Supplementary Materials for "Statehouse Democracy without the Electoral Connection"

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. Next, I accessed additional newspapers, as well as the transcripts or online story archives of local TV news stations, via NewsBank. The full corpus comprises 290 newspapers and 239 TV stations, including broadcast transcripts from 171 stations and online articles for an additional 68 stations.

A.1 Automated Text Analysis Procedure

Text analysis results in Section 2.2 of the main paper 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, or a TV media market overlaps. Names 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 (2021). Finally, I conducted an automated search of all 48.2 million articles and transcripts in the corpus for stories referencing one or more legislators.

A.2 Details of Newspaper Content Analysis

The content analysis used a random sample of newspaper articles identified in the automated search process as mentioning state legislators. The sample contains 494 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. Below, I report the list of topics identified, including non-policy stories.

Table A1: Story Topics: Full List

Topic	Pct.	Topic	Pct.	Topic	Pct.
Elections (General)	11.9%	Transportation	3.7%	Election Policy	1.6%
Education	11.0%	Social Welfare	3.5%	Civil & Family Law	1.0%
Crime & Criminal Justice	9.3%	Business & Econ. Dev.	3.1%	Rules and Procedure	0.9%
Budget	8.6%	Process (General)	3.1%	Immigration	0.9%
Health Care	8.1%	District Community Event	2.8%	Agriculture	0.9%
Civil Rights & Liberties	6.9%	Elections (Results)	2.5%	Multiple Policies	0.9%
Taxes	6.7%	Gambling	2.2%	Foreign Affairs	0.6%
Government Operations	5.8%	Ethics	2.2%	Campaign Finance	0.5%
Environment & Energy	5.7%	State-Local Gov. Relations	2.0%	Interstate Relations	0.4%
Labor & Employment	4.6%	Other	1.7%	State-Federal Relations	0.4%
Personal Lives	4.6%	Commemorative	1.7%		

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

Table A2 reports the questions used to determine if stories contain evidence of original reporting or watchdog reporting. These are largely drawn from accounts of watchdog journalism and news production (especially Bennett and Serrin 2005; Boydstun 2013) Stories are coded as Original or Watchdog if at least one response is coded as "yes."

Table A2: Coding Reporting Style

Original Reporting
Is there at least one interview, quote, or attempt to speak with politicians or their press offices?
Is there at least one interview, quote or attempt to speak with other experts or members of the community?
Is there evidence of on-the-ground, in-person reporting?
Watchdog Reporting
Does the story reference requesting or consulting public records or other documents?
Does the story bring in outside data or evidence (including published reports and government data)?
Does the story fact check or push back against claims made by politicians?
Does the story add context to policy problems or solutions, or include discussion of real-world effects of government action?
Does the story unearth new problems, wrongdoing, or government negligence (e.g., from investigative reporting)?

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 similarly follow Snyder and Strömberg (2010). ViewerShare_{md} is estimated 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.

SRDS Data: For some robustness tests in Appendices F and G, 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

$$\Pr(\text{Vote}_i = 1) = \beta_0 + \beta_1 \text{Dem}_i + \varepsilon_i \tag{A4}$$

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; 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 any of the following policies: "background checks," "ban assault rifles." Because these questions differ dramatically, 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):

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

$$
\begin{aligned}
\alpha^j_g &\sim N(0, \sigma^2_g) \text{ for all } g \text{ and } j \\
\alpha^{\text{district}}_d &\sim N(\alpha^{\text{state}}_{s[d]} + \text{s}(\text{Evang}_d) + \text{s}(\text{RepVote}_d) + \text{s}(\text{UrbanPct}_d) + \text{s}(\text{Income}_d), \sigma^2_{\text{district}}) \\
\alpha^{\text{state}}_s &\sim N(\alpha^{\text{region}}_{m[s]}, \sigma^2_{\text{state}}) \qquad \alpha^{\text{region}}_m \sim N(0, \sigma^2_{\text{region}})
\end{aligned}
\tag{A5}
$$

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. 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).

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:

$$\Pr(\text{District}_i = d \mid \text{ZIP}_i = z, \text{County}_i = c) = \frac{\text{Population}_{dzc}}{\sum_{d=1}^{D} \text{Population}_{dzc}}. \tag{A6}$$

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

E Descriptive Statistics of Analysis Variables

Table A5: Descriptive Statistics: Main Results

Statistic	Mean	St. Dev.	N
Congruence	0.066	0.108	326,133
TVCongruence	0.056	0.081	341,178
% College	0.308	0.144	341,272
% 65+	0.150	0.043	341,272
% Under 30	0.392	0.057	341,272
% Black	0.120	0.168	341,272
% Hispanic	0.123	0.156	341,272
% Other	0.085	0.083	341,272
% Urban	0.581	0.373	341,272
Log Pop. Density	3.372	2.063	341,272
Tenure	5.223	5.537	341,272
Freshman	0.389	0.488	341,272
10+ Years	0.133	0.339	341,272
Leadership	0.029	0.168	341,272

Table A6: Descriptive Statistics: Knowledge

Statistic	Mean	St. Dev.	N
Correct	0.037	0.189	2,507
White	0.718	0.450	2,507
Black	0.110	0.314	2,507
Hispanic	0.098	0.297	2,507
Other Race	0.074	0.261	2,507
Female	0.562	0.496	2,507
High School	0.250	0.433	2,507
No High School	0.028	0.166	2,507
Some College	0.330	0.470	2,507
30-44	0.253	0.435	2,507
45-64	0.334	0.472	2,507
65+	0.205	0.404	2,507
Log Pop. Density	3.861	1.829	2,507
Years in City	16.043	14.690	2,507

Table A7: Descriptive Statistics: Electoral

Statistic	Mean	St. Dev.	N
Incumbent Running	0.783	0.412	20,027
Incumbent Unopposed	0.262	0.440	20,027
Rolloff Rate	0.109	0.101	20,027
Turnout	0.564	0.137	19,611
% College	0.302	0.141	19,611
% 65+	0.152	0.046	19,611
% Under 30	0.392	0.062	19,611
% Black	0.101	0.156	19,611
% Hispanic	0.111	0.145	19,611
% Other Non-White	0.075	0.092	19,611
% Urban	0.588	0.375	19,415
Log Pop. Density	3.562	2.191	19,415
Incumbent	0.058	0.101	18,487
Dem. Pres. Vote	0.047	0.070	18,235
Nationalization	−0.157	0.159	15,222

F Robustness of Responsiveness Results

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

F.1 Baseline Dyadic Responsiveness

Table A8 reports baseline responsiveness results for the five policy areas in the main paper. The positive, significant coefficients on Opinion show that legislators are more likely to vote in favor of policies if their constituents are more supportive of them.

Table A8: Baseline Responsiveness to Public Opinion

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion	1.31**	1.58**	0.03**	0.84*	0.19**
	(0.07)	(0.18)	(0.01)	(0.42)	(0.02)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	92,633	3,810	224,694	5,029	6,657
Adj. R^2	0.48	0.40	0.52	0.56	0.44

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

F.2 Results without Measurement Error Correction

Table A9: Newspaper Congruence and Responsiveness: without Error Correction

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × Congruence	1.60**	1.13	0.15**	4.88**	0.48**
	(0.28)	(0.75)	(0.02)	(1.62)	(0.13)
Opinion	2.02**	-0.04	0.01*	0.20	0.18**
	(0.09)	(0.11)	(0.01)	(0.43)	(0.02)
Congruence	-1.01**	-0.09	0.12**	-4.29**	0.12
	(0.17)	(0.41)	(0.02)	(1.46)	(0.10)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	92,633	3,810	224,694	5,029	6,657
Adj. R^2	0.48	0.40	0.52	0.56	0.44

Table A10: TV Market Congruence and Responsiveness: without Error Correction

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × TVCongruence	1.44**	2.01*	0.16**	-1.71	0.42**
	(0.38)	(1.02)	(0.03)	(5.97)	(0.14)
Opinion	2.03**	-0.05	0.02**	0.80	0.20**
	(0.09)	(0.11)	(0.01)	(0.45)	(0.02)
TVCongruence	-0.96**	-0.85	0.13**	1.66	0.18
	(0.24)	(0.53)	(0.04)	(5.39)	(0.15)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	92,633	3,810	224,694	5,029	6,657
Adj. R^2	0.48	0.40	0.52	0.56	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.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 A11: 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 Model Specifications

F.4.1 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.

Table A12: 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

Table A13: TV 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 × TVCongruence	-0.44	2.97**	1.56**	1.54**	0.68**	4.04*	1.53
	(0.31)	(0.85)	(0.41)	(0.45)	(0.21)	(2.38)	(2.52)
Opinion	-1.32**	1.59**	0.97**	0.67**	0.75**	1.79**	2.98**
	(0.08)	(0.27)	(0.09)	(0.09)	(0.08)	(0.23)	(0.41)
TVCongruence	0.08	-0.68**	-0.88**	-0.49**	-0.47**	-2.72*	-1.06
	(0.14)	(0.16)	(0.23)	(0.17)	(0.16)	(1.63)	(1.73)
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	92,633	77,637	156,100	68,594	156,100	5,343	1,314
Adj. R^2	0.48	0.48	0.53	0.53	0.53	0.42	0.53

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

F.4.2 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 Tables A14 and A15, I present versions of the main results that include a control for party. Though coefficients are generally not statistically significant, these tables show that the media strengthens policy responsiveness, even beyond the above and beyond party.

Table A14: Newspaper Congruence and Responsiveness with Party Control

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × Congruence	0.16	0.64	0.07**	4.54**	0.03
	(0.13)	(0.69)	(0.01)	(1.23)	(0.05)
Opinion	0.22**	0.47**	-0.02**	-0.09	0.00
	(0.04)	(0.15)	(0.00)	(0.30)	(0.01)
Congruence	-0.14*	-0.19	0.01	-4.06**	-0.03
	(0.08)	(0.42)	(0.02)	(1.11)	(0.06)
Democrat	-0.66**	0.61**	0.49**	0.49**	0.84**
	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	86,808	3,551	214,513	4,896	6,387
Adj. R^2	0.72	0.63	0.65	0.67	0.81

Table A15: TV Market Congruence and Responsiveness with Party Control

	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × TVCongruence	0.14	1.42*	0.08**	-5.82	-0.27**
	(0.19)	(0.86)	(0.02)	(4.81)	(0.10)
Opinion	0.23**	0.38**	-0.02**	0.60*	0.01
	(0.04)	(0.15)	(0.00)	(0.34)	(0.01)
TVCongruence	-0.10	-0.73	0.02	5.31	0.07
	(0.11)	(0.51)	(0.02)	(4.35)	(0.08)
Democrat	-0.66**	0.61**	0.49**	0.49**	0.83**
	(0.01)	(0.02)	(0.00)	(0.02)	(0.01)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	92,171	3,612	223,417	5,029	6,656
Adj. R^2	0.71	0.63	0.65	0.67	0.80

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. $^*p < 0.05$; $^{**}p < 0.01$.

F.4.3 Separate Results by Chamber

Table A16: 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. $^{*}p < 0.05$; $^{**}p < 0.01$.

Table A17: TV Market Congruence and Responsiveness by Chamber

	Upper Chambers				
	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × TVCongruence	0.77	0.96	0.13**	3.90	-0.03
	(0.60)	(1.71)	(0.04)	(12.18)	(0.27)
Opinion	1.01**	1.52**	0.02	1.52	0.22**
	(0.14)	(0.43)	(0.01)	(2.39)	(0.04)
TVCongruence	-0.56	-0.57	0.14**	-3.34	0.28
	(0.38)	(0.98)	(0.05)	(11.02)	(0.22)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	22,273	726	50,116	1,254	1,586
Adj. R^2	0.50	0.47	0.53	0.53	0.48

	Lower Chambers				
	Restrict Abortion	Same-sex Marriage	Stricter Gun Laws	Police Body Cameras	Minimum Wage
Opinion × TVCongruence	0.70	3.41*	0.18**	-1.50	0.58**
	(0.49)	(1.91)	(0.03)	(8.01)	(0.22)
Opinion	1.35**	1.39**	0.02**	0.91*	0.17**
	(0.09)	(0.23)	(0.01)	(0.49)	(0.02)
TVCongruence	-0.57*	-1.84*	0.14**	1.49	-0.15
	(0.31)	(1.10)	(0.05)	(7.22)	(0.18)
District Ctrls.	X	X	X	X	X
Legislator Ctrls.	X	X	X	X	X
N	69,923	2,886	173,334	3,775	5,071
Adj. R^2	0.47	0.38	0.52	0.57	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 Robustness of Electoral Connection Results

This appendix reports several robustness tests for the political knowledge and electoral behavior results. 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.

G.1 Sample Comparison with Close-Election Design

In Table A18, I show that district-years included in a close election design (using a bandwidth of 0.07, following Myers 2025) differ from those that are excluded. Most notably, close-election districts have many more white residents than do excluded districts. They are also slightly better educated and wealthier. Finally, they are more suburban, as evidenced by high % Urban but much lower population density (the U.S. Census Bureau classifies all built-up areas as urban and others as rural). These districts are quite different from those where elections are not close.

Table A18: Characteristics of Close-Election RDD Sample

	Close Districts	Remaining Districts
Population Density (per mile^2)	157.67	221.31
% Urban	58.1%	58.83%
% High School	90.6%	87.81%
% College	32.8%	29.44%
% Black	6.1%	12.00%
% Hispanic	9.0%	11.21%
% Under 30	37.7%	39.46%
% 65+	15.6%	14.89%
Med. Income	$36,427.90	$33,111.42
Number of Districts	4,669	18,743

G.2 Information Results: 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 A19 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).

Next, I vary the source of circulation data used to construct Congruence. As discussed in Appendix F.3, 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 A19 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 2025). The only specification I find that approaches the results from Myers (2025) requires not only using the SRDS data but also excluding the district-level covariates that are necessary to estimate the effect of congruence.

Table A19: 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

Table A20: 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. $^*p < 0.05$; $^{**}p < 0.01$.

In Table A20, 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 A21: 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: Dependent variable is whether respondents correctly name their state representative. Models include state fixed effects. Standard errors, in parentheses, are clustered by district. $^{*}p < 0.05$; $^{**}p < 0.01$.

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.3). In Table A21, I show results where respondents are correct if they name any representative that overlaps their county and Zip code.

G.3 Electoral Behavior Results: Alternative Specifications

Turning to the electoral connection results, in Table A23, 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 A22 reports similar robustness tests for TV congruence and finds that the results in the paper are not sensitive to the exclusion of covariates.

Table A22: Alternative Specifications of Electoral Results for TV

	Rolloff			Incumbency			Nationalization		
Congruence	**-0.00**	0.00	-0.00	-0.02	-0.03	-0.02	**0.01**	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	**(0.02)**	(0.02)	(0.02)
Congruence×				**-0.00**	0.00	0.00			
Incumbent				**(0.02)**	(0.02)	(0.02)			
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	18,289	18,487	18,289	22,318	22,356	22,318	14,198	14,346	14,198
Adj. R^2	0.67	0.67	0.66	0.88	0.88	0.88	0.46	0.47	0.46

Table A23: 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×	**-0.00**	0.00	0.00	0.02	0.02	0.02
Incumbent	**(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	0.01**	0.01**	0.01**	0.00	0.00	0.00
(w/ Party)	(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. $^{*}p < 0.05$; $^{**}p < 0.01$.

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 A24: Congruence and Voting Behavior by Chamber

	Upper Chambers					
	Rolloff		Incumbency		Nationalization	
	Newspaper	TV	Newspaper	TV	Newspaper	TV
Congruence	-0.00 (0.02)	-0.06 (0.05)	-0.00 (0.04)	-0.05 (0.10)	0.03 (0.04)	0.03 (0.10)
Congruence× Incumbent			-0.02 (0.02)	-0.07 (0.04)		
Incumbent (w/ Party)			0.02** (0.01)	0.02** (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

	Lower Chambers					
	Rolloff		Incumbency		Nationalization	
	Newspaper	TV	Newspaper	TV	Newspaper	TV
Congruence	-0.00 (0.01)	0.01 (0.02)	-0.04 (0.02)	-0.02 (0.06)	0.00 (0.02)	-0.04 (0.06)
Congruence× Incumbent			0.00 (0.02)	0.01 (0.05)		
Incumbent (w/ Party)			0.01** (0.00)	0.01** (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

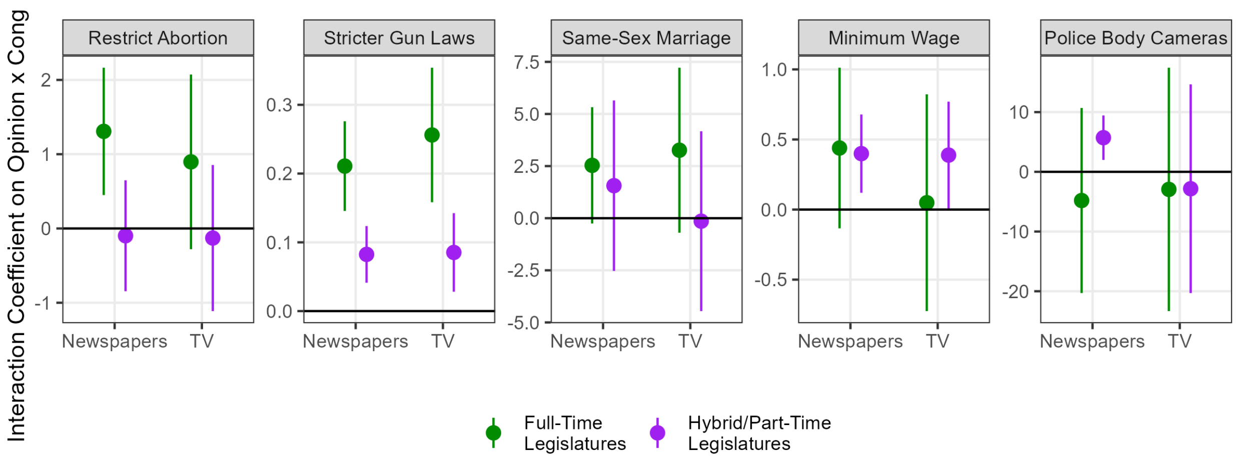
Note: Results from OLS regressions. Dependent variables are rolloff in state legislative elections (columns 1-2); two-party Democratic vote share for state legislature (columns 3-4), and vote nationalization (columns 5-6). All models include fixed effects for district, year, and quintiles of % Urban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

H Suggestive Evidence of Alternative Mechanisms

H.1 Responsiveness and Legislative Professionalization

In Figure A1, I consider whether the media's effect on responsiveness varies with legislative professionalization. I use the National Conference of State Legislatures classification to distinguish full-time from part-time and hybrid legislatures and re-run the main models from the paper, interacting all variables with professionalization. I find suggestive evidence consistent with media effects in representation in full-time 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 A25 shows that full-time and hybrid legislatures tend to have more full-time and session reporters. This difference is largely driven by newspaper reporters.

Figure A1: Responsiveness Results by Professionalization



Note: Results from OLS regressions where the dependent variable is roll-call votes. Models interact opinion and congruence with two-level legislative professionalization and include bill fixed effects. 95% confidence intervals from district clustered standard errors and account for measurement error in Opinion.

Table A25: Professionalization and Press Corps Size

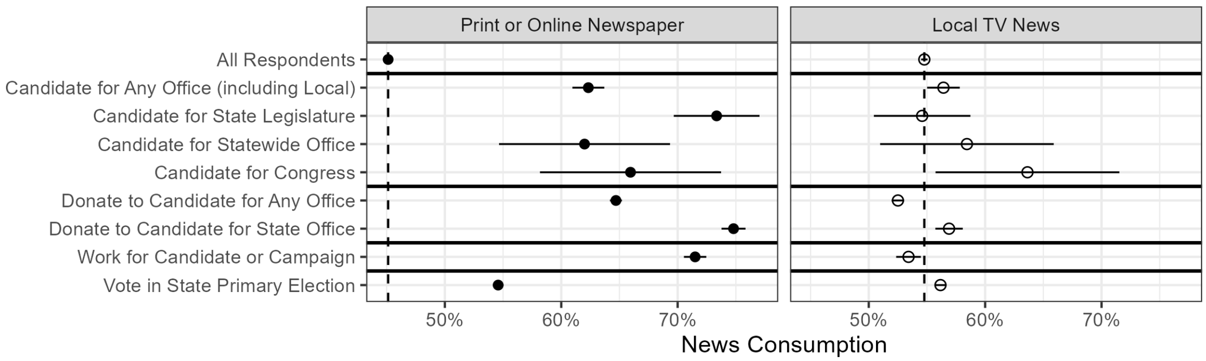
	Total Reporters (2014)	Newspaper Reporters (2014)	TV Reporters (2014)	Total Reporters (2022)
Full-Time	10.38*	3.66*	0.62	13.66**
	(3.93)	(1.76)	(0.76)	(4.06)
Hybrid	0.02	0.01	-0.00	0.05
	(0.03)	(0.01)	(0.01)	(0.03)
Legislature Size				
	50	50	50	50
N	0.07	0.02	-0.08	0.15

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.2 News Consumption among Political Elites

Figure A2 shows that elites are more likely read newspapers and (to a lesser degree) watch local TV news than is the general public, according to a pooled sample of the 2010-2020 CES.

Figure A2: Local Media Consumption among the Public and Political Elites



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