The Power of Partisanship in Brazil:

Evidence from Survey Experiments

(Supplemental Information)

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Abstract

This document contains the following items mentioned in the main text of the paper, but not included due to space limitations:

- Evidence of partisanship from observational data
- Information on randomization procedures and balance
- Robustness of results to the inclusion of controls
- Internal validity checks on the online survey results
- Results from the pilot study conducted with college students

Partisanship in Observational Data

The PT is clearly in a league of its own both in terms of aggregate number of identifiers and the "boundedness" of its label. However, a skeptic might still wonder what "boundedness" means in practice—and thus whether party labels are truly meaningful. Do attachments to political parties in Brazil shape voter behavior? Conventional wisdom says that if party labels are "real,'' they should shape voters' opinions about incumbent governments, and their vote choices.

A quick look at surveys taken over the past 20 years provides preliminary evidence that party labels have the effects they 'should'—shaping perceptions of governments and candidates. Figure A.1, for instance, shows that since 1989, evaluation of the president's job performance is substantially higher among those who identify with his/her party than for identifiers with the main opposition party, with all others (identifiers of other parties and non-identifiers) roughly in between.

Likewise, Figure A.2 shows that in the first round of all the six presidential elections since 1989, identifiers of the PT were considerably more likely to vote for PT candidates (Figure 2(a)),

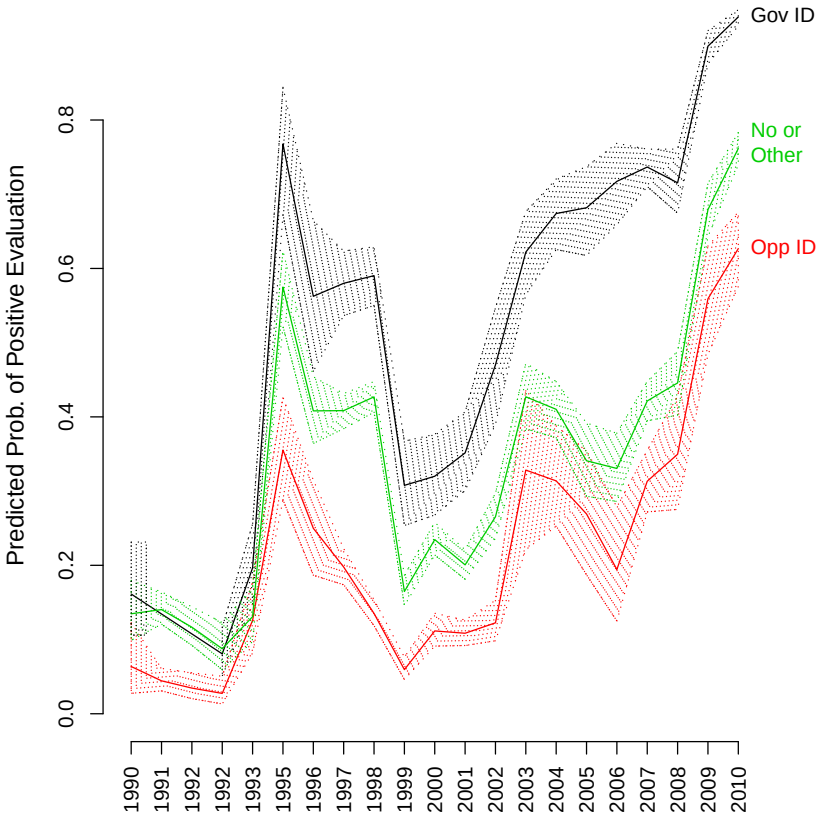
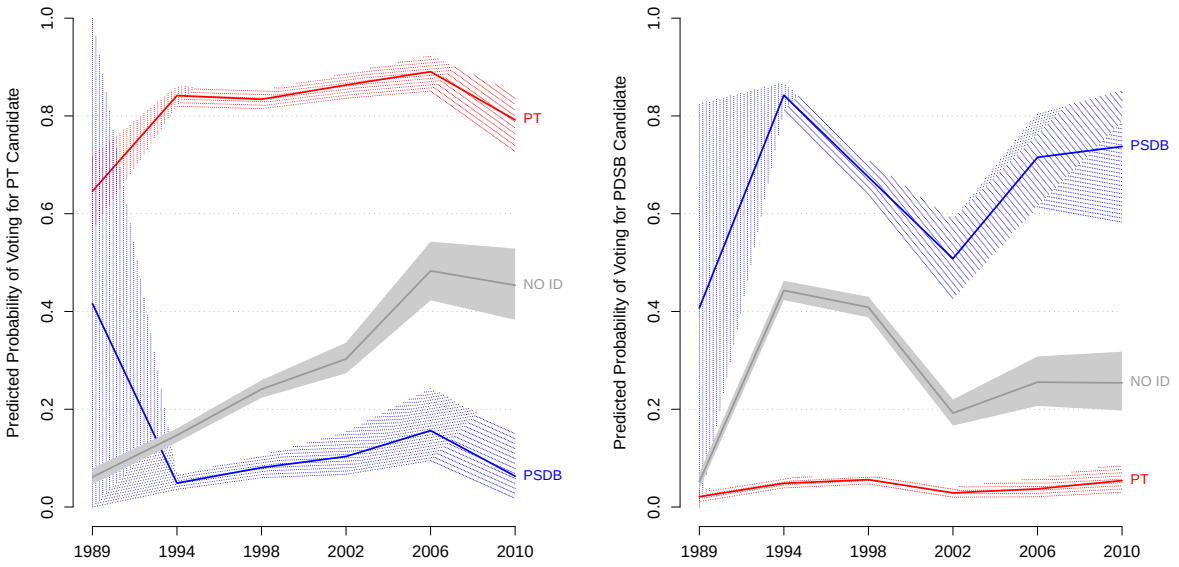


Figure A.1: Government Evaluations Given Party Identification

Figure shows the predicted probabilities of rating the government as good or very good given that one self-identifies with the president's party, with the main opposition party, or with some other or no party. Data are from Datafolha surveys spanning a period of twenty years. Probabilities were estimated using a logit regression of positive evaluation on party ID, income, age, sex and type of municipality. Predicted probabilities were obtained by holding other variables at their modal category. The PT was considered the main opposition party prior to 2002, and the PSDB afterwards. There are not enough identifiers with the president's party in 1991 and 1992 to estimate these probabilities. 95% confidence intervals are shown.



(a) Vote for PT Candidates

(b) Vote for PSDB Candidates

Figure A.2: Probability of Voting for PT and PSDB Candidates Given Party ID

Figures show the predicted probabilities of voting for the party that one self identifies with in presidential elections, using a series of Data Folha surveys spanning all six presidential elections since redemocratization. Probabilities were estimated using a multinomial logit regression of voting intention on party ID, income, age, sex and type of municipality. Dependent variable was a three category vote intention variable in the 1st round of each presidential election (PT candidate, PSDB candidate, and other candidates). The main independent variable was party id, coded as PT, PSDB, PMDB, other, no party. Predicted probabilities were computing by setting other variables to their modal category. Behavior of PMDB and "other" identifiers is almost identical to that of non-identifiers, and not shown for simplicity. 95% confidence intervals are shown.

and considerably less likely to vote for PSDB candidates. The same applies to identifiers with the PSDB (Figure 2(b)). Identifiers of other parties are not shown in these figures, but behave almost exactly like those with no party identification.

These differences are non-trivial and suggest that party ID in Brazil shapes voters' actions and perceptions of the political world just as the conventional definition of partisanship suggests it should. However, these results should be taken as evidence of an "association" between party ID and patterns of behavior, because the data and research design do not allow us to identify causal relationships. More specifically, our analysis ignores both potential endogeneity between partisanship and the behavior that we observe and potential reporting biases that might lead respondents to project consistency in their answers. To eliminate such problems, which bedevil observational evidence, we adopted the experimental research designs in the paper.

Randomization of Treatment Assignment

Random assignment of treatment is the most crucial aspect of experimental manipulations. In both samples, we randomized the assignment of treatment ex-ante. In the BEPS sample, we randomized application of the treatment within each sector that was sampled in the survey. However, it does not suffice that the treatment be randomly dispensed, it is important that the randomization balance at least over observable pre-treatment variables.

Balance, is the property of a sample, and not that of a hypothetical population . Hence, there is no absolute threshold or test to determine whether a sample is balanced, and t-statistics have no relevance the assessment (Ho, Imai, King & Stuart 2007) even though they are commonly used in experimental literature.. Furthermore, balance on causally important variables is more important than on marginal variables.

With no obvious way to assess balance, we provide several balance statistics for our experimental samples (Sekhon 2011). The first two columns in Table A.1 report the means of each variable in the control and treatment groups.[1] The third column reports the standardized difference (using the pooled variance), followed by a variance ratio of treatment over control, a difference of means T-test, the Kolmogorov-Smirnov test for non-categorical variables, and its bootstrapped p-values.

A well balanced sampled would also show a variance ratio of 1 and insignificant difference of means and Kolmogorov-Smirnov tests. Differences between the mean values in our internet sample a generally small, though the treatment group contains more respondents in the 2–5 minimum wage bracket (and less in the 5-8mw bracket) than the control group, as well as respondents that score slightly higher in the activism index. In the BEPS sample too there were more people in the 2-5 minimum wage bracket, but at the expense of the lowest (below 2 mw) bracket. Otherwise, balance was very good.

We also performed the Hansen & Bowers (2008) omnibus test, which provides a single statistic for assessing balance. Despite the slight imbalances reported above, this omnibus test produced a p-value of 0.297 (d^2=17.38, df=15) for the online sample, indicating that treatment and control groups are statistically indistinguishable from each other. The result was likewise positive for the BEPS sample (d^2=18.15, df=16, p-value=0.315).

[1] For our categorical variables, a baseline category is omitted from the table.

Table A.1: Balance on Selected Variables Between Control and Pooled Treatment Groups

(a) Online Sample

	Treated	Control	St. Diff.	Var Ratio	T-Test	KS	KS p-value
Income							
02-05 SM	0.34	0.30	1.47	1.08	0.03		
05-08 SM	0.15	0.19	-1.60	0.81	0.01		
08+ SM	0.25	0.25	-0.07	1.00	0.92		
Region							
N	0.05	0.04	0.62	1.21	0.36		
NE	0.23	0.25	-0.50	0.96	0.43		
S	0.20	0.21	-0.22	0.98	0.74		
SE	0.42	0.41	0.29	1.01	0.65		
Gender							
0.66	0.63	0.60	0.98	0.35			
Age	32.30	31.80	0.54	1.09	0.41	0.03	0.47
Activism	3.32	3.17	1.16	1.04	0.08	0.05	0.09

(b) BEPS

	Treated	Control	St. Diff.	Var Ratio	T-Test	KS	KS p-value
Income							
02-05 SM	0.37	0.31	1.78	1.09	0.03		
05-08 SM	0.09	0.09	0.12	1.02	0.89		
08+ SM	0.03	0.05	-1.86	0.53	0.03		
Region							
N	0.15	0.15	-0.12	0.98	0.89		
NE	0.24	0.25	-0.14	0.99	0.87		
S	0.19	0.21	-0.60	0.94	0.48		
SE	0.28	0.25	0.69	1.05	0.41		
Gender							
Male	0.46	0.46	0.04	1.00	0.97		
Vote in 2010							
Rousseff	0.56	0.54	0.46	0.99	0.59		
Serra	0.32	0.32	0.01	1.00	0.99		
City Size							
Large	0.26	0.27	-0.04	1.00	0.96		
Medium	0.11	0.11	0.24	1.04	0.77		
Small	0.28	0.26	0.51	1.04	0.54		
Age	41.51	41.31	0.18	0.94	0.83	0.03	0.79
Knowledge	1.69	1.82	-1.30	1.00	0.12	0.04	0.22

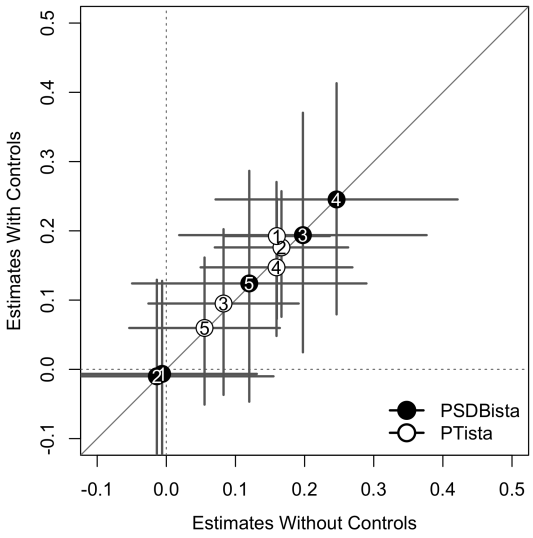


Figure A.3: Treatment Effects With and Without Controls

Figure shows effects, and 95% confidence intervals, of receiving 'some' treatment on agreement of PT and PSDB identifiers with their party, estimated with and without controls. Controls included age, gender, and a political activism/interest index. Numbers indicate the experimental items as follows: 1-Minimum Wage; 2-Investment Protection; 3-Gvt. Financing of Pvt. Companies; 4-Offshore Oil; and 5 Venezuela in Mercosul.

Robustness of Results to Controls

Given than randomization worked well, it is not surprising that estimates of effects hardly change with the inclusion of individual level controls. Figure A.3 reports estimates of treatment effects obtained with and without the inclusion of controls. Estimates without controls, which are reported in the main paper, were obtained through simple linear probability models. Estimates with controls were obtained from logit regressions. Including controls reduces marginally the confidence intervals, some some precision is lost by dropping cases with missing values on the control variables. Most of very small differences reported in the Figure are due, in fact, to slightly different samples due to to this missingness. For simplicity, we report estimate of effects for receiving "some treatment" only, on partisans of both the PT and the PSDB.

Internal Validity Checks for The Online Study

Our results from the online survey demonstrate that party cues can shape voter attitudes even in a relatively young and notoriously fragmented party system like Brazil's. These effects come from both in-group and out-group cueing, suggesting both that PT and PSDB partisanship constitute forms of social identity, producing in-group cueing effects, and that the context of competition

between these two parties produces polarizing out-group cueing effects.

Respondents were randomly assigned to treatments, our manipulations were trivial, and results are statistically significant. However, we gave respondents no incentives to speak their true opinions on the issues. Consequently, consistency bias, social adequacy, or anticipation of what researchers consider a "correct" answer could have driven their answers to the experimental questions.[2]

We do not think respondents could have guessed our intentions (McDermott 2011). We purposefully did not mention the word politics in our ads, and we did not provide any indication of the type of questions that would be asked. However, to identify consistency bias in response patterns—which would call into question the internal validity of our argument—we randomized the placement of the party identification question in the online survey. Half of respondents answered the party ID question before the experimental questions, while the other half answered it after.

Consistency bias could go in either direction: respondents could misrepresent their opinions on the issues to be consistent with their previously stated party preference, or vice-versa. That is, respondents might sincerely answer the party ID question but then project consistency once the policy issues came up. Likewise, faced with the policy questions first, respondents could then project consistency by saying that they identify with a party whose positions they agreed with the most. However, this latter form of consistency bias is unlikely—respondents who had not yet been asked their partisan preference have no ex ante incentive to misrepresent their positions on the issues.

Moreover, this second form of consistency bias would be difficult to actually do, for at least three reasons. First, respondents would have had to keep track of their answers to five questions. Second, more importantly, it is reasonable to assume that respondents are more likely to misrepresent their opinion on arcane issues than misrepresent their party identification. Misrepresenting one's party preference is a stronger lie than misrepresenting one's opinion on relatively low-salience policies. Third, given Brazil's highly fragmented system, respondents could simply claim to sympathize with a different party that shares the same opinions as their preferred party but which was not mentioned in the survey.

In any case, if our assumption about respondents being more likely to misrepresent their opinions on issues than misrepresent their party ID is correct, we should be able to spot consistency bias by comparing the effects of cues on respondents who answered the party ID question before or after the experimental questions. If consistency bias is a problem, then the effects of cues on re-

[2]These are potential violations of the "dominance" condition required by induced-value theory. See, for instance, the discussion in Morton & Williams (2010, p.362)

spondents who answered the party identification question before the experimental questions should be larger than the effects of cues on respondents who answered it after the experimental questions.

We find no evidence that this is the case. Table A.2 presents the difference between the (absolute) effects of cues on the "early" and "late" groups. Positive values indicate larger effects in the late group, while negative values suggest larger effects in the early group—and would be compatible with strategic misrepresentation of preferences. There is no evidence of strategic manipulation of answers, bolstering our claim that cueing effects are real. Respondents are not projecting consistency by strategically choosing policies backed by the party they support.

Table A.2: Differences in Effects of Pooled Cues Given Placement of Party ID Question

	Effect Late − Effect Early	
	PSDBistas	PTistas
1 Minimum Wage	-0.044	-0.119
2 Investment Protection	-0.190	0.151
3 Gvt. Financing Pvt. Companies	-0.003	0.038
4 Offshore Oil	0.017	-0.021
5 Venezuela in Mercosur	0.233	0.048
Average	0.003	0.019

Positive values indicate that effects are larger in the group that answered the party ID question later in the survey (after the experimental questions).

If respondents were projecting consistency by misrepresenting their party preference, we would also expect to see a larger number of identifiers with the PT and/or the PSDB amongst those that received the party ID question late after the experimental questions, simply because 3/4 of all respondents saw information about the PT and/or the PSDB position (all of those who fell into one of the three treatment conditions), but no respondents saw information about any other party's position.

Again this is not the case. Those who answered the party ID question *after* the experimental questions were very slighlty *less* likely to identify with either the PSDB or PT. The difference was -0.01 (SE=0.01) for all respondents. Even after excluding those in the control groups (i.e. those who saw no party cues in the experimental questions), the difference was also -0.01 (SE=0.02). Neither difference is statistically significant, and both are in the opposite direction of what would be expected if respondents were strategically misrepresenting their partisan affiliation after seeing the experimental questions. Overall, our robustness checks reinforce support for our main finding, that PT and PSDB cues have the predicted effects.

Survey-Experiment With College Students

As a first approach to a multi-item questionnaire, we conducted a simple survey experiment with a non-representative sample of college students. Subjects were recruited in 10 public and private universities in Rio de Janeiro and Niterói throughout June–August 2011. We administered a short pen-and-paper survey that included five party cueing experimental questions, whose wording was almost identical to the online experiment questions described in Table 2 of the paper. This survey was administered in the classroom, after hearing a standard script delivered by the researchers.

Conditions in the classroom were not ideal: the stakes involved were low, no incentives were provided, and the attention devoted to the task was not always high. These facts, however, should make it harder for us to find any effect of subtle variations in the wording of the five questions. The fact that we find any results at all gives us confidence that we were capturing a real underlying phenomenon, and lead to the better implemented online survey design.

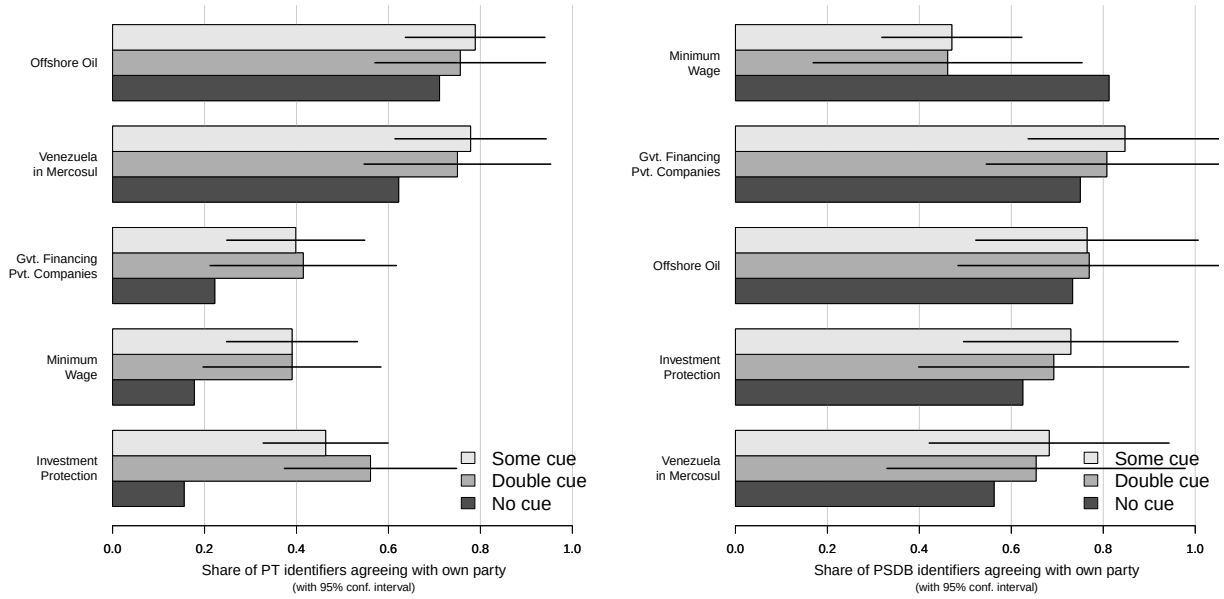
This sample of convenience is very different from the overall Brazilian population. Not only are subjects considerably wealthier and younger than average, but party identification with both of the two main parties is lower, and identification with the PSOL and the PV is considerably higher than in the overall population, and identification with the PT and the PSDB lower, which led to small N problems, as described in Table A.3).

Table A.3: Size of Sample of Interest by Treatment Category

	National Sample		College Students			
	Control	DBL Cue	Control	PT Cue	PSDB Cue	DBL Cue
PTistas	169	127	45	39	43	41
PSDBistas	37	30	16	22	37	26
Other	64	68	104	98	87	106
Non-Partisan	355	371	278	270	228	236
Total	625	596	451	438	401	412

We randomized treatment before distributing the questionnaires in each classroom, therefore, we did not randomize conditionally on party identification. We found good balance between the treatment condition with respect to several important covariates.

Figures A.4 is equivalent to Figure 3 in the paper. As in those results, the effects of cues was clearer on PT identifiers, but these respondents also showed lower levels of baseline support for their party's position. In general, we see that whenever this baseline support was lower, the cues produced larger effects, suggesting that ceiling effects might limit our capacity to observe treatment



(a) PT Partisans

(b) PSDB Partisans

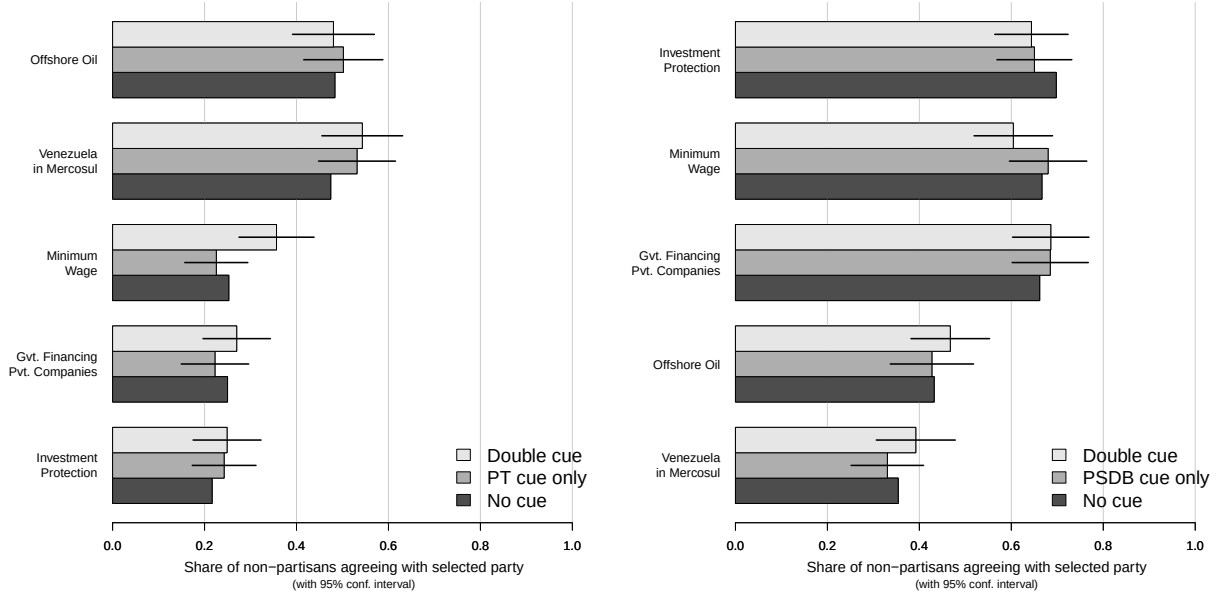
Figure A.4: Effects of Party Cues on Partisans of the PT and the PSDB

Figures shows levels of agreement between partisans and their own party, as well as effects of cues, for partisans of the the PT and the PSDB. Items are ordered by baseline level of agreement with each party, so the ordering differs across panels. 95% bootstrapped confidence intervals are also shown.

effects.

Cueing effects were always in the correct direction for both parties, with the sole exception of the minimum wage question in the case of PSDB supporters. Overall, all the patterns observed in the PSDB are compatible with a problem of lack of statistical power. With high baseline agreement and very small numbers, the deck was stacked against finding any statistically significant result.

Results for non-partisans are shown in Figure A.5 (equivalent to Figure 5), and corroborate the finding in the paper. Non-partisans are rarely ever affected by cues, and do not react more to PT cues than to PSDB cues.



(a) Agreement with the PT

(b) Agreement with the PSDB

Figure A.5: Effects of Party Cues on Non-Partisans

Figure shows effects of receiving a single- or double-cue on the agreement of non-partisans with the position of the two main parties (PT on the left, and PSDB on the right panel). Items are ordered by baseline level of agreement with each party, so the ordering differs across panels. 95% bootstrapped confidence intervals are also shown.

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