Media Reflect!

Policy, the Public, and the News

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Appendix

Measuring Public Preferences for Policy

Our measures come from the General Social Survey (GSS), which was fielded from 1973 to 1994 in every year except 1979 and 1981), and then in even-numbered years thereafter. We are able to use data through 2018 given the availability of media data. In each survey, the GSS asks about preferences for spending in various areas using the following item:

> We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether we are spending too much money on it, too little money, or about the right amount.
>
> Do you think the government is spending too much, too little or about the right amount on [e.g., health care]?'

Respondents are asked about a range of spending domains, some of which appear in each survey, including those of special interest to us – defense, welfare, and health.[1]

Although the survey question directly taps relative preferences, responses provide only very general information, i.e., on a three-point scale. We have discussed this at some length in our earlier work (Soroka and Wlezien 2010), but what is most important to reiterate here is that many respondents select the middling option, indicating that the government is spending "about the

[1] We rely on the primary representative sample in each year, ignoring minority oversamples. We do not apply survey weights, which are an issue since the GSS began employing a two-stage subsampling design for nonresponse beginning in 2004, as we have to use the subsample that receives the specific items of interest, e.g., the "welfare" spending question instead of the "assistance to the poor" variant. For more information about the GSS methodology, see www3.norc.org/gss. Note that applying survey weights makes little difference to the time series.

right amount." (Depending on the spending category and year, between 15 and 60 percent of respondents select this position.) Thus, the median respondent frequently appears to be happy with the policy status quo, and this may not change even as conditions and policy change. This apparent support for current spending levels nevertheless encompasses a good amount of variation, from those who are very close to preferring less to those who are equally close to favoring more. As such, the "true" median preference can change over time even as the median response to the survey remains the same.

It is possible to identify this public preference – at least the changes over time – by aggregating individual responses and focusing on the mean. We also can take the difference between the percentage of respondents wanting more and the percentage of respondents wanting less—technically, the percentage saying we are spending "too little" minus the percentage saying we are spending "too much." Indeed, the latter measure is *perfectly correlated* with the mean over time and provides a more intuitive empirical referent, in terms of actual percentages of the public, and we adopt this approach and refer to the percentage difference measures as indicators of "net support" for spending.

Note that GSS these data are not available in every year, and so we estimate equations with the raw time series as well as with those using linear interpolation to fill in missing values. Results differ only slightly, so we focus in the text on estimates that rely on interpolated data, but include results using raw data in Table A2, below. Mean values for the interpolated measures of preferences are -6.58 (StDev=16.63) for defense, -25.43 (StDev =12.02) for welfare, and 60.40 (StDev =8.87) for health. These reflect large differences in support for spending change over the time period, and also greater variation in preferences for defense, especially relative to health.

Measuring Budgetary Policy

As noted above, our measures of spending policy are drawn from the OMB of the US government. Specifically, we rely on the budget authority specified in appropriations, not the outlays that result from those decisions and spill out over time, the former of which are more directly under the control of elected officials. The budget functions literally match the spending preference categories in the cases of defense and health; for welfare, three subcategories of "Income Security" were excluded: "general retirement and disability insurance," "federal employee retirement and disability," and "unemployment compensation." The measures are in constant or "real" dollars. Specifically, we use price-adjusted data based on the OMB's

Consumer Price Index (CPI) . There are other transformations to the spending series worth considering as well. Some past work uses changes in per-capita spending, controlling for change in the size of the US population, for instance. Diagnostic analyses indicate that making this additional adjustment makes little difference to the results below, and so we opt for the aggregated values in this article.

We standardize the measures, expressing levels of spending as standard deviations from the zero-centered mean, estimated by domain, based on the observed variation in each domain-specific time series over the roughly 40 years examined here. Standardizing the spending series in this way makes no difference to their trends over time, of course; it simply expresses values using a different metric. There are several advantages of doing this, such as a more straightforward comparison of coefficients across domains for which the levels of spending differ quite dramatically.

Measuring The Media Policy Signal

The media policy signal is based on methods outlined in some detail in Neuner, et al (2019), Soroka and Wlezien (2019), Dun, et al. (2021), and Soroka and Wlezien (2022). As noted above, it relies on the hierarchical implementation of three content analytic dictionaries to sentences in articles about defense, welfare or health policy. The first dictionary captures mentions of spending:

Spend: allocat*, appropriation*, budget*, cost*, earmark*, expend*, fund*, grant*, outlay*, resourc*, spend*

The second captures mentions of upward or downward direction of change:

Up: accelerat*, accession, accru*, accumulat*, arise, arose, ascen*, augment*, boom*, boost*, climb*, elevat*, exceed*, expand*, expansion*, extend*, gain*, grow*, heighten*, higher, increas*, jump*, leap*, multiply*, peak*, rais*, resurg*, rise*, rising, rose, skyrocket*, soar*, surg*, escalat*, up, upraise*, upsurge*, upward*)

Down: collaps*, contract*, cut*, decay*, declin*, decompos*, decreas*, deflat*, deplet*, depreciat*, descend*, diminish*, dip*, drop*, dwindl*, fall*, fell, fewer, lose*, losing, loss*, lost, lower*, minimiz*, plung*, reced*, reduc*, sank, sink*, scarcit*, shrank*, shrink*, shrivel*, shrunk, slid*, slip*, slow*, slump*, sunk*, toppl*, trim*, tumbl*, wane*, waning, wither*

The third captures mentions of each policy domain, keeping in mind that articles are already identified based on mentions of one of the three policy domains, described below):

Defense: army, navy, naval, air force, marine*, defense, military, soldier*, war*, cia, homeland, weapon*, terror, security, pentagon, submarine*, warship*, battleship*,

destroyer*, airplane*, aircraft, helicopter*, bomb*, missile*, plane*, service men, base*, corps, iraq, afghanistan, nato, naval, cruiser*, intelligence

Welfare: welfare, social assistance, food stamp*, social security, income assistance, security income program, ssip, infants and children program, earned income tax credit, eitc, temporary assistance, tanf

Health: healthcare, health care, obamacare, affordable care act, medicare, medicaid, health insurance program, chip, health administration

The application of these dictionaries is examined in much more detail in Soroka and Wlezien (2022), where we also test the reliability and validity of each dictionary, and compare the hierarchical dictionary codes with results from machine-learning approach

These dictionaries are applied to full-text articles gathered from 17 of the highest-circulation newspapers in the US newspapers, three of which claim national audiences, and seven of which cover large regions in the northeastern, southern, midwestern, and western parts of the country: *Arizona Republic*, *Arkansas Democrat-Gazette*, *Atlanta Journal-Constitution*, *Boston Globe*, *Chicago Tribune*, *Denver Post*, *Houston Chronicle*, *Los Angeles Times*, *Minneapolis Star-Tribune*, *New York Times*, *Orange County Register*, *Philadelphia Inquirer*, *Seattle Times*, *St. Louis Post-Dispatch*, *Tampa Bay Tribune*, *USA Today*, and *Washington Post*.) Articles are identified based on the following combined subject and keyword searches in Lexis-Nexis:

Defense: "STX001996 or BODY(national defense) or BODY(national security) or BODY(defense spending) or BODY(military spending) or BODY(military procurement) or body (weapons spending)," where STX001996 is the National Security subject code.

Welfare: "N64000CC OR BODY(food stamp) OR BODY(income assistance) OR BODY(social assistance) OR BODY(social security) OR BODY(medicaid) OR BODY(medicare)," where N64000CC is the Social Assistance and Welfare subject code.

Health: "STX000833 OR BODY(health care)," where STX000833 is the Health Care Policy subject code.

As with budgetary data, measures of the media policy signal are standardized by domain.

Tests of Stationarity

In theory, public preferences are stationary time series, the linear combination of spending and the public's underlying preferred level of spending, which we expect to be cointegrated, where the former follows the latter over time (Soroka and Wlezien 2004; 2005; 2010). For defense, welfare, and health, Augmented Dickey Fuller (ADF) tests indicate that spending is integrated in levels but stationary in differences. This can be seen in Table A1, and allows us to estimate the equations described in the text that employ differenced spending variables.

Table A1. Dickey-Fuller Tests of Stationarity

	Levels		Changes	
	Lagged DV coefficient[a]	Test Statistic[b]	Lagged DV coefficient[a]	Test Statistic[b]
Policy				
Defense	-.063 (.040)	-1.600, p=.484	-.485 (.144)	-3.355, p=.013
Welfare	-.235 (.100)	-2.341, p=.411	-.928 (.161)	-5.784, p=.000
Health	-.287 (.120)	-2.396, p=.382	-.927 (.164)	-5.651, p=.000
Media				
Defense	-.280 (.120)	-2.329, p=.163		
Welfare	-.544(.149)	-3.653, p=.005		
Health	-.414 (.132)	-3.128, p=.025		
Preferences (interpolated)				
Defense	-.264 (.087)	-3.038, p=.032		
Welfare	-.227 (.091)	-2.499, p=.116		
Health	-.202 (.082)	-2.453, p=.127		
Preferences (raw)				
Defense	-.462 (.176)	-2.626, p=.088		
Welfare	-.471 (.158)	-2.973, p=.037		
Health	-.388 (.161)	-2.415, p=.138		

[a] Cells contain OLS coefficients with standard errors in parentheses.
[b] Cells contain Dickey-Fuller or augmented Dickey-Fuller test statistics with MacKinnon p-values.

Turning to news coverage, given the construction of the variables, we also expect those measures to be stationary, and to at least partly mediate the thermostatic effects of spending on preferences (Soroka and Wlezien 2022). ADF tests also support this expectation, particularly for welfare and health (see Table A1); for defense the coefficient for lagged media coverage is appropriately negative but not sufficiently reliable (p=0.16) to credit given the statistical demands of unit root tests. To further investigate, we conduct a KPSS test using Newey-West automatic bandwidth selection implemented in Stata, which does not reject the null of trend (or level) stationarity ($p > 0.10$). Given our expectations and the results of the various diagnostics, we assume that news coverage of defense spending change is stationary. Note that there is evidence of slight trend in the News series in each domain, though this is apparent only as drift (via the intercept term) for health and not the other areas when conducting ADF tests. When each of the series is detrended, tests more clearly reject a unit root in all domains – for defense (p=0.07), for welfare (p=0.01), and for health (p<.01). Using detrended News variables makes little difference

to the results of the analyses, and mostly serves to strengthen and clarify the patterns demonstrated in the paper. See Table A1 above.

Table A2. Bivariate Correlations Between Variables

	$Media_t$	$Policy_t$ [a]
All domains		
$Media_t$	—	—
$Policy_t$ [a]	.387***	—
$Public_t$	.485***	.152
Defense only		
$Media_t$	—	—
$Policy_t$ [a]	.624***	—
$Public_t$	.447***	.466***
Welfare only		
$Media_t$	—	—
$Policy_t$ [a]	.326*	—
$Public_t$	.492**	.289
Health only		
$Media_t$	—	—
$Policy_t$ [a]	.206	—
$Public_t$	.526***	.159

[a] First difference of spending decisions taken in year t for fiscal year t+1. Cells contain Pearson's r coefficients. ***p < .01; **p < .05; *p < .1

Diagnostics for preferences are more difficult, as there is a good amount of missing data, as mentioned in the text and described further just above in the appendix. For the analysis in Tables 1 and 2, we use the series including interpolated data, so it important to report results of diagnostics using those series, but tests ignoring the (14) missing observations provide additional, perhaps more useful information. These are reported in Table A1. There we can see that using interpolated series, the coefficients (in the third set of results in the table) are expectedly negative – and substantially so – in all cases but significant only for defense; they are too unreliable to credit for welfare and health. Ignoring missing data, results in the final set of results in Table A1 are more likely to reject the null of a unit root, particularly for defense and welfare; for health, the *p*-value is 0.14. KPSS tests for both the interpolated and raw series do not reject level (or trend) stationarity of health preferences ($p>.10$), however, and given our theoretical expectations and the totality of the evidence, we infer that these also are stationary for the purposes of this analysis.

Bivariate correlations between media, policy, and public preference variables (all measured at time *t*) are included in Table A2.

Results by Policy Domain

Coefficients in Tables 1 and 2 are based on models that pool our data across all three policy domains. Below, Table A3 reports results of Table 1 models for each policy domain separately. Given small sample sizes, there is less evidence of significant effects across the public, policy, and media in each domain. Even so, and most importantly given our argument above, we see evidence of the reflecting role of media in the positive coefficients for opinion in column 3, which are significant for welfare and health though not for defense.

Table A3. Basic Granger Causality Tests by Domain

Defense	**Dependent Variable**		
	$Public_t$	**$Policy_t$[a]**	**$Media_t$**
$Public_{t-1}$	0.727***	0.004	0.004
	(0.090)	(0.003)	(0.008)
$Media_{t-1}$	1.338	0.083+	0.525**
	(1.708)	(0.047)	(0.156)
$Policy_{t-1}$[a]	-8.657	0.192	0.937
	(6.327)	(0.175)	(0.578)
Constant	-2.846+	0.068	0.003
	(1.580)	(0.044)	(0.144)
N	38	39	38
Adjusted R2	0.679	0.317	0.507
Welfare	**Dependent Variable**		
	$Public_t$	**$Policy_t$[a]**	**$Media_t$**
$Public_{t-1}$	0.744***	0.006	0.042*
	(0.110)	(0.005)	(0.016)
$Media_{t-1}$	1.097	0.009	0.248
	(1.106)	(0.047)	(0.164)
$Policy_{t-1}$[a]	-7.825+	0.003	0.028
	(4.125)	(0.175)	(0.610)
Constant	-5.044+	0.185	1.183**
	(2.872)	(0.121)	(0.425)
N	38	39	38
Adjusted R2	0.629	-0.009	0.284

Health	Dependent Variable		
	$Public_t$	$Policy_t$[a]	$Media_t$
$Public_{t-1}$	0.816***	-0.001	0.035*
	(0.102)	(0.004)	(0.017)
$Media_{t-1}$	0.598	0.040	0.388*
	(0.906)	(0.035)	(0.148)
$Policy_{t-1}$[a]	-6.176	0.020	0.923
	(4.437)	(0.169)	(0.726)
Constant	11.588+	0.072	-1.575
	(5.845)	(0.222)	(0.956)
N	38	39	38
Adjusted R2	0.711	-0.034	0.408

[a] First difference of spending decisions taken in year t for fiscal year t+1 (or year t-1 for fiscal year t). Cells contain regression coefficients with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

Results Ignoring Missing Data

Table A4 reports results from re-estimating the regressions in Table 1 dropping all cases where there are missing values for public preferences. (The analyses in the text rely on linear interpolation to impute missing data.) This means that we focus on a time series of 27 observations of preferences instead of 41, in practice 24 observations since we only have media data from 1980; while imperfect, the analysis ignoring the missing data offers a basic check on the consequences of our imputation. Dropping the cases does change some of the results, particularly for statistical significance, which may not be surprising given that the sample is reduced by about 40%. Consider that imputing missing data by simply carrying forward the most recent lagged observations produces results closer to those in Table 1, e.g., the estimated coefficient for public preferences in the media equation is larger (0.012) and more reliable (p=.05). Also note that results of similar analyses following the more fine-grained specifications used in Table 2 of the article are more confirming.

Table A4. Granger Causality Tests Ignoring Missing Data

	Dependent Variable		
	$Public_t$	**$Policy_t$[a]**	**$Media_t$**
$Public_{t-1}$	0.484***	0.004	0.010
	(0.099)	(0.003)	(0.008)
$Media_{t-1}$	2.338	0.080+	0.288*
	(1.432)	(0.046)	(0.116)
$Policy_{t-1}$[a]	-8.475*	0.084	0.833*
	(4.071)	(0.133)	(0.329)
Constant	0.484***	0.004	0.010
	(0.099)	(0.003)	(0.008)
N	72	69	72
Adjusted R2	0.948	0.103	0.454

[a] First difference of spending decisions taken in year t for fiscal year t+1 (or year t-1 for fiscal year t). Cells contain regression coefficients with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

Results Using Detrended Media Signal Data

Table A5 reports results from re-estimating the regressions in Table 1 using a detrended media signal. The signal is detrended by regressing the media signal on a simple fiscal year (counter) variable to capture slight secular trend in media coverage, and then using the residuals from this model in place of the original media signal. Results differ only marginally from those in Table 1, confirming that our findings are not driven by a trend in the media signal.

Table A5. Granger Causality Tests with De-Trended Media Signal

	Dependent Variable		
	$Public_t$	**$Policy_t$[a]**	**$Media_t$**
$Public_{t-1}$	0.744***	0.004*	0.018**
	(0.054)	(0.002)	(0.007)
$Media_{t-1}$	1.048	0.040	0.431***
	(0.673)	(0.024)	(0.088)
$Policy_{t-1}$[a]	-7.752**	0.120	0.550
	(2.727)	(0.097)	(0.355)
Constant	-2.766*	0.075+	0.149
	(1.111)	(0.039)	(0.145)
N	114	117	114
Adjusted R2	0.975	0.119	0.480

[a] First difference of spending decisions taken in year t for fiscal year t+1 (or year t-1 for fiscal year t). Cells contain regression coefficients with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

On Media Effects

The estimated media effects in Table 2 disappear when we include more "current" measures of media coverage in the model, i.e., early year *t* coverage when analyzing opinion and late year *t* coverage when assessing spending change. See Table A6 for results.

Table A6. Revised Analyses based on the Timing of Measurement, Using More Proximate Measures of Media Content

	Dependent Variable	
	$Public_t$	**$Policy_t$[a]**
$Public_t$	---	0.004*
		(0.002)
$Public_{t-1}$	0.707***	---
	(0.053)	
$Media_{t,\ Early}$	2.185**	---
	(0.782)	
$Media_{t-1,\ Early}$	---	0.003
		(0.035)
$Media_{t-1,\ Late}$	0.036	0.059+
	(0.777)	(0.034)
$Policy_{t-1}$[a]	-8.928**	0.141
	(2.652)	(0.091)
Constant	-2.913**	0.080*
	(1.076)	(0.038)
N	114	117
Adjusted R2	0.977	0.181

[a] First difference of budgetary policy decisions taken in year t for fiscal year t+1 (or year t-1 for fiscal year t). Cells contain regression coefficients with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.

In the preference equation in the first column of the table, the coefficient for coverage early in year *t* is 2.18 (s.e.= 0.78), just about double that for coverage late in year *t-1* from the first column of Table 2, the estimate for which drops to 0.04 (s.e.=0.78) when both are included in the equation. This matters because early and late coverage appear to respectively reflect concurrent opinion and policy, as we see in columns 3 and 4 of Table 2, and coverage across half-years is highly correlated (r=0.84). These additional analyses thus suggest that opinion in year *t* is not independently responsive to prior coverage; or, at a minimum, it is not perfectly clear that media coverage causes preferences. (The reverse is not true, e.g., estimating the separate effects of

preferences in year *t-1* and the difference in preferences between *t-1* and *t*, the latter of which coverage of policy change in year t could possibly influence, continues to show significant positive effects of the former on coverage.) Also see Wlezien (N.d.) for more on the interrelationships between public opinion and the news.

For policy, in the second column of Table A6, the coefficient for coverage late in year *t* is 0.06, roughly the same as the coefficient for coverage early in year *t* in Table 2, the latter of which drops to 0.00 (0.03). Given that the news late in a year reflects decisions taken earlier that year (see Table 2), this is exactly what we would expect to observe, and the correlation between media coverage early and late in the year may help explain the apparent media effects on policy in Table 2. Also note that estimating the separate effects of preferences in year *t-1* and the changes in preferences between year *t-1* and year *t* in the policy equation shows positive effects of the former, which is much as we expect given the results in Table 1 and supportive of real public opinion effects.