When Do Voters Sanction Corrupt Politicians?*

Marko Klašnja
Georgetown University
marko.klasnja@georgetown.edu

Noam Lupu
Vanderbilt University
noam.lupu@vanderbilt.edu

Joshua A. Tucker
New York University
joshua.tucker@nyu.edu

October 23, 2017

Abstract

A rich body of work has explored a number of factors that affect the extent to which corrupt politicians are held accountable by voters. However, most of these studies examine only one factor in isolation. As a result, we lack a sense of their relative importance and whether and how they condition each other. To address this problem, we embedded conjoint candidate choice experiments into surveys in Argentina, Chile, and Uruguay. We test the importance of two factors thought to mitigate voters' willingness to punish candidates for corrupt behavior: how widespread corruption is and whether corruption brought side benefits. We find that corruption accusations indeed decrease support for candidates, much more so than economic performance or partisanship. We also find that informing respondents that corruption is widespread does not help to mitigate the corruption sanction. However, candidates accused of corruption who brought jobs to their constituency are punished substantially less, especially by citizens with lower socioeconomic status.

Do voters sanction corrupt politicians at the ballot box? In principle, elections should allow voters to vote corrupt candidates out of office (Besley 2007; Ferejohn 1986). However, the empirical evidence from the increasingly rich literature is mixed. While some studies find that voters hold corrupt politicians to account (e.g. Banerjee et al. 2011; Ferraz and Finan 2008; Klašnja et al. 2016; Weitz-Shapiro and Winters 2013; Welch and Hibbing 1997), others show that voters are sometimes willing to condone or ignore corruption (e.g. Banerjee and Pande 2007; Chang et al. 2010; Golden 2010; Klašnja 2017).

Motivated by such mixed evidence, recent studies have been focusing on factors that mitigate the electoral punishment of corrupt politicians. For example, voters appear more willing to forgive malfeasance by politicians belonging to their preferred party (e.g. Anduiza et al. 2013; Solaz et al. 2017).[1] Many of these studies have also been driven by growing sophistication in the use of experiments in political science research (for example, Klašnja and Tucker 2013; Weitz-Shapiro and Winters 2015; Weschle 2016, as well as several contributions in this issue). These experimental approaches allow researchers to estimate the causal impact of, for example, hearing that a hypothetical candidate for office has been accused of taking a bribe. They thus overcome inferential challenges that plagued earlier observational work and advance our empirical understanding of the role of corruption in citizens' voting decisions.

In this paper, we experimentally study the importance of two factors that may mitigate the willingness of voters to punish candidates for corrupt behavior, but that have so far been studied observationally or theoretically. Namely, we ask to what extent the negative repercussions for a candidate of corruption allegations are mitigated when: (a) corruption is perceived to be widespread, potentially inducing voters to ignore corruption and focus on other aspects of politicians' performance or character (e.g. Klašnja et al. 2017); and (b) when corruption is reported to have brought direct benefits (in the form of jobs) to the constituency, potentially making voters willing to trade these benefits off for corruption (e.g. Barberá et al. 2016).

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[1] For recent reviews of this literature, see De Vries and Solaz (2017) and Niczyporuk et al. (2018).

Studying these potential mitigators experimentally allows us to make causal inferences that were not possible previously.

To date, most experimental studies of how corruption affects voting have tested one or two mitigating factors in isolation, with several important limitations. These designs provide little information about the relative importance of different factors that mitigate corruption voting. Without being able to compare different mitigators, our understanding of the scope of voters' corruption sanctioning remains partial and incomplete. Research that focuses on a single factor also cannot shed light on important potential interactions between different mitigating factors, as well as with other contextual features or individual-level characteristics. For example, do corrupt side benefits mitigate the punishment of corruption only in a weak economy, when they may be particularly valuable? Finally, treatment effects in experiments including only one or two treatments may be compounded, or even confounded, by other important factors that influence corruption voting but that are left out (Dafoe et al. 2017). For example, partisan bias in corruption sanctioning may be amplified in the absence of information about how credible the source of corruption allegations is, something which may itself be shaped by partisanship (e.g. Botero et al. 2015; Weitz-Shapiro and Winters 2017).

To address these limitations, we employ a conjoint experimental design (Hainmueller et al. 2014), which allows us to randomize a larger number of experimental treatments within the same vignette. Using this design, we can directly compare multiple factors on the same scale, explore theoretically interesting interactions, and avoid compounded treatments. We first put the extent of electoral sanctioning in context – something rarely done by previous studies – by comparing it to two well-known factors known to influence vote choice: the state of the economy, and the candidates' party affiliation. We next compare the magnitude of the potentially mitigating contextual treatments – the prevalence of corruption and the provision of side benefits – with two other commonly posited individual-level mitigators: copartisan bias in evaluations and a voter's tolerance for corruption (e.g. Ludwin-Peery and Tingley 2014). We also exploit the conjoint design to examine interesting interactions between our mitigating treatments and other

characteristics of our rich vignette scenario and of our respondents. Moreover, the conjoint design allows us to experimentally control for a number of other features that we know can affect corruption voting, such as the source of corruption allegations or the characteristics of a challenger. All of these features ensure that our design generalizes beyond many existing experimental studies of corruption and voting. However, to increase the external validity of our findings, we also embedded our experiment in three countries with very different recent experiences with political corruption: Argentina, Chile, and Uruguay.

We report five sets of results. First, we find strong evidence of corruption sanctioning: all else equal, accusations of corruption decrease support for a candidate by 65%. Second, in our data, this effect is several magnitudes larger than either the effect of the economy or that of partisan preference, suggesting a particularly strong distaste for corruption. Third, while informing respondents that corruption was prevalent in a candidate's province does not alter the extent of corruption sanctioning, mentioning that corruption may have brought construction jobs to the municipality noticeably mitigates the negative impact of corruption on candidate support – by 25%. Fourth, the size of this mitigating effect is as large as the mitigation observed among individuals who find bribes justifiable, and considerably larger than the partisan bias in corruption evaluations.[2] Finally, while the mitigation due to corrupt side benefits broadly applies to a variety of contexts and respondent characteristics, it is somewhat more pronounced among lower-educated citizens and those with lower wealth, who are more likely to benefit from the type of side-benefits (construction jobs) presented in our vignette.

Collectively, these results advance our understanding of the effect of corruption on voting behavior in the three important ways. First, they provide continued comparative evidence in support of the notion that voters – all else equal – are inclined to sanction candidates for corruption, and in a part of the world where it is often posited that patronage networks would make such behavior unlikely (Luna 2010; Stokes 2005; Stokes et al. 2013). Second, they show

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[2] An important caveat regarding the partisanship comparison is that partisanship is generally quite weak in two of the three countries in which we conducted our experiments (Lupu 2013, 2015).

that a long posited potential mitigator of corruption voting – the belief that corruption is common – does not decrease sanctioning of corrupt candidates, despite theoretical reasons to think that it might (Klašnja et al. 2017). Conversely, the results show that one way by which candidates can avoid paying (as large) a penalty for corruption is to make sure that corruption produces at least some clear material benefit for the voters, as opposed to only the politician. Moreover, our conclusions in all three cases are based on cleaner estimates of causal effects than most previous studies due to our ability to rule out many possible confounding effects.

Mitigating Corruption Voting

Do voters sanction corrupt politicians? And under what circumstances might they fail to sanction them? We begin by reviewing the relevant theoretical arguments about the influence of corruption on voting behavior, and the contextual and individual-level factors that may mitigate citizens' proclivity to sanction corrupt politicians.[3]

At the most basic level, standard accounts of *corruption voting* suggest that less corruption is usually more desirable (e.g. Besley 2007; Klašnja et al. 2016). Therefore, the basic prediction is that voters are expected to punish politicians deemed to be corrupt and reward politicians who refrain from and/or strive to combat corruption.[4]

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

Existing evidence in support of this basic prediction is mixed. While some studies find that corruption reduces support for a politician (Ferraz and Finan 2008; Hirano and Snyder 2012; Klašnja 2015; Reed 1999; Welch and Hibbing 1997), others have shown that voters may fail to

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[3] We do not focus on institutional factors that may attenuate the electoral sanctioning of corruption (see, for example Chang and Golden 2007; Kunicova and Rose-Ackerman 2005; Schwindt-Bayer and Tavits 2016).

[4] It is possible that the basic assumption underlying this expectation is flawed, and that voters may reward corrupt behavior. Our basic test accommodates such an alternative. We also outline some of the possible reasons when discussing our expectations regarding moderating factors below.

sanction malfeasance (Banerjee and Pande 2007; de Figueiredo et al. 2012; Klašnja 2017; Manzetti and Wilson 2007). Therefore, we seek to examine several factors that may cause voters to forgive corruption. In particular, we exploit our experimental design to primarily focus on two contextual mitigating factors that have so far only been studied observationally or theoretically.

One reason voters may not punish corrupt politicians is the prevalence of corruption in the wider context. Greater prevalence of corruption may mitigate electoral sanctioning of corruption for several reasons. Voters may be more cynical about corruption when it is widespread, choosing to ignore it and focus on other aspects of politician's performance or character (e.g. Rose and Peiffer 2015).[5] In a highly corrupt society, voters may also come to believe that corrupt politicians are more effective at navigating the system than clean politicians (Bauhr and Charron 2017; Klašnja et al. 2017). Moreover, when corruption is widespread, bad politicians may crowd out honest ones (Caselli and Morelli 2004), making voters lose trust in the political system, and be pessimistic about the availability of clean alternatives (Chong et al. 2015; Meirowitz and Tucker 2013; Seligson 2002; Svolik 2013). These arguments set up the following expectation:

- *H2a*: Voters will punish candidates less for corruption when corrupt behavior is reported to be widespread.

Another argument suggests the opposite expectation. Informing voters that corruption is widespread might increase the salience of corruption perceptions in voters' minds, and consequently increase the weight of corruption considerations in vote choice (Klašnja et al. 2016), a process observed more generally for politically salient phenomena (Iyengar 1990; Krosnick 1988). It is also possible that by suggesting corruption is a larger problem – precisely because it is widespread – the candidate's corrupt behavior could be viewed by the voter as even more damaging. These arguments suggest an alternative hypothesis:

- *H2b*: Voters will be more likely to punish corrupt candidates when corrupt behavior is reported to be widespread.

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[5] Related, voters with high priors about corruption may be inured to corruption stimuli like a corruption scandal (Arias et al. 2017).

Despite the clear theoretical predictions, it is difficult to establish empirically the effect of perceived prevalence of corruption when relying on observational data. For example, voters in higher-corruption contexts may be conceptualizing corruption differently than voters in low corruption settings (Pavão 2016), and it may be this rather than corruption prevalence per se that accounts for any differences in the sanctioning of corruption. Moreover, prevalence of corruption and voter behavior are likely part of a jointly determined equilibrium, where voter indifference to corruption is both a cause and a consequence of the aggregate level of corruption (Klašnja et al. 2017). Our research design helps to address these challenges.

The second contextual mitigating factor we seek to examine experimentally is the provision of corrupt side benefits to voters. A corrupt politician may be forgiven for stealing if he or she also shares some of the corruption rents with voters (Barberá et al. 2016; Klašnja et al. 2017; Konstantinidis and Xezonakis 2013). More broadly, and related, scholars have found that corruption may be less harmful to malfeasant politicians when they also deliver good performance, whether in terms of local public good provision (Boas et al. 2017; Chauchard et al. 2017; Pereira and Melo 2015; Weitz-Shapiro and Winters 2013), or in terms of the state of the economy more generally (Klašnja and Tucker 2013; Li et al. 2015; Manzetti and Rosas 2015; Zechmeister and Zizumbo-Colunga 2013). Altogether, these arguments suggest the following expectation:

- $H3$: Voters will punish candidates less for corruption when corrupt behavior is alleged to have brought benefits to the constituents.

While intuitive, this expectation is also difficult to establish observationally. For example, the nature of corruption-induced benefits in the real world may be systematically different from benefits provided by a clean politician, and voters' reactions to, say, direct side payments financed by corrupt rents may not be comparable to their reactions to social spending secured through a legitimate legislative process. Furthermore, that voters would forgive politicians for corruption during good economic times but not during bad times may be a reflection of voters' preference for

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ability over honesty rather than their willingness to trade-off corruption for benefits. Our approach allows us to control these aspects and hone in on the causal effect of corrupt side benefits.[6]

Most existing experimental studies of corruption and voting behavior focus on only one or two factors while leaving out others. This approach leaves incomplete our understanding of the importance of corruption and the scope conditions in which it may affect the voting calculus. For example, even if we find that corruption significantly affects the intended vote choice in our vignette, this effect may be rather small in magnitude relative to some other factor, such as the performance of the economy while the politician is in office.[7] Similarly, even if we find that the prevalence of corruption or the corrupt side benefits mitigate any negative effect of corruption, the question remains as to how large such mitigation is relative to other well-known mitigating factors, such as citizens' co-partisan bias in evaluating corruption.

Therefore, in addition to reexamining the basic effect of corruption on voting, and experimentally studying the mitigating effects of our two contextual factors, we seek to benchmark these effects against other important features of experimental context and our respondents. First, to situate the basic corruption effect, we compare it to the effects of two other factors: (a) the performance of the economy, and (b) the partisan affiliation of the candidates. These two factors capture the two most widely studied determinants of vote choice: the economic and the partisan vote. The corruption vote component could be considered part of a third major dimension of the voting decision – the effect of candidate valence (in this case, honesty; Ansolabehere and Snyder 2000; Stokes 1963).

Second, to put the magnitude of the two mitigating factors we focus on into perspective, we compare them to two other commonly studied mitigating factors originating at the individual level: (a) the respondents' copartisan bias in corruption evaluations, and (b) the effect of

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[6] Klašnja and Tucker (2013) and Weitz-Shapiro and Winters (2013) experimentally examine related but broader tradeoffs between economic or public-good performance and corruption. Konstantinidis and Xezonakis (2013) also employ a survey experiment to examine the trade-off between corruption and benefits to voters arising from fiscal transfers or clientelist exchanges. However, none of these studies focuses on explicit corruption-related benefits.

[7] For example, Klašnja and Tucker (2013) find that corruption has a substantially smaller effect on voting than does the state of the economy in an experiment in Moldova. By contract, in an identical experiment in Sweden, the effect of corruption is of comparable magnitude as that of the economy.

respondents' predetermined justifiability of bribery. Citizens' copartisan bias in corruption evaluations refers to the extent to which they are less inclined to sanction corruption by candidates belonging to their preferred party compared to (identical) candidates belonging to some other party. This partisan bias has been shown repeatedly in previous observational and experimental studies (Anderson and Tverdova 2003; Anduiza et al. 2013; Charron and Bågenholm 2016; Klašnja and Tucker 2013; Rundquist et al. 1977; Solaz et al. 2017).

The second individual-level mitigating factor we benchmark against is the extent to which respondents' tolerance of corruption lessens their proclivity to sanction corrupt politicians. Straightforwardly, those who are more inclined to find bribery justifiable, whether through cultural, familial, or environmental influences, should be less likely to punish corruption (Barr and Serra 2010; Fisman and Miguel 2007; Hauk and Saez-Marti 2002; Ludwin-Peery and Tingley 2014; Simpser 2017).

We thus study both the factors that mitigate corruption voting and benchmark these effects against other factors thought to affect vote choice an corruption voting. Our conjoint experimental design both addresses some of the shortcomings of prior research and provides the analytical leverage to make causal inferences.

Evidence from Conjoint Experiments in the Southern Cone

To test our hypotheses and compare the magnitudes across and interaction among different factors, we fielded candidate choice conjoint experiments embedded in nationally representative surveys in three Southern Cone countries: Argentina, Chile, and Uruguay. All three surveys were fielded between March and May 2017 as part of LAPOP's AmericasBarometer.[8]

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[8] The AmericasBarometer conducts high quality, nationally representative public opinion surveys across the Americas every two years. Each country study includes a minimum of 1,500 observations, interviews are conducted face-to-face, and all interviews are audited extensively to ensure quality. Importantly for our purposes, all of the interviews were conducted using electronic questionnaires, which allowed us to ensure complete randomization.

We focus on these three countries because they offer useful contextual variation. All three have similar political systems and demographic characteristics: presidential multiparty systems, middle-income levels of wealth, comparatively high rates of economic inequality, and ethnically homogeneous societies. This allows us to use similar candidate vignettes across the three countries. At the same time, they vary greatly on two dimensions that prior studies suggest matters to corruption voting: party system strength and perceptions of corruption. Argentina has a fragmented party system with low levels of mass partisanship (Lupu 2016a). 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 eroded dramatically in recent years (Luna and Altman 2011; Lupu Forthcoming). Contrasting these two party systems, Uruguay's has been remarkably stable in recent years, and mass attachments to the parties is more widespread (Buquet and Piñeiro 2014; Lupu 2015).

With respect to corruption, there is substantial variation across these three countries.[9] 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

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Further information on sampling procedures, data collection, and response rates is available at www.vanderbilt.edu/lapop/.

[9] Citizen perceptions are based on an item in the AmericasBarometer survey (asked before our experiment): "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 list the proportion of respondents who said that more than half or all politicians are corrupt. 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.; see Lupu 2017). We list the proportion who reported having been asked to pay a bribe by any one of these officials.

both Chile and Uruguay (and 7% and 6%, respectively), but substantially higher (16%) in Argentina, which is just below the regional average.[10]

In sum, although the three Southern Cone countries in which we fielded our experiment are similar in terms of political institutions and demographics, they differ dramatically in terms of party strength and perceived corruption, factors that are thought to affect and mitigate corruption voting. If we find similar results across these different settings, we can be more confident that those results are not just unique to one country (Slater and Ziblatt 2013).

Experimental designs are useful because they help us avoid pitfalls of relying on observational data on electoral sanctioning of corruption, where voters' observed choices may be correlated with many other factors, contextual and individual, that influence how corruption affects voter behavior. To isolate the causal effects of corruption and the mitigating effects we outlined above, we need to ensure that those other factors are not confounding our analysis.

The conjoint design, in which researchers ask respondents to choose between two hypothetical candidates while randomizing certain characteristics of the environment and the candidates, present one effective way to identify our causal effects of interest (Hainmueller et al. 2014, 2015). 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. Unknown to the respondents, within the text of the vignette we randomly varied six characteristics of the candidates and the electoral environment: each candidate's gender (female or male), party affiliation (left party, right party, or independent),[11] corruption record (accused of taking bribes or praised for efforts to stamp out bribery in their administration), the information source for the corruption allegation (left or right newspaper, or judicial officials;

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[10] 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.

[11] In Argentina, the parties were Partido Justicialista (left) and Propuesta Republicana (right) and the newspapers were *Página 12* (left) and *La Nación* (right). In Chile, the parties were Nueva Mayoría (left) and Chile Vamos (right) and the newspapers were *La Nación* (left) and *El Mercurio* (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, the parties were the Frente Amplio (left) and the Partido Nacional (right) and the newspapers were *El País* (left) and *La República* (right).

applicable only when a candidate is accused of corruption), a potentially mitigating corruption factor (the corruption prevalence or the creation of construction jobs; also applicable only when a candidate is accused of corruption), and the state of the economy (improved or worsened since the last election; applicable only to the incumbent). We randomly varied each of these attributes independently for each of the two candidates, which allows us to simultaneously estimate the causal effect of each characteristic (Hainmueller et al. 2014).[12]

The text of the vignette presented to respondents in Argentina is below (the vignettes shown in Chile and Uruguay were nearly identical). The random variables and their values are enclosed in square brackets.

> 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 [<right party>/<left 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

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[12] Following Hainmueller et al. (2014), we conducted several diagnostic checks on our experiments. To check for *profile order effects*, we reran our analysis interacting each randomized characteristic with a variable indicating whether the candidate appeared first or second (see Table A5 in the Supplementary Appendix). Only the negative effect of candidate gender may in part be an artifact of profile order (the effect of the state of the economy is strongly driven by profile order, but this is by design, as it only applies to the incumbent). We also verified that the *random assignment* of vignette characteristics was successful by finding predominantly null results when regressing several respondent demographics (gender, age, and years of education) on the randomized vignette characteristics they received (see Table A6 in the Supplementary Appendix). 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.

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 [<right party>/<left party>/independent].[13] 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 we varied randomly in the experiment cover a wide range of factors that have been identified to shape the effect of corruption on voting. This is the second important advantage of the conjoint design. By randomizing 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).

After showing respondents the vignette with the two candidate profiles, we asked our key outcome question: "If you had to choose between these two candidates, who would you vote for?" While this outcome only represents a hypothetical vote choice and may not necessarily extend into real-world situations (Boas et al. 2017), others have shown that revealed choices in

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[13] In Chile, the last name Arias is less common, so we used Soto.

conjoint experiments can be informative of respondents' relevant preferences (Carnes and Lupu 2016; Chauchard et al. 2017; Hainmueller et al. 2015).[14]

Do Voters Sanction Corrupt Politicians?

Did citizens in Argentina, Chile and Uruguay sanction corruption? How much weight did they put on corruption relative to other common dimensions of vote choice? And did contextual factors like widespread corruption or corrupt side benefits mitigate any sanctioning of corruption? To answer these questions, we treated each hypothetical candidate in our experiments as a unique case (that is, there were two candidates for every respondent), following Hainmueller et al. (2014).[15] We then estimated ordinary least square models[16] relating respondents' hypothetical vote choices to indicators for corruption, source of corruption allegation, potential mitigating factors (corruption prevalence and corrupt jobs), as well as the state of the economy, candidates' gender, and party affiliation. We pooled estimates across the three countries, and also included country dummy variables in our specifications.

Figure 1 examines our basic expectation about corruption formulated in $H1$ – that respondents' support for a corrupt candidate should decline relative to a clean candidate. As discussed in the previous section, we also seek to put this effect into context by comparing it to the size of the effect of economic performance (for hypothetical incumbents only) and the extent of support for copartisan candidates relative to non-copartisans.

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[14] Since our experiment probes respondents' preferences on a sensitive topic, another concern is that their responses may exhibit social desirability bias. By varying a rich set of attributes and thus making it more difficult for respondents and interviewers to infer the key topics of interest, conjoint designs are well-suited to minimize such concerns (Hainmueller et al. 2014). Besides, as our results below reveal, our respondents refrained from sanctioning corruption in predictable ways.

[15] Our results were qualitatively similar when we treated elections rather than candidates as the unit of analysis. Consistent with our results in Figure 1, in hypothetical elections pitting a corrupt candidate against a clean candidate, respondents voted for a corrupt candidate only 11% of the time. Also, similar to our findings in Figure 2, respondents were considerably less inclined to sanction corrupt candidates when corruption brought jobs (support was at 29%), but not as much when corruption was presented as widespread (support was at 16%).

[16] Our results were substantively identical when we used logistic regression models instead of OLS, given that the vote choice is binary. See Table A4 in the Supplementary Appendix.

The top estimate in Figure 1 is strongly consistent with $H1$: corruption causes a large drop in respondents' vote probability, from about 53% for clean candidates to 18% for corrupt candidates, a drop of 35 percentage points (or a 65% reduction). The other two estimates, of the effects of the economy and copartisanship, respectively, are not surprising (and help increase our confidence in our research design). The poor state of the economy, relative to a good economy, also negatively affects respondents' probability of voting for a candidate, whereas belonging to the respondents' preferred party increases support for a candidate. All of these effects are statistically significant at $p < .05$.[17]

Figure 1: Corruption, Economy, and Partisanship as Determinants of Vote Choice
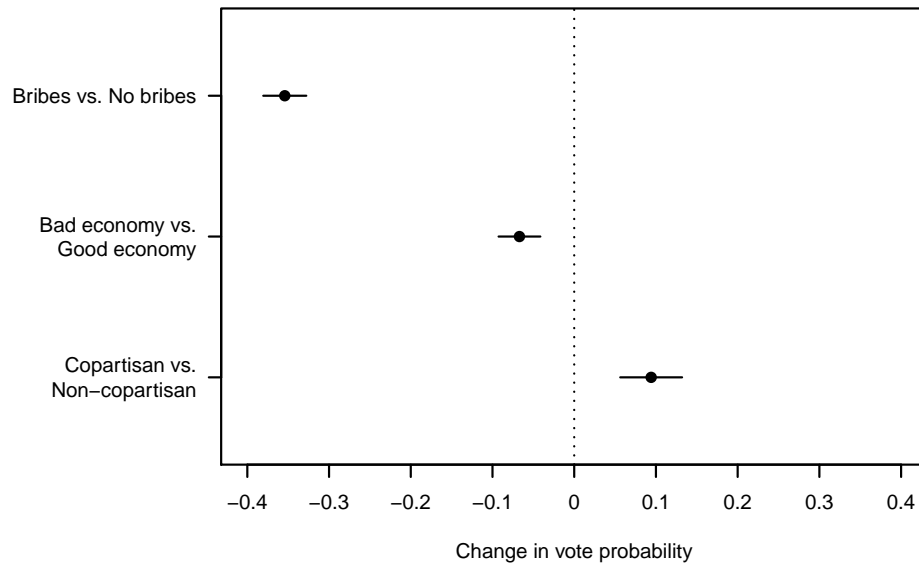
Values represent the difference in respondents' propensity for supporting a hypothetical candidate based on the conjoint vignette characteristics. Lines represent the 95% confidence interval estimated using standard errors clustered by respondent. Estimates are based on ordinary least squares regression models reported in Table A1 in the Supplementary Appendix.

What is striking in Figure 1, however, is how much larger the effect of corruption is relative to the effects of the economy and partisanship – about 5 and 3.5 times, respectively (in

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[17] 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 A7 the multiple-testing corrected significance levels based on the Benjamini-Hochberg procedure (Benjamini and Hochberg 1995). The interpretation remains the same for all but the gender treatment effect.

absolute terms). The smaller magnitude of the effect of partisanship is likely mainly a consequence of weak partisan attachments among citizens in the Southern Cone (Lupu 2016b), particularly in Argentina and Chile where the effect is close to zero and statistically null (see Figure A1 in the Supplementary Appendix, which replicates Figure 1 by country).[18] Moreover, the effect of corruption is pulled upward by a particularly strong effect in Uruguay (about 25% and 45% larger – and statistically significantly so – than in Argentina and Chile, respectively, see Figure A1), the least corrupt of the three countries.

This is consistent with some previous empirical evidence (Klašnja and Tucker 2013) and theoretical arguments that citizens in countries with better governance should also exhibit greater sensitivity to accountability failures (Ashworth et al. 2017; Klašnja et al. 2017). That said, our corruption effect is similar in magnitude to several other experimental studies from the region (Boas et al. 2017; Weitz-Shapiro and Winters 2013, 2017). What is novel, however, is how large the effect is relative to that of the economy, in contrast to previous findings (Klašnja and Tucker 2013). While probing this sharp contrast is beyond the scope of this paper, it is an interesting question for future research.

## Prevalence and Side Benefits as Mitigating Factors

Figure 1 clearly demonstrates the negative impact of corruption on respondents' support for a candidate. However, are there contextual circumstances in which this negative effect is mitigated? In Figure 2, we evaluate the evidence for hypothesis $H2a$ on the mitigating effect of corruption prevalence (and its alternative, $H2b$, on the absence of a mitigating effect of this contextual factor), and hypothesis $H3$, on the mitigating effect of corrupt side benefits. As benchmarks against which to compare the size of these mitigating effects, we also examine the

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[18] 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 developed democracies with stable patterns of partisanship. As a consequence, only 6% of our respondents are designated as copartisans of the hypothetical candidate they evaluated in our experiment.

extent of mitigation due to any partisan bias in corruption sanctioning, and due to respondents'
general justifiability of bribery.

Figure 2: Contextual and Individual Factors Mitigating the Effect of Corruption on Vote
Choice
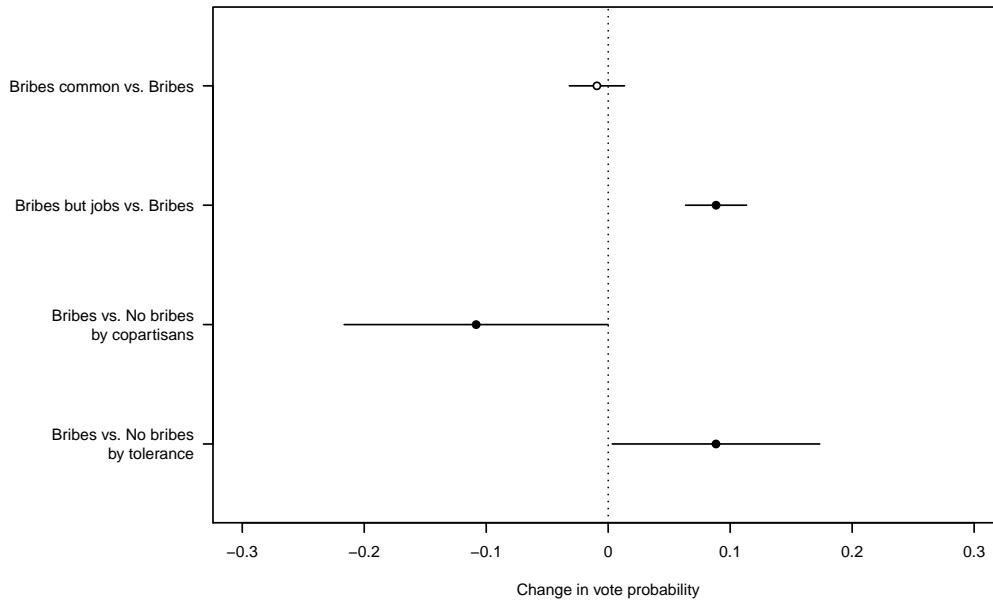
Values represent the difference in respondents' propensity for supporting a hypothetical
candidate based on the conjoint vignette characteristics. Lines represent the 95%
confidence interval estimated using standard errors clustered by respondent. Estimates
are based on ordinary least squares regression models reported in Table A2 in the
Supplementary Appendix. Black (gray) dots represent estimates significant at $p < .05$
($p < .1$), while hollow dots represent insignificant estimates.

There are several takeaways from Figure 2. First, informing respondents that corruption
was widespread in a candidate's province did nothing to mitigate the negative effect of corruption
(the top estimate in Figure 2). The effect is substantively very close to zero, and it is estimated
quite precisely. This evidence is thus inconsistent with $H2a$.[19] It is also inconsistent with $H2b$, in
that information on corruption prevalence does not increase the corruption sanctioning either. It is

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[19] Another, suggestive piece of evidence of the lack of mitigating effect of corruption prevalence comes from
country-by-country variation. Despite variation in the (perception of) prevalence of corruption across the three
countries (see footnote **??** above), we find no evidence of mitigation due to the corruption prevalence treatment in
any country. If anything, corruption prevalence somewhat increases the sanctioning of corruption in Argentina (by
around 4 percentage points, significant at $p < .075$), but leaves the effect of corruption unchanged in Chile and
Uruguay.

plausible that the two countervailing effects are cancelling each other out, leading to a zero net effect.

The second takeaway is that the mention of jobs created through corrupt means noticeably mitigates the negative effect of corruption on voter support. While the presence of such side benefits does not entirely eliminate the sanctioning of corruption, it reduces it by 25%, increasing the average support for a corrupt candidate from 18% to 27%.[20] This mitigating effect is present in all three countries, and to a very similar extent (there is no statistically significant difference across countries), bolstering our confidence of uncovering a general pattern.

The bottom two estimates in Figure 2 show the two often-studied individual-level mitigators: the potential partisan bias in corruption sanctioning, and the effect of individuals' predetermined tolerance of bribery. To measure tolerance for bribery, we use a question in the AmericasBarometer survey – asked before the experiment was presented – that measured the extent to which respondents find bribes justifiable.[21] In contrast to a number of previous studies (e.g. Anderson and Tverdova 2003; Anduiza et al. 2013; Solaz et al. 2017), we do not observe copartisan bias in the propensity to punish corrupt politicians; quite the opposite: the respondents in our experiment were on average *more* likely to punish corrupt candidates from the party they feel the closest to than from other parties (by about ten percentage points).[22] This result is less surprising, however, in light of the fact, discussed above, that citizens' partisan attachments are extremely weak in our three countries.[23] On the other hand, in line with the arguments made by

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[20] Note that Figure 2 shows a positive marginal effect because it is comparing the effect of "bribes but jobs" to just bribes. The average level of support for a candidate in the bribes but jobs condition is therefore bigger than the only bribes condition (27% vs. 18%), but not nearly as large as the support for a candidate in the "no bribes" condition (53%).

[21] The question was, "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").

[22] This is mainly driven by identifiers with right parties and by Uruguayans. This suggests that right partisanship in Argentina and Chile is stronger than left partisanship (in terms of its effects on behavior), which seems reasonable since left parties have in recent decades been far less coherent and cohesive in those countries.

[23] We get a similar (substantively larger, but statistically weaker) result when focusing on the effect of the bad state of the economy, also in contrast to other studies finding partisan bias in economic evaluations (see e.g. Anderson 2007; Tucker 2006).

previous studies (e.g. Barr and Serra 2010; Ludwin-Peery and Tingley 2014; Simpser 2017), those who find bribes justifiable are on average less likely to punish corrupt politicians.

The third takeaway from Figure 2 is that the extent of mitigation produced by corrupt side benefits is sizeable, when measured against – and alongside – other potential mitigating factors. Namely, the mitigating effect of "corrupt but jobs" is virtually identical in size as that arising from individuals' tolerance of bribery. It is important to note, however, that one mitigating factor is not compounded by the other: when the corrupt jobs treatment is interacted with individuals' bribe tolerance, the extent of mitigation is more than doubled (the interaction term is significant at $p < .025$), such that on average the support for a corrupt candidate rises from 17% when neither factor is present, to between 23% and 25% when either factor is present alone, to 39% when both factors are present together.

Conditions Amplifying the Mitigating Effect of Side Benefits

Given the relative – and absolute – importance of corrupt side benefits in mitigating the extent of corruption sanctioning, we explore several conditions that may plausibly make this type of mitigation particularly pronounced. The results are collected in Figure 3, and we proceed by discussing the logic and the findings for each test in turn.

First, in our vignette, the corrupt side benefit is in the form of construction jobs, which are usually performed by individuals with lower education and wealth.[24] It is plausible that such respondents may be more willing to trade off corruption for jobs than other respondents.[25] We find some – though not very strong – evidence in support of this expectation, as seen in the top two estimates in Figure 3. The mitigation of corruption sanctioning due to corrupt construction jobs is somewhat greater for both lower-educated respondents (top estimate) and those with lower wealth (second from the top), by five and four percentage points, respectively (both effects are

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[24] Unfortunately, we do not have information on respondents' occupation that would have allowed us to examine in more detail the heterogeneous effects across occupation groups.

[25] This expectation is similar to the observation that clientelist benefits are mainly targeted toward poor and lower-skilled citizens (Stokes 2005).

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Figure 3: What Conditions Amplify the Mitigating Effect of Corrupt Side Benefits?
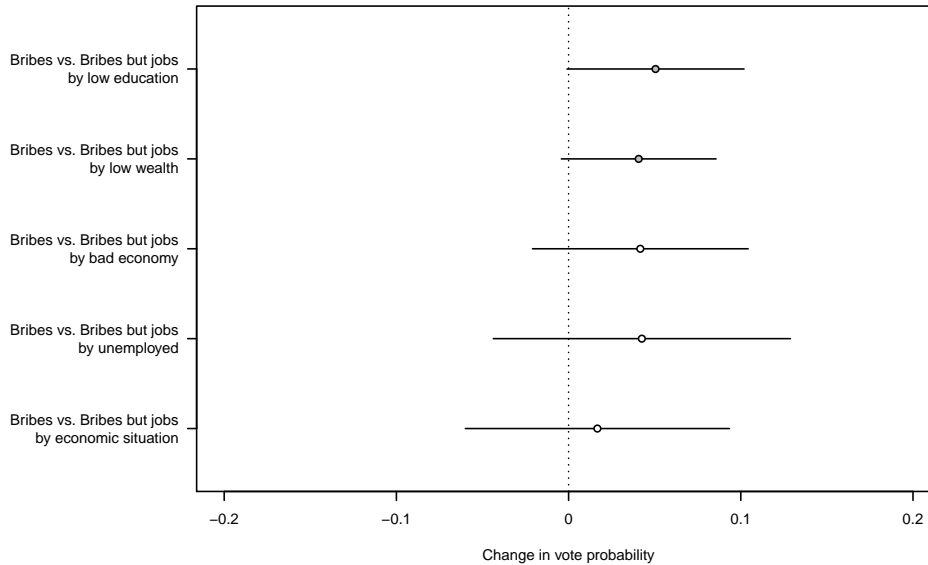
Values represent the difference in respondents' propensity for supporting a hypothetical candidate based on the conjoint vignette characteristics. Lines represent the 95% confidence interval estimated using standard errors clustered by respondent. Estimates are based on ordinary least squares regression models reported in Table A3 in the Supplementary Appendix. Black (gray) dots represent estimates significant at $p < .05$ ($p < .1$), while hollow dots represent insignificant estimates.

significant at $p < .1$, as indicated by gray dots).[26] For example, the average support for a corrupt candidate who provided jobs is 22% among the more highly educated, and 27% among the lower-educated respondents. Both of these effects persist even when controlling for the other characteristic, suggesting that both education and wealth carry explanatory power.

It is further possible that new jobs are particularly valuable during economic downturns, even if secured through projects that benefited politicians corruptly. This suggests that the mitigating effect of jobs created corruptly may be stronger when the state of the economy is poor than when it is good. As discussed above, we randomized the state of the economy in our conjoint experiment, allowing us to explore this interaction causally. While the third estimate in Figure 3 is indeed positive (and of similar magnitude as the effect of respondents' education and wealth),

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[26] We define respondents as having low education if they completed less than secondary school, and with lower wealth if they are in one of the bottom three quintiles of the wealth distribution. Our measure of wealth is the score based on the factor analysis of 13 items capturing ownership of a number of consumer goods and assets by a respondent's household (such as a cellular phone or a vehicle).

suggesting a greater mitigating effect of corrupt jobs in a poor economy than in a good economy, it is however statistically imprecise.

Another possibility, related to the previous argument but evaluated at a more micro level, is that citizens in a precarious economic situation are more willing to forgive corruption when it brings about jobs than respondents living in more stable economic circumstances. The bottom two estimates in Figure 3 examine this possibility with two different measures of precariousness: being unemployed, and the extent to which income covers one's basic needs.[27] As with the state of the economy, we do not find precise evidence consistent with these expectations (the point estimate for being unemployed is similar in size to that of low education, but not statistically significant).

What is particular interesting about the findings in Figure 3 is that they seem to fit with both egotropic and sociotropic approaches to economic (Kinder and Kiewiet 1981; MacKuen et al. 1992) and corruption (Klašnja et al. 2016) voting. On the one hand, we do see evidence that people who are (potentially) more likely to benefit from the availability of construction jobs are more likely to mitigate their negative electoral response to corruption, which would be a very pocketbook type of concern. On the other hand, even the statistically significant mediators do not account for all of the mitigating effect; overall, the findings in Figure 3 suggest that the mitigating effect of corrupt side benefits is quite broad, applying to a variety of scenarios and respondents.[28] This suggests that even those who are not likely to directly benefit from the "jobs" part of the corruption trade-off are still willing to at least somewhat mitigate their opposition to the corrupt candidate, suggesting that some form of societal benefit is being rewarded, or, more precisely, leading to a weaker punishment. Together with the observation that we find this mitigating effect in all three countries, we conclude that it is a plausibly general phenomenon.

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[27] The latter is based on the following question in the AmericasBarometer survey: "The salary that you receive and total household income... (1) Is good enough for you and you can save from it, (2) Is just enough for you, so that you do not have major problems, (3) Is not enough for you and you are stretched, (4) Is not enough for you and you are having a hard time." We code respondents as being in a precarious economic situation if they indicated category (4), chosen by 10.1% of the sample.

[28] While the expectations are theoretically less clear, we similarly do not find heterogeneous effects based on the source of the corruption allegation, or the gender or party affiliation of the candidate.

Conclusion

Studying the effect of corruption on voter behavior is challenging. Observational studies suffer from well-known problems of identification, especially since more popular incumbents may be more likely to engage in electorally risky behavior like corruption. Experimental studies of the factors affecting corruption voting, on the other hand, have so far focused on one hypothesized variable at a time. This too is limited by confounding: if we study, for instance, the effect of corruption alone, we may not see how it is conditioned by factors like partisanship. Moreover, we would not have a benchmark against which to evaluate the corruption effect; is the effect of corruption small or large compared to other known influences on vote choice?

Our conjoint experimental design allows us to address these limitations. We find that corruption accusations indeed decrease support for candidates, much more so than economic performance or partisanship. We also find that informing respondents that corruption is widespread does not help to mitigate the corruption sanction. However, candidates accused of corruption who brought jobs to their constituency are punished substantially less, especially by citizens with lower socioeconomic status.

Our findings are based on three countries in Latin America's Southern Cone. But the consistency of our findings across these three cases is striking. Although they are similar on many institutional and demographic dimensions, they vary considerably in terms of the extent (and public perceptions) of corruption. And yet, in all three of these cases, citizens appear to sanction corruption the same way. This suggests that our findings ought to generalize beyond these individual countries.

Nevertheless, there are doubtless scope conditions on our inferences from the Southern Cone. For one, self-reported partisanship in our cases was low by comparison to rates typical in many developed democracies. This doubtless explains why we do not see partisanship mitigating much of the corruption sanction in our cases. Of course, the only way to know would be to replicate similar conjoint experiments in developed democracies, and this ought to be a priority for future studies of corruption voting. Our study has provided the cleanest evidence to date that

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side benefits and not corruption prevalence mitigate corruption sanctioning, but it is crucial to know whether this obtains elsewhere. An ideal design would be to include a conjoint experiment like ours on a large cross-national survey project.

Future studies might also leverage more of the findings of our own experiment. Given our focus on factors that mitigate corruption sanctioning, 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, the side benefit we focused on in our experiment is construction jobs, but one could imagine incumbent corruption producing other kinds of side benefits instead. Future conjoint experiments could compare the effects of different potential side benefits. The design also extends easily to including comparisons with other candidate characteristics, such as race or class, or policy platforms.

These kinds of experiments are an important step toward understanding when voters will sanction corrupt politicians. Around the world, voters continually reelect incumbents known to be involved in corruption, maintaining bad equilibria in which corruption goes unpunished. If we are to break the vicious cycles of corruption and impunity, we need to understand how corruption voting works, and when voters will be moved to sanction corrupt representatives. Studies like this are an important step in this direction.

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