# When Do Voters Sanction Corrupt Politicians?

Word count: 3,999

**Abstract**

A growing body of research explores the factors that affect when corrupt politicians are held accountable by voters. Most studies, however, focus on one or few factors in isolation, leaving incomplete our understanding of whether they condition each other. To address this, we embed rich conjoint candidate choice experiments into surveys in Argentina, Chile, and Uruguay. We test the importance of two contextual factors thought to mitigate voters' punishment of corrupt politicians: how widespread corruption is and whether it brings side benefits. Like other scholars, we find that corruption decreases candidate support substantially. But we also find that information that corruption is widespread does not lessen the sanction applied against corruption, whereas information about the side benefits from corruption does, and does so to a similar degree as the mitigating role of permissible attitudes toward bribery. Moreover, those who stand to gain from these side benefits are less likely to sanction corruption.

Do voters punish corrupt politicians at the ballot box? In principle, elections allow voters to vote corrupt candidates out of office (Besley 2007). But the empirical evidence is mixed: while some studies find evidence of punishment (e.g. Klašnja 2015), others do not (e.g. Chang et al. 2010). As a result, recent studies focus on identifying factors that mitigate the electoral sanctioning of corruption. For example, voters appear less willing to sanction corrupt politicians who belong to their preferred party (e.g. Anduiza et al. 2013). In this paper, we expand this emerging literature on the mitigating factors of *corruption voting*.[1]

We use survey experiments to examine two important mitigating factors that have so far been studied only observationally or theoretically. We evaluate the extent to which corruption voting is mitigated when: (a) corruption is perceived to be widespread, potentially inducing voters to ignore it and focus on other aspects of politicians' performance or character; and (b) corruption brings direct benefits to the constituency, potentially incentivizing voters to trade off these benefits for electoral support.

Survey experiments on vote choice do not necessarily match real-world voting (Boas et al. 2019), partly because voters need to coordinate their expectations (Arias et al. 2019; Chang et al. 2010). But they do reveal something about public preferences (Hainmueller et al. 2015), in themselves important objects of investigation. Here we are less interested in the overall effect of corruption on voting than we are in different mitigating factors, results that are less likely to be driven by response biases. Moreover, studies like Ferraz and Finan (2008; but see Avis et al. 2016: 21) and Klašnja (2015) have found substantial electoral costs for corrupt politicians, so it is unlikely that anti-corruption voting only appears in the artificial context of survey experiments.

Our second contribution is design-based. To date, most experimental studies on corruption and voting have tested one or two mitigating factors in isolation. Such approaches have several important limitations: (a) they provide little information about the relative importance of different mitigating factors; (b) they cannot shed light on potential interactions between different mitigating factors; (c) treatment effects in such designs may be compounded, or even confounded,

[1] Corruption voting is defined as the effect of corruption on voting behavior, analogous to the term economic voting.

by other important factors that influence corruption voting but are left out (Dafoe et al. 2018).

To address these limitations, we employ a conjoint experimental design that randomizes a larger number of experimental treatments within the same vignette (Hainmueller et al. 2014).[2] We exploit this design to place our mitigating factors of interest in context by comparing them to two other mitigating factors: co-partisanship and voters' general tolerance of corrupt behavior. We also examine interactions between our mitigating treatments and other factors, while controlling for a range of other features we know can affect corruption voting. These features ensure that our results generalize beyond prior experimental studies of corruption and voting. To increase the external validity, we also ran our experiment in three countries with different recent experiences with political corruption: Argentina, Chile, and Uruguay.

We find strong evidence of corruption voting: accusations of corruption decrease support by 65% compared to a candidate praised for efforts to stamp out corruption. While informing respondents that corruption was widespread in a candidate's province does not mitigate the sanctioning of candidates for corruption, mentioning that corruption has brought construction jobs to the municipality – what we call *side benefits* – does, by a substantively meaningful 25%. The size of this mitigating effect is as large as the mitigation observed among individuals who find bribes justifiable, and considerably larger than co-partisanship.[3] Finally, while the mitigation due to side benefits broadly applies to a variety of contexts and respondent characteristics, it is more pronounced among citizens who are more likely to benefit from such rents.

## Mitigating Corruption Voting

Under what circumstances do voters sanction corrupt politicians? To begin, standard accounts of *corruption voting*—where corruption is understood as a misuse of public resources

[2] Three recent studies use a similar approach but focus on different research questions (Breitenstein 2019; Mares and Visconti Forthcoming; Martin Forthcoming).

[3] An important caveat is that partisanship is fairly weak in two of the three countries in which we conducted our experiments (Lupu 2015).

for personal and/or political gains—suggest that less corruption is more desirable (e.g. Besley 2007). Thus our first, baseline, hypothesis is:

- $H1$: Allegations of corrupt behavior will reduce support for a candidate.

Existing evidence in support of this basic prediction is mixed. One mitigating factor may be the prevalence of corruption in the wider context. When corruption is widespread, voters may choose to ignore it and focus on other aspects of a politician's performance or character (e.g. Rose and Peiffer 2015). They may also believe that there are few or no clean alternatives on offer (Meirowitz and Tucker 2013), or that corrupt politicians are more effective than clean politicians (Klašnja et al. 2018):

- $H2a$: Voters will punish corrupt candidates less when corrupt behavior is perceived as widespread.

Other studies suggest the opposite. Informing voters that corruption is widespread may increase the salience of corruption in voters' minds (Klašnja et al. 2016), a process observed more generally for political phenomena (e.g. Iyengar 1990):

- $H2b$: Voters will punish corrupt candidates more when corrupt behavior is perceived as widespread.

It is difficult to establish empirically the effect of perceived corruption prevalence with observational data. Voters in higher-corruption contexts may conceptualize corruption differently than voters in low-corruption contexts (Pavão 2018). And voter indifference to corruption may be both a cause and a consequence of its prevalence. Our research design helps to address these challenges.

The second mitigating factor we examine is the provision of side benefits to voters. Corrupt politicians may be forgiven if they share some of the rents with voters (Barberá et al. 2016). More broadly, punishment may be lower when incumbent performance is otherwise good (e.g. Klašnja and Tucker 2013; Zechmeister and Zizumbo-Colunga 2013):

- $H3$: Voters punish corrupt candidates less when corrupt behavior brings side benefits to constituents.

This expectation is also difficult to establish observationally. Corruption-induced benefits may be systematically different from benefits provided by a clean politician, eliciting distinct reactions from voters. Voters may forgive corrupt politicians during good times but not during bad times because of their preference for ability over honesty rather than because of their willingness to trade off corruption for benefits. Our approach allows us to control these aspects.[4]

Our survey experiment also benchmarks the two potential factors mitigating corruption voting, by comparing them to two other mitigating factors originating at the individual level: (a) co-partisanship, whereby an individual may be less likely to sanction a corrupt politician from their preferred party (Anduiza et al. 2013; Solaz et al. 2019); and (b) an individual's general tolerance for corruption (e.g. Barr and Serra 2010).

## Conjoint Experiments in the Southern Cone

We fielded conjoint candidate choice experiments embedded in nationally representative surveys in Argentina, Chile, and Uruguay. All three surveys were fielded between March and May 2017 as part of LAPOP's AmericasBarometer and include just over 1,500 respondents each.[5]

We focus on these three countries because they offer useful contextual variation. All three have similar political systems and demographics, allowing us to use similar candidate vignettes. Yet they vary in theoretically meaningful ways: Argentina and Chile have lower levels of mass partisanship but higher levels of corruption perceptions than Uruguay, while bribery is considerably more frequent in Argentina than it is in Chile or Uruguay. Given such variation, to

[4] Klašnja and Tucker (2013), Konstantinidis and Xezonakis (2013), and Weitz-Shapiro and Winters (2013) experimentally study somewhat related tradeoffs between performance and corruption.

[5] Section A2 of the appendix provides further information about the countries and surveys, presents the wording of the vignette and other survey items used in our analysis, gives details on each experimental treatment, and reports on diagnostic checks for our experiment. All of our analyses use the full sample.

the extent that we find similar results across these different settings, we can be more confident that they are not just unique to a particular context.

In our experiment, we presented survey respondents with a short vignette about two hypothetical mayoral candidates, an incumbent and a challenger, running in a local election. Within the text of the vignette, we randomly varied six characteristics of the candidates and the electoral environment: (1) candidate gender, (2) party affiliation (left party, right party, or independent), (3) corruption record (accused of taking bribes or praised for efforts to stamp out bribery in their administration), (4) the information source for the bribery allegation/praise (left or right newspaper, or judicial officials), (5) a potentially mitigating corruption factor (corruption prevalence or the creation of construction jobs; applicable only when a candidate is accused of corruption), and (6) the state of the economy (improved or worsened since the last election; applicable only to the incumbent). We randomized each attribute independently for each candidate, allowing us to simultaneously estimate the causal effect of each characteristic (Hainmueller et al. 2014). After showing respondents the vignette, we asked our key outcome question: "If you had to choose between these two candidates, for whom would you vote?"[6]

## When Do Voters Sanction Corrupt Politicians?

To estimate treatment effects, we treat each hypothetical candidate as a unique case (i.e. there are two candidates for every respondent), following Hainmueller et al. (2014) (standard errors are clustered by respondent). We then estimate OLS models relating respondents' choices to indicator variables for each treatment.[7] We pool estimates across the three countries and

[6] Since our experiment probes preferences on a sensitive topic, responses may exhibit social desirability bias. With a larger set of treatments, conjoint designs lessen this concern by making it more difficult for respondents and interviewers to infer the key topics of interest (Hainmueller et al. 2014). Moreover, corruption voting in our experiment varies in predictable ways, suggesting that social desirability is not driving our results. Our analysis omits respondents who answered "Don't Know" or did not respond to the question (7% of our sample).

[7] Our results are substantively similar when using logistic regression models (Table A5).

include country dummies in our specifications.[8] Since the economic performance attribute only applies to the incumbent candidate, we also control for incumbency and an interaction between economic performance and incumbency.[9]

The top estimate in Figure 1 is strongly consistent with $H1$: corruption causes a large drop in respondents' support, from about 53% for clean candidates to 18% for corrupt candidates—a 65% reduction.[10] The effects of the economy and co-partisanship are also not surprising: a poor economy decreases the probability of support;[11] belonging to a respondent's preferred party increases it.[12]

While the corruption effect is sizable, it is similar in magnitude to other experimental studies from the region (for a review, see Boas et al. 2019). We suspect that the smaller magnitude of the partisanship effect is a consequence of weak partisanship, particularly in Argentina and Chile, where the effect is close to zero and statistically null (see Figure A2).[13] Corruption voting also seems to be large relative to the effect of the economy, in contrast to some previous findings (Klašnja and Tucker 2013). But it is difficult to know how respondents envisioned the improved and worsened economic conditions signaled in the vignette, or how the magnitudes of these economic changes compared to the magnitude of corruption. Probing these contrasts is an interesting question for future research.

[8] Table A7 shows the treatment effects by country; the relative extent of mitigation due to corruption prevalence or side benefits is similar across countries.

[9] Unsurprisingly, economic performance affects incumbents and challengers differently (Figure A1).

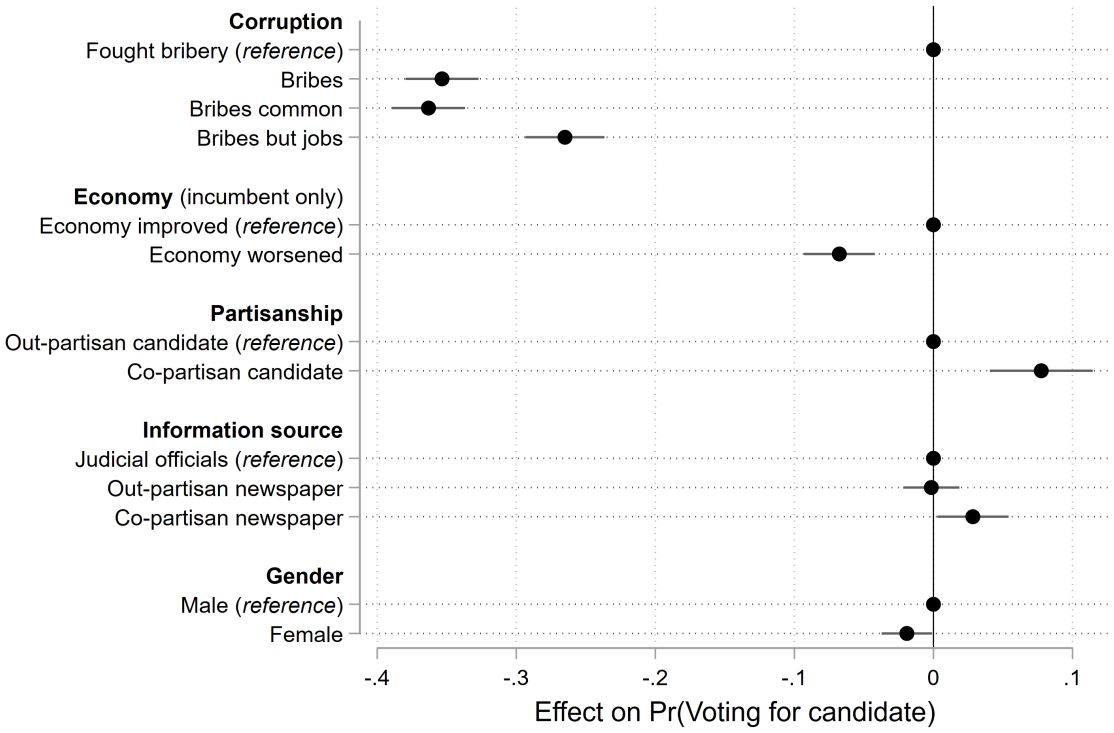
[10] Figures A3 and A4 offer some, though noisy, evidence that corruption sanctioning varies by respondents' perceptions of corruption and bribe experience. We find no consistent evidence that the corruption treatment effect varies by the information source (Figure A5).

[11] Figure A7 shows little variation in the economy treatment effect by actual and perceived economic performance.

[12] The conjoint design implies a relatively large number of tests, but our key results are robust to multiple-testing adjustment (Table A6).

[13] Only around 3% of respondents in Argentina and Chile report identifying with a political party. This share rises to 13% in Uruguay but is still low compared to many other democracies.

**Figure 1:** Conjoint experiment results




Values represent the difference in respondents' propensity for supporting a hypothetical candidate based on each vignette characteristic. Lines represent 95% confidence intervals estimated using standard errors clustered by respondent. Estimates are based on OLS regressions reported in Table A4.

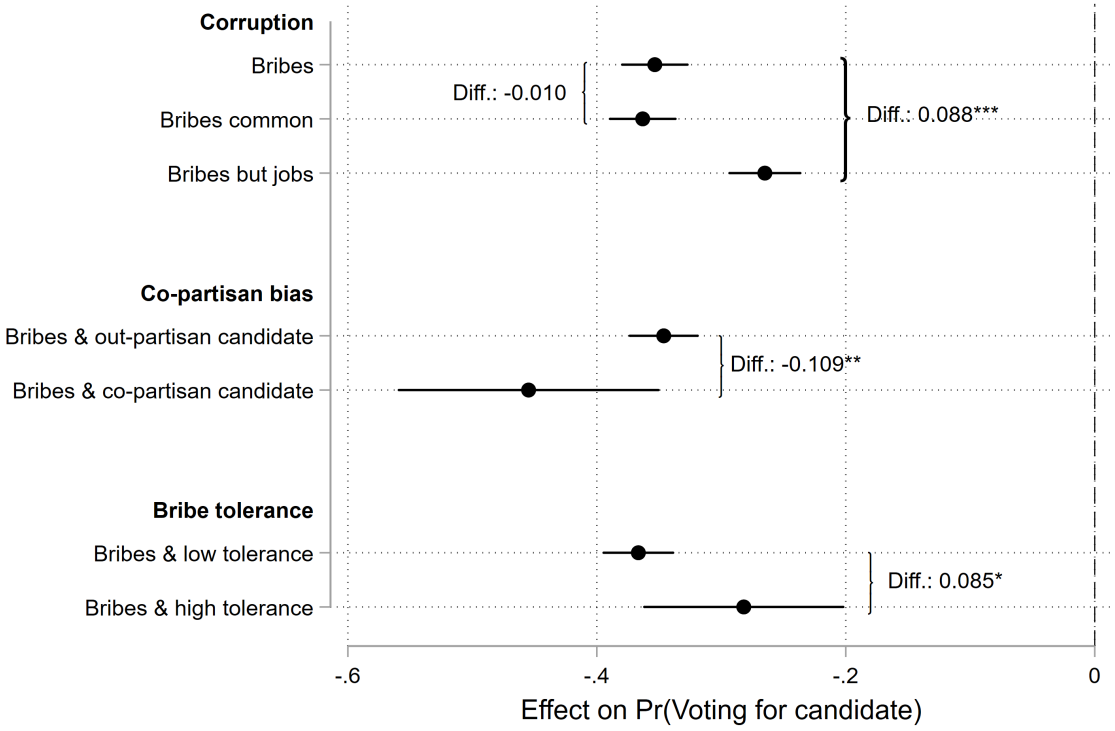
## Prevalence and Side Benefits as Mitigating Factors

In Figure 2, we evaluate the evidence for hypotheses $H2a$ (and $H2b$) and $H3$ on the mitigating effects of corruption prevalence and side benefits. As benchmarks, we also examine the mitigation due to co-partisanship and tolerance of corruption.

Informing respondents that corruption is widespread in a candidate's province (the treatment we refer to in the Figures as 'bribes common') did nothing to mitigate the sanctioning of corruption: the difference between the top two values, the basic corruption treatment and the prevalence treatment, is not statistically significant. This evidence is thus inconsistent with $H2a$ and $H2b$.[14]

[14] We examined whether this null result may be due to ceiling effects from respondents' already high corruption perceptions, but find no evidence for this possibility (Figure A3).

**Figure 2:** Contextual and individual factors mitigating corruption voting




Values represent the difference in respondents' propensity for supporting a hypothetical candidate based on each vignette characteristic. Lines represent 95% confidence intervals estimated using standard errors clustered by respondent. Estimates are based on OLS regressions reported in Table A4. Brackets list the difference between effects. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Information about jobs created through corruption (the treatment we refer to in the Figures as 'bribes but jobs'), however, noticeably mitigates the sanctioning of corruption: the difference between the first and third values, the basic corruption treatment and the side benefits treatment, is positive and statistically significant. The mention of corruption reduces the likelihood of voting for a candidate by 36 percentage points, but the side benefit of jobs decreases the corruption penalty from 36 to 27 percentage points (a 25% decrease). This mitigating effect is present in all three countries, and to a very similar extent (Table A7), bolstering our confidence in uncovering a general pattern.

The second and third sets of values in Figure 2 show the effects of co-partisanship and corruption tolerance. We do not observe co-partisan bias in the propensity to punish corrupt politicians (the difference between the two values under Co-partisan bias); quite the opposite:

respondents were on average *more* likely to punish co-partisan corrupt candidates, by nearly eleven percentage points.[15] This result is less surprising, however, given that partisan attachments are fairly weak in our three countries.

On the other hand, citizens who are more tolerant of corruption are on average less likely to sanction corrupt politicians. To measure corruption tolerance, we use a binary survey item (asked before the experiment) that measured the extent to which respondents find it justifiable to pay a bribe (see Section A5 of the appendix for wording). The extent of mitigation produced by corrupt side benefits is virtually identical in size to that arising from individuals' tolerance of bribery. Note that these two mitigating factors appear to be additive: when the corrupt jobs treatment is interacted with individuals' bribe tolerance, the extent of mitigation is essentially doubled and statistically significant.

## Conditions Amplifying the Mitigating Effect of Side Benefits

Figure 3 explores the conditions that may make the mitigating effect of corruption's side benefits particularly pronounced. In our vignette, the corrupt side benefit takes the form of construction jobs, typically positions held by individuals with lower levels of education and wealth.[16] We indeed find that such respondents are more willing to trade off corruption for jobs than other respondents, as shown in Figure 3. The mitigating effect of side benefits on sanctioning corruption is six percentage points higher among respondents with lower levels of education, a result that is in line with Truex (2011). And it is five percentage points higher among less wealthy respondents.[17] Both of these effects persist even when controlling for the other characteristic.

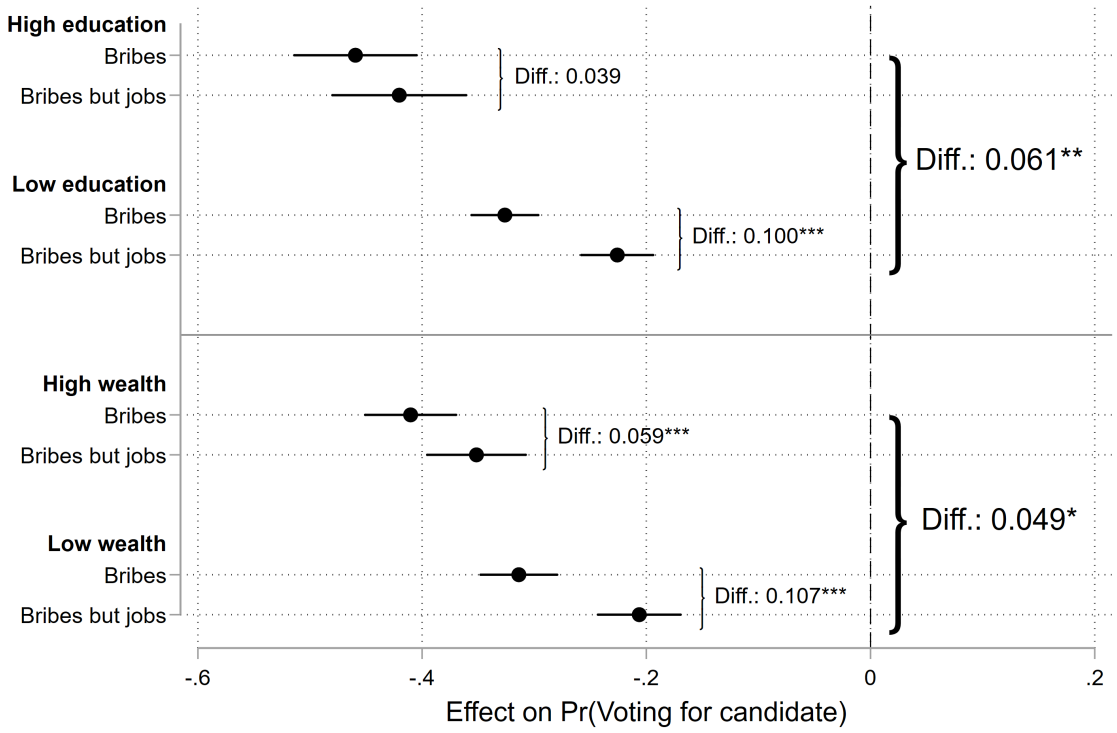
Other measures of economic vulnerability generally seem to amplify the mitigating effect

[15] This is mainly driven by identifiers with right parties and by Uruguayans, suggesting that right partisanship in Argentina and Chile is stronger than left partisanship (in terms of its effects on behavior).

[16] Unfortunately, we do not have information to examine the heterogeneous effects across occupation groups.

[17] We define respondents as having low education if they completed less than secondary school and low wealth if they are in the bottom three quintiles of the wealth distribution. All other respondents are coded as high. Wealth is a score based on factor analysis of 13 items capturing household ownership of consumer goods and assets (such as a cellular phone or a vehicle).

**Figure 3:** What conditions amplify the mitigating effect of side benefits?




Values represent the difference in respondents' propensity for supporting a hypothetical candidate based on each vignette characteristic. Lines represent 95% confidence intervals estimated using standard errors clustered by respondent. Estimates are based on OLS regressions reported in Table A4. Brackets list the difference between effects. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

of side benefits on corruption sanctioning, though with less statistical precision. Figure A6 shows that economic downturns (based on our experimental manipulation) and being unemployed correlate positively with accepting corruption in exchange for construction jobs. Overall, these findings suggest that beneficiaries from the side benefits of corruption are somewhat less likely to sanction it, although the mitigating effect of corrupt side benefits is quite broad, applying to a variety of scenarios and respondents.[18]

[18] We do not find heterogeneous effects based on the source of the corruption allegation, or the gender or party affiliation of the candidate (see Figure A5, Table A2, and Figure A10).

# Conclusion

Studying the effect of corruption on voter behavior is challenging. Observational studies potentially suffer from problems of identification, especially since more popular incumbents may be more inclined to engage in corruption. Experimental studies, on the other hand, have mostly focused on one hypothesized variable at a time. This too is limiting because other important factors may be omitted.

Our conjoint experimental design addresses these limitations. Drawing on data from identical experiments in Argentina, Chile, and Uruguay, we find that corruption accusations indeed strongly (negatively) affect candidate support. Influencing respondents' perceptions about how widespread corruption is does not alter the corruption sanction. However, corrupt candidates who are reported to have brought jobs to their constituency are punished substantially less, especially by citizens with lower socioeconomic status. This mitigating effect is as large as that among citizens who find bribes justifiable, and much larger than the inclination to forgive corruption by candidates from one's preferred party.

There are doubtless scope conditions on our inferences from the Southern Cone. For one, self-reported partisanship in our cases is low in comparison to rates typical in many developed democracies. This may help explain why we do not see partisanship mitigating much of the corruption sanction. It would thus be beneficial to replicate similar conjoint experiments in other democracies with higher levels of partisanship.

Future studies might also leverage other aspects of our experiment. For example, our design included partisan media sources of corruption accusations, but we do not dwell on those results here. Scholars interested in those findings, and perhaps the individual characteristics that condition its effects, could further analyze our experiments. Indeed, the data are already publicly available through LAPOP.

Finally, our conjoint design could be extended to include additional conditions. For instance, future experiments could compare the effects of different side benefits from corruption, or include comparisons with other candidate characteristics, such as race or class, or policy

platforms.

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