

Online Appendix for “When Do Voters Sanction Corrupt Politicians?”

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A1 Country Information

We fielded candidate choice conjoint experiments embedded in nationally representative surveys in three Southern Cone countries: Argentina, Chile, and Uruguay ($N = 1,528$, $1,625$, and $1,514$, respectively). All three surveys were fielded as part of LAPOP's AmericasBarometer. The AmericasBarometer conducts high quality, nationally representative public opinion surveys across the Americas every two years. Interviews are conducted face-to-face, and all interviews are audited extensively to ensure quality. Importantly for our purposes, the interviews were conducted using electronic questionnaires, which allowed us to ensure complete randomization. Further information on sampling procedures, data collection, and response rates is available at www.vanderbilt.edu/lapop/.

We focus on the three Southern Cone countries because they offer useful contextual variation. On the one hand, all three have similar political systems and sociodemographic characteristics: presidential multiparty systems, middle-income levels of wealth, comparatively high rates of economic inequality, ethnically homogeneous societies, and populations with similar demographics (for the latter, see Table A1). These similarities allowed us to use very similar candidate vignettes across the three countries without having to explicate a number of institutional or contextual factors.

On the other hand, these three countries vary on two relevant dimensions for the study of corruption voting (and its moderators): corruption, and party system strength. This variation helps us evaluate how generalizable our findings are. That we find similar results across these different settings (see Section A4) increases our confidence that those results are not just unique to one type of context (Slater and Ziblatt, 2013). In terms of corruption, as a result of several high-profile corruption scandals in 2016, nearly 73% of Chileans think that more than half of the country's politicians are involved in corruption. A similar 66% of Argentines think that corruption is widespread in their country, doubtless partly the result of persistent corruption allegations against the former president and members of her administration. On the other hand, only 40% of Uruguayans think that corruption is widespread in their country, a rate that is lower than in the United States. Corruption victimization, on the other hand, is low in both Chile and Uruguay (and 7% and 6%, respectively), but higher (16%) in Argentina, which is just below the regional average.¹ These perceptions and experiences with corruption are summarized in Table A1.

In terms of party strength, Argentina has a fragmented party system with low levels of mass partisanship (Lupu, 2016). Two parties currently anchor electoral politics, but they are both also deeply internally divided and unstable. Competing factions regularly present alternative candidates or lists in elections. Chile's party system is similarly anchored by two large coalitions, but the two coalitions are also fraying and voters' attachments to these historical brands has

¹ Citizen perceptions are based on an item in the AmericasBarometer (asked before the experiment) measuring corruption perceptions; see Section A5 for details. Citizen experiences of corruption are based on a series of items in the survey that asked respondents whether they had been asked to pay a bribe by a number of different public officials (e.g., police officers, public hospital staff, municipal bureaucrats, etc.). We list the proportion who reported having been asked to pay a bribe by any one of these officials. For comparison, note that in 2016, Transparency International's Corruptions Perceptions Index ranked Uruguay at 21, Chile at 24, and Argentina at 95 out of 176 countries.

Table A1: Summary statistics

	All	Argentina	Chile	Uruguay
Female	0.51	0.50	0.51	0.52
Age	43.53	41.90	42.18	46.51
Less than high school	0.79	0.78	0.78	0.81
Wealth (in bottom 3 quintiles)	0.60	0.59	0.61	0.59
Unemployment	0.13	0.09	0.16	0.14
Bribes justifiable	0.12	0.10	0.17	0.10
High corruption perception	0.60	0.66	0.72	0.40
Very high corruption perception	0.22	0.26	0.29	0.10
Bribe paid	0.10	0.16	0.07	0.06
Observations	4,667	1,528	1,625	1,514

Sample summary statistics for respondents' sociodemographic characteristics, as well as corruption perceptions, attitudes, and experiences. Except respondent age, all the variables are binary. The averages incorporate population weights developed by LAPOP. "High corruption perception" shows the proportion of respondents who said that "more than half" or "all" politicians are corrupt (see Section A5 for details). "Very high corruption perception" only uses the latter category ("all politicians are corrupt").

eroded dramatically in recent years (Luna and Altman, 2011). Contrasting these two party systems, Uruguay's has been more stable in recent years, and mass attachments to the parties are more widespread (Buquet and Piñeiro, 2014; Lupu, 2015).

A2 Experimental Details

Vignette Text and Treatment Components

In our candidate choice experiments, we presented survey respondents with a short vignette about two hypothetical mayoral candidates, an incumbent and a challenger, running in a local election. As mentioned in the text, we randomly varied six characteristics of the candidates and the electoral environment:

- **Gender:** female or male.
- **Party affiliation:** left party, right party, or independent. The parties were as follows: in Argentina, *Partido Justicialista* (left) and *Propuesta Republicana* (right); in Chile, *Nueva Mayoría* (left) and *Chile Vamos* (right). (Technically, these are coalitions, not parties, but all non-independent candidates run as members of one of these coalitions, so voters are accustomed to seeing these kinds of affiliations); in Uruguay, *Frente Amplio* (left) and *Partido Nacional* (right).

- **Corruption record:** accused of taking bribes or praised for efforts to stamp out bribery in their administration.
- **Information source for the corruption allegation:** left-leaning or right-leaning newspaper, or judicial officials. The newspapers were as follows: in Argentina, *Página 12* (left) and *La Nación* (right); in Chile, *La Nación* (left) and *El Mercurio* (right); in Uruguay, *El País* (left) and *La República* (right).
- **Mitigating corruption factor:** corruption prevalence or the creation of construction jobs; applicable only when a candidate is accused of corruption.
- **State of the economy:** improved or worsened since the last election; applicable only to the incumbent.

The original Spanish and translated English texts of the vignette presented to respondents are provided below. The random variables and their values are enclosed in square brackets. Angle brackets represent placeholders for proper nouns that varied by country.

Spanish version:

Imagine que usted está votando en una elección para intendente con dos candidatos. Las condiciones económicas del departamento han [mejorado/ empeorado] desde la última elección.

[María/Alberto] López es [la/el] actual intendente [del <left party> / del <right party> / y es independiente] que se presenta a la re-elección. [El diario <left newspaper> / El diario <right newspaper> / La justicia] [ha alabado los esfuerzos de López por castigar a los empleados públicos que acepten coimas a cambio de concesiones públicas durante su mandato / ha acusado a López de aceptar coimas a cambio de concesiones públicas durante su mandato / ha acusado a López de aceptar coimas a cambio de concesiones públicas durante su mandato, una práctica que entonces era común en todo el departamento / ha acusado a López de aceptar coimas a cambio de concesiones públicas, pero algunos sugieren que esta práctica también trajo empleo en construcción al departamento].

[La otra candidata es Isabel/ El otro candidato es Juan] Arias [del <left party> / del <right party> / y es independiente]. Arias había sido intendente del departamento antes de que López asumiera. [El diario <left newspaper> / El diario <right newspaper> / La justicia] [ha alabado los esfuerzos de Arias por castigar a los empleados públicos que acepten coimas a cambio de concesiones públicas durante su mandato / ha acusado a Arias de aceptar coimas a cambio de concesiones públicas durante su mandato / ha acusado a Arias de aceptar coimas a cambio de concesiones públicas durante su mandato, una práctica que entonces era común en todo el departamento / ha acusado a Arias de aceptar coimas a cambio de concesiones públicas durante su mandato, pero algunos sugieren que esa práctica también trajo empleo en construcción al departamento].

English translation:

Imagine that you are voting in an election for mayor with two candidates. The economic conditions of the municipality have [improved/worsened] since the last election.

[María/Alberto] López is the incumbent [⟨left party⟩ / ⟨right party⟩ / independent] mayor running for reelection. [The newspaper ⟨left newspaper⟩ / The newspaper ⟨right newspaper⟩ / Judicial officials] [praised López's efforts to punish public employees accepting bribes in exchange for public concessions / accused López of accepting bribes in exchange for public concessions during [her/his] term / accused López of accepting bribes in exchange for public concessions during [her/his] term, a practice that was then common throughout the province / accused López of accepting bribes in exchange for public concessions during [her/his] term, but some suggest that this practice brought construction jobs to the municipality].

The other candidate is [Isabel/Juan] Arias from [⟨left party⟩ / ⟨right party⟩ / independent].² Arias had been the mayor of the municipality before López took office. [The newspaper ⟨left newspaper⟩ / The newspaper ⟨right newspaper⟩ / Judicial officials] [praised Arias's efforts to punish public employees accepting bribes in exchange for public concessions / accused Arias of accepting bribes in exchange for public concessions during [her/his] term / accused Arias of accepting bribes in exchange for public concessions during [her/his] term, a practice that was then common throughout the province / accused Arias of accepting bribes in exchange for public concessions during [her/his] term, but some suggest that this practice brought construction jobs to the municipality].

Taken together, these six characteristics of the candidates and the context cover a wide range of factors that have been identified to shape the effect of corruption on voting. By randomizing such a rich set of relevant features, we could ensure that our respondents were not conflating different factors in a way that may obscure our treatment effects of interest. For example, our design ensures that when hearing about corruption bringing jobs, our respondents were not also inferring a particular state of the economy, a particular party affiliation of a candidate (e.g. a left party focused on creating jobs by any means), or a particular source of the allegation (e.g. a partisan-friendly newspaper).

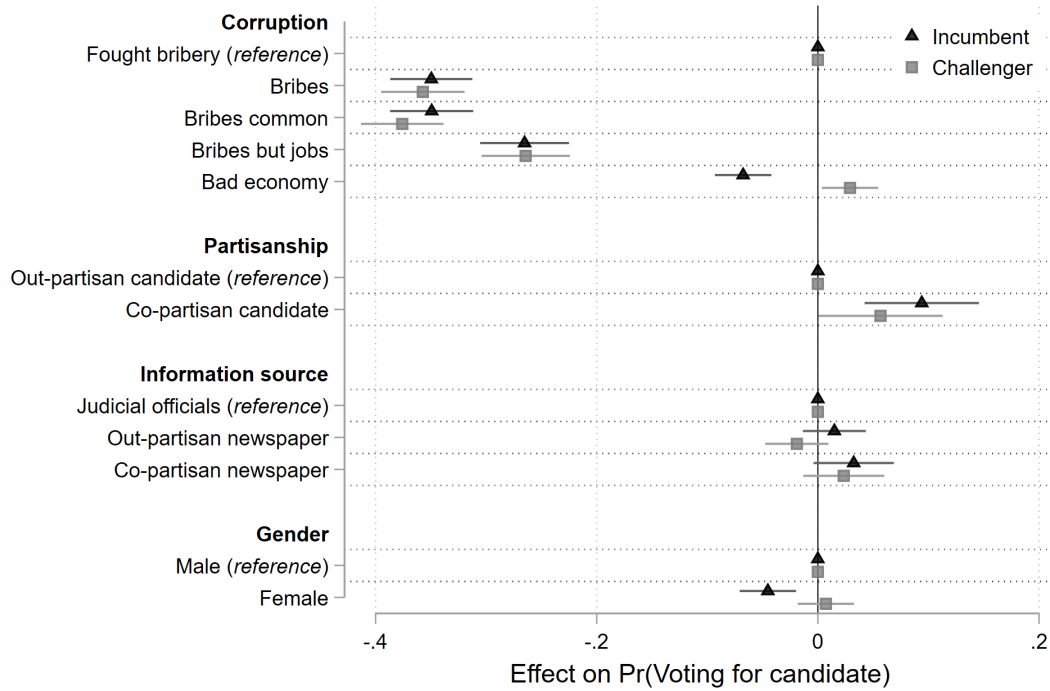
Another advantage of the conjoint design relative to many simpler survey-experimental designs is that its richness should make it more difficult for respondents to guess the subject of researchers' investigations, and therefore less likely to give socially desirable answers (i.e. refrain from endorsing potentially controversial attributes such as corruption) (Hainmueller et al., 2014). Moreover, we showed to respondents only one candidate pair (as opposed to multiple paired comparisons commonly shown in other conjoint studies), eliminating the possibility of respondents learning across the vignettes about what the objective of the experiment was.

² In Chile, the last name Arias is less common, so we used Soto.

Validity Tests

Following Hainmueller et al. (2014), we conducted several diagnostic checks on the implementation of our conjoint experiment. To check for *profile order effects*, we reran our analysis separately for the incumbent and the challenger profile, given that the incumbent profile was always shown first. As seen in Figure A1, only the negative effect of candidate gender may in part be an artifact of profile order. Since the information about the economy only referred to the incumbent, it is unsurprising that the economy treatment yields different effects for incumbents and challengers. A worsening economy decreases support for the incumbent candidate; it also increases support for the challenger (to a smaller degree than it hurts incumbents), indicating that the economy overall is a non-negligible factor driving the respondents' paired choices.

Figure A1: Diagnostic check for profile order effects



Estimates are shown separately for incumbent and challenger profiles. The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

Given that the economy (by construction) and candidate gender treatments exhibit order effects, Table A2 verifies that our experimental results are unchanged when we include the interactions between the economy treatment or the gender treatment with all the other treatments (columns 2 and 3, respectively; column 1 shows the results from the baseline specification as a benchmark).³

³ Table A2 also shows that corruption sanctioning in our experiment is not strongly moderated by economic perfor-

Table A2: Results with economy and gender interactions

	Main model		Interactions w/ economy		Interactions w/ female	
Bribes	-0.35***	(0.01)	-0.36***	(0.02)	-0.34***	(0.02)
Bribes common	-0.36***	(0.01)	-0.37***	(0.02)	-0.35***	(0.02)
Bribes but jobs	-0.26***	(0.01)	-0.27***	(0.02)	-0.24***	(0.02)
Co-partisan candidate	0.08***	(0.02)	0.09***	(0.03)	0.09***	(0.03)
Economy worsened	0.03**	(0.01)	-0.03	(0.03)	0.03*	(0.02)
Incumbent profile	0.05***	(0.02)	0.05***	(0.02)	0.09***	(0.02)
Economy worsened \times Incumbent profile	-0.10***	(0.02)	-0.10***	(0.02)	-0.12***	(0.03)
Out-partisan newspaper	-0.00	(0.01)	-0.02	(0.02)	-0.02	(0.01)
Co-partisan newspaper	0.03**	(0.01)	-0.02	(0.02)	0.01	(0.02)
Female	-0.02**	(0.01)	-0.03**	(0.01)	0.02	(0.03)
Bribes \times Economy worsened			0.01	(0.03)		
Bribes common \times Economy worsened			0.01	(0.03)		
Bribes but jobs \times Economy worsened			0.02	(0.03)		
Co-partisan candidate \times Economy worsened			-0.03	(0.04)		
Out-partisan newspaper \times Economy worsened			0.03	(0.02)		
Co-partisan newspaper \times Economy worsened			0.09***	(0.03)		
Female \times Economy worsened			0.02	(0.02)	-0.00	(0.03)
Chile \times Economy worsened			0.01	(0.02)		
Uruguay \times Economy worsened			0.02	(0.02)		
Bribes \times Female					-0.04	(0.03)
Bribes common \times Female					-0.04	(0.03)
Bribes but jobs \times Female					-0.04	(0.03)
Co-partisan candidate \times Female					-0.03	(0.04)
Incumbent profile \times Female					-0.07***	(0.03)
Economy worsened \times Incumbent profile \times Female					0.04	(0.04)
Out-partisan newspaper \times Female					0.03	(0.02)
Co-partisan newspaper \times Female					0.03	(0.03)
Chile \times Female					0.02	(0.02)
Uruguay \times Female					-0.03	(0.02)
Constant	0.52***	(0.02)	0.55***	(0.02)	0.50***	(0.02)
Observations	8668		8668		8668	

The first column shows the results from our baseline specification. Columns 2 and 3 add interactions of all the treatments with the economy treatment and the candidate gender treatment, respectively. Standard errors are clustered by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

We also verified that the *random assignment* of vignette characteristics was successful in Table A3, by finding predominantly null results when regressing the predetermined respondent characteristics (gender, age, and years of education) on the randomized vignette characteristics they received.

Some of the other diagnostic checks recommended in Hainmueller et al. (2014) were not applicable to our research design. Our study could not exhibit *carryover effects*, since our experiments presented each respondent with only one pair of candidates rather than multiple pairs in succession (as in Hainmueller et al., 2014). Also, we could not test for attribute order effects since our experiments used a pair of fixed-format candidate profiles, not listing attributes in random order. However, we do not expect attribute order effects to bias our results, since respondents had to read through all of the attributes for each of the candidate profiles.

A3 Full Tabular Results

Table A4 shows the full tabular results underlying the estimates shown in Figures 1, 2 and 3 in the text (coefficients on country dummies are omitted as they are not of substantive interest).⁴

A4 Additional Results

This section contains a number of additional results briefly referenced in the text.

Table A5 shows that our key results, obtained with an OLS regression model, are substantively identical if we use a logit model (given that our outcome variable is binary).

Since our conjoint design includes a relatively large number of treatments, our analysis involves multiple tests. To check that our inferences about statistical significance are not an artifact of repeated tests, we show in Table A6 that the statistical interpretation remains the same for all treatment effects except gender, even after accounting for the false discovery rate (based on the procedure outlined in Benjamini and Hochberg 1995).

Figure A2 replicates Figure 1 from the paper, but disaggregates the results by country. In general, the treatment effects are reasonably similar across countries, the most notable difference

mance, in contrast to some prominent findings with observational data (Anderson and Tverdova, 2003; Zechmeister and Zizumbo-Colunga, 2013). One concern is that this is an artifact of our research design involving a hypothetical scenario. We think this is unlikely for two reasons. First, several other studies utilize hypothetical vignettes and find that corruption sanctioning *is* strongly moderated by economic factors (Breitenstein, 2019; Klačnja and Tucker, 2013). We thus believe that the absence of such moderating effects in our study is unlikely to be driven by the research design choice, and more likely to be driven by contextual differences relative to those other studies (conducted in different countries than those in our sample). Second, our study does find other moderating effects, most notably of the provision of corrupt side benefits. If the hypothetical nature of our experiment makes moderating effects unlikely, than we would not expect to find this moderating effect either.

⁴ While the economy treatment technically only applies to the incumbent, it was shown outside of either candidate's profile, at the beginning of the vignette (see the vignette text in Section A2). Unless we are examining the effect of the economy, we assign the same value of the economy treatment to both candidate observations for each respondent, which allows us to use the entire dataset to estimate the other treatment effects. In the estimation model, we include an interaction between the incumbent profile and the economy treatment dummy.

Table A3: Randomization checks

	Gender	Age	Education
Bribes	0.01 (0.01)	0.54 (0.51)	0.15 (0.11)
Bribes common	0.00 (0.01)	0.20 (0.51)	0.11 (0.11)
Bribes but jobs	0.02 (0.01)	0.46 (0.52)	-0.07 (0.11)
Co-partisan candidate	-0.03 (0.02)	4.81*** (0.87)	0.24 (0.18)
Economy worsened	-0.01 (0.01)	0.66 (0.50)	-0.08 (0.11)
Incumbent profile	0.00 (0.00)	-0.05 (0.03)	-0.01 (0.00)
Economy worsened \times Incumbent profile	0.00 (0.00)	-0.03 (0.05)	-0.00 (0.01)
Out-partisan newspaper	0.02 (0.01)	0.12 (0.41)	-0.17* (0.09)
Co-partisan newspaper	-0.02 (0.02)	0.67 (0.52)	-0.11 (0.11)
Female	0.03*** (0.01)	-0.23 (0.36)	-0.07 (0.08)
Observations	9328	9326	9300

Outcome variables are respondents' pre-treatment demographic characteristics, indicated in the column headers. Standard errors are clustered by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

Table A4: Coefficient Estimates for Figures 1-3

	Figure 1	Figure 2	Figure 2	Figure 3	Figure 3
Bribes	-0.35*** (0.01)	-0.35*** (0.01)	-0.37*** (0.01)	-0.46*** (0.03)	-0.41*** (0.02)
Bribes common	-0.36*** (0.01)	-0.36*** (0.01)	-0.37*** (0.01)	-0.49*** (0.03)	-0.43*** (0.02)
Bribes but jobs	-0.26*** (0.01)	-0.26*** (0.01)	-0.28*** (0.02)	-0.42*** (0.03)	-0.35*** (0.02)
Co-partisan candidate	0.08*** (0.02)	0.16*** (0.04)	0.08*** (0.02)	0.08*** (0.02)	0.08*** (0.02)
Economy worsened	0.03** (0.01)	0.03** (0.01)	0.03** (0.01)	0.03** (0.01)	0.03** (0.01)
Incumbent profile	0.05*** (0.02)	0.05*** (0.02)	0.05*** (0.02)	0.05*** (0.02)	0.05*** (0.02)
Economy worsened \times Incumbent profile	-0.10*** (0.02)	-0.10*** (0.02)	-0.10*** (0.02)	-0.10*** (0.02)	-0.10*** (0.02)
Out-partisan newspaper	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)
Co-partisan newspaper	0.03** (0.01)	0.03** (0.01)	0.02* (0.01)	0.03** (0.01)	0.03** (0.01)
Female	-0.02** (0.01)	-0.02** (0.01)	-0.02** (0.01)	-0.02** (0.01)	-0.02** (0.01)
Chile	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Uruguay	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Bribes \times Co-partisan candidate		-0.11** (0.06)			
Bribes common \times Co-partisan candidate		-0.07 (0.05)			
Bribes but jobs \times Co-partisan candidate		-0.13** (0.06)			
High tolerance			0.02 (0.03)		
Bribes \times High tolerance			0.08** (0.04)		
Bribes common \times High tolerance			0.00 (0.04)		
Bribes but jobs \times High tolerance			0.12*** (0.05)		
Low education				-0.12*** (0.02)	
Bribes \times Low education				0.13*** (0.03)	
Bribes common \times Low education				0.16*** (0.03)	
Bribes but jobs \times Low education				0.19*** (0.03)	
Low wealth					-0.11*** (0.02)
Bribes \times Low wealth					0.10*** (0.03)
Bribes common \times Low wealth					0.11*** (0.03)
Bribes but jobs \times Low wealth					0.15*** (0.03)
Constant	0.52*** (0.02)	0.52*** (0.02)	0.52*** (0.02)	0.62*** (0.03)	0.58*** (0.02)
Observations	8668	8668	8566	8668	8668

Column 1 shows the estimates underlying Figure 1; these results are also used for the top-most set of estimates in Figure 2; columns 2 and 3 show the results for the middle and bottom-most sets of estimates plotted in Figure 2; columns 4 and 5 show the results for top and bottom sets of estimates plotted in Figure 3, respectively. Standard errors are clustered by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

Table A5: Estimates with OLS and Logit

	OLS	Logit
Bribes	-0.353*** (0.01)	-0.353*** (0.01)
Bribes common	-0.363*** (0.01)	-0.363*** (0.01)
Bribes but jobs	-0.265*** (0.01)	-0.265*** (0.01)
Co-partisan candidate	0.078*** (0.02)	0.077*** (0.02)
Economy worsened	0.029** (0.01)	-0.019*** (0.01)
Incumbent profile	0.053*** (0.02)	0.052*** (0.02)
Economy worsened \times Incumbent profile	-0.097*** (0.02)	-0.068*** (0.01)
Out-partisan newspaper	-0.002 (0.01)	-0.001 (0.01)
Co-partisan newspaper	0.028** (0.01)	0.029** (0.01)
Female	-0.019** (0.01)	-0.019** (0.01)
Chile	-0.008 (0.01)	-0.008 (0.01)
Uruguay	0.002 (0.01)	0.002 (0.01)
Observations	8668	8668

Estimates in the first column are from on an ordinary least squares regression model based on our main specification. The second column shows the marginal effects from a logit model. Estimates are substantively identical, and we therefore use OLS estimates in the text and the rest of this appendix. Standard errors are clustered by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

Table A6: Multiple-Comparison Correction for Main Estimates

	Point Estimate	Original p -value	Sig. at $p < .05$	B-H p -value threshold	Sig. at B-H $p < .05$
Bribes	-0.353	0.000	1	0.004	1
Bribes common	-0.363	0.000	1	0.008	1
Bribes but jobs	-0.265	0.000	1	0.013	1
Co-partisan candidate	0.078	0.000	1	0.021	1
Worsened economy	0.029	0.025	1	0.029	1
Incumbent profile	0.053	0.001	1	0.025	1
Worsened economy \times incumbent	-0.097	0.000	1	0.017	1
Out-partisan source	-0.002	0.883	0	0.050	0
Co-partisan source	0.028	0.030	1	0.033	1
Female	-0.019	0.039	1	0.038	0

Results of the Benjamini-Hochberg multiple-comparison correction. The first column shows the point estimates for each coefficient from our main specification. The second and third columns show the original p -values and their significance at $p < .05$ (1=significant), respectively. The fourth column shows the false discovery rate-adjusted threshold for significance according to the Benjamini-Hochberg (“B-H”) procedure (Benjamini and Hochberg, 1995). This procedure orders the p -values of all the effects from lowest to highest, and designates as statistically significant only those p -values that satisfy the condition $p_k \leq \frac{k}{m} \alpha$, where k is the position in the order of each p -value, m is the number of effects, and $\alpha = .05$ is the target significance level. The final column shows the B-H corrected significance at $p < .05$.

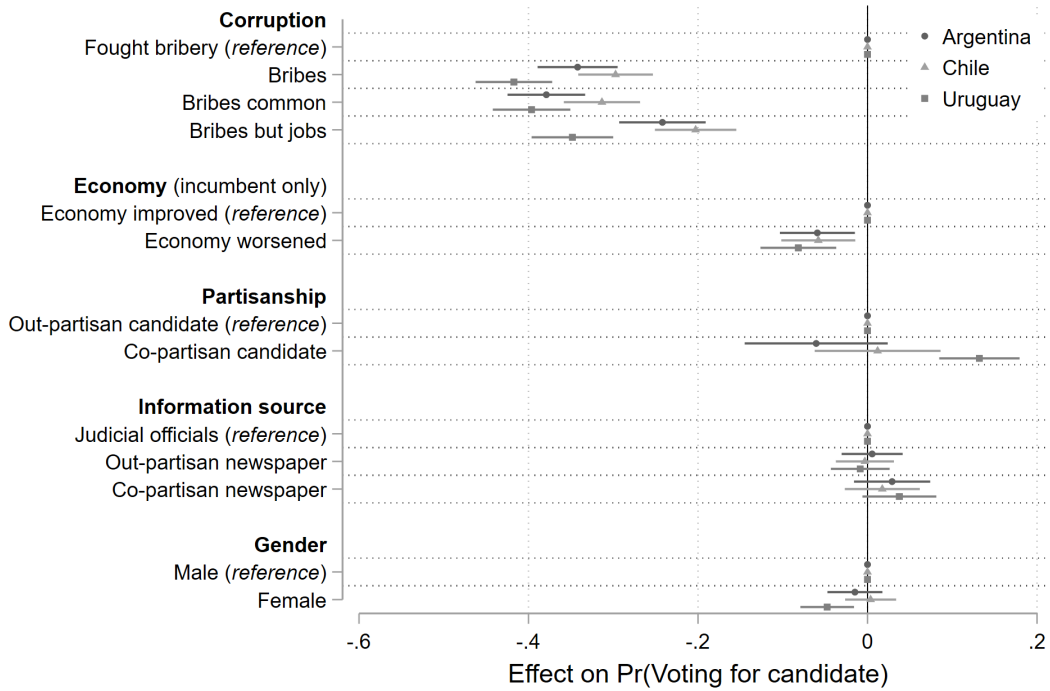
being in the positive effect on candidate support of their co-partisanship status, which is evident in Uruguay, but not in Argentina or Chile.

Table A7 zooms in to the corruption treatment effects by country. The first three columns show the estimates by country; the last three columns show the differences between country-level estimates (Argentina vs. Chile, Argentina vs. Uruguay, Chile vs. Uruguay). The top panel shows the average marginal effects of each treatment condition (bribes, bribes common, and bribes but jobs) relative to the control condition (fought bribery); the same quantities are shown at the top of Figure A2. The bottom panel shows the difference between the mitigating corruption treatments (bribes common and bribes but jobs) and the basic corruption treatment (bribes), all relative to the control condition (fought bribery).

There are two main takeaways from Table A7. First, the extent of corruption sanctioning is generally higher in Uruguay than in either Argentina or Chile, particularly for the basic corruption treatment and the ‘jobs’ mitigating treatment. Because of these differences, we included in our analyses the country dummies, so that our results are primarily driven by within-country variation in the outcome variable. The second observation from Table A7 is that despite the country differences in the absolute magnitude of corruption sanctioning, the last three columns in the lower panel indicate that the *relative* extent of mitigation due to corruption prevalence or side benefits is quite similar across countries. Overall, differences in the pretreatment environment in each country do not strongly influence the key patterns of our interest—the mitigation of corruption sanctioning.

A related question is whether, even within countries, respondents’ pretreatment perceptions of corruption and experiences with paying a bribe influence their reaction to our

Figure A2: Treatment Effects, by Country



The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

corruption treatments. Figure A3 compares the corruption treatment effects among respondents with high and low corruption perceptions; Figure A4 does the same among respondents who have and have not paid a bribe. Both figures show some, but noisy evidence that corruption sanctioning varies by these pretreatment characteristics.

In Figure A5, we further examine whether our corruption treatment effects vary by the randomized source of information (judicial officials, left-leaning or right-leaning newspapers). As in Figure 1 in the paper, instead of showing the results for the raw information attributes, we code the newspapers with respect to the respondents' own left-right leanings into a co-partisan and out-partisan newspaper source. Overall, bribe accusations against a candidate elicit corruption sanctioning of very similar magnitude irrespective of the information source.

In Figure 3 in the paper, we showed the heterogeneities in the mitigation in corruption sanctioning due to corrupt side benefits. In particular, this mitigation was somewhat higher among less educated and less wealthy respondents. In Figure A6, we do find similarly-sized, but imprecisely estimated, heterogeneities along two other dimensions of economic vulnerability: in economic downturns (top panel of Figure A6), and among unemployed (vs. employed) respondents (lower panel of Figure A6). These null effects, however, do indicate that the mitigating effect of corrupt side benefits is relatively broad.

While the treatment effect of the randomized state of the economy is statistically

Table A7: Corruption Treatment Effects, by Country

	By country			Diff. across countries		
	Argentina	Chile	Uruguay	Argentina vs. Chile	Argentina vs. Uruguay	Chile vs. Uruguay
Corruption treatments						
Bribes	-0.34*** (0.02)	-0.30*** (0.02)	-0.42*** (0.02)	-0.05 (0.03)	0.08** (0.03)	0.12*** (0.03)
Bribes common	-0.38*** (0.02)	-0.31*** (0.02)	-0.40*** (0.02)	-0.07** (0.03)	0.02 (0.03)	0.08** (0.03)
Bribes but jobs	-0.24*** (0.03)	-0.20*** (0.02)	-0.35*** (0.02)	-0.04 (0.04)	0.11*** (0.04)	0.15*** (0.03)
Diff. in corruption treatments						
Bribes common vs. bribes	-0.04* (0.02)	-0.02 (0.02)	0.02 (0.02)	-0.02 (0.03)	-0.06** (0.03)	-0.04 (0.03)
Bribes but jobs vs. bribes	0.10*** (0.02)	0.10*** (0.02)	0.07*** (0.02)	0.01 (0.03)	0.03 (0.03)	0.03 (0.03)

Standard errors are clustered by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

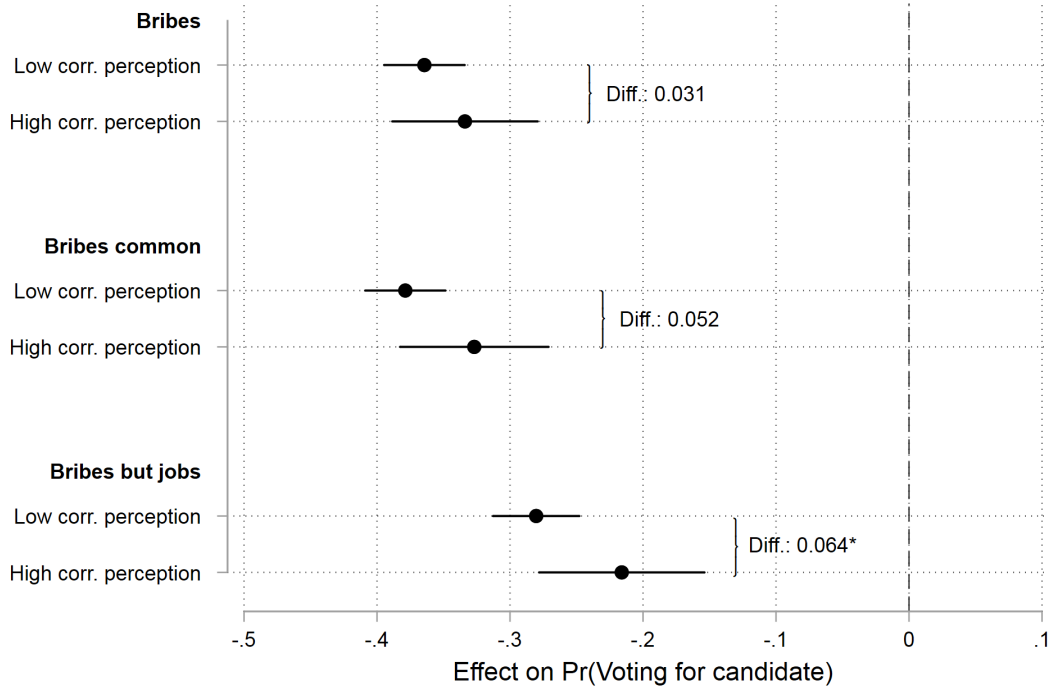
significant, it is relatively small in magnitude (see Figure 1 in the text). It may be that respondents in either (a) countries with a weaker actual economic performance, or (b) who perceive the economy to have worsened, did not find our ‘improving economy’ treatment condition credible. To evaluate these possibilities, Figure A7 examines the variation in the economy treatment effect (the baseline category is the improving economy and the effect is for the worsening economy; we show the effect on the vote for the incumbent profile). At the top, we compare the pooled estimate to the effect disaggregated by country. Below that, we compare the effect for respondents who perceived the economy to have worsened to those who did not, both pooled and by country.⁵ Overall, the figure reveals little variation in the treatment effect, implying that respondents in different countries and with different economic perceptions reacted similarly to the randomized state of the economy.

Our analyses employ a regression-based approach to estimating causal quantities in conjoint experiments, as outlined by Hainmueller et al. (2014). An alternative approach, especially applicable to estimating causal interactions in conjoint experiments, is outlined by Egami and Imai (2019). This approach is most clearly beneficial when the conjoint experiment includes several attributes with both a large number of levels and no natural baseline category; for example, when the attributes include a profile’s occupation or policy platform. However, in our experiment, all the attributes have a natural baseline category, and none of our attributes has a large number of values.

Nonetheless, in Figure A8 we compare the results from the conventional regression-based approach (in the left panel) and the Egami and Imai’s (2019) ANOVA-based approach (the right

⁵ For economic perceptions, we use the LAPOP item SOCT2, reproduced in Section A5 below.

Figure A3: Corruption Treatment Effects, by Respondents' Corruption Perception



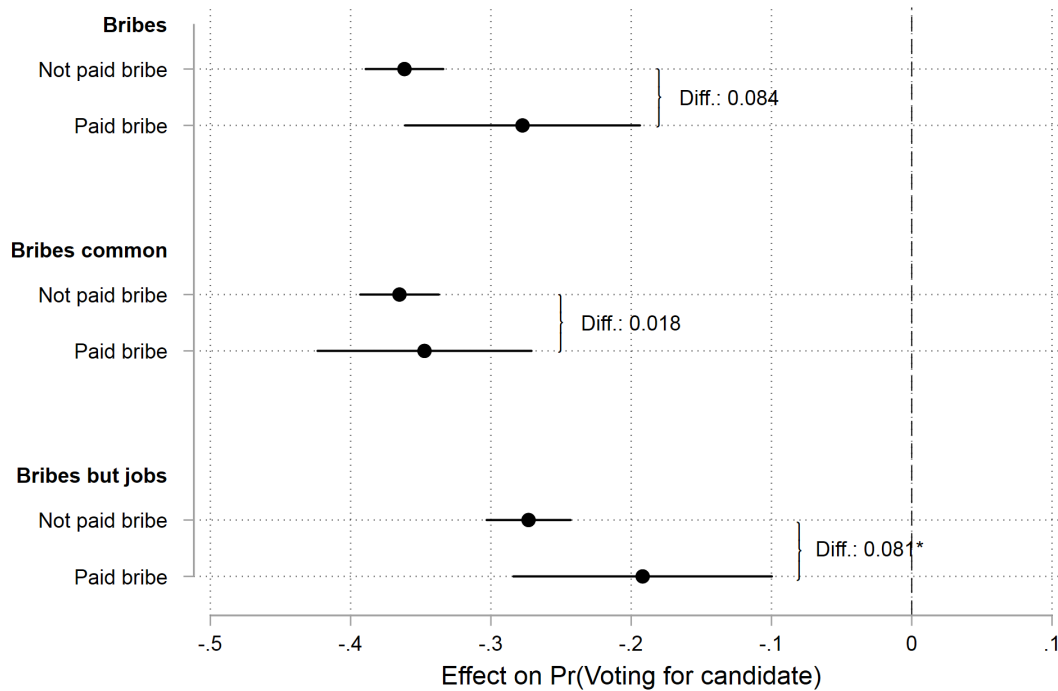
The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent. Brackets list the difference between effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

panel) for one of the treatment-by-treatment causal interactions of interest—between the candidate's partisanship and the corruption treatments (which we evaluated in Figure 2 in the paper). The patterns are quite similar.⁶

As discussed in the text, the effect of candidate co-partisanship (relative to a respondent's partisan affiliation) on candidate support is relatively weak in our experiment, and we find no evidence of co-partisan bias in corruption sanctioning. We speculate that such weak partisan effects are due to weak partisan attachments in the Southern Cone countries. Another possibility may be that the respondents' partisan affiliation is not captured well. We measured it originally based on the survey item in LAPOP (VB11) asking respondents which party (if any) they feel closest to (which we matched to the candidate party affiliations in our vignette). Alternatively, LAPOP includes an item (L1) that asks respondents to place themselves on a left-right scale, with

⁶ In Egami and Imai's (2019) approach, all the attributes are assumed to be independently randomized, which is not the case in our experiment, as the 'bribes common' and 'bribes but jobs' scenarios are conditional on a candidate being accused of receiving bribes. Egami and Imai (2019) recommend that in such cases analysis be limited to a subset of data where the full-randomization assumption is satisfied. This would entail eliminating the control condition in our corruption treatment from the analysis. The differences in results between the conventional approach and their approach are similarly minor if we only focus on the mitigating factors and eliminate the comparison between the basic corruption treatment and the control condition.

Figure A4: Corruption Treatment Effects, by Respondents' Bribe Experience

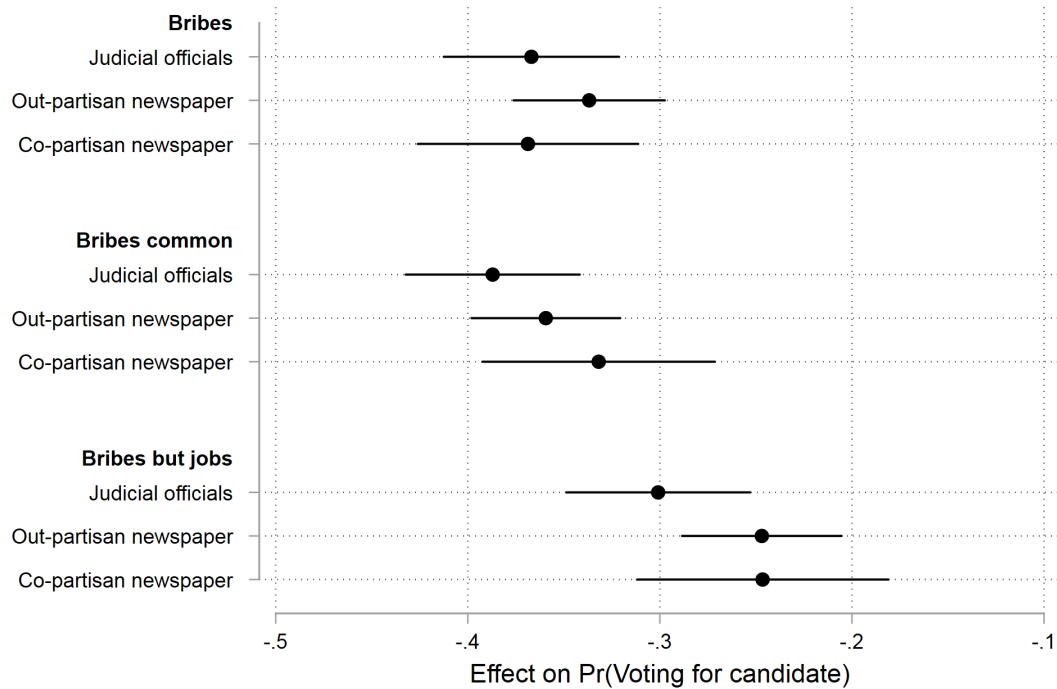


The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent. Brackets list the difference between effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

1 being most to the left, and 10 most to the right. We coded respondents choosing 3 or less as left-leaning, and those choosing 7 or more as right-leaning, for another measure of partisan affiliation. As before, we code the candidates as co-partisan and out-partisan relative to respondent partisanship. The top panel of Figure A9 compares the average marginal effects of the two alternative measures of co-partisanship; the bottom panel of Figure A9 compares the moderating effect on corruption sanctioning of candidate co-partisanship based on the two alternative measures. Results with the left-right measure are similar to the results with the party ID measure, though generally less precise and somewhat smaller in magnitude.

Figure A10, in turn, examines whether candidates from left-wing parties in our vignette are treated differently than candidates from right-wing parties (relative to independents)—by co-partisan (vs. out-partisan) respondents. We show the results for the party ID measure (the results for the left-right measure are once again similar). The differences in the treatment effects are uniformly minor.

Figure A5: Corruption Treatment Effects, by Information Source



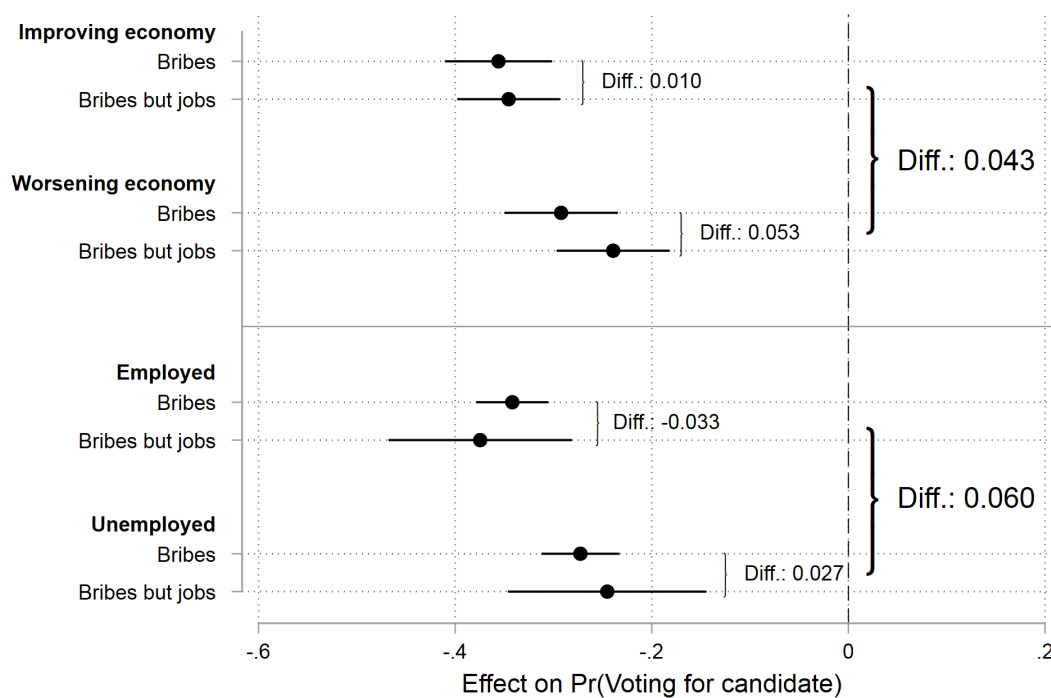
The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

A5 Auxiliary Question Wording

We replicate here the wording of the auxiliary questions and the coding of the variables based on them which we use for the analyses referenced here and in the text.

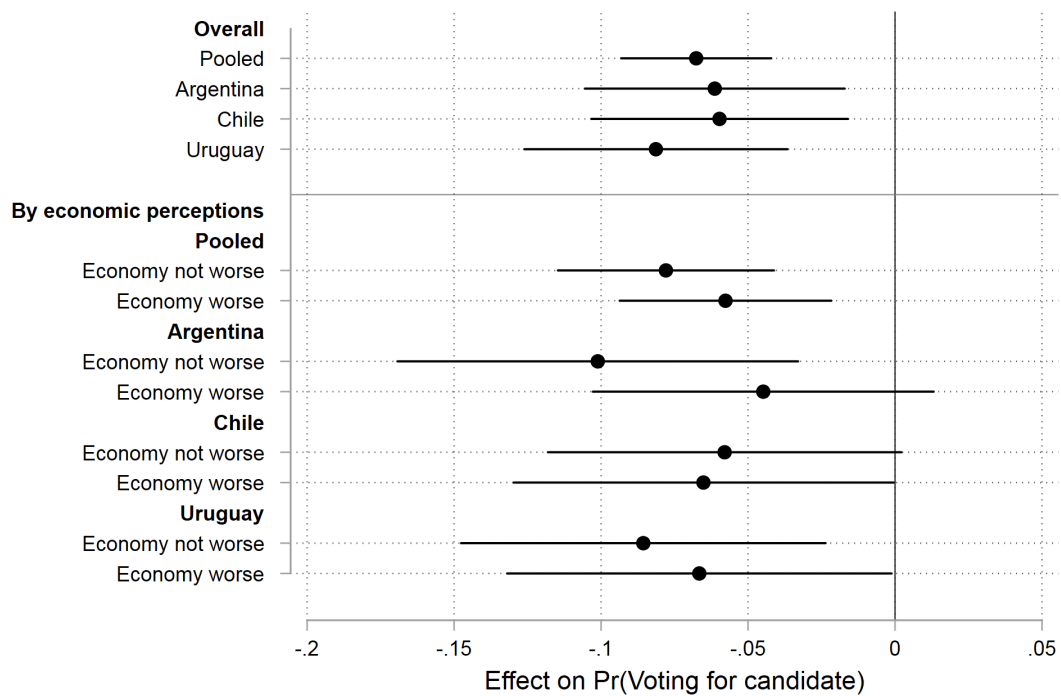
- **Tolerance of corruption:** “Do you think given the ways things are, sometimes paying a bribe is justified?” The response options were “yes” or “no.” 12.34% of respondents answered “yes” (1.39% did not provide an answer or chose “don’t know”).
- **Corruption perception:** “Thinking of the politicians of [country], how many of them do you believe are involved in corruption?” The answer options were “none,” “less than half,” “half,” “more than half,” or “all.” We code as having high corruption perceptions those respondents who said that more than half or all politicians are corrupt.
- **Economic perception:** “Do you think that the country’s current economic situation is better than, the same as or worse than it was 12 months ago?” The answer options were “better,” “same,” or “worse.” We combine the “same” and “worse” categories.

Figure A6: What Conditions Amplify the Mitigating Effect of Side Benefits?



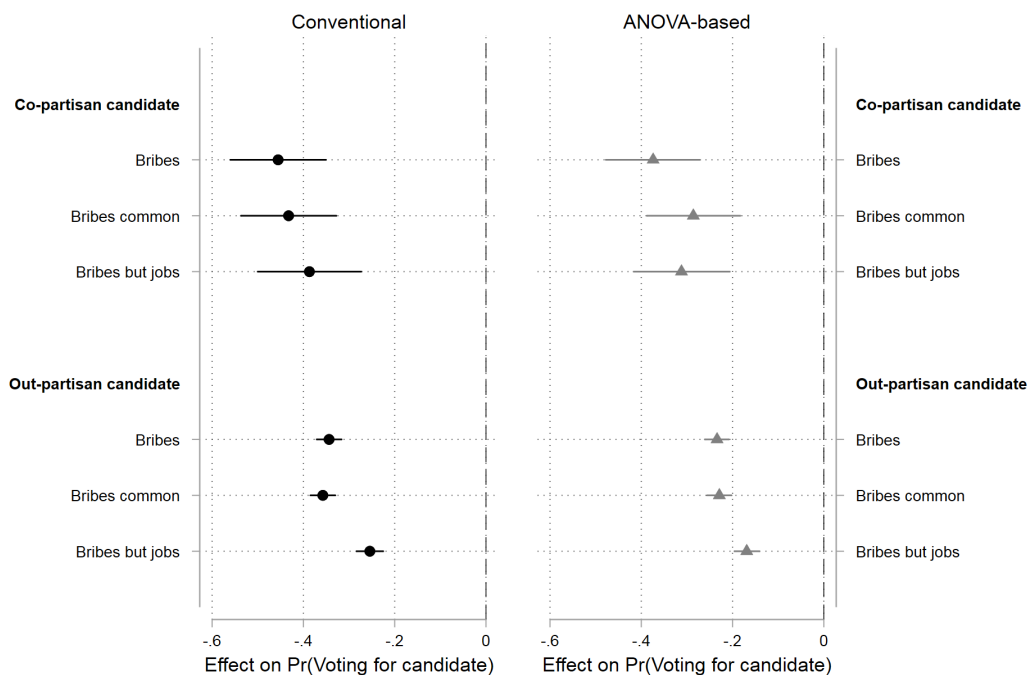
The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent. Brackets list the difference between effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

Figure A7: Economy Treatment Effect, by Country and Respondents' Retrospective Economic Perceptions



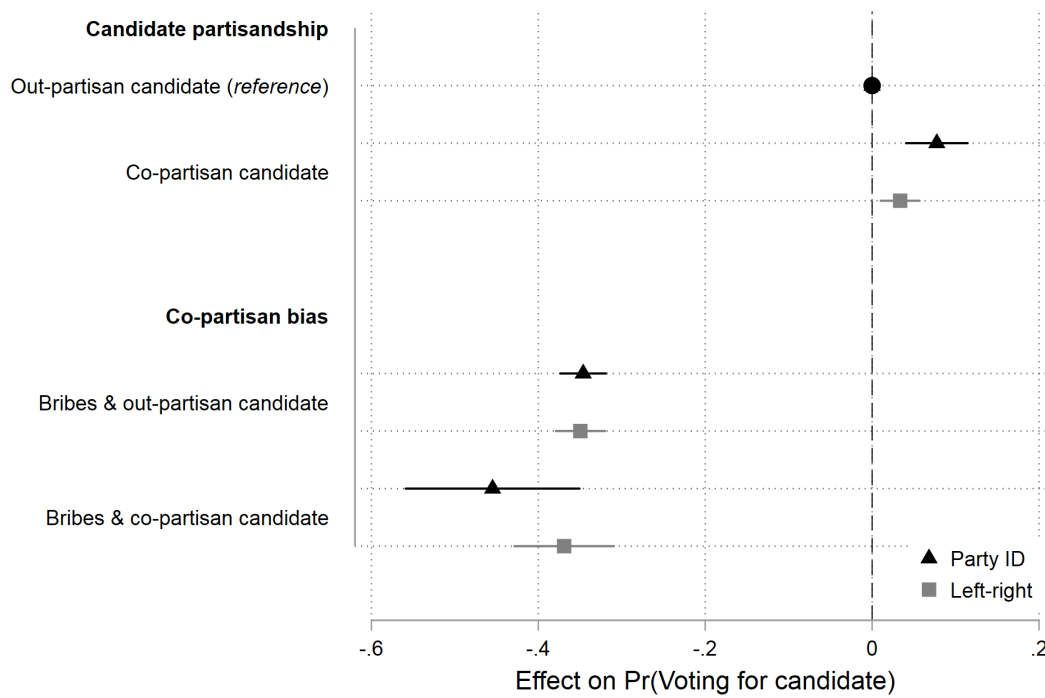
The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

Figure A8: Conventional Analysis vs. ANOVA-based Approach by Egami and Imai (2019)



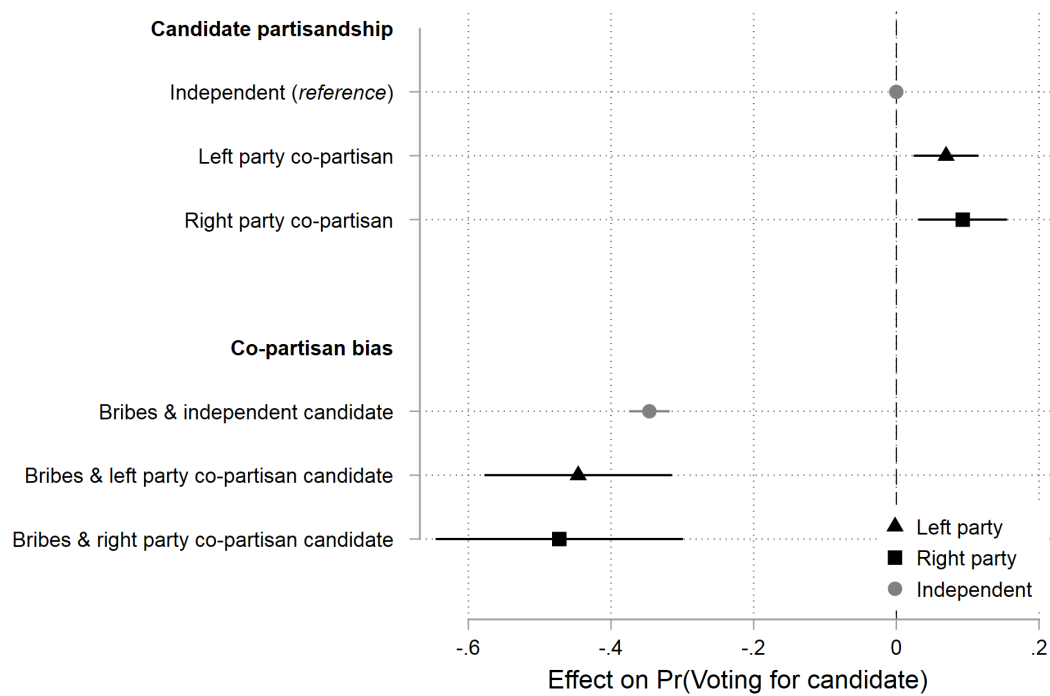
The left panel shows the estimates from a conventional regression-based approach to analyzing conjoint experiments (Hainmueller et al., 2014); the right panel from the ANOVA-based approach outlined in Egami and Imai (2019). The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

Figure A9: Candidate Partisanship and Co-Partisan Bias Effects for Alternative Respondent Partisanship Measures



The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

Figure A10: Left-Party vs. Right-Party Candidate Effects



The horizontal lines show the 95% confidence intervals, based on standard errors are clustered by respondent.

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