

# In-Group Loyalty and the Punishment of Corruption

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## Abstract

This study suggests that in-group loyalty, defined as the degree to which people favor their own group over others, undermines the punishment of corruption. We present evidence from two studies. First, we utilize a real-world corruption scandal involving the ruling party in Spain that broke during survey fieldwork. People exposed to the scandal withhold support from the incumbent, but in-group loyalty based on partisanship weakens this effect. Second, we explore in-group loyalty beyond partisanship through laboratory experiments. These experiments artificially induce group identities, randomly assign the group identity of candidates and shut down any instrumental benefits of in-group loyalty. The experimental evidence suggests that people support corrupt candidates as long as they share a group identity and are willing to sacrifice material payoffs to do so. Our findings have important implications. Most importantly perhaps, they suggest that candidates can get away with corruption by engaging in identity politics.

## Keywords

corruption and patronage, elections, public opinion, and voting behavior, European politics

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## Introduction

Corruption, commonly defined as the misuse of public office for private gain, is a global phenomenon.<sup>1</sup> Transparency International, a nongovernmental organization dedicated to combating corruption by providing free and easily accessible information, estimates that 6 out of 7.4 billion people currently live in countries with a serious corruption problem.<sup>2</sup> The same organization, based on results from its 2013 corruption barometer, suggests that people across the globe view corruption as a serious problem and generally disapprove of it (Hardoon & Heinrich, 2013). While free, fair and periodic elections are thus expected to curb corruption, empirical tests of this expectation have produced inconclusive results (e.g., De Vries & Solaz, 2017; Fisman & Golden, 2017; Mares & Young, 2016). Some evidence suggests that corrupt activities take a considerable electoral toll on incumbents (e.g., Fackler & Lin, 1995; Ferraz & Finan, 2008; Klačnja, Tucker, & Deegan-Krause, 2016; Krause & Méndez, 2009; Winters & Weitz-Shapiro, 2013), while other work demonstrates that people often reelect politicians they know to be corrupt, not only in developing democracies (Carlin, Love, & Martinez-Gallardo, 2015a; Manzetti & Wilson, 2007; Zechmeister & Zizumbo-Colunga, 2013) but also in more established ones (e.g., Chang, Golden, & Hill, 2010; Reed, 1996; Rundquist, Strom, & Peters, 1977; Vivyan, Wagner, & Tarlov, 2012). This raises the question of why corruption often goes unpunished.

A wealth of studies aims to address this question by focusing on institutional characteristics, such as the electoral system, district magnitude, or clarity of responsibility (e.g., Chang & Golden, 2007; Kunicová, Rose-Ackerman, 2005; Persson, Tabellini, & Trebbi, 2003; Tavits, 2007). These studies implicitly assume that people would punish corruption if they could. Recent work challenges this assumption by demonstrating that people often discard corruption information based on the source or the degree to which the source is perceived as credible (Botero, Cornejo, Gamboa, Pavao, & Nickerson, 2015; Klačnja, 2017; Weitz-Shapiro & Winters, 2017). We extend this work beyond information by focusing on the role of social group identities. Specifically, we suggest that in-group loyalty, broadly defined as the degree to which people favor their own group over others, undermines the punishment of corruption. The claim that group identities undermine the accountability for corruption is not new. On the contrary, it is a well-established finding in the literature that voters' partisan allegiances mitigate the effect of corruption on electoral decision-making (Anderson & Tverdova, 2003; Anduiza, Gallego, & Munoz, 2013; Ecker et al., 2016). The novelty of our contribution lies in the fact that (a) we extend the idea beyond partisanship to suggest that in-group loyalty more generally hampers the punishment of corruption, and (b)

we suggest that in-group effects can be expressive in nature and persist even when no instrumental benefits are provided.

Extending the focus to in-group loyalty more generally helps us understand the lack of punishment of corruption in contexts where partisanship is less important. Party systems in developing democracies are often less institutionalized, party labels are less informative and other group identities are more salient (Chandra, 2007; Lupu, 2016; Stokes, 2001). This study suggests that loyalty based on other group identities, such as language, ethnicity, or religion, may go a long way in explaining why corruption may often go unpunished in these contexts. Moreover, by rooting our expectation in social identity theory we shed more light on the mechanism that drives the relationship between in-group loyalty and the lack of punishment of corruption. Existing work focusing on partisanship suggests that instrumental calculations are key. Voters trade their support for a corrupt candidate against preferred policies (Myerson, 1993; Rundquist et al., 1977) or economic performance (Carlin et al., 2015a; Carlin, Love, & Martinez-Gallardo, 2015b; Manzetti & Wilson, 2007; Zechmeister & Zizumbo-Colunga, 2013). We consider the possibility that in-group loyalty may still hamper accountability for corruption even when no such instrumental benefits are provided. Taking our cue from social identity theory (Tajfel, 1981) and notions of expressive partisanship (Huddy, Mason, & Aarøe, 2015) we suggest that people may support a corrupt candidate simply because they share the same group identity. If people gain some positive utility from supporting someone from their in-group simply because doing so allows them to protect or advance their group status, this can easily offset any possible losses based on candidate corruption.

Exploring whether in-group loyalty weakens the punishment of corruption in absence of instrumental benefits is also important from a normative viewpoint. If in-group loyalty weakens the punishment of corruption because of instrumental calculations, such behavior might not be normatively undesirable, but if voters fail to punish corruption because of knee-jerk expressive reasons, this behavior might be socially undesirable from a viewpoint of normative democratic theory. Furthermore, if corruption is tolerated due to expressive concerns, the costs of corruption for candidates might decrease as a result because no instrumental benefits need to be provided to garner support of in-groups.

We explore the relationship between in-group loyalty and the punishment of corruption by presenting evidence from two studies: one based on observational data from Spain (Study 1), and one based on laboratory experiments (Study 2) conducted in the United Kingdom. In Study 1, we compare voter reactions to a major corruption scandal, the Bárcenas scandal, involving the incumbent party in Spain and examine how in-group

loyalty based on partisanship conditions these reactions. The results show that in-group loyalty weakens the punishment of corruption, while out-group loyalty increases it. Exploiting the scandal in Spain allows us to causally identify how corruption information affected incumbent support and how this is conditioned by in-group loyalty based on partisanship in a real-world setting. As such, we contribute to both the internal and external validity of existing work on partisan effects that is either based on observational data, potentially suffering from endogeneity, or on survey experiments using hypothetical scenarios that might lack credibility.

Exploiting a naturally occurring scandal does not allow us to determine whether in-group loyalty weakens the punishment of corruption because of instrumental calculations or expressive reasons however. To examine whether in-group loyalty is based on expressive considerations, we turn to the laboratory (Study 2). This experimental setting allows us to carefully control the setting in which voters make decisions, artificially induce group identity, and randomly assign the group identity of the candidates voters face. Specifically, we employ a minimal group paradigm and create groups based on people's preferences for painters, Klee or Kandinsky (Tajfel, 1970). In doing so, we join a growing group of political scientists who employ experimental games to study political behavior (e.g., Duch, Przepiorka, & Stevenson, 2015; Habyarimana, Humphreys, Posner, & Weinstein, 2007; Huber, Hill, & Lenz, 2012; Landa & Duell, 2015). We designed our experiments in such a way that participants (a) were informed about candidate corruption, (b) could not update their group identity in line with corruption information, (c) could not infer any policy positions from the group identity of candidates, and (d) could not monetarily gain from candidate corruption, in fact they automatically lose as a result of it. This arguably constitutes a hard test for our expressive in-group loyalty explanation. To foreshadow, our experimental evidence suggests that voters support candidates they know to be corrupt as long as they share a group identity and are even willing to sacrifice material payoffs to do so. These findings are important as they suggest that voters reelect a corrupt candidate not only to gain policy or other benefits that they would not have received otherwise but also because of their expressive in-group loyalty. These results dovetail with existing work regarding the punishment of unethical behavior in psychology and experimental economics as well as on group-serving biases in retrospective voting in political science (e.g., Bazerman & Gino, 2012; Healy & Malhotra, 2013).

These findings have important implications for our understanding of the punishment of corruption. First, they suggest that people will be relatively immune to corruption allegations involving their own group. This overall does not bode well for the idea that elections can curb corruption and

suggests that other institutional solutions, such as the strengthening of the judiciary, might be more appropriate (Lederman, Loayza, & Soares, 2005). Second, due to the fact that our experiments are based on a minimal group paradigm, not only partisanship but also people's loyalties to social groups more generally hamper the punishment of corruption. This is important because partisanship has been shown to be less influential in shaping political behavior in developing democracies (Lupu, 2016; Stokes, 2001). In such contexts, in-group loyalty based on other social group identities may still hamper the punishment of corruption (e.g., Banerjee & Pande, 2007). Finally, the findings presented here also inform our understanding of candidate behavior. When voters overlook corruption only in exchange for policy benefits, candidates have to incur some costs for corruption by credibly committing to their policy pledges. Yet the key insight of this study is that even in the absence of these benefits voters can be tolerant of corruption provided that the incumbent belongs to their in-group. This suggests that the costs of political malfeasance might be much lower than previously assumed as candidates can get away with corruption through identity-based mobilization rather than by having to rely on costly programmatic or clientelistic strategies.

We proceed as follows. First, we review the literature and highlight our theoretical contribution. Second, we outline the research design and present the results of our first study utilizing a recent corruption scandal in Spain. In a third step, we turn to the design and the results of our second study based on laboratory experiments. Finally, we conclude by outlining the implications of our findings for the study of the relationship between corruption and electoral accountability more generally.

## **In-Group Loyalty, Accountability, and Corruption**

Existing work suggests that the effective electoral punishment of corruption is not commonplace (De Vries & Solaz, 2017; Fisman & Golden, 2017). Why might voters punish corruption some of the time, while they condone it at other times? An important answer to this question stresses the role of voters' partisan allegiances. A study by Anderson and Tverdova (2003), for example, uses survey data from over 16 countries to show that while citizens in countries with higher levels of corruption overall display more negative evaluations of the political system, this negative effect is much less pronounced among government supporters. More recent work by Ecker and colleagues (2015) relying on survey data from 20 parliamentary elections in Western and Central Eastern Europe demonstrates that support for incumbent parties is lower among voters who perceive corruption to be high, but that these effects are much smaller among government partisans. Survey experimental work by

Anduiza and colleagues (2013) echoes these results. The authors find that the use of partisan labels affects voters' judgments of which acts were considered to constitute corruption or not. Actions taken by the in-party were viewed as less corrupt when party labels were provided. Moreover, these partisan effects were most pronounced among citizens with lower levels of political sophistication who were less likely to view the actions of their own party as corrupt (e.g., Klašnja, 2017; Weitz-Shapiro & Winters, 2017). The authors argue that these effects are due to the fact that voters either doubt the credibility of corruption information or attach little importance to it. This is because they care more about other benefits that the party provides, such as issue congruence or economic performance.

Perceived in this way, partisan effects are largely consistent with classical models regarding the lack of punishment of corruption that stress instrumental calculations. An experimental study by Rundquist and colleagues already in 1977 demonstrated that when it comes to corruption people engage in an implicit trade (e.g., Myerson, 1993).<sup>3</sup> Although people may generally disapprove of corrupt conduct, they need to weigh their corruption evaluations against policy benefits, and this may result in an overall positive balance in favor of reelection. As a result, voters might prefer a corrupt candidate who represents their policy positions to a clean candidate who does not (see also Muñoz, Anduiza, & Gallego, 2016; Peters & Welch, 1980; Welch & Hibbing, 1997). The same mechanism applies to economic performance. Carlin et al. (2015a) and Zechmeister and Zizumbo-Colunga (2013) show, for instance, that Latin American presidents are less likely to be punished for corruption and are more likely to survive political scandals when the economy performs well (e.g., Carlin et al., 2015b; Manzetti & Wilson, 2007b). This type of explanation again fits with an instrumental understanding of partisanship. Understood in this way, partisanship acts like a running tally of party performance, party's ideological stances, and voter's proximity to the party in terms of preferred policies (e.g., Abramowitz, 2010; Bafumi & Shapiro, 2009; Fiorina, 1981).

In addition to this instrumental view, scholars have understood partisanship as expressive in nature based on a social group identity (Green, Palmquist, & Schickler, 2002; Huddy et al., 2015). This expressive understanding of partisanship has its roots in social identity theory (Tajfel & Turner, 1979; Tajfel 1981).<sup>4</sup> The basic idea here is that people tend to perceive themselves and others through categories such as ethnicity, language, religion, gender, partisanship, or other forms of group division and exhibit strong psychological motivations to endorse existing group membership. Turner, Hogg, Oakes, Reicher, and Wetherell (1987, p. 42) have described this motivation as a need among group members "to differentiate their own groups positively from

others to achieve a positive social identity.” Individuals derive psychological benefits from affiliating themselves with social groups, such as status, honor, prestige, or increased self-esteem (e.g., Akerlof & Kranton, 2000). People’s in-group loyalty gives rise to a desire to positively distinguish the group from others and to advance or protect in-group status (Tajfel, 1981).<sup>5</sup> Especially within competitive environments, such as the political competition over scarce public resources in elections, in-group loyalty may even bring about out-group hostility. Out-group hostility and in-group loyalty become closely linked under perceived group threat because group defense is a key facet of cooperation for which identities developed in the first place (Brewer & Caporael, 2006).

Evidence from experimental economics and psychology stresses the importance of in-group loyalty, especially in the context of punishment of unethical behavior. Bernhard, Fischbacher, and Fehr (2006), utilizing lab-in-the-field experiments with two small and cohesive tribes in Papua New Guinea,<sup>6</sup> for example, demonstrate that social norm violators who belong to the punisher’s group are punished less than those who do not belong to the punisher’s group. These behavioral differences in punishment of antisocial behavior are also reflected in the beliefs potential norm violators have about possible punishment (Bernhard et al., 2006, pp. 219-221). Since then a series of studies conducted in a variety of other country contexts show that in-group loyalty increases people’s willingness to condone unethical behavior (Gino, Gu, & Zhong, 2009; Wright, Dinsmore, & Kellaris, 2013). A recent experimental study by Hildreth and colleagues (2016) suggests that these in-group loyalty effects are much more pronounced in competitive environments because people wish their in-group to win. This result echoes classical findings based on group-threat that suggest that threats emerging from competition over scarce resources foster negative attitudes toward the out-group (Blalock, 1967).

There are good reasons to believe that in-group loyalty based on expressive considerations also matters for the punishment of corruption. Corruption allegations pose a serious threat to in-group status and the in-group’s hold on power. When it comes to electoral behavior, classical and contemporary work suggests that voters assign an important role to social identities, based on ethnicity, language, religion or ideology, when evaluating candidates and deciding who to vote for (e.g., Abrajano & Alvarez, 2005; Campbell et al., 1960; Chandra, 2007; Lazarsfeld, Berelson, & Gaudet, 1944; Schnakenberg, 2014). More specifically, recent experimental work by Landa and Duell (2015) demonstrates the importance of group identity for electoral accountability. The authors conduct laboratory experiments to show that voters who share a group affiliation with a candidate are more willing to ignore both

effort and competence in reelection decisions compared with when they are faced with an out-group candidate. Finally, studies examining prosocial preferences, such as intergroup trust, have shown that in-group loyalty effects increase with political competition (Carlin & Love, 2016; Rand et al., 2009). When we apply the notion of in-group loyalty to the punishment of corruption, we expect that, although voters may generally disapprove of corruption, during an election when different groups compete over scarce resources, their in-group loyalty to candidates trumps their disapproval of corruption even in the absence of instrumental benefits. If people derive some positive utility from supporting an in-group candidate because doing so allows them to protect or advance their group status, this can compensate for any potential losses due to candidate corruption. We would expect to find little electoral punishment of corruption among those voters who share a group identity with a candidate, while the same candidate will be punished for an identical indiscretion by out-group voters.<sup>7</sup>

Based on the extensive literature highlighting the important role of expressive group identity in political behavior and social preference formation, we suggest that there might be two channels through which in-group loyalty is expected to weaken the punishment of corruption: *in-group serving biases* and *in-group status*. Retrospective voting studies, largely inspired by social group identity theory, suggest that voters have strong in-group serving biases when it comes to performance evaluations (Evans & Andersen, 2006; Healy & Malhotra, 2013; Rudolph, 2003). While voters tend to reward the in-group for positive outcomes, they absolve it of blame for negative outcomes. The notion of in-group serving biases also fits with motivated reasoning approaches (Taber & Lodge, 2006) that outline a process of selective information processing driven by automatic affective ties, in this case based on in-groups. Work on in-group status, stemming mostly from behavioral economics, suggests that people have social preferences such that they gain some utility from the utility of others, particularly members of their own in-group (for an overview, Charness & Rabin, 2002 of such models). Chen and Li (2009) for example show that social preferences are higher when individuals are paired with members of their in-group. Moreover, Shayo (2009), in his work on group identity and attitudes toward redistribution, suggests that individuals derive utility from having wealthy, high-status group members and that this might explain low levels of support for redistribution among poor groups. If we extend Shayo's insights to wealth obtained through corruption, this would imply that voters gain some utility from the fact that an in-group candidate benefits from corruption even if they personally do not benefit or even lose out. Based on the idea of in-group serving biases, we would expect people to discount the information they receive about corruption of an in-group candidate, while based on the notion of in-group status, we would

expect voters to even reward corruption of their in-group candidate. If in-group loyalty is largely based on in-group serving biases, we would expect to find *no effect* of corruption on support for an in-group candidate, while when it is based on in-group status, we would expect a *positive effect* of corruption on support for an in-group candidate.

## Evidence From Two Studies

Examining the relationship between in-group loyalty, corruption, and accountability is far from straightforward. Survey data suffer from social desirability bias. Respondents are not likely to admit that they approve of corrupt activities. Cross-national data on corruption and election results suffer from endogeneity. Publicly elected officials often have the ability to redesign electoral rules in such a way that favor corrupt activities, and electoral volatility might cause corruption by reducing people's time horizons and their incentives to be honest. Moreover, aggregate observational studies make it hard to discern why individual voters reward or penalize corruption, at least partly due to ecological inference problems. To circumvent these issues, we aim to isolate the effect of in-group loyalty on the electoral punishment of corruption by combining observational data from Spain with laboratory experiments from the United Kingdom.

In our first study, we utilize the fact that a major corruption scandal in Spain involving the incumbent party was released during the fieldwork of the Spanish module of the sixth round of the European Social Survey (ESS).<sup>8</sup> This allows us to compare the reactions of those who were interviewed before and after the scandal broke (see also Ares & Hernández, 2017). Moreover, we can explore the effect of in-group loyalty by examining whether those who identify with the government party react differently compared with opposition partisans. This setup aids us to overcome some of the limitations that arise from studying the relationship between in-group loyalty based on partisanship, corruption, and accountability by correlating people's survey responses about corruption with their vote intentions. Our first study is important as it allows us to isolate the causal effect of corruption and the role in-group loyalty plays in a real-world setting, but it also comes with limitations. First, we cannot observe an identical corruption scandal involving the opposition party so we cannot rule out that government identifiers are just overall less likely to punish corruption. Second, to be certain that the treatment and control group are fully comparable we would ideally have a measure of in-group loyalty, here party identification, for all respondents before and after the scandal. The fact that this information is not available, could introduce measurement error that is related to the treatment. Third, and perhaps most importantly for our endeavor here, is that we cannot rule out the possibility that a possible in-group loyalty effect is based on instrumental concerns. A lack of punishment of the government party may be because in-partisans expect benefits.<sup>9</sup>

To tackle these issues and to isolate the expressive aspect of in-group loyalty, we follow the lead of a growing group of political scientists who employ experimental games to study the precise causal mechanism underlying individual-level electoral decision-making (e.g., Huber et al., 2012; Landa & Duell, 2015). What is more, the use of laboratory experiments is particularly useful when studying corruption. If a researcher aims to study corrupt behavior or its effects, problems arise because the subject of study is carefully hidden from the researcher's eyes (Olken, 2009). To tackle this, social scientists have recently begun to use alternative approaches to gather empirical data on corruption, or unethical behavior more generally, by means of laboratory experiments (for overviews, see Abbink, 2006; Bazerman & Gino, 2012). Comparisons between laboratory and field experiments on corruption suggest that laboratory results are remarkably externally valid (Armantier & Boly, 2008; Englmaier & Gebhardt, 2016). In laboratory experiments, it is possible to create analogous, although stylized, environments that mimic real-life corruption scenarios, and to obtain data in a controlled manner. In political economy experiments like ours, it is also common practice to reward subjects in proportion to the payoffs they have achieved in the experiment (Levitt & List, 2007). This feature ensures that subjects have proper incentives to maximize their payoffs and to make careful decisions. It creates the right set of incentives to observe their actual behavior rather than stated preferences as measured through surveys, which is a rarity when studying the relationship between corruption and accountability. Given that we know that people's preferences about the punishment of corruption diverge greatly from their actual behavior relying on revealed rather than stated preferences is crucially important. Finally, laboratory experiments have proven to be a useful tool for exploring in-group effects (Lane, 2016). In our laboratory experiments, we are able to keep the information and benefits voters receive constant, isolate the effect of in-group loyalty, and exclude instrumental trade-offs between candidates and voters. We are also able to examine the extent to which voters punish in-group and out-group candidates for the *identical* corrupt act while keeping everything else constant. We now proceed to detail the specific empirical procedures used in each study and discuss the results for each study in turn.

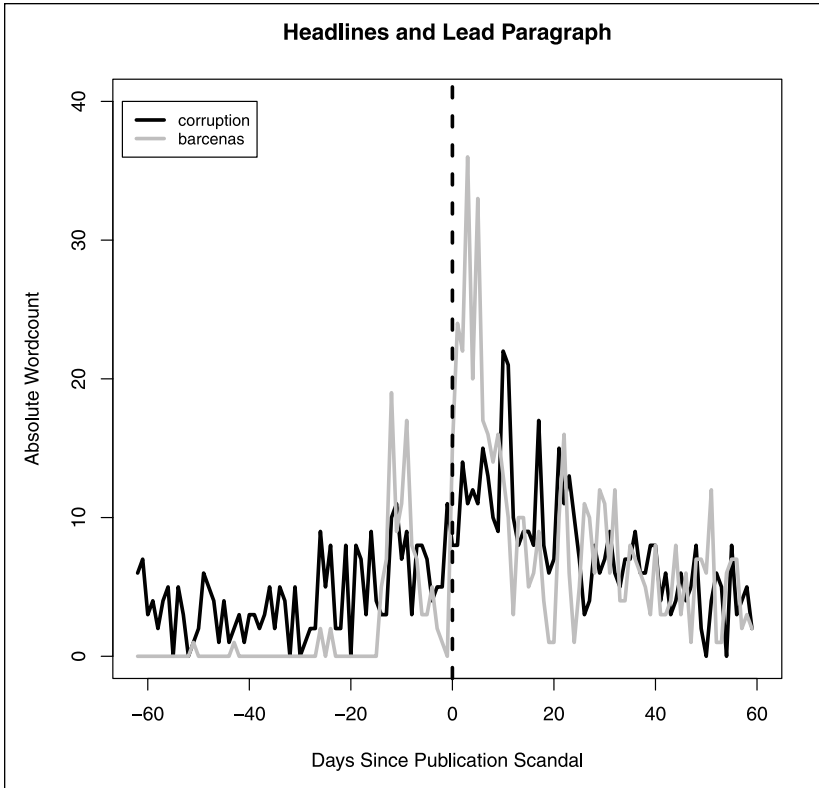
## Results From Study 1: The Bárcenas Scandal in Spain

In our first study, we utilize the fact that a major corruption scandal, the Bárcenas scandal, involving the incumbent party, the Partido Popular (PP), in Spain was released during the fieldwork of the Spanish module of the sixth round of the ESS.<sup>10</sup> The scandal uncovered widespread corruption, namely, the widespread misappropriation of funds, by many high-ranking politicians in the PP including

the current prime minister, Mariano Rajoy, and the previous prime minister José María Aznar. On January 31, 2013, internal party documents handwritten by the party treasurer, Luis Bárcenas, were leaked to the most widely circulated Spanish newspaper, *El País*. The documents revealed a parallel bookkeeping system operated by the party for decades (1980-2009) that recorded undeclared and illegal cash donations to Swiss bank accounts. These accounts involved many high-ranking PP officials including current and past prime ministers, and were used to pay them bonuses as well as cover daily party expenses. The publication of the so-called Bárcenas papers, *los papeles de Bárcenas*, triggered widespread and long-lasting news coverage in Spanish and international media. Figure 1 provides the word counts of the terms *corruption* and *Bárcenas* that feature in the headline or lead paragraphs of all articles published in *El País* each day for a time period of 60 days before and after the scandal broke on January 31, 2013. The data are based on a Lexis-Nexis search. The latter date is marked by the dotted vertical line in Figure 1. The word counts show a clear peak in mentions of “Bárcenas” and to a lesser extent also of “corruption” directly after the scandal broke. This suggests that the scandal led to a significant shift in media attention in the immediate aftermath of the publication of the papers, but that the attention faded after about 2 weeks. The scandal left a permanent mark on Spanish politics. On November 22, 2013, a High Court judge investigating PP’s finances argued that circumstantial evidence of a parallel accounting system existed, and passed on this information to the Court of Auditors.<sup>11</sup> Since then, Luis Bárcenas is serving a jail sentence,<sup>12</sup> several other high-ranking party officials have admitted to wrongdoing,<sup>13</sup> and the PP was condemned for using misappropriated funds to renovate their party headquarters.<sup>14</sup>

The Bárcenas papers were published in the midst of the Spanish fieldwork for the sixth round of the ESS that started on January 23, 2013.<sup>15</sup> This allows us to compare the reactions of those who were interviewed before and after the papers were published.<sup>16</sup> The date respondents were interviewed serves as the variable that assigns individuals to the treatment or control group. Respondents who were interviewed before January 31, 2013, the date the papers were released, were assigned to the control group and those interviewed after are assigned to the treatment group. Formally, the treatment indicator can be defined as:

$$T_i = \begin{cases} 0 & \text{if observation } i \text{ received the control,} \\ & \text{i.e., was interviewed before January 31, 2013} \\ 1 & \text{if observation } i \text{ received the treatment,} \\ & \text{i.e., was interviewed after January 31, 2013.} \end{cases}$$



**Figure 1.** Word counts of the terms *Bárcenas* and *corruption* in El País before and after the scandal.

We restrict the treatment period to 2 weeks ending on February 14, 2013, as the data presented in Figure 1 suggest that after that date the media attention to the scandal faded. To test the robustness of our findings, Tables B.1-2 in the Supplemental Information present results based on alternative cutoff points in time, 1 and 3 weeks, and show that the results are very similar. The ESS questionnaire does not include a vote intention question so we operationalize incumbent support by relying on a government approval item (for question wordings and descriptive statistics of all variables used in the analysis, see Tables A.1-2 of the Supplemental Information).<sup>17</sup> We capture the conditioning effect of in-group loyalty by comparing the reactions to the Bárcenas papers of respondents who self-reported to identify with the PP to those who identify with an opposition party or no party at all. While we expect

**Table 1.** Balance Statistics.

|                           | Control | Treatment | <i>p</i> value | <i>n</i> |
|---------------------------|---------|-----------|----------------|----------|
| Identification with PP    | 0.30    | 0.26      | .26            | 868      |
| Turnout previous election | 0.70    | 0.72      | .69            | 943      |
| Economic satisfaction     | 2.16    | 2.08      | .58            | 947      |
| Age                       | 49      | 47        | .07            | 952      |
| Education                 | 13.08   | 12.84     | .63            | 940      |
| Income                    | 2.09    | 2.14      | .53            | 940      |
| Religiosity               | 4.48    | 4.39      | .70            | 948      |
| Gender                    | 0.53    | 0.50      | .44            | 952      |
| Employment                | 0.35    | 0.44      | .01            | 952      |

PP = Partido Popular.

opposition or nonpartisans to withdraw support for the incumbent following the scandal, we expect this effect to be nonexistent or even in the opposite reaction for incumbent partisans, PP identifiers.

This design relies on a core assumption, namely that the timing of the interviews across the fieldwork period should be unrelated to the outcome, that is to say treatment status is conditionally independent of the potential outcomes. We test this assumption by means of balance and placebo tests. The balance statistics are presented in Table 1. We report the results from two-sided means difference tests of pretreatment variables.

These balance tests reveal that respondents in the treatment and control group are very similar, except for age and employment. Respondents in the control group are likely to be older and out of work compared with the treatment group. This result is not entirely surprising given that it is less difficult to reach respondents at home when they are older or out of work, and hence they are more likely to be interviewed earlier in the fieldwork period. To account for this potential selection effect, we add age and employment status as an individual-level covariate to our regression analyses next to other individual-level characteristics, such as gender, education, income, employment status, economic satisfaction, and religiosity. Table C.1 in the Supplemental Information shows the results including all individual-level covariates. For the placebo tests, we randomly select two dates in the fieldwork period as cutoff points to create a placebo treatment variable. Respondents interviewed 2 weeks before the placebo dates are assigned to the placebo control group, while those who were interviewed 2 weeks after are assigned to the placebo treatment group. The results of the placebo tests reveal that no differences in the outcome variable between the control and treatment group

exist at other dates chosen at random. Due to space constraints, these results are reported in Tables F.1 and F.2 of the Supplemental Information.

Moreover, we also check the robustness of our main results by using a matching technique. We employ the nearest-neighbor matching as an optimization method for finding the closest (or most similar) individuals.<sup>18</sup> These results are reported in the Table D.1 in the Supplemental Information and show that our main results hold when we employ matching. Finally, due to the fact that for those exposed to the scandal partisanship was measured *ex-post*, we have to be sensitive toward the possibility that respondents may have updated their incumbent party identification accordingly (for compelling evidence to this effect in Brazil, see Winters & Weitz-Shapiro, 2014, 2015). We examined whether the PP identification predicts treatment assignment of respondents (while including other covariates) as a way to explore the possibility that respondents might have been less likely to state that they identify with the PP after the scandal was released. These results are presented in Table E.1 of the Supplemental Information and show that PP identification has no significant effect on treatment assignment.

We begin our analysis by comparing means between the control and treatment group, and then turn to regression analyses. The outcome variable captures respondents' approval with the PP government on an 11-point scale ranging from 0 (not satisfied at all) to 10 (very satisfied). The average approval with the government overall is very low, namely, 2.518. This is not surprising given that at the time the survey was conducted, in early 2013, Spain found itself in a deep economic recession. Unemployment was very high at over 25%, and reached staggering levels for the youth with one in two of those below 25 being out of work.<sup>19</sup> The average government approval among those who were exposed to the Bárcenas scandal is 2.280 versus 2.722 among those not exposed. The difference in approval of the government between the treatment and control group is with  $-0.442$  almost half a point on the 11-point scale and statistically significant ( $p = .024$ ). Moreover, whereas the difference in means between treatment and control group is with  $-0.192$  rather modest and fails to reach statistical significance ( $p = .651$ ) for those respondents identifying with PP, it is twice the size,  $-.388$ , and statistically significant ( $p = .039$ ) for those not identifying with PP. This suggests that PP identifiers might have been willing to overlook the corruption scandal involving "their" party, while those who identify with other or no parties at all are not.

We now turn to the results of regression analyses presented in Table 2. Government approval is the dependent variable and the treatment indicator (Ti) the main variable of interest. Model 1 in Table 2 presents the average treatment effect of being exposed to the Bárcenas scandal on government approval. The results indicate that being exposed to the scandal reduces

**Table 2.** Exposure to Bárcenas Scandal and Government Approval by Government Party Identification.

| Independent variables | Dependent variable: Government approval (0-10) |                  |                  |
|-----------------------|--|------------------|------------------|
|                       | (1)  | (2)              | (3)              |
| Treatment (Ti)        | -0.442** (0.195)                               | -0.354** (0.164) | -0.468** (0.180) |
| Constant              | 2.722*** (0.170)                               | 0.526 (0.411)    | 0.027 (0.477)    |
| Individual covariates |  | ✓                | ✓                |
| Region fixed effects  |  |                  | ✓                |
| Observations          | 933  | 907              | 907              |
| R <sup>2</sup>        | .004   | .339             | .360             |

Table entries are ordinary least squares regression coefficients with standard errors in parentheses. The time fixed effects used are dummies for the day of the interview was taken. Individual-level covariates included are age, gender, education, income, employment status, economic satisfaction, and religiosity.

\* $p \leq .1$ . \*\* $p \leq .05$ . \*\*\* $p \leq .01$ .

approval by about half-a-point (-.442) on an 11-point scale. Spanish voters do update their evaluation of the government after being exposed to widespread and long-lasting corruption of the ruling party. Model 2 includes the treatment indicator while controlling for a set of individual-level covariates. Finally, Model 3 includes the treatment indicator, individual-level covariates, as well as region fixed effects (see Table C.1 in the Supporting Information for the full results). The results show that government approval was lower among respondents who were exposed to the scandal compared with those who were not.

Table 3 presents the results for being exposed to the scandal and the interaction with those who identify with the incumbent party versus those who do not. Model 1 shows the results for government approval, our outcome variable of interest, while Model 2 shows the results for satisfaction with the economy. This second outcome variable serves as a placebo test as we do not expect it to be affected by a corruption scandal involving the incumbent party. The results for Model 1 show that those who were exposed to the scandal and identified with opposition parties or no party at all display lower government approval, roughly -0.495 on an 11-point scale. The interaction between being exposed and self-reported PP identification is positive and statistically significant suggesting that the effect of exposure is different for PP identifiers. In fact, the difference in being exposed versus not is positive for self-reported PP identifiers, ATE = 1.537, SE = 0.866 in Model 1, and significant at a  $p \leq .10$  level suggesting that in-group partisans might even reward corruption of in-group

**Table 3.** Exposure to Bárcenas Scandal and Government Approval and Economic Satisfaction by Government Party Identification.

| Independent variables                                    | Government approval | Economic satisfaction |
|--|---------------------|-----------------------|
|  | (1)                 | (2)                   |
| Treatment (Ti)   | -0.495*** (0.180)   | 0.064 (0.175)         |
| Government identifier                                    | 1.727*** (0.370)    | 1.475*** (0.352)      |
| Treatment × Government                                   | 0.769* (0.433)      | -0.425 (0.417)        |
| Constant   | 0.279 (0.451)       | 2.755*** (0.426)      |
| Average Treatment Effect (ATE) for government identifier | 1.537* (0.866)      | -0.850 (0.833)        |
| Region fixed effects                                     | ✓                   | ✓                     |
| Individual covariates                                    | ✓                   | ✓                     |
| Observations   | 907                 | 919                   |
| R <sup>2</sup>   | .431                | .104                  |

Table entries are ordinary least squares regression coefficients with standard errors in parentheses. The time fixed effects used are dummies for the day of the interview was taken. Individual-level covariates included are age, gender, education, income, employment status, economic satisfaction, and religiosity.

\* $p \leq .1$ . \*\* $p \leq .05$ . \*\*\* $p \leq .01$ .

members in line with the notion of in-group status. That said, given that these results do not allow us to rule out that the in-group effects are driven by instrumental considerations, we should be careful with this interpretation. In fact, the results presented in the next section suggest that when we shut down instrumental considerations, voters do not punish candidates with whom they share a group identity for corruption but also do not reward them. Our findings in Model 1 suggest that in-group partisans do not withdraw support from the incumbent after being exposed to the corruption scandal, while out-group partisans do. The results presented in Model 2 show that no significant treatment effect exists for economic satisfaction. Moreover, while government identifiers are overall more positive about the state of the economy compared with opposition or non-partisans, corroborating a well-known result (e.g., Evans & Andersen, 2006; Rudolph, 2003), no statistically significant interaction effect exists between being exposed to the scandal and identifying with the government. The fact that we find significant treatment and in-group loyalty effects only for government approval increases our confidence that we are really tapping into the punishment of corruption.

## **Results From Study 2: Laboratory Experiments on Corruption**

We now turn to the results of our second study based on laboratory experiments run in the United Kingdom.<sup>20</sup> This experimental setting allows us to carefully control the setting in which voters take decisions, artificially induce their group identity, and randomly assign the group identity of the candidates they face. We designed our experiments in such a way that participants (a) were informed about the corruption of candidates, (b) could not update their group identity in line with corruption information, (c) could not infer any policy positions from the group identity of candidates, and (d) could not monetarily gain from candidate corruption, in fact they automatically lost as a result of it. This arguably constitutes a hard test for the expressive in-group loyalty explanation. In our experiment, five subjects play five blocks of five rounds in which three subjects are randomly assigned as voters and two as candidates.<sup>21</sup> These roles remain fixed throughout the experiment.<sup>22</sup> In each round, three voters decide by majority which candidate wins the election.

The experimental game works as follows. At the start of every round, each candidate receives funds, namely 18 tokens, between one and six of which are valuable (red tokens, exchange rate 4 tokens = £1), while the rest is worthless (blue tokens). The number of valuable tokens a candidate receives is a random draw from a uniform distribution between one and six and resembles the rolling of a six-sided dice. Candidates know the number of valuable tokens they receive and are asked to send these to voters. Valuable tokens are designed to benefit voters as they are doubled and divided equally among all voters. Group welfare is thus maximized when candidates send all valuable tokens they receive to voters and minimized when candidates keep all valuable tokens for themselves. This rules out the possibility that voters can benefit directly from candidate corruption, or that corruption can be traded off against other benefits (such as higher social welfare, for example). Each candidate decides how many of the valuable tokens they send to voters and how many to embezzle and keep for herself. Candidates have a monetary incentive to get elected as they receive a bonus of one valuable token when elected, but can increase their payoff by embezzling. If candidates decide to keep valuable tokens, they risk being caught in a corruption audit. If candidates are caught in an audit (because at least one of the flipped tokens proved to be valuable), they lose all of the tokens they kept, and voters are informed about their misconduct. Candidates face such a corruption audit every round. The audit works as follows: two of the tokens kept by candidates are flipped, and the flipping of the tokens will reveal if none, one, or both tokens that were kept are valuable. Voters and candidates are informed about all these procedures (see part J.2 of section J in the Supporting Information for the experimental instructions).

To decide who to vote for, voters receive information about candidate corruption, namely, if a candidate was caught in a corruption audit or not. This information is captured by a variable coined *Caught* that takes on the value of 1 when a candidate is caught and 0 when a candidate is not caught. The way in which corruption is operationalized in our experiment is in line with the widespread definition of corruption, namely, as the misuse of public office for private gain (Fisman & Golden, 2017, see also Note 1). Being corrupt is not randomly assigned as it depends on candidate behavior. Yet whether candidates are caught in a corruption audit or not is randomly assigned. Random assignment of candidate corruption would make corruption unrelated to the actual behaviour of candidates, rendering this information meaningless to voters who attempt to find out about candidate quality.

Next to corruption information, voters also receive information about their group identity and the group identity of the two candidates they face. We induce group identity to the game by relying on a minimal group paradigm (Tajfel, 1970). At the start of the experiment, all participants are asked to choose between five sets of two paintings, one by Klee and one by Kandinsky. We assign candidates and voters to two groups based on their painter preferences. At the beginning of the experiment, we randomly assign voters to groups of three. The group composition of voters remains stable across the experiment. Recall that participants play five blocks in total so a set of voters faces five pairs of candidates throughout the experiment. We randomly assign the group identity of the candidates that voters face by reassigning two different candidates to a different set of voters at the beginning of each block of five rounds. The advantage of using painter preferences to construct group identities is that they are shown to be largely uncorrelated to those individual-level covariates associated with important social groups, such as gender or ethnicity, for example (Lane, 2016). Indeed, Table G.2 in the Supplemental Information shows that individual-level covariates are balanced across the group identities based on painter choices. We create a variable *Same Identity* that takes on a value of 1 when voters face a candidate of the same group and 0 when voters face a candidate from a different group. This is a standard way in which group identity is artificially induced in the laboratory (for an overview, see Lane, 2016). It is important for us to use trivial groupings based on painter preferences to ensure that participants cannot infer policy positions from the candidate's group identity. This is to rule out that voters are trading off corruption for policy benefits. Moreover, it is important to note that the candidates can only allocate funds to *all* voters. They cannot favor their in-group by specifically targeting them. These elements allow us to completely shut down instrumental considerations on the side of voters and focus on the expressive aspect of in-group loyalty that we are interested in.

Finally, we control for the number of tokens that both candidates send to voters. In all of our analyses, we add a variable *Difference in Tokens* that captures the difference between the tokens sent by the two candidates. This variable takes on the same value for each candidate, but differs in sign. All else being equal, we expect voters to support the candidate who sends them the largest number of valuable tokens.

What is essential for estimating the effect of in-group loyalty on the electoral punishment of corruption is that we examine decisions of voters as a function of the activities of the candidates in our experiment. To do so, we have constructed a stacked data matrix of voter–candidate dyads that allows us to model the reactions of voters as a function of the activities of candidates. This stacked data matrix includes two lines for every voter for every election decision, that is, for every round. The first line provides the attributes of a voter and links them to those of Candidate 1, while the second links them to those of Candidate 2. The relevant candidate attributes include how many tokens they sent, if they were caught in a corruption audit, and which group they belonged to, i.e., Klee or Kandinsky. The relevant voter attributes are vote choice and in-group loyalty. In a postexperimental questionnaire, we also solicited other individual background variables like gender, age, and risk aversion. The results presented here are robust against the inclusion of these factors (see Table H.1 in the Supplemental Information) and painter groups are balanced with respect to these background characteristics (see Table G.2 in the Supplemental Information).

Analyzing vote choices by means of stacked data matrices is commonplace in voting behavior research that use discrete choice modeling. Alvarez and Nagler (1998) suggest that conditional logit models, unlike (multinomial) logit models, allow for the examination of the interaction of individual voting decisions and candidate characteristics. They suggest that probit estimations have the additional advantage of allowing the modeling of individual- and candidate-specific variables simultaneously while making less strict assumptions about the data. Hence, we examine our experimental data using a probit estimation. Specifically, we use a panel probit model with individual random effects to deal with the fact that we have repeated observations for each individual voter. We add fixed effects for round and block to control for any temporal order in the data (due to learning of voters across rounds, for example). Note that we also reestimated our models with a linear probability model and the results remain robust (see Table I.1 in the Supplemental Information).

Table 4 presents the results from panel probit estimations with individual random effects and fixed effects for round and block. The analysis includes the three variables discussed earlier, namely, *Caught*, *Same Identity*, and

**Table 4.** Corruption and Vote Choice.

| Independent variables     | Dependent variable: Vote for candidate |
|---------------------------|--|
| Caught                    | -0.392** (0.163)                       |
| Same identity             | 0.180** (0.086)                        |
| Caught × Same ID          | 0.436* (0.237)                         |
| Difference tokens         | 0.344*** (0.036)                       |
| Constant                  | -0.055 (0.276)                         |
| Caught effect for same ID | 0.044 (0.180)                          |
| Observations              | 1,168                                  |
| Number of subjects        | 30                                     |

Table entries are panel probit regression coefficients with standard errors clustered on participants in parentheses. This regression includes fixed effects for rounds, blocks, and groups. Standard errors in parentheses.

\* $p \leq .1$ . \*\* $p \leq .05$ . \*\*\* $p \leq .01$ .

*Difference in Tokens*, as well as the interaction between *Caught* and *Same Identity* (*Caught* × *Same ID*). The latter interaction represents the key result we are interested in, namely, does the effect of being caught in a corruption audit differ when voters face an in-group versus an out-group candidate. We expect to find a negative and statistically significant coefficient for *Caught*, as being caught in a corruption audit should decrease the likelihood that a voter would vote for that candidate, and a positive and statistically significant coefficient for *Same Identity*, as we expect voters to display a higher likelihood to vote for a candidate with whom they share a group identity, as well as for *Difference in Tokens*, as we expect voters to vote for the candidate who sends them more valuable tokens. The results in Table 4 suggest that this is indeed what we find. Moreover, we expect a positive and statistically significant coefficient for the interaction term between *Caught* and *Same Identity* as this would suggest that the negative effect of a candidate being caught in a corruption audit on the likelihood of voting for this candidate is mitigated by the fact that the voter shares the same group identity with the candidate. While the effect of being caught in a corruption audit for an out-group candidate, which is represented by the coefficient for the variable *Caught*, is  $-0.392$  and statistically significant, the same effect for an in-group candidate is virtually zero. This is based on subtracting the coefficient for the variable *Caught* from the coefficient for the interaction term *Caught* × *Same ID*:  $0.436 - 0.392 = 0.044$ . This suggests that while voters punish out-group candidates for corruption, they do not punish in-group candidates even knowing that by doing so they lose out monetarily.

In a final step, we explore the channels through which expressive in-group loyalty might weaken the electoral punishment of corruption, namely, in-group serving biases or in-group status. Recall that based on the idea of in-group status that people might gain some utility from having wealthy and high-status members in their in-group, we would expect in-group loyalty to lead people to reward corruption (*positive effect*). Based on the notion of in-group serving bias, we would expect no punishment of corruption (*no effect*) because voters are expected to discount any “bad” information they receive about their in-group candidate. The results presented in Table 4 show a null effect for being caught in a corruption audit for in-group candidates. This is in line with the in-group serving bias expectation. In-group loyalty seems to weaken the electoral punishment of corruption because voters discount corruption information when an in-group candidate is involved, while they do punish corruption of out-group candidates for the identical indiscretion.

## Discussion and Conclusion

Democratic elections allow citizens to freely sanction or reward politicians in a periodic manner and are therefore widely believed to have a constraining effect on corruption. As conventional wisdom has it, elections provide voters with the means to do away with their corrupt leaders. Yet we often find that voters (re)elect corrupt politicians. This study maintains that while much of the literature to date focused on the role of information or policy benefits, another important aspect has been overlooked, namely, the importance of expressive social group identities. Existing evidence mainly focuses on the role of partisanship and considers situations in which corruption is traded off against some instrumental benefit, such as preferred policies or economic performance. In this study, we considered the possibility that in-group loyalty more broadly defined hampers the punishment of corruption even when no such instrumental benefits are provided. Our evidence suggests that in-group loyalty can be expressive in nature. Although voters may generally dislike corruption, when different groups compete over scarce resources during an election, in-group loyalty trumps the disapproval of corrupt activities simply because voters attach some positive utility to backing their in-group candidate. This lack of punishment of corruption in the absence of instrumental benefits seems mostly to be due to the fact that voters react differently to identical corrupt activities of in-group candidates compared with out-group candidates. While they punish out-group candidates for corruption, they discount corruption information concerning candidates with whom they share a group identity.

By relying on evidence from two studies, one employing observational data from Spain involving a large-scale corruption scandal involving the ruling party and another based on data from laboratory experiments in the United Kingdom, we demonstrate that in-group loyalty weakens the electoral punishment of corruption. In the Spanish case, we show that in-group loyalty based on partisanship decreases the likelihood of sanctioning the ruling party for corruption, while out-group identification increases it. In the laboratory setting, we are able to isolate these in-group effects by carefully controlling the setting in which voters make decisions and the information they receive and focusing on the expressive aspect of in-group loyalty. Our experimental game allows us to shut down instrumental considerations of in-group loyalty as group identities were artificially induced in the laboratory (here based on a trivial distinction between people's painter preferences) so voters could not infer any policy positions from them, identities could not be updated throughout the game, and candidates could not specifically target benefits toward their in-group members. Relying on a minimal group paradigm not only helps us to show how expressive in-group loyalty weakens the electoral punishment of corruption, but also allows us to test the generalizability of these in-group effects beyond partisanship. This latter aspect of our results is important as it extends the importance of in-group loyalty to contexts where partisanship is a less salient social group identity.

Our findings speak to a large body of work regarding the punishment of unethical behavior which suggests that in-group loyalty increases people's willingness to condone rather than punish unethical behavior, and that these effects are most pronounced in competitive environments (Bernhard et al., 2006; Carlin & Love, 2016; Gino et al., 2009; Hildreth et al., 2016; Rand et al., 2009; Wright et al., 2013). We extend these insights to the punishment of corrupt activities of politicians. Moreover, our results corroborate classical and contemporary work in electoral behavior highlighting that voters assign an important role to expressive social identities when evaluating candidates for public office (e.g., Abrajano & Alvarez, 2005; Campbell & et al., 1960; Chandra, 2007; Huddy et al., 2015; Kaufman, 2004; Landa & Duell, 2015; Lazarsfeld et al., 1944). Although much of this work suggests that group identity considerations lead to efficient decision-making by providing information shortcuts, in the case of corruption we suggest that in-group loyalty may lead to negative effects, namely the condoning of corrupt activities. Finally, our findings dovetail existing work on in-group-serving biases in electoral accountability due to partisanship. Accumulated evidence over the past three decades has shown that partisanship produces systematic biases in what political information citizens attend to and how that information is interpreted and evaluated (for an overview, see Healy & Malhotra,

2013). Partisanship, for example, colors people's perceptions of the economy because individuals who are attached to the governing parties are likely to perceive the economy more positively than individuals attached to an opposition party (e.g., Evans & Andersen, 2006; Rudolph, 2003). We demonstrate that in-group loyalty more generally clouds the sanctioning of candidate corruption and that this seems to be due to the fact that in-group members discount corruption information involving a candidate from the same in-group.

Moreover, our finding that in-group loyalty without the provision of any specific benefits trumps voters' condemnation of corruption complements existing understandings of why corruption often goes unpunished at the ballot box. Existing work suggests that voters may not punish corruption because they lack (credible) information or because they are trading off corruption against a preferred policy platform or performance. In this understanding, a lack of punishment of corruption is not due to the fact that voters may not care about corruption, but a reflection of a lack of (credible) information or instrumental calculations. In this case, the behavior of voters might not be normatively undesirable. Yet the evidence presented here suggests that voters may fail to punish candidate corruption even when they are fully informed about corruption, receive no side-payments, or cannot infer any policy benefits based on partisan leanings of candidates. Presented with the same corruption information, voters treat the information involving a candidate of their in-group differently compared with a candidate from the out-group, namely, they discount it. This evidence fits with motivated reasoning approaches (Taber & Lodge, 2006) that suggest that voters engage in a process of selective information processing driven by automatic affective group ties. This behavior might be undesirable from a normative democratic theory perspective.

Finally, the lack of punishment of corruption due to expressive in-group loyalty is important, because it suggests that politicians can get away with corruption perhaps more easily than previously assumed. When the lack of punishment of corruption is largely the result of the quantity and quality of information that is available to voters, having a free press will no doubt help. When voters trade off corruption against policy benefits, corruption is likely costly for candidates as they have to credibly commit to their policy pledges. Yet when the lack of punishment of corruption is the result of expressive in-group loyalty, the costs of corruption for candidates are rather low, and much lower than previously assumed. Simply, playing the identity card suffices. This suggests that elections are not an effective institutional deterrent against corruption, but rather that other institutional solutions, like the establishment of a strong and independent judiciary, may prove more important.

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## Notes

1. Corruption is most commonly defined as the misuse of public office for private gains (Rose-Ackerman, 2006). This definition is extensively used in political science but does raise some questions. For example, does “misuse” signify that the act is a deviation from legal standards, from moral standards, or perhaps from both, and do private gains only relate to public officials themselves or also to their family, friends, or their party? Here we interpret private gains to include both direct material benefits to the official, like money, for example, as well as more indirect gains, such as increased reelection prospects or aiding family, friends, party affiliates, or members of one’s own social or ethnic group to obtain certain material benefits. Moreover, we construe misuse as the use of office for illegal purposes, but also more broadly as the misappropriation of public resources to enhance the private returns of the official.
2. These are countries that score 50 out of a 100 in Transparency’s International Corruption Perceptions Index indicating serious levels of public sector in 2015.
3. In addition, a well-established literature demonstrates the importance of vote-buying and material side payments (see Mares & Young, 2016, for an overview). Yet the exchange of material benefits is a costly, difficult to organize, and voters may ultimately not comply.

4. Here we follow Huddy, Mason, and Aarøe (2015). Green, Palmquist, and Schickler (2002) view partisanship as a “social identity” but do not necessarily follow the predictions of “social identity theory” such as in-group bias and positive differentiation.
5. In-group loyalty develops based on naturally occurring in-groups, due to language, ethnicity, gender, or ideology, but also arises from trivial categorizations based on a coin toss or preferences for abstract paintings (Tajfel, 1970, 1984).
6. This environment is particularly useful as tribes in Papua New Guinea exhibit strong group identities based on language and local customs.
7. These expectations dovetail with the formal work on ethnic identities by Banerjee and Pande (2007). The authors present a theoretical model to suggest that as a society becomes more ethnically polarized and citizens vote along ethnic lines rather than any other factor, criminal activities of candidates of the same ethnic group are less likely to be punished electorally. If political polarization occurs among ethnic lines, corruption is less likely to be punished as voters “feel an instinctive pull towards their co-ethnics” (Banerjee & Pande, 2007, p. 5). The by-product of this might be a selection effect. Parties alter their candidate choices in line with ethnic polarization as the probability of winning an election increases with ethnicity rather than quality, and thus the quality threshold for candidates is lowered.
8. Other work, such as Legewie (2013), utilizes the fact that an event, the Bali bombings, took place during the fieldwork of the ESS as a means to identify how terrorist attacks affect the perception of immigrants for example.
9. To separate in-group loyalty from the trading off explanation, we would compare two social groups that exhibit equally high (or low) proximity to the government party, but are expected to differ based on the benefits they receive from the government party’s policies. This essentially would require an exogenous policy change to delineate such social groups at the same time as the corruption scandal broke. To our knowledge, no such policy change occurred in Spain at this time.
10. Other work, such as Legewie (2013), utilizes the fact that an event, the Bali bombings, took place during the fieldwork of the ESS as a means to identify how terrorist attacks affect the perception of immigrants, for example.
11. [http://elpais.com/elpais/2013/11/22/inenglish/1385130382\\_146272.html](http://elpais.com/elpais/2013/11/22/inenglish/1385130382_146272.html) (accessed July 26, 2016).
12. <http://www.bbc.co.uk/news/world-23088204> (July 26, 2016).
13. <https://www.theguardian.com/world/2013/jan/31/spanish-prime-minister-secret-payments?INTCMP=SRCH> (accessed July 26, 2016).
14. [http://articles.chicagotribune.com/2013-05-31/news/sns-rt-us-spain-corruption-bre94u0yf-20130531\\_1\\_swiss-accounts-luis-barcenas-party](http://articles.chicagotribune.com/2013-05-31/news/sns-rt-us-spain-corruption-bre94u0yf-20130531_1_swiss-accounts-luis-barcenas-party) (accessed July 26, 2016).
15. The fieldwork of the Spanish module in the sixth round of the European Social Survey (ESS) was conducted between January 23, 2013, that is, about a week prior to the publication of the Bárcenas papers, and May 14, 2013.
16. For similar type of application to trust in politicians, see Ares and Hernández (2017).

17. A government's approval function is an indicator that has long been considered as keystone of political accountability in democracies.
18. Closeness is expressed in terms of a dissimilarity function: The less similar the individuals, the larger the function values (Rubin, 1973). It selects for each treated individual the control individual with the smallest distance from the treated individual.
19. See <https://data.oecd.org/unemp/unemployment-rate.htm> and <https://data.oecd.org/unemp/youth-unemployment-rate.htm> (accessed July 26, 2016).
20. We conduct laboratory experiments by using a variation of the Attorney General Game introduced by Azfar and Nelson (2007).
21. Due to a technical problem with one of the computers, one group of five participants could not participate in the second block of our treatment experimental session.
22. Group compositions are fixed throughout the block, but there is a rematching of participants at the beginning of each round.

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