

Online Appendix  
for  
**THE LUXURY GOODS VOTE**  
Why Left Governments are Punished More for Economic  
Downturns

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**Abstract**

Voters often punish incumbent parties for poor economic performance; whether they treat left and right governments differently has been less clear. We leverage both observational and experimental data to establish an empirical regularity: voters, on average, punish left-of-center incumbents more severely for economic downturns than their counterparts on the right. A luxury goods model of voting best explains this regularity. When times get tough, voters prioritize economic security over luxury goods policies – socially desirable but post-material policies most often associated with the left that voters support in abundant times. We reach this conclusion after running a ‘tournament of theories’ subjecting plausible rival hypotheses to empirical tests. The data suggest that the vote swing against the left in downturns does not arise from clientelism, partisan accountability, issue ownership or voters’ fear of redistribution but rather from an aversion to spending on luxury goods policies when times are lean.

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## Appendix

### A Objective economic data

We run our analysis with both objective (this section) and subjective (Section ?? in the paper) economic measures to establish that left parties leading governments are indeed punished more severely for downturns.

We use observational data from two sources – the first three modules of the Comparative Studies of Electoral Systems (CSES) and the Eurobarometer Mannheim Trendfile (*The Comparative Study of Electoral Systems*, 2013; Schmitt and Scholz, 2002). We restrict our sample to major developed democracies with stable party systems. This yields a sample of 51 elections in 20 countries for our base CSES sample.<sup>1</sup> Each dataset has advantages and drawbacks. CSES surveys are conducted only during national elections, providing a glimpse at what voters are paying attention to at a moment when politics is particularly salient. They encompass a wide range of countries with comparable questions asked in each country in a module. The CSES, however, only covers elections from 1996-2011 and has few repeated surveys within countries.

The Eurobarometer, in contrast, provides us with semi-annual surveys from fewer (eight) countries that have participated since the early 1970s<sup>2</sup> but many more surveys over a longer time period (1970-1999). Since the Eurobarometer conducted two surveys per year in each country, most do not correspond to an actual election period with the attendant increase in the salience and awareness of political issues.

The two data-sets complement each other and offer distinct advantages over hypothesis testing with a single dataset. Not only does the the use of two datasets reassure readers

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<sup>1</sup>Australia 1996, 2004, 2007; Austria 2008; Belgium 1999(Flanders), 1999(Wallonia), 2003; Canada 1997, 2004, 2008; Denmark 1998, 2007; Finland 2003, 2007, 2011; France 2002; Germany 1998, 2002, 2005, 2009; Greece 2009; Ireland 2002, 2007; Italy 2006; Japan 1996; Netherlands 1998, 2002, 2006, 2010; Norway 1997, 2001, 2005, 2009; New Zealand 1996, 2002, 2008; Portugal 2002, 2005; Spain 1996, 2000, 2004, 2008; Sweden 1998, 2002, 2006; USA 1996, 2004, 2008.

<sup>2</sup>The included countries are Belgium, Denmark, France, Germany, Ireland, Italy, Netherlands, and the UK.

that reported results are not an artifact of single dataset found through atheoretical data-mining but the ability to show an effect with different measures, samples and time periods increases confidence in the generalizability of a finding.

## **A.1 Variables**

We employ vote choice (or, for the Eurobarometer data, intended vote) for the head of government's party as the dependent variable. In most cases this is the prime minister's party but in presidential systems it is the president's party. We do this in order to focus on the party most clearly responsible for economic outcomes. This is consistent with [Duch and Stevenson \(2008\)](#) who conjectured that the most important posts, namely that of the PM's office and the Finance Minister, enable clearer attribution of responsibility, and [Duch, Przepiorka and Stevenson \(2015\)](#) who demonstrate with experimental work that voters primarily punish decision makers with proposal power.<sup>3</sup>

The election-year change in the unemployment rate serves as our key explanatory variable for the first part of our observational data analysis. The election year is defined as the election quarter as well as the three preceding quarters. In order to reduce measurement error – since elections can fall early in a quarter and unemployment can change after that – we measure only changes up to the quarter before the election. Thus the difference between unemployment in the quarter preceding the election and the quarter four quarters before the election constitutes our measure.<sup>4</sup>

We choose unemployment as our objective economic indicator because of its direct relevance to voters. The loss (or potential loss) of employment presents a much larger financial burden than does, for example, a slower aggregation of national wealth (i.e., GDP growth). Inflation offers an alternative but has been negligible for over two decades in developed economies and, consequently of low political salience. Growth in GDP does

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<sup>3</sup>also see [Fisher and Hobolt \(2010\)](#).

<sup>4</sup>Models run on CSES data calculate the change in aggregate unemployment using IMF International Financial Statistics quarterly data; unemployment figures in the EB data are calculated from quarterly OECD harmonized unemployment figures.

not appear in two of the partisan voting models that we wish to test (*clientelism* and *partisan accountability*). Various measures related to unemployment have also featured in much of the more innovative work in comparative political economy recently (e.g., [Rehm, Hacker and Schlesinger, 2012](#); [Alt and Lassen, 2014](#)).

We further reason that accountability entails reward or punishment for *changes* in the general welfare of the electorate, thus we use the *change in unemployment rate* rather than the unemployment rate. This is consistent with the well-known finding that voters are more attuned to the direction of change in the unemployment rate than the unemployment rate itself ([Conover, Feldman and Knight, 1986](#)). Employing the alternative, the level of employment, would also risk measuring cross-national differences more than within-nation differences in our pooled panel since unemployment usually changes slowly within nations but differs markedly across them. In employing change in unemployment as our key independent variable, we also remain consistent with the first paper to posit a luxury goods vote ([Stevenson, 2002](#)) and recent results that could be interpreted to support it ([Dassonneville and Lewis-Beck, 2013](#)).

Note that the use of an objective macroeconomic indicator – change in the unemployment rate – comes with both costs and benefits. It offers the advantage of associating voting with observable economic outcomes at least loosely connected to economic policy and it frees us from the vagaries of question choice in election surveys – for example, only module 1 of the CSES includes a question on economic perceptions. The cost, of course, is the loss of variation within election studies. Every respondent in a given election study or Eurobarometer country-survey is assigned the same economic value. Thus, country- and election-study fixed-effects models are not possible and the economic effects that we estimate with the change in unemployment variable are driven by cross-national and cross-survey variation. We acknowledge these drawbacks and square the circle by running models using perceived economic deterioration as the key explanatory variable in Section 4 in the main text.

Economic performance is our key independent variable but our theory posits that its influence on the vote is conditioned by the ideological position of the governing party. We recognize that scales differ across countries and adjust for this by measuring the governing party’s position as its deviation from the position of the median party in each election on a right-left scale (*LeftDeviation*).<sup>5</sup> This calculation is possible because the CSES surveys ask respondents to locate the parties participating in the recent election on a ten-point left-right scale, which we reverse so that higher values correspond to greater leftness. Our basic measure of perceived lead-party leftness is the difference between the lead party and the party each respondent placed at the median.<sup>6</sup> The CSES also reports parties locations on an ideological scale, as given by a pool of country experts. This enables us to create a second “expert” *LeftDeviation* measure of how far the governing party deviates to the left of the median to contrast with the “perceived” measure.

Party position measures are often, and justifiably, criticized, both on grounds of validity and reliability (Lo, Proksch and Gschwend, 2013). As our research design cannot avoid employing governing party positions, we ameliorate this problem by demonstrating robustness to three measures of party position. Our third measure is used in our Eurobarometer analysis and comes from the Comparative Manifestos Project (CMP) left-right placement (Volkens et al., 2011) which we again reversed so that greater number indicate greater leftness. Again, in order to account for the relative leftness of the lead party, right-left placement is measured as the distance from the parliamentary median party. This could theoretically range from -100 to +100. In order to make the coefficients more easily comparable to the CSES results above, we divided the right-left scores by 10 so that

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<sup>5</sup>Other infidelities nevertheless remain with the cross-national comparison of party positions (Lo, Proksch and Gschwend, 2013). Ideally we would estimate a “within” model so that cross-national party position variation does not influence estimates but this is not possible with country-year aggregate economic data (unemployment) that do not vary across subjects within election surveys. We do accomplish this with the perceived economic data later in our analysis.

<sup>6</sup>Since many respondents only placed a single party and that to be included in our analysis they had to place the lead party on the right-left scale, a nontrivial proportion of lead (and only lead) parties are placed at the median, as can be seen in the histogram in the first panel of Figure 1. Nevertheless, this method improves comparisons between elections.

the possible range goes from -10 to +10.<sup>7</sup> While this party placement measure, as well as the CSES expert placement measure, offer us additional means of testing our hypotheses, it is important to note a substantial drawback with both of them: CMP and expert placement measures are at the party-level, so do not vary across respondents, which deprives us of within-panel variation and subjects these measures to some of cross-national comparability problems discussed in [Lo, Proksch and Gschwend \(2013\)](#).

We readily concede that ideological measures of party placement are not conceptually interchangeable with a party reputation for luxury goods provision. It is wholly possible for even extreme left parties to focus on, say, redistribution rather than luxury goods. Nevertheless, it is most often left parties that support luxury good spending leading to a highly positive association between leftness and proposals for luxury goods spending. We take this distinction seriously and distinguish between leftness and luxury goods provision in the experimental section of the paper.

Rounding out our specification, and with one notable exception, we have a set of control variables that are likely orthogonal to our unemployment and party placement measures but included out of convention. The exception is *PolicyDistance* which measures the perceived ideological distance between each respondent and the governing party. Respondents in the CSES surveys placed both the lead party and themselves on the same scale. The remaining control variables are largely self-explanatory and found in both the CSES and EB datasets: *Unemployed*, a dummy intended to capture egotropic rather than sociotropic effects, *Age*, *Female*, *Education*, and *Income*.

## A.2 Analysis

Table 1 presents our results using an objective economic measure, the change in the unemployment rate. Models (1) through (5) rely on CSES data and respondent-reported vote as the dependent variable. Model (6) employs Eurobarometer Trendfile data and, since most of its semiannual surveys do not correspond with elections, vote intention rather

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<sup>7</sup>The observed range goes from about -7.5 to +7.5.

than vote as the dependent variable. In all models, we estimate simple binary logistic regressions of voting (vote intention) for the lead party, with standard errors clustered by election survey. To avoid arbitrarily giving more weight to election surveys with more respondents, we weight our models by election survey sample size.

Model (1) estimates a minimalistic economic voting model and shows results characteristic of cross-national election research with aggregate economic measures: while correctly signed, the effect of the change in the unemployment rate on voting for the lead party is far from statistically significant. This is unsurprising, well documented in the literature (Paldam, 1991; Anderson, 2007), and the grounds for a shift to individual-level perceived economic measures in much research (Lewis-Beck and Stegmaier, 2000; Duch and Stevenson, 2008). Cross-national economic voting results are highly context dependent (Powell and Whitten, 1993). The variance in  $\Delta Unemployment$  is largely cross-national in the CSES data since there are few elections in each country, given the short (1996-2011) timespan of the panel. In contrast, the Eurobarometer data have more within-country (but between surveys within each country) variation than cross-national variation given the small number of countries but large number of surveys within each.

Models (2) through (6) consider the conditional effect of change in unemployment on the vote. Not only do they test for an effect of the economy on the vote for the incumbent party but they probe the possibility that voters hold left and right governments differently accountable for the economy. Both effects can theoretically exist simultaneously, a sort of double jeopardy for the left from economic downturns. Voters can punish left incumbents like most other incumbents for a poor economy but additionally punish them because they are associated with luxury good policies.

The main relationship of interest to us is the *conditional* effect of the economy on the vote for lead parties at different degrees of leftness. It is this relationship that tests for asymmetric effects depending on the partisanship of the government and can begin to distinguish between the rival theories expounded in Section 2 and Table 1 in the main text. The simple (not conditional) standard errors on the interaction coefficients

	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta Unemployment$	-0.039 (0.072)	-0.063 (0.077)	-0.061 (0.069)	-0.101 (0.079)	-0.030 (0.090)	-0.253*** (0.066)
<i>Left Deviation</i>		0.010 (0.016)	0.003 (0.016)	0.031 (0.018)	0.040 (0.046)	0.013 (0.015)
$\Delta Unem * LeftDeviation$		-0.013 (0.018)	-0.010 (0.018)	-0.028 (0.018)	-0.041 (0.041)	-0.021 (0.019)
<i>Unemployed</i>	-0.099 (0.109)	-0.093 (0.110)	-0.090 (0.090)	-0.091 (0.103)	-0.134 (0.105)	-0.290*** (0.045)
<i>Age</i>			0.006*** (0.002)	0.009*** (0.002)	0.008*** (0.002)	0.001 (0.003)
<i>Female</i>			0.069* (0.032)	0.092** (0.032)	0.104*** (0.031)	-0.032* (0.015)
<i>Education</i>			-0.115*** (0.023)	-0.118*** (0.024)	-0.119*** (0.028)	-0.062*** (0.005)
<i>Income</i>			0.084* (0.037)	0.069 (0.036)	0.067 (0.040)	0.054*** (0.006)
<i>Policy Distance</i>				-0.386*** (0.111)	-0.372*** (0.111)	
<i>Constant</i>	-0.698*** (0.081)	-0.730*** (0.082)	-0.737** (0.246)	0.094 (0.358)	0.037 (0.387)	-1.241*** (0.072)
<i>Data</i>	CSES	CSES	CSES	CSES	CSES	EB
<i>DependentVar.</i>	Vote	Vote	Vote	Vote	Vote	Vote Intent
<i>Party Placement</i>	–	Perceived	Perceived	Perceived	Expert	CMP
<i>N.Obs.</i>	71617	57891	50483	49278	47181	158196
<i>N.Elections</i>	51	48	48	48	46	205†
<i>N.Countries</i>	20	18	18	18	18	8
<i>CorrectPred.(%)</i>	66.91	67.66	68.01	71.72	71.49	76.11
<i>BIC</i>	90934	72855	62717	55124	52837	171733

Table 1: *Unemployment change, party placement and lead-party vote. Dependent variable is vote for the largest governing party. OECD only. Binary logit. Standard errors, in parentheses, clustered by election. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . † Country surveys, not elections, in the Eurobarometer (EB) sample.*

in models (2) to (6) show no evidence of a statistically significant relationship. Logit interaction coefficients, however, are often misleading (Ai and Norton, 2003) and do not report whether a variable in an interaction has a significant effect at some value of the conditioning variable. To evaluate this relationship, we plot out the marginal effect of a one-point change in our unemployment variable on the probability of voting for the

lead governing party at all values of leftness (*LeftDeviation*) in our sample, using three different measures of party placement.

The three panels in Figure 1 are based on Models (4) to (6) in Table 1. All interval-level covariates other than that for lead-party leftness (*LeftDeviation*) are set at their sample means and categorical covariates are set at their modes. Thus, the marginal predicted probabilities in the first two (CSES) panels are estimated for a 48 year old, employed male with middle-category income who has completed secondary education and estimates that he is 2.6 points away from the lead party on a ten-point ideological scale. The histograms show the sample distribution of the lead party “leftness” (*LeftDeviation*) – its deviation from the median party in parliament as assessed by respondents, CSES experts and CMP experts, respectively.

The panels depict graphically what we already observed about the non-partisan economic vote in Table 1. When the lead party is at the median of parliamentary parties (*LeftDeviation*=0), the economic vote is insignificant in the CSES sample but substantively large and significant in the Eurobarometer sample. All three models, however, estimate an increasingly negative effect of a unit rise in unemployment on the change in the probability of voting for the incumbent governing party as incumbent party leftness increases. This is consistent with our assertion that leftness magnifies the economic vote for lead governing party. The first panel (Model 4 in Table 1) employs CSES data and a party position variable based on the individual-level perceptions of survey respondents. A one point increase in the change in the unemployment rate corresponds to a statistically significant decrease in the probability of voting for the lead governing party once it is more than two points to the left of the parliamentary median. For a lead party two points to the left of the median, this corresponds to an approximately 2.5 point drop in the probability of receiving our mean respondent’s vote; at 10 points to the left of the median, this figure increases to an 8-point drop.

We have already discussed the measurement problems with cross-national party location variables and the problem of no within-panel variation with objective economic

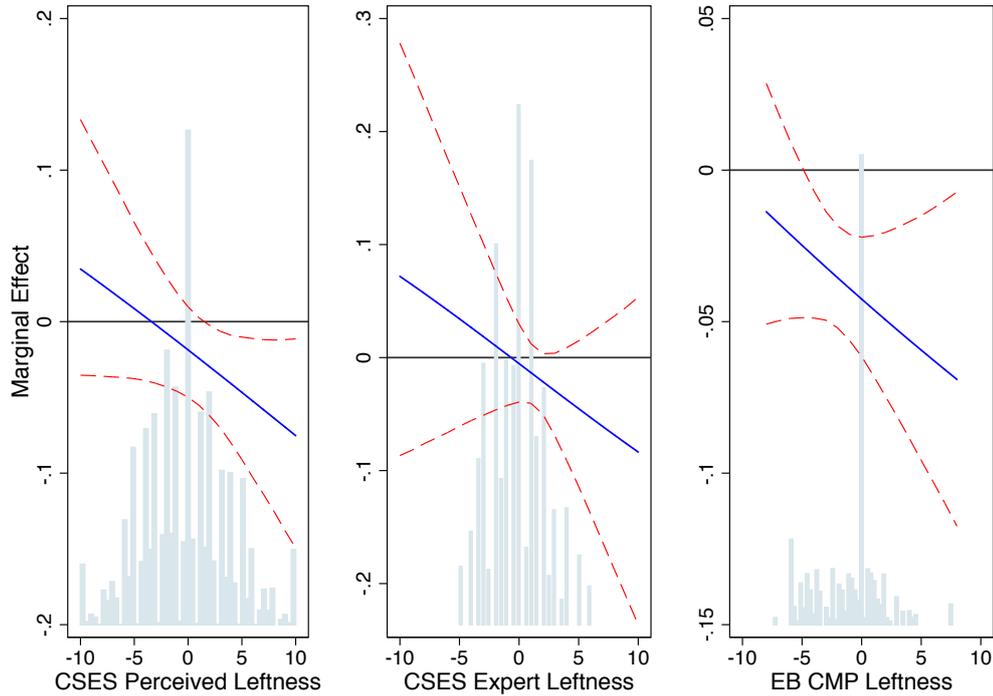


Figure 1: *Marginal effect of a one-point increase in the unemployment rate on respondents' probability of voting for the lead party at different leftward deviations of the lead party from the parliamentary median party. Estimated from the Models (4), (5) & (6), respectively, in Table 1. Figures estimated at covariate means; categorical variables set to mode.*

measures (we address both of these in the next section) but one other possible source of estimation bias arises in Model (4). The conditioning variable for the perceived party position (*LeftDeviation*) might be endogenous to the respondent's vote choice. Objective measures of party position certainly introduce measurement error and rely even more heavily on cross-national variation but they are certainly exogenous to respondent's vote choice. Model (5), shown in the second panel of Figure 1, replicates Model (4) but substitutes in CSES expert party placements in place of respondent placements. Changes in unemployment appear statistically insignificant regardless of governing party leftness but the estimated marginal effects are notably similar to those from Model (4). It seems that the shift to exogenous expert party placement primarily has an effect on the uncertainty of our estimates – unsurprisingly given the aggregate-level data assigned to individuals

and cross-national variation – but only a modest effect on the estimates themselves. Endogeneity bias, at most, has a modest effect in Model (4).

The third panel in Figure 1, based on Model (6), offers a third test of the same relationship, again using objective economic data, but a wholly different dataset, the Eurobarometer Mannheim Trendfile. Whereas the three merged CSES modules used in the previous models offer relatively broad coverage (18-20 countries) with few election surveys (mean of 2.6) within each, the Eurobarometer data offer narrower coverage (8 countries) but with many surveys (mean of 25.6) within each. This frequency of coverage is, of course, only possible by conducting surveys regularly (semi-annually) regardless of the proximity of elections, with the consequence that the dependent variable is now *vote intention* for the lead governing party. Unlike the CSES modules, the Eurobarometer also does not ask respondents to place themselves and parties on an ideological scale. The *Policy Distance* variable therefore falls out of the model.

Testing our relationship of interest on a completely different dataset offers several advantages. It is less likely that the results are an artifact of a given dataset discovered by atheoretical data fishing, so readers enjoy greater confidence that the hypothesis tests are valid. Additionally, the use of a second dataset with markedly different sample coverage increases confidence in the generalizability of the findings. The Eurobarometer data also offer a third advantage for our purposes: a third distinct measure of party placement. Models (4) and (5) placed parties via respondent estimates and expert opinions, respectively. The Eurobarometer data used for Model (6) places parties on an ideological scale based on party manifestos, as estimated in the Comparative Manifestos Project (CMP). As with expert placements, the CMP placements provide no individual-level variation but a third measure should increase confidence that results are robust to variation in party-placement methods.

The third panel in Figure 1 provides such reassurance. It displays the predicted marginal effects on the probability of voting for the lead governing party for an individual with sample mean values for the model covariates (and modal values for categorical vari-

ables): A middle-aged (40-44 year old), middle-income, employed (unemployed=0) female who stopped full-time education between the ages of 17 and 18. Despite the expectation that the salience of the economy for voting might be lower in surveys that do not correspond with elections, Panel 3 shows a substantively similar result for far-left parties and a stronger effect for centrist and even right-of-center incumbents. A double effect — both the conventional economic vote and what we argue is the luxury goods vote — is most evident here: Voters punish nearly all lead parties for a deteriorating economy but they punish parties of the left more. A party located at the parliamentary median can expect a four-point lower probability of receiving the vote of our simulated average voter when the change in unemployment increases by a point; a party at the extreme left would see its probability drop by over six percentage points; while even a lead party of the middle-right would expect a two-point drop.

### **A.3 Assessment**

Considered together, the model estimates in Table 1 and Fig 1 suggest that voters do punish left-of-center lead parties more severely than their counterparts on the right when the economy deteriorates. Moreover, the farther to the left a party is positioned, the greater its punishment — although, in the case of model (5), the effect for the change in unemployment on the vote is never statistically significant. The substantive magnitude of this effect for the left parties placed farthest from their parliamentary medians is similar regardless of whether the model was estimated with CSES data and respondent placement of parties, CSES data and expert placement of parties or Eurobarometer data and party manifesto (CMP) placement of parties. A governing lead party of the far-left can expect a drop of eight, five, and six percentage points, respectively, in the probability of receiving the average respondent's vote when the change in the unemployment rate increases by one point. Voters punish left governments more than right governments in downturns.

So what do these first results suggest about partisan theories of voting? The *clientelism* hypothesis predicts that voters should increase support for left governing parties when unemployment rises, so is refuted by the data. Voters faced with an increase in general unemployment do not, on average, increase their support for parties most likely to prioritize reducing unemployment (i.e., the left). Personal experience of unemployment indeed does increase support for the left as shown elsewhere (Margalit, 2013, and in Section 4.3 of the main text ), but this effect is insufficiently large to outweigh the contrary turn toward the right of the more numerous employed voters.

The *Luxury goods voting*, *class interests*, *issue ownership* (for competence managing the economy) and *partisan accountability* hypotheses, in contrast, all correctly predict a decrease in support for the left. Such observational equivalence requires different tests, found in sections 5 and 6 of the main text. At this point, however, we can tentatively conclude two things: Voters do seem to punish left incumbents more than their counterparts on the right during downturns and all potential mechanisms except for *clientelism* are compatible with this finding.

## B Luxury Goods Survey Experiment

1. Two large parties are competing in a national election. The **[governing/opposition]** party advocates strong funding for **[luxury good policies / basic left policies]**. The **[opposition/governing]** party advocates reducing taxes and cutting back on programs such as those above. Thus, their platforms are:

**[GOVERNING/OPPOSITION] PARTY:** Fully fund programs (e.g., **[luxury good policies / basic left policies]**)

**[OPPOSITION / GOVERNING] PARTY:** Reduce taxes, cut back on programs (e.g., **[luxury good policies / basic left policies]**)

On the scale below, please indicate how inclined are you to vote for the GOVERNING PARTY.

1. very unlikely
2. .
3. .
4. .
5. equal chance of voting for either party
6. .
7. .
8. .
9. .very likely

2. Under the stewardship of the same governing party mentioned in the previous question, the economy goes into a DEEP RECESSION. The government still proposes fully funding the arts and programs such as those mentioned above. You earn enough to meet the needs of your family but you are not wealthy and have to monitor your budget closely. The economic slowdown has reduced your income and a friend of yours has lost his job as the unemployment rate has increased. Given the new economic situation and assuming that you will vote, how likely are you to vote for the governing party?

1. very unlikely
2. .
3. .
4. .
5. equal chance of voting for either party
6. .
7. .

8. .
9. .very likely

Table 2: Policy examples used in experiment

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luxury good policies:	“the arts, programs for troubled teenagers and the reintroduction of native animal species to local forests”
basic left policies:	“workers compensation for injured workers, vocational training for the unemployed and better unemployment benefits”

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*Policy examples only vary for the left party, which can be either in government or opposition.*

## B.1 Double jeopardy experiment: D-in-D estimates

	all	left	left-basic	left-lux	right-v-basic	right-v-lux
$Vote_{t-1}$	0.931*** (0.015)	0.956*** (0.027)	1.003*** (0.033)	0.926*** (0.039)	0.942*** (0.019)	0.888*** (0.045)
$Treatment$	0.002 (0.117)	0.000 (0.170)	-0.001 (0.203)	-0.024 (0.254)	0.020 (0.135)	-0.021 (0.283)
$Period$	0.004 (0.117)	-0.005 (0.168)	-0.000 (0.203)	-0.024 (0.250)	0.034 (0.135)	-0.037 (0.289)
$Treatment * Period$	-1.088*** (0.165)	-1.573*** (0.241)	-1.034*** (0.287)	-2.199*** (0.363)	-0.971*** (0.192)	-0.315 (0.400)
$Constant$	0.366** (0.119)	0.293 (0.218)	-0.019 (0.258)	0.517 (0.332)	0.230 (0.124)	0.456 (0.278)
$N.Observations$	500	244	120	124	132	124
$N.Respondents$	250	122	60	62	66	62
$R^2$	0.885	0.852	0.891	0.846	0.955	0.766

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table 3: *Effects of a recession treatment on the incumbent party vote under various incumbent ideology and policies. Difference in differences. Amazon Mechanical Turk sample. Standard errors in parentheses. \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ .*

## B.2 Summary statistics for Mechanical Turk sample

Table 4: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
$Vote_t$	5.102	2.696	1	9	500
$Vote_{t-1}$	5.348	2.673	1	9	500
<i>Left Right Self Placement</i>	4.06	2.046	1	9	500
<i>PID : Close to Dems</i>	0.284	1.28	-2	2	500
<i>PID : Close to Reps</i>	-0.795	1.273	-2	2	498
<i>Education</i>	3.468	0.845	1	5	500
<i>Sex(fem = 2)</i>	1.54	0.499	1	2	496
<i>Financial situation of HH</i>	2.4	1.071	1	5	500

Table 5: Summary statistics. Participants observed pre- and post-treatment. 250 participants in total.

## C Funding preferences survey

The survey was conducted using a Qualtrics interface. The text is reproduced below.

### C.1 Questionnaire wording

(1) Given current economic circumstances, please indicate your preference for more or less governmental spending by moving the slider for each policy item to the number indicating how much you would decrease or increase spending in that area. NEGATIVE numbers indicate a preference for cutting spending; zero corresponds to no change; POSITIVE numbers indicate a preference for increasing spending.

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-3	=	decrease spending radically
-2	=	decrease spending substantially
-1	=	decrease spending modestly
0	=	no change in spending
1	=	increase spending modestly
2	=	increase spending substantially
3	=	increase spending radically

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- International development aid
- Border police for limiting immigration
- Income tax rebates and reductions
- Infrastructure investment (roads, bridges, broadband)
- Reintroduction of native species to original habitats
- Monitoring and enforcement of CO2 emissions limits for industry
- Arts funding (museums, philharmonic, opera, etc.)
- Subsidies for childrens daycare
- Military Spending
- Medical research
- Unemployment benefits
- Subsidies and tax rebates for foreign investors
- Welfare benefits for the poor
- Retraining programs for unemployed workers

- Social inclusion programs and training for marginalized social groups
- Temporary suspension of payroll withholding for social security and pensions
- Subsidized school lunches for poor children

(2) The economy goes into a DEEP RECESSION. Your family still earns enough to meet its basic needs but you have to cut back and monitor your budget closely. The economic slowdown has reduced your income and a friend of yours has lost his job as the unemployment rate has increased. You are concerned about the security of your own familys employment.

Given these new circumstances, please indicate your preference for less or more governmental spending by filling in the blank to the left of each policy item with a number from the scale in the table below. NEGATIVE numbers indicate a preference for cutting spending; zero corresponds to no change; POSITIVE numbers indicate a preference for increasing spending.

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-3	=	decrease spending radically
-2	=	decrease spending substantially
-1	=	decrease spending modestly
0	=	no change in spending
1	=	increase spending modestly
2	=	increase spending substantially
3	=	increase spending radically

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[REPEAT SAME POLICY LIST]

[A standard battery of demographic questions follows (see descriptive statistics).]

## C.2 Funding preference survey descriptive statistics

Table 6: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Close to Democratic Party	0.228	1.199	-2	2	92
Close to Republican Party	-0.769	1.25	-2	2	91
Left-Right Self-Placement	-0.598	1.568	-3	3	92
Financial situation of HH	1.538	1.009	0	4	91
Children less than 18 in HH	0.598	1.054	0	4	92
Age in years	36.337	10.253	22	67	92
Sex (Female=1)	0.446	0.497	0	1	92

Table 7: *Funding preference survey summary statistics. Amazon Mechanical Turk Sample.*

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