives in each possible legislative district. Appendix D.3 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 then follow the usual poststratification procedure (Lax and Phillips 2009*b*).

D.3 Matching the CES to Legislative Districts

The CES includes granular location data for respondents, including state, ZIP code, and county, but not state legislative districts. I use ZIP codes and counties to match respondents probabilistically to districts. My approach is similar to Steelman and Curiel (2023). 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 sometimes updated by the United States Postal Service to aid in mail delivery. However, the U.S. Census Bureau collects data by ZIP Code Tabulation Areas (ZCTAs), which approximate ZIP codes.

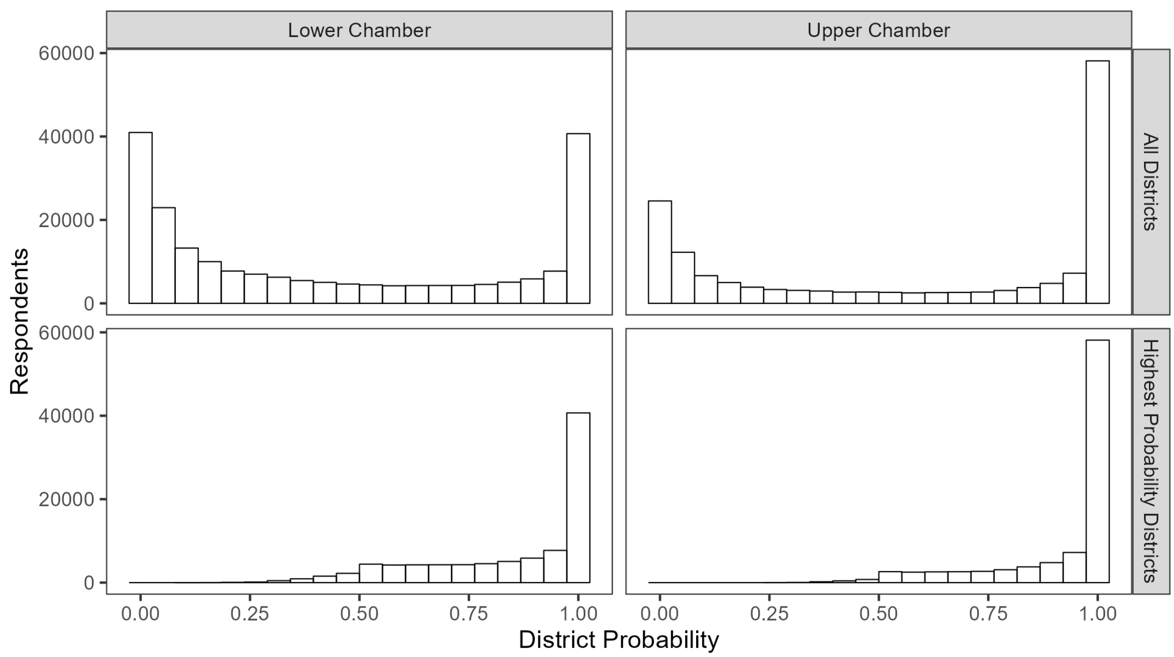
For each CES respondent, I use GIS software to find the overlap of their ZIP code (substituting ZCTA) and county. I then identify all districts that intersect with this area, and find the population of each district-ZCTA-county combination by aggregating up from Census blocks. I use these population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

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

Probabilities sum to 1 for each respondent separately for lower- and upper-chamber districts.

Figure A2 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 A2: District Matching Probabilities in the CES

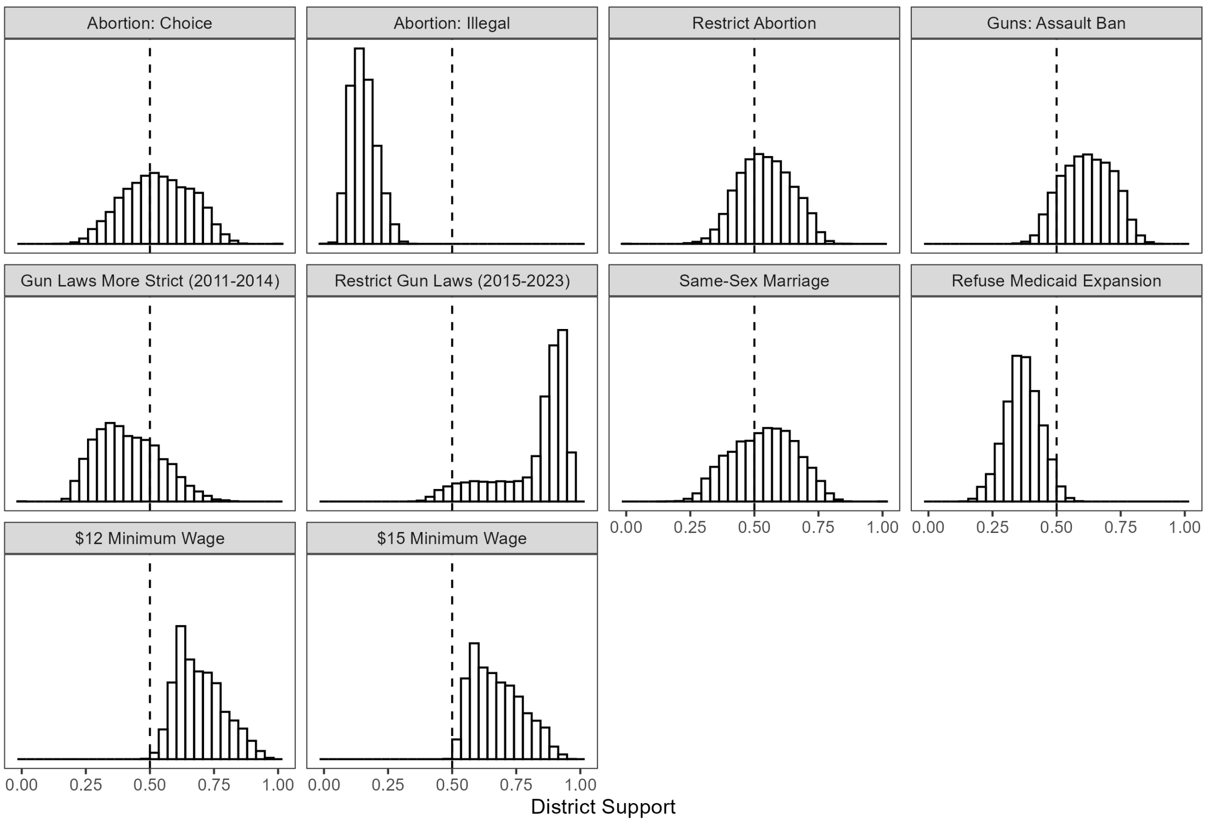


Note: Histograms report the distribution of district probabilities for all respondents of the CES.

D.4 Distribution of District Opinion

Below, in Figure A3, I report the distribution of district-level opinion estimates by policy domain and question, over the full time series of the sample. Note that the distributions for some variables are much more skewed than others (e.g., the composite restriction of gun access based on support for specific policies beginning in the 2014 CES). In the case of guns, this seems to be caused by high levels of mass support for requiring background checks. For this reason, I present results using alternative opinion measures (e.g., support for an assault weapons ban) in Appendix F.4 below.

Figure A3: Distribution of Opinion by District



Note: Histograms report the distribution of district-level support for each policy.

E Descriptive Statistics of Analysis Variables

Below, I report simple descriptive statistics for variables used in the main analysis and in the heterogeneous effects section of the paper. Descriptive statistics (i.e., distributional information) for public opinion measures is in Appendix D.4 above.

Table A5: Descriptive Statistics of Responsiveness Data

Statistic	Mean	St. Dev.	N
Congruence	0.066	0.108	327,683
% College	0.308	0.144	342,966
% 65+	0.150	0.043	342,966
% Under 30	0.392	0.057	342,966
% Black	0.120	0.168	342,966
% Hispanic	0.123	0.156	342,966
% Other	0.086	0.082	342,966
% Urban	0.581	0.372	342,966
Log Pop. Density	3.370	2.064	342,966
Tenure	5.222	5.535	342,966
Freshman	0.390	0.488	342,966
10+ Years	0.133	0.339	342,966
Leadership	0.029	0.168	342,966
Professionalization (Squire)	0.287	0.203	342,966
Staff per Member	4.730	4.911	342,966
Democrat	0.447	0.497	342,966
Majority Party	0.655	0.475	342,966

F Robustness of Responsiveness Results

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

F.1 Results without Measurement Error Correction

The table below replicates the main results using a single model, without accounting for measurement error in the process of estimating opinion. The results generally replicate (although the effect on same-sex marriage is not statistically significant at the 95% level in this specification).

Table A6: Newspaper Congruence and Responsiveness: without Error Correction

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage	All Issues
Opinion ×	1.56**	1.16	0.63**	4.88**	5.34**	0.05**
Congruence	(0.29)	(0.75)	(0.08)	(1.62)	(1.48)	(0.02)
Opinion	1.99**	0.01	0.06*	0.20	2.07**	0.24**
	(0.10)	(0.11)	(0.03)	(0.43)	(0.20)	(0.00)
Congruence	-0.91**	-0.19	-0.36**	-4.29**	-3.70**	0.01
	(0.18)	(0.41)	(0.06)	(1.45)	(1.01)	(0.01)
District Ctrls.	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X
N	93,384	3,810	225,686	5,036	6,659	334,575
Adj. R^2	0.48	0.40	0.52	0.56	0.44	0.44

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; uncertainty in Opinion measure is not propagated in the models. $^{*}p < 0.05$; $^{**}p < 0.01$.

F.2 Results using Observed Coverage

The main empirical strategy in the paper uses the congruence of media markets and legislative districts to exogenously identify the effect of news coverage on legislator behavior. Section 3 of the manuscript explains the reasons this approach is preferred. However, below I find similar results using a more blunt measure. Specifically, I replace congruence with the percent of all stories about the legislature that mention each individual legislator, using the news archive and automated text analysis procedure described in Appendix A. Because my sample of newspapers includes multiple newspapers in some communities, I only include the share of stories in the newspaper that covered each legislator most frequently (i.e., if a legislator is mentioned in 2% of one newspaper's coverage but 3% of another's, I used data only from the latter). I also exclude legislators who are not mentioned in any stories.

These results are generally consistent with those reported in the main text using congruence.

Table A7: Observed Coverage Frequency and Responsiveness

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × Pct. Stories	2.06**	3.54*	0.35**	12.38*	0.23
	(0.58)	(2.08)	(0.07)	(5.86)	(0.39)
Opinion	1.23**	1.70**	0.02**	0.38	0.21**
	(0.08)	(0.26)	(0.01)	(0.44)	(0.02)
Pct. Stories	-1.22**	-1.88	0.20**	-10.90*	-0.26
	(0.31)	(1.43)	(0.07)	(5.16)	(0.37)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	51,575	1,797	139,906	2,822	4,337
Adj. R^2	0.48	0.39	0.52	0.56	0.47

Note: Dependent variable is roll-call votes. Models include bill fixed effects. Standard errors are clustered by district and account for measurement error in Opinion. $^{*}p < 0.05$; $^{**}p < 0.01$.

F.3 Alternative Newspaper Circulation Data for Congruence

Newspaper results use congruence constructed from Alliance for Audited Media (AAM) ciruclation data for 2011-2022. An alternative source of data is the Standard Rate and Data Service (SRDS). SRDS data have been preferred by some other scholars (e.g, Peterson 2019) because they include more small newspapers that are less likely to participate in the AAM.

SRDS data have two key limitations for this study. First, SRDS data only exist through 2018, so out-of-date data must stand in for 2019-22. Second, small newspapers that appear in SRDS but not in AAM are less likely to have resources to fund full-time state capitol coverage. So, adding these newspapers may not actually be informative of the effects of news coverage *in state capitols*.

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. The lone exception is abortion, where the coefficient is not statistically significant and is close to zero (notably much smaller in magnitude than the effect of opinion on roll-call votes).

Table A8: Congruence and Responsiveness with SRDS Circulation Data

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × SRDS Congruence	-0.17	1.05**	0.06**	4.00*	0.17**
	(0.18)	(0.43)	(0.01)	(1.80)	(0.07)
Opinion	1.40**	1.41**	0.02**	0.48	0.17**
	(0.08)	(0.20)	(0.01)	(0.45)	(0.02)
SRDS Congruence	-0.06	-0.38*	0.16**	-3.47*	0.18**
	(0.12)	(0.21)	(0.02)	(1.61)	(0.08)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	92,200	3,612	223,471	5,029	6,668
Adj. R^2	0.48	0.40	0.52	0.56	0.45

Note: Dependent variable is roll-call votes. Models include bill fixed effects. Standard errors are clustered by district and account for measurement error in Opinion. $^{*}p < 0.05$; $^{**}p < 0.01$.

F.4 Alternative Measures of Opinion

This section reports the main responsiveness results using several alternative measures of opinion for those issues where opinion is a composite of different questions over the years. All results are robust to the alternative measures. Note that Abortion: Choice refers to support for allowing abortions "as a matter of choice," and so the negative coefficient is consistent with responsiveness.

Table A9: Newspaper Congruence and Responsiveness with Alternate Opinion Measures

	Abortion: Choice	Abortion: Illegal	Guns: Assault Ban	Guns: More Strict	Guns: Restrict	Min. Wage: \$12	Min. Wage: \$15
Opinion × Congruence	-0.97**	2.87**	1.49**	1.81**	0.59**	3.93**	3.37*
	(0.24)	(0.60)	(0.31)	(0.36)	(0.13)	(1.63)	(1.90)
Opinion	-1.25**	1.70**	0.94**	0.62**	0.75**	1.63**	2.80**
	(0.08)	(0.28)	(0.09)	(0.09)	(0.08)	(0.23)	(0.41)
Congruence	0.38**	-0.59**	-0.80**	-0.59**	-0.38**	-2.64**	-2.27*
	(0.11)	(0.11)	(0.18)	(0.13)	(0.11)	(1.12)	(1.26)
Years	2011-2022	2011-2014, 2017-2022	2015-2022	2011-2014	2015-2022	2017-2020	2021-2022
District Ctrls.	X	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X	X
N	87,269	72,617	150,279	65,509	150,279	5,141	1,247
Adj. R^2	0.49	0.49	0.53	0.53	0.53	0.43	0.52

F.5 Separate Results by Chamber

This section reports the main results separately for upper and lower legislative chambers. The sign and direction of all effects are identical to those from the models pooling chambers in the main text, though in upper chambers, some effects are no longer statistically significant. These are on issues with reduced sample sizes for data using the much smaller upper chambers.

Table A10: Newspaper Congruence and Responsiveness by Chamber

	Upper Chambers				
	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × Congruence	1.36**	0.95	0.19**	0.00	0.35*
	(0.45)	(1.68)	(0.03)	(6.33)	(0.17)
Opinion	0.87**	1.39**	0.01	1.62	0.18**
	(0.14)	(0.42)	(0.01)	(2.47)	(0.04)
Congruence	-0.85**	-0.01	0.12**	-0.02	0.06
	(0.28)	(1.01)	(0.04)	(5.72)	(0.15)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	21,115	726	48,455	1,228	1,533
Adj. R^2	0.51	0.48	0.53	0.54	0.48

	Lower Chambers				
	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × Congruence	0.81*	3.96**	0.13**	5.84**	0.39*
	(0.37)	(1.29)	(0.02)	(1.75)	(0.17)
Opinion	1.33**	1.40**	0.02*	0.27	0.16**
	(0.10)	(0.22)	(0.01)	(0.48)	(0.02)
Congruence	-0.60**	-2.01**	0.11**	-5.08**	0.14
	(0.23)	(0.76)	(0.03)	(1.57)	(0.14)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	65,718	2,825	166,091	3,668	4,855
Adj. R^2	0.48	0.39	0.52	0.58	0.43

Note: Results from OLS regressions where the dependent variable is roll-call votes for lower-chamber legislators. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district and account for measurement error in Opinion. $^{*}p < 0.05$; $^{**}p < 0.01$.

G Television Congruence Results

The main manuscript is primarily concerned with the effects of newspaper congruence on responsiveness because newspapers are much more engaged in covering state legislatures than other media. In this appendix, I show that legislators are also more responsive to public opinion when local TV newscasts face similar incentives to cover them.

To do so, I adapt the measure of Congruence to Designated Market Areas (DMAs, commonly called media markets) defined by the Nielsen Company, which correspond to the reach of broadcast television stations in a given market (Moskowitz 2021). Not all residents of a DMA may watch local TV news.

I compute TVCongruence_d using the formula

$$\text{TVCongruence}_d = \sum_{m=1}^{M} \text{ViewerShare}_{md} \times \text{MarketShare}_{md}, \tag{A6}$$

where ViewerShare_{md} is the share of potential viewers of TV stations in market m who live in district d. Because TV viewership data is prohibitively expensive, I use population as a proxy for viewership, following Snyder and Strömberg (2010); Campbell, Alford and Henry (1984). So,

$$\text{ViewerShare}_{md} = \frac{\text{Population}_{md}}{\text{Population}_m} \tag{A7}$$

and $MarketShare_{md}$ is defined as in Equation (4). In almost all cases, $\text{MarketShare}_{md} = 1$ because districts are not split across markets, so TVCongruence_d for most districts is equivalent to ViewerShare_{md}.

The table below reports results following the specification in Table 4 of the main paper, substituting TVCongruence_{it} for Congruence_{it}, for each legislator i's district in year t.

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage	All Issues
Opinion × TVCongruence	0.51	2.13	0.17**	0.66	0.34*	0.03
	(0.38)	(1.24)	(0.03)	(6.53)	(0.17)	(0.02)
Opinion	1.28**	1.44**	0.02**	0.82	0.18**	0.21**
	(0.08)	(0.20)	(0.01)	(0.46)	(0.02)	(0.00)
TVCongruence	-0.42	-1.13	0.13**	-0.48	0.05	0.02
	(0.24)	(0.72)	(0.04)	(5.89)	(0.14)	(0.02)
District Ctrls.	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X
N	93,384	3,810	225,686	5,036	6,659	334,575
Adj. R^2	0.48	0.40	0.52	0.56	0.44	0.44

Note: Results from OLS regressions where the dependent variable is roll-call votes. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district and account for measurement error in Opinion. $^{*}p < 0.05$; $^{**}p < 0.01$.

Positive coefficients across all five issues are consistent with TV news attention strengthening responsiveness, and in most cases (except police body cameras) are of a similar magnitude to the newspaper results reported in the main manuscript. However, the results are not statistically significant at the 5% level for three of five policy areas, and the pooled model.

There are two likely explanations for why these TV results differ from newspapers. First, local newspapers produce more coverage focused on state politics compared to TV broadcasts, as I showed in Figure A1. The resulting effects of this coverage, or of the behaviors of journalists working for newspapers on legislators, may be stronger. A second explanation is that using population, rather than direct television viewership, to estimate TVCongruence introduces measurement error that contributes to inflated standard errors. Nevertheless, across issues, a similar pattern emerges for the effect of local TV market congruence: Where legislators are more likely to be covered by broadcast reporters, they are also more responsive to their constituents.

H Heterogeneous Effects

This section reports full models for heterogeneous effects regressions, as well as tests of statistical significance for the differences between levels of moderating variables reported in Section 6.3 of the main manuscript.

First, I report regression results for the heterogeneous effects reported in the main paper.

Table A11: Full Regression Results for Heterogeneous Effects Models

	Professionalism	Staffing	Tenure	Leadership	Party	Majority Party
Opinion × Congruence	0.11**	0.10**	0.04	0.04*	0.03*	0.03
	(0.03)	(0.03)	(0.06)	(0.02)	(0.02)	(0.04)
Opinion	0.20**	0.21**	0.20**	0.21**	0.20**	0.18**
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Congruence	0.00	-0.07**	0.16**	0.01	0.01	-0.18**
	(0.03)	(0.03)	(0.05)	(0.02)	(0.01)	(0.03)
Prof. × Opinion × Congruence	-0.20*					
	(0.09)					
Hi Staff × Opinion × Congruence		-0.07*				
		(0.04)				
Tenure × Opinion × Congruence			0.00			
			(0.01)			
First 2 Yrs × Opinion × Congruence			-0.06			
			(0.07)			
10+ Yrs. × Opinion × Congruence			0.10			
			(0.09)			
Leadership × Opinion × Congruence				-0.03		
				(0.06)		
Dem. × Opinion × Congruence					0.02	
					(0.04)	
Majority × Opinion × Congruence						0.00
						(0.04)

(continued)

	Professionalism	Staffing	Tenure	Leadership	Party	Majority Party
Opinion × Professionalism	0.03					
	(0.02)					
Opinion × High Staff		-0.00				
		(0.01)				
Opinion × Yrs. in Office			0.00			
			(0.00)			
Opinion × First 2 Yrs.			0.03**			
			(0.01)			
Opinion × 10+ Yrs.			0.02			
			(0.01)			
Opinion × Leadership				0.02**		
				(0.01)		
Opinion × Majority						0.05**
						(0.01)
Congruence × Professionalism	0.02					
	(0.07)					
Congruence × High Staff		0.10**				
		(0.03)				
Congruence × Yrs. in Office			-0.03**			
			(0.01)			
Congruence × First 2 Yrs.			-0.14**			
			(0.06)			
Congruence × 10+ Yrs.			-0.29**			
			(0.09)			
Congruence × Leadership				0.13**		
				(0.04)		
Congruence × Dem.					-0.05*	
					(0.03)	
Congruence × Majority						0.28**
						(0.03)
Leadership	0.04**	0.04**	0.04**	0.03**	0.05**	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Democrat					0.18**	
					(0.00)	
Majority						0.06**
						(0.00)
Years in Office	-0.00	-0.00	0.00	-0.00	-0.00**	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
First 2 Years	0.01*	0.01**	0.01*	0.01*	0.01*	0.01*
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
10+ Years	0.00	0.00	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
District Ctrls.	X	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X	X
N	317,822	317,822	317,822	317,822	317,763	317,822
Adj. R^2	0.43	0.43	0.44	0.43	0.45	0.44

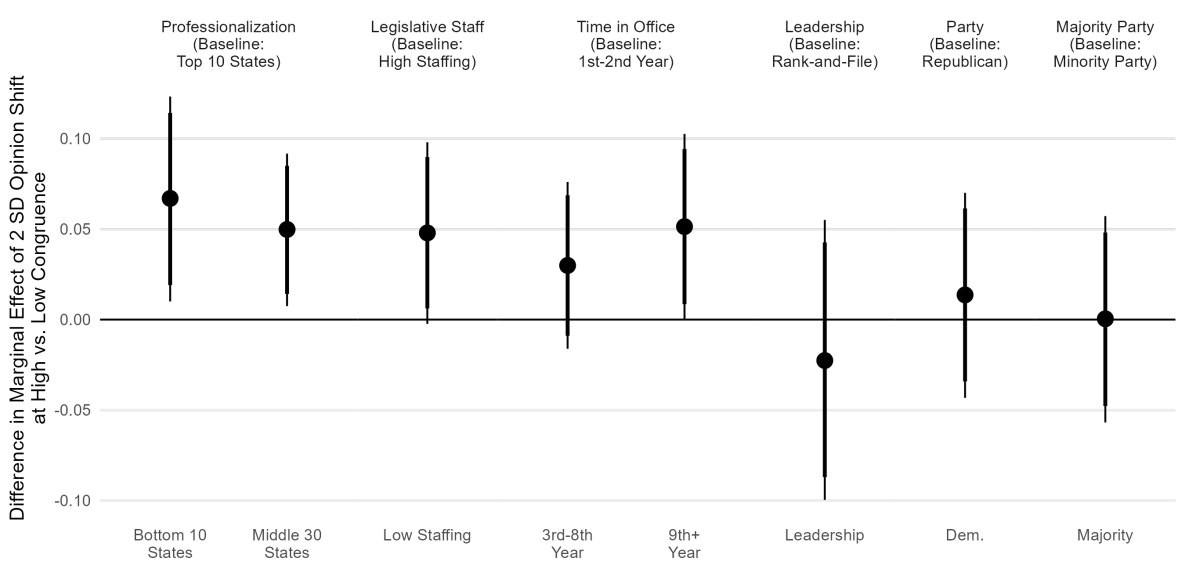
Note: Results from OLS regressions where the dependent variable is roll-call votes. All models include bill fixed effects. Standard errors, in parentheses, are clustered by district and account for measurement error in Opinion. $^{*}p < 0.05$; $^{**}p < 0.01$.

Next, I turn to tests of statistical significance for the differences between marginal effects reported in the main paper. Because these involve triple-interactions, testing statistical significance is not straightforward. In reporting heterogeneous effects in the paper, the "effect" of congruence was defined as the difference between the effect of a 2 standard deviation increase in public opinion on legislator roll-call votes in high- vs. low-congruence districts. That statistic was visible as the gap between the points in Figures 5 and 6.

In order to test whether the marginal effects are statistically distinguishable at different levels of moderating variables (e.g., professionalization, tenure in office, party, etc.), we must test whether the difference between those gaps vary at the different levels of the variable. The figure below reports these differences, as well as standard errors. I compute standard errors by bootstrapping and using the method of composition, as described in Section 5 of the main manuscript.

We can see that the effect of congruence on responsiveness is greater in the 10 least professionalized states, as compared to the 10 most professionalized states. Likewise, we see a slightly lesser effect on the middle 30 states as compared to the most professionalized states. These differences are statistically significantly greater than zero. We see a similarly sized effect of being a high-staffing state, which is significant at the 90% level (though not at the 95% level). And the effect of congruence is similarly greater among legislators in their ninth or greater years in office than those serving in their first or second terms.

Figure A4: Statistical Significance of Group Differences in Heterogeneous Effects Analysis



Note: Points report the differences between the marginal effect of high vs. low congruence at the stated baseline and at a different value of the moderator, noted on the x-axis. Error bars cover 90% and 95% confidence intervals.

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