

The Corrosive Effect of Corruption on Trust in Politicians: Evidence From a Natural Experiment

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Abstract

Although several studies have revealed a negative correlation between corruption and political trust, we lack reliable evidence isolating the causal effect of corruption on trust, especially outside the lab. In this paper we capitalize on the causal identification advantages generated by the fact that a major corruption scandal (the Bárcenas case) was uncovered during the fieldwork of a representative survey in Spain. Given that the day at which survey interviews were conducted is as if-random, the uncovering of the scandal provides an exogenous source of variation in levels of corruption and represents a unique opportunity to assess the causal effect of a corruption scandal. Our results indicate that: (i) the scandal had a substantial negative effect on citizens' levels of trust in politicians; (ii) the effect of the scandal is stronger in the days following its disclosure; (iii) the effect of the scandal is independent from individuals' partisan preferences.

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INTRODUCTION

Political corruption generates severe dysfunctions for the functioning of democracies, since it subverts some of their basic principles like equality before the law, or the fairness and impartiality of political processes. In fact, the absence of corruption is considered paramount for high-quality governments, and numerous scholars have identified damaging political, social, and economic effects of corruption (Anderson and Tverdova, 2003; Jain, 2001; Linde, 2012; Rothstein and Teorell, 2008; Seligson, 2002).

Given its harmful effects, in the presence of corruption we would expect citizens to develop negative orientations towards the political system or its institutions, and to withdraw their electoral support from corrupt politicians. However, some scholars have argued that corruption, or at least some types of corruption, may provide certain benefits and, as such, corruption would not necessarily have a negative impact on citizens' attitudes and behaviors (see Jain, 2001, pp. 92–93). Empirical studies have not provided a univocal answer about the impact of corruption on citizens' electoral behavior or their attitudes towards the political system. In the case of the punishment of corrupt politicians at the voting booth, the identified effects of corruption range from null to moderate (see e.g. Fernández-Vázquez et al., 2015; Kauder and Potrafke, 2015; Riera et al., 2013). With regards to corruption and political support, studies have generally identified a significant correlation between corruption and negative orientations towards the political system (see e.g. Erlingsson et al., 2015; Morris and Klesner, 2010; Seligson, 2002). However, in the case of the analysis of the relationship between corruption and political attitudes, most prior studies fail to isolate the causal effect of corruption, or corruption scandals, on citizens' political support. This is due to the fact that the causal relationship between corruption, or perceptions of corruption, and political support is likely to be endogenous

and complex, making it extremely difficult to analyze empirically (Villoria et al., 2013, p. 86).

In this paper we rely on the fact that the '*Bárcenas scandal*', one of the most relevant and visible corruption scandals that occurred in Spain, was uncovered by the Spanish newspaper 'El País', and was immediately picked up by all other national media outlets during the fieldwork of the sixth round of the European Social Survey (ESS). This allows us to capitalize on a quasi-experiment to study the consequences of a major corruption scandal on citizens' trust in politicians. The timing of the survey is used as a source of exogenous variation in corruption levels, which allows us to assess the causal effect of corruption on citizens' trust politicians. This design brings us closer to the ideal for the study of the consequences of corruption (Jain, 2001, p. 91), since it allows us to compare the consequences of a corruption scandal against what would have happened had the corruption scandal not been uncovered. Moreover, in contrast to experiments conducted in the lab, this research design allows us to assess the causal effect of a real-world scandal on a representative sample of the Spanish population.

Based on the idea that corruption scandals like the *Bárcenas scandal* —which involved key political actors of the governing party 'Partido Popular' (PP)— signal an abuse of the power entrusted by citizens to political authorities, our main expectation is that the corruption scandal will have a negative effect on citizens' trust in politicians. However, the corruption scandal should have been more present in citizens' minds in the weeks just after it was uncovered, since at that time the scandal was intensively covered in the media. This leads us to expect a decay of the effects of the scandal on citizens' trust in politicians as we move further away from the day in which the scandal was uncovered. At the same time, we do not expect an equal reaction from all citizens to the scandal. Anduiza, Gallego and Muñoz (2013) have shown that partisanship moderates the extent

to which citizens tolerate corruption. Hence, we expect that the impact of the scandal will differ depending on whether citizens support the party involved in the scandal (Partido Popular, PP) or not.

This paper proceeds as follows. In the next section we lay out the theoretical framework for the analysis of the impact of corruption and corruption scandals on citizens' political support, we introduce the main characteristics of the *Bárcenas* scandal, and present our research hypotheses. Next, we summarize our identification strategy. The fourth section presents the results of the empirical analyses. The fifth section summarizes different robustness checks that address the assumptions that must be met to identify the causal effect of corruption. The last section concludes.

THEORY

A negative reaction to corruption?

Large scale corruption scandals are instances at which the wrongdoings of politicians become widely known to the public. Confronted with these cases, or any other increase in corruption, rational citizens should update their attitudes towards the political system based on their judgment of the behavior of political actors. This is in fact the hypothesis that has been put forward by institutional theories, which argue that political attitudes such as trust in politicians are endogenous to the political system and will depend on what political authorities do and how citizens perceive and evaluate their performance (Erlingsson et al., 2015; Mishler and Rose, 2001). Evaluations of the perceived output and performance of political institutions should have an impact on citizens' orientations towards them. Institutions that perform well and produce desirable outputs (e.g. guaranteeing citizens' freedom, institutional fairness) should garner support, while the misuse of public office should lead to skepticism and distrust.

The literature on the impact of corruption on citizens' attitudes and behavior provides us, though, with contrasting expectations about the effects of corruption. While some scholars argue for a corrosive effect of corruption (e.g. Della Porta, 2000), an opposing functionalist strand maintains that corruption can have positive consequences and in fact foster positive evaluations of institutional performance (e.g. Bayley, 1966). According to those who expect a corrosive effect of corrupt practices, corruption undermines the fundamental principles of democratic accountability, equality and openness (Anderson and Tverdova, 2003; Chang and Chu, 2006). Corrupt institutions lose their autonomy and their credibility and become personal tools of the corrupt elites (Chang and Chu, 2006). This malfunctioning of institutions should be reflected in how citizens evaluate the performance of institutions. Functionalist perspectives on corruption, on the contrary, argue that corruption can function as the 'grease' of the bureaucratic machine, increasing the efficiency of government (see Jain, 2001, pp. 92–93). According to this perspective, public officials will, for example, be more helpful and effective under corruption, since they are paid directly in the form of bribes. Empirical findings, however, have tended to favor the former strand arguing for negative, or at least, null effects of corruption, and some have even argued that there is no single empirical study reporting a positive effect of corruption on citizens' attitudes towards the political system (Kumlin and Esaiasson, 2012, p. 269)

In attempting to assess the impact of corruption on political support existing research has mainly estimated the impact of aggregate measures of corruption, or measures of citizens' perceptions and direct experiences with corruption, on indicators of political support. The studies by Anderson and Tverdova (2003) and Hakhverdian and Mayne (2012) uncover a negative association between aggregate measures of corruption at the national level —as measured by the Corruption Perception Index (CPI)— and

individual levels of political support. Mishler and Rose (2001) report similar findings for Post-Communist democracies, for which they find that countries with higher aggregate corruption levels suffer of lower levels of aggregate trust in institutions and, at the individual level, perceived corruption is also associated to lower trust in institutions (see also Linde, 2012). Chang and Chu (2006) find similar corrosive effects of citizens' perceptions of corruption on political trust in Asian democracies, even if it was initially proposed that corruption could have less of an impact in the East Asian context. Similar conclusions are reached in Seligson's (2002) study of Latin American countries, based on an analysis of individual experiences with corruption (as reported by survey respondents) and an index tapping generalized support for basic institutions of government. Focusing on Mexico, Morris and Klesner (2010) also indicate that individuals with stronger perceptions of corruption express lower levels of political support. In the case of Spain, Villoria et al. (2013) have shown a significant negative correlation between citizens' perceptions of corruption and their satisfaction with democracy and institutional trust.

The proposition of a negative effect of corruption is prominent not only in research studying the general impact of corruption, but also in studies addressing the impact of specific scandals, although, in this case, much more attention has been paid to the potential negative electoral consequences of those scandals than to their impact on citizens' attitudes towards the political system. Bowler and Karp (2004) show that being aware of the House-Bank scandal involving US legislators undermined popular support towards US Congress, and that residents of UK districts where their members of parliament had been involved in different scandals were more likely to have negative attitudes towards politicians. Similar findings are reported by Chanley et al. (2000) who conclude that scandals associated with the US congress lead to declining trust in

government. Pharr (2000) also finds that reports of Japanese officials' misconduct were negatively correlated with citizens' political satisfaction. In the most comprehensive analysis of the impact of political scandals on citizens' attitudes to date, Kumlin and Esaiasson (2012) show, using data for different European countries, that after elections in which political scandals are widely discussed during the campaign citizens' appear to be less satisfied with the way democracy works, although the impact of scandals is reduced as the number of cumulated past scandals increases. In the case of Spain, Solé-Ollé and Sorribas-Navarro (2014) conclude that scandals involving local politicians are likely to reduce citizens' trust in local governments.

The negative association between corruption and individual-level political support appears repeatedly across different institutional contexts. Is this, however, sufficient evidence of a negative causal effect of corruption on political trust? Although these studies provide relevant insights about the impact of corruption, most of them do not isolate its causal effect on citizens' attitudes. A common strategy of past research has been to operationalize corruption based on individual-level experiences with corruption (e.g. Seligson, 2002) or on measures of perceived corruption, either at the individual-level as measured by survey items (e.g. Chang and Chu, 2006; Villoria et al., 2013), or at the aggregate level with indicators such as the Corruption Perception Index (CPI) (e.g. Anderson and Tverdova, 2003; Hakhverdian and Mayne, 2012). The main limitation of these research designs is the threat of endogeneity, due to the potential mutual causality between perceptions of corruption and political support. These analyses based on cross-sectional data explicitly treat the effect of corruption on political attitudes as exogenous. However, it is likely that low political support could increase citizens' perceptions of corruption. High levels of corruption, perceptions of corruption and institutional mistrust generate a vicious circle of mutual reinforcement (Morris and Klesner, 2010). Under low

levels of political trust citizens could develop a higher degree of tolerance towards corruption and, thus, be more likely to engage in corrupt practices. Hence, lower levels of political trust would not only increase perceptions but also actual experiences of corruption. These different attitudes and perceptions nurture a vicious circle that enhances the difficulty of disentangling the causal effect of corruption on political support. Aggregate measures of perceptions of corruption (such as the CPI) could also be affected by overall levels of political support within the country since they are gauged through cross-sectional surveys.

A similar problem affects scandal studies that rely on measures of scandal recall to operationalize scandal exposure (Bowler and Karp, 2004). In this case, citizens with lower levels of trust could be more likely to be exposed to news about the scandal, and also be more likely to remember the scandal when interviewed. Other studies on scandals compare the political support of individuals living in countries or districts that were and were not affected by a given scandal (Kumlin and Esaiasson, 2012; Solé-Ollé and Sorribas-Navarro, 2014).¹ These studies suffer from the problem that it is hardly arguable that corruption scandals are randomly distributed across countries or districts. Although techniques such as matching may alleviate this problem (Solé-Ollé and Sorribas-Navarro, 2014), there is still a concern for omitted variable bias since, first, there are potential unobservable factors affecting corruption and the outcome of interest, and, second, the limited theory on scandals and political support does not provide proper guidance about potential control variables (Kumlin and Esaiasson, 2012, pp. 276–277). The studies by Chanley et al. (2000) and Pharr (2000) adopt a time-series approach and compare the level of political support of citizens living in the same country across time. The limitation

¹ Kumlin and Esaiasson (2012) include different waves of Eurobarometer surveys in their pooled sample. As a consequence, they also include repeated observations from the same countries, which at some points had scandal elections and at some other points had non-scandal elections.

of these studies is that they either rely on a limited number of data-points (Pharr, 2000) or they need to pool different measures of political support gauged through different questions of different polling firms to generate a sufficiently long time-series (Chanley et al., 2000). In any case, one of the main drawbacks of most of these studies is that the measurement of citizens' levels of political support is made months or even years after the scandal occurred. This is problematic since, as we will show below, the effects of scandals are likely to fade as time goes by, and also because other extraneous events that might affect political support may occur between the scandal and the measurement of citizens' levels of support.

Recently there have been some attempts to disentangle the causal effect of corruption on political support using experimental methods. Maier (2010) conducted an experiment on college students to assess the impact of exposure to information about a real case political scandal. This study finds evidence of a negative effect of the scandal on evaluations of politicians and the party involved, but not on the evaluations of institutions and the political system. Conversely, Régner and Floch's (2005) study showed, using a similar research design, that exposure to scandals reduced trust in politicians, although only among individuals with high levels of political knowledge. These results, however, are limited in their external validity, since respondents are a sample of college students and, even if the experiment was based on a real political scandal, we are uncertain of whether these results would be replicated in a context where exposure to the scandal information is not directly manipulated by the researcher. The recent study by Botero et al (2015) improved on this aspect by using a survey-experiment in Colombia. Their results show that a candidate involvement in a fictitious corruption scandal is likely to reduce trust towards the candidate, and the effect is stronger if information is conveyed through newspapers.

As a difference from these previous analyses in this paper we rely on a natural experiment to assess the impact of a real and large-scale corruption scandal on trust in politicians among a representative sample of the Spanish population. In order to do so, we adapt to the study of corruption scandals the research design that Legewie (2013) implemented to analyze the impact of terrorist attacks on attitudes towards migrants. Using a similar approach, a recent study by Zhang (2015) has shown that the prosecution and dismissal of corrupt politicians boosts citizens' support for the regime in authoritarian countries.

The Bárcenas case and Spaniards' trust in politicians

Scandals associated to political corruption have been pervasive in Spain during the last decade. According to the Spanish Institute for Sociological Research (CIS) trimestral barometers corruption has become one of the main concerns of Spaniards (after unemployment).

One of the most relevant and visible corruption scandals of the last decades has been the Bárcenas case. This scandal became widely known in Spain after the most read daily newspaper '*El País*' leaked on January 31st a significant amount of internal documents from the Partido Popular (PP). These documents uncovered a system to keep a parallel bookkeeping of illegal donations, which were used to pay cash bonuses to party members, among other irregularities. The leaked documents directly involved Luis Bárcenas (treasurer of the party at that time) in the scheme, as well as other relevant members of the party. The documents leaked by *El País* on January 31st 2013 were facsimiles allegedly written by Bárcenas, which suggested that high-ranking PP officials had received undeclared money from the party, among other irregularities. These documents came to be known as 'los papeles de Bárcenas' (the Bárcenas documents), and they covered the period from 1990 to 2009 and indicated that these extra-cash payments

to party members had taken place regularly over those decades. After the publication in *El País* of these documents, the news of the scandal were promptly picked up by national and international media, and this scandal became a front-page matter in all national newspapers in the days following the leaks.

News of the scandal had already appeared initially on the 18th of January 2013, when the newspaper *El Mundo* published a piece indicating that Mr. Bárcenas had a secret bank account in Switzerland. However, the leak by *El País* was the first to incorporate the internal documents and details that clearly suggested that the scandal affected not only the treasurer of the party, but also many other high-ranking PP members, including some with important responsibilities in the national government. As a consequence, after January 31st the scandal even casted doubts on the stability of the Spanish government (the main opposition party asked for the resignation of the Prime Minister on February 3rd), and forced the PP and government officials to provide explanations about the scandal. In fact, under intense pressure from the media and opposition parties, on February 4th PP Prime Minister Mariano Rajoy admitted (referring to the scandal documents) “that nothing was true, except for a few things”.

(FIGURE 1 HERE)

To have a better picture of how the Bárcenas affair rapidly rose in salience on January 31st, figure 1 displays a graphical representation of the relative frequencies of Google searches about the scandal retrieved from Google Trends. This figure shows the relative number of searches for the term ‘Caso Bárcenas’ by day in Spain (for January and February 2013). The Y-axis in this graph measures the relative number of searches relative to the maximum number of searches in the period included (100). In this case the maximum number was reached precisely between January 31st and February 1st. As the

graph clearly shows, there was a peak in searches for the term ‘Caso Bárcenas’ on the day when *El País* leaked the bookkeeping documents. This is consistent with the great salience with which this issue was covered in the Spanish media. This figure also reveals that there was a relative increase in the number of searches when *El Mundo* presented a piece about Bárcenas, but the issue became then less salient only to peak at the end of January coinciding with the disclosure by *El País* of the Bárcenas documents. The fieldwork of round 6 of the ESS started on the 23rd of January 2013, so the sudden rise in the salience takes place during the fieldwork period, which allows us to establish a comparison between those who were exposed to the Bárcenas affair and those who were not.

The prominence of the scandal in the media, as well as its relevance due to the fact that it involved high-ranking government officials, lead us to expect that the scandal should have informed and modified citizens’ judgment of the trustworthiness of politicians. As we argue above, corruption scandals clearly signal an abuse of the power entrusted by citizens to political authorities. As a consequence, when exposed to scandals citizens’ should rationally update their trust in politicians. Hence, we expect that

H₁: Exposure to the Bárcenas scandal should reduce citizens’ levels of trust in politicians

After the disclosure of the Bárcenas documents by *El País* all other media intensively covered the scandal during the following weeks. However, the intensity of the coverage decreased after a few weeks (Google trends in figure 1 also indicate a decreasing interest in the scandal). Hence, scandal related considerations should have been more immediately salient or accessible on citizens’ minds in the days or weeks that followed the uncovering of the scandal. Given that citizens’ are likely to base their attitudinal

orientations towards the political system on top-of-the-head considerations (Zaller, 1992), we expect that the scandal should have informed citizens' trust in politicians to a greater extent in the weeks that just followed the disclosure of the scandal. This leads us to expect that:

H₂: The effects of the treatment (exposure to the Bárcenas scandal) should be weaker for those interviewed further away from the date when the scandal was uncovered

Individuals have certain predispositions that make them react differently to political information and events (Zaller, 1992). Using an experimental design Anduiza et al. (2013) have shown that partisanship is likely to moderate citizens' tolerance towards corruption. Those who identify with a party involved in a corruption scandal tend to filter information and attribute lower importance to that scandal. As a consequence, citizens' reaction to scandals in terms of the trust they express in politicians should vary depending on whether they sympathize with the party involved (Partido Popular) or with any other party. Hence, we expect that

H₃: The effects of the treatment (exposure to the Bárcenas scandal) should be weaker for supporters of the Partido Popular (PP)

DATA AND METHODS

Identification strategy

The analyses in this paper draw on data from the Spanish subsample of the sixth round of the ESS, which was fielded between January 23rd and May 12th 2013, and includes a

question on how much individuals trust politicians, which is measured in an 11 points scale where 0 means no trust at all in politicians and 10 means absolute trust.²

As mentioned earlier, one of the main difficulties in identifying the causal effect of corruption on trust in politicians is to find exogenous variation in corruption or perceptions of corruption. In contrast to lab experiments, in our case, we do not need to generate exogenous variation in corruption ourselves, since we capitalize on the Bárcenas case being uncovered while the ESS was being fielded. This identification strategy relies crucially on the assumption that the timing at which respondents were interviewed is as-if random, so that the uncovering of the Bárcenas case provides an exogenous source of variation for the levels of corruption to which respondents are exposed. As such, the day at which the interview was conducted serves as the variable that assigns individuals to the treatment and the control group. Those who were interviewed before the scandal was uncovered are assigned to the control group, and those who were interviewed after the scandal are assigned to the treatment group. Hence, we assume that given the visibility and ubiquity of the Bárcenas affair everyone, even those least informed about politics, was exposed to the scandal after January 31st. Therefore, the treatment variable can be defined as:

$$D_i = \begin{cases} D_i = 0 & \text{(control group) if subject } i \text{ was interviewed before January 31}^{\text{st}}. \\ D_i = 1 & \text{(treatment group) if subject } i \text{ was interviewed between January 31}^{\text{st}} \\ & \text{and February 28}^{\text{th}}. \end{cases}^3$$

² Details about the coding of all variables used in this paper can be found in Appendix A.

³ We do not include respondents interviewed after the 28th of February in the treatment group for two reasons. First, we expect the treatment to have an effect mainly on those who were interviewed closer to the date of the treatment (see Legewie (2013)). Second, because including individuals that were interviewed further away from the event increases the likelihood that potential sources of bias (mainly reachability bias) play a more relevant role on the assignment to treatment.

One of the crucial assumptions to reliably identify the causal effect of the treatment is that the timing of the survey is exogenous, and hence independent of other third variables that could affect levels of trust in politicians (thus generating systematic differences in trust between the treatment and control group). Potential threats to this assumption come from differential reachability of the sampled survey respondents, since not all sampled individuals are equally reachable for an interview. There are certain individuals that are harder to reach, and for this reason they might be likely to be interviewed at later stages during the fieldwork period. If these factors that determine individuals' reachability are related to political trust, this will generate systematic differences between the treatment and the control group that are unrelated to the treatment, and might bias our estimate of the effect of the corruption scandal. To assess the plausibility of this assumption (ignorability assumption), we conduct a series of balance tests between the treatment and control groups on several pre-treatment variables. Moreover, we also estimate the effect of the treatment controlling for a set of pre-treatment covariates and on a matched sample on these same covariates. Since we also have information on how many times an individual refused to be interviewed before it was possible to complete the interview, we also take this information into consideration in the models as a robustness check.

Another important assumption in this identification strategy is the exclusion restriction, which requires that the outcome variable (trust in politicians) is affected by the treatment a subject receives (having been exposed to the Bárcenas affair or not) but is not otherwise influenced by their assignment to the treatment or control status (i.e. by the time at which the interview was conducted). The effect of having been interviewed before or after the 31st of January on trust in politicians should only go through the treatment

(having been exposed to the corruption scandal) conditional on covariates. This implies that there should not be any time trends on trust in politicians that are not driven by the exposure to the scandal. This could be problematic to the extent that there were other factors that might change through time and affect trust in politicians. In that case, the differences we observe between the treatment and control groups in terms of trust could be a consequence of time-varying factors apart from the scandal.

To evaluate this potential threat we test, through a simulation of fictitious placebo events, whether there is a significant difference in trust at other dates chosen at random. Furthermore, the plausibility of this assumption can also be studied by comparing the treatment and control group on other outcome variables unrelated to the treatment. If we do not find differences on them we can be more confident that no other relevant event took place simultaneously to the corruption scandal that could contaminate the estimated effect of corruption scandal on trust in politicians.

Size of treatment and control groups and balance on covariates

Since the assignment of survey respondents to the treatment or control group depends on the day at which the survey was conducted, the number of individuals in each group depends on the distribution of respondents across the fieldwork period. The distribution of respondents in the control and treatment group is unbalanced, with more respondents in the treatment than in the control group. Specifically, there are 230 individuals in control, and 1,198 individuals in treatment (see figure 2 for further details about the distribution of respondents)

On average, the treatment and control groups should not differ on pre-treatment covariates that could be associated to trust in politicians. To test for this, we perform

balance tests along a set of covariates. Table 1 displays the results for two-sample t-tests for equality of means between the treatment and control groups. The pre-treatment covariates considered are: years of education completed, gender, age, employment status, participation in the last national election and having voted for the winning party (PP) in the last national election. The party supported by respondents can affect political trust, with supporters of the incumbent party usually displaying higher levels of political trust (Anderson and Tverdova, 2003). If the control and treatment group were imbalanced, with more supporters of the ruling party on either of the two, this could bias the estimation of the treatment effect since part of the difference between these two groups could be attributed to the different relative number of winners and losers in each of them.

(TABLE 1 HERE)

As the results on table 1 indicate, most of the pre-treatment covariates are similarly distributed within the control and treatment groups. The only statistically significant difference in means between the two groups appears for employment status. There are more respondents in paid work in the treatment group and more respondents outside of the labor market in the control group. In terms of having participated in the last national election, supporting the winning party, age, gender and years of education there are no statistically significant differences. Two-sample Kolmogorov-Smirnov tests for equality of distribution functions were conducted for the same variables, obtaining equivalent results (results not shown). These tests indicate that the potential selection bias (the reachability bias) appears to exert only a minor influence on whether individuals were interviewed before or after the scandal. This supports the plausibility of the ignorability assumption, and provides support for the characterization of the event as a natural experiment. Some of the analyses presented in the next section, nevertheless, also include controls for these pre-treatment variables.

Due to the multistage sampling procedure implemented in the ESS, and because the scandal broke early during the fieldwork period, some of the Autonomous Communities (regions) in Spain were surveyed after treatment only. Therefore, there are no individuals in control in those regions. For this reason, as a robustness check we re-estimate our analyses using only those regions in which there are observations both in the treatment and control groups.⁴ Nevertheless, having found that there are only minor imbalances on the pre-treatment covariates gives us confidence that the regional imbalance is ignorable. If the regional sampling generated bias, this should be reflected in the distribution of these pre-treatment variables.

RESULTS

The causal effect of the Bárcenas corruption scandal (H_1)

Figure 2 summarizes average levels of trust in politicians (a moving average of 2 days) across the fieldwork period in January and February, separately for the treatment and the control groups. In the same graph we plot a histogram of the number of respondents per day. The x-axis ranges from -10 to 30, and the value 0 corresponds to January 31st. As this figure shows, there is a substantial drop in average trust in politicians on January 31st. Before the Bárcenas scandal broke the average level of trust in politicians in the control group is of 2.3 points (on the 11 point scale from 0 to 10), after the scandal, the average trust in the treatment group is of 1.8 points. Thus, there is a -0.5 point difference between the two groups, a reduction in trust that is quite remarkable given the already low levels of trust of most Spanish citizens before the scandal.

⁴ The analysis conducted including only individuals living in regions with observations both in treatment and control do not alter the substantive results and lead us to the same conclusions (results not shown but available upon request).

(FIGURE 2 HERE)

Table 2 presents the results from a regression analysis, in which the treatment indicator (D) is the main variable of interest. The first column reports the results of an OLS regression model including only the treatment indicator. The coefficient associated to it indicates that respondents who were interviewed after *El País* uncovered the Bárcenas scandal have, on average, less trust in politicians. The difference is of 0.48 points and statistically significant at $p < 0.01$. The second model includes controls for the pre-treatment covariates on which the balance tests were estimated (since one of these variables proved to be imbalanced between the control and treatment group). Once we control for these covariates we observe that the size of the coefficient for the treatment indicator is slightly reduced, but it remains statistically significant and substantively large. Individuals interviewed after the Bárcenas scandal are 0.39 points less trustful of politicians. Model 3 includes region fixed-effects since, as we had pointed out, some respondents of some regions were only interviewed in the treatment period, and regional imbalances could bias the estimate of the treatment effect. In this third model the difference between those who were exposed to the Bárcenas affair and those who were not is of -0.45 points. This difference is significantly larger than the difference between election winners and losers, and it is equivalent to a difference of more than 17 years of education. In line with our first hypothesis this model, hence, shows that the Bárcenas scandal had a substantive negative effect on individuals' trust in politicians.⁵

⁵ We have also analyzed the effect of the Bárcenas scandal using propensity score kernel based matching, estimated using the same pre-treatment covariates introduced in model 2. To increase the reliability of the treatment effects obtained through the matching procedure the common support restriction is imposed on the matched units. The estimated treatment effect obtained from the matching estimator equals -0.389 and is significant at $p < 0.05$. Standard errors for the matching estimator are based on bootstrapped standard errors (200 replications). The matched sample is restricted to regions that were surveyed both in treatment and control.

(TABLE 2 HERE)

Changes in the treatment effect over time (H_2)

The results from the previous models are based on a comparison in which the treatment group is composed of individuals interviewed on January 31st or during the month of February. In order to test our second hypothesis we estimate a series of regression models in which the time interval for the treatment group is moved away from the event by 5 days at each point. The control group remains the same for all the estimations, while the interval for the treatment group increases. For example, for the first estimation the treatment group comprises those interviewed between January 31st and February 6th, for the second estimation it comprises those interviewed between January 31st and February 11th. This process is repeated until the final date of the fieldwork (May 12th) is reached. This allows us to analyze whether the effect of the treatment weakens as we incorporate to the treatment group individuals interviewed further away from the day when the scandal was uncovered. The model estimated to calculate these effects is specified with the same pre-treatment covariates included in model 3 of table 1.⁶

(FIGURE 3 HERE)

Figure 3 summarizes how the estimate for the treatment indicator (D) changes as individuals interviewed at a latter point in time are incorporated to the treatment group. The trend line in figure 4 indicates that there is a decay of the effect over time. When we restrict the treatment group to those interviewed just after the scandal was uncovered (until February the 6th), the difference in trust in politicians between those who were

⁶ We have re-analyzed the balance on pre-treatment covariates computing means comparisons (t-tests) between the original control group and the enlarged control group, which includes all individuals interviewed after January 31st. Again, we only find statistically significant differences between the two groups for employment status, for which there are more respondents in paid work in the treatment group and more individuals outside the labor market in the control group.

exposed to the scandal and those who were not is of 0.50 points and statistically significant at $p < 0.05$. Over time, as we incorporate to the treatment group individuals who were interviewed further away from when the scandal was uncovered, the size of the coefficient D decreases in size. In fact, when those interviewed in May are incorporated to the treatment group, the treatment effect shrinks to -0.32 and ceases to be statistically significant at conventional levels. In line with our second hypothesis, these results support the idea that the scandal informed citizens' evaluations of the trustworthiness of politicians to a greater extent on the days and weeks that followed the publication of the Bárcenas documents.

Treatment effect heterogeneity (H_3)

To test the third hypothesis about the potential moderating role of partisan predispositions, in model 4 of table 1 we specify an interaction between the treatment indicator (D) and the election winner variable, which captures whether the respondent voted for the PP or not in the last elections.⁷ Although partisanship (i.e. identifying or being close to PP) would be a more adequate indicator of partisan predispositions, we rely on the party voted for because partisanship could be affected by the treatment itself. The interaction coefficient indicates that there are no significant differences in the effect of being exposed to the Bárcenas scandal between those who voted for PP and those who did not. Hence, contrary to our expectations, the treatment effect seems to be the same independently of the party supported by the respondent.

There are two potential reasons why our results might not support the hypothesis that the impact of a corruption scandal should be weaker for those individuals who support the party involved in the scandal. The first of these reasons is scandal-specific. In

⁷ Among those who did not vote for PP we also include those who did not vote in the last elections.

line with Zaller's (1992) considerations, the intensity, clarity and simplicity of the scandal allegations, as well as the fact that all media outlets independently of their ideological leaning reported on the scandal may have reduced the partisan bias in the perceptions of the scandal. Moreover, some of the scandal allegations were supported by leaked documents, and some members of PP even acknowledged the veracity of some of these documents. This might have increased the credibility of the scandal even for PP voters, which should have reduced the partisan bias in the perception of the seriousness of the scandal. Furthermore, in a context of high media polarization, the fact that both left- and right-wing leaning media outlets provided extensive coverage of the scandal may have signaled to PP supporters that the scandal allegations were not an attempt of left-biased media to discredit PP. The second reason why we might not find an interaction effect is country-specific. Before the scandal, Spaniards not supporting the government party already had low levels of political trust (average 2.09). This might have predated the impact of the scandal for those not supporting PP, since for these individuals there was already little trust in politicians to be lost before the scandal was uncovered.

ROBUSTNESS CHECKS

Placebo tests

An important robustness check in these analyses is to check that the treatment indicator is actually capturing the effect of the corruption scandal and is not just capitalizing on a trend of declining trust in politicians over time. To check for this we perform a simulation of 1000 random (placebo) events during the whole survey fieldwork. Drawing from a random uniform distribution we generate 1000 fictitious events with the condition that, as in our analysis, the control group comprises 8 days before the fictitious event took place and the treatment group comprises 28 days after that event. The placebo events can

take place at any point in time between February 1st and April 14th. Figure 4 summarizes the random distribution of these events. The second step in the simulation is to recover the parameter of interest (\tilde{D}) and its associated t-statistic (\tilde{t}) for each of the placebo events, which are estimated using the fully specified model (i.e. model 3 table 2). Table 4 summarizes the results of the simulation. These results reveal that 11 percent of the simulations produced a treatment effect equal or higher than the one estimated with the original event, and that in 4.8 percent of the simulations this effect was statistically significant at least at $p < 0.05$. It was expected that a certain percentage of the simulated events would produce similar results to the ones obtained with the original scandal. Since the events are generated at random, a certain proportion of them fall close to the original event date, and, thus, the simulated control and treatment groups resemble the original control and treatment groups. Hence, the fact that only 4.8 percent of the random events produce statistically significant treatment effects that are equal or higher to the ones originally estimated, and the fact that in none of the simulations we obtain both a treatment parameter \tilde{D} and a t-statistic \tilde{t} that are higher than the ones originally estimated, adds further plausibility to our causal estimate of the corruption scandal.

(FIGURE 4 HERE)

(TABLE 3 HERE)

Effects on other outcome variables

As a next robustness check, we analyze the impact of the treatment indicator on outcome variables that should be unaffected by the Bárcenas affair. If we find an effect on these variables, this would cast doubts on our treatment indicator and could indicate that the difference we find in trust in politicians between the treatment and the control group is not due to the Bárcenas scandal but is rather associated to other unobserved differences

between the treatment and control groups, or to another event that took place contemporarily to the publication of the Bárcenas documents. To test for this, we run a first model in which we assess the impact of the corruption scandal on trust in the United Nations, which should not be affected by scandals involving Spanish politicians. Moreover, we fit a second model in which the dependent variable reflects how important it is for respondents that the government protects all citizens against poverty. This is a measure of respondents' preferences that could be associated to other characteristics of respondents that have been frequently associated to trust (e.g. age, education level) but which should be unaffected by the Bárcenas scandal. In fact the regression models in table 4 (models 2 and 3) indicate that exposure to the Bárcenas scandal is not associated to any of these two outcome variables. In both of them the coefficient associated to the treatment indicator is of reduced magnitude and not statistically different from zero.

(TABLE 4 HERE)

Reachability bias

One of the threats to the ignorability assumption comes from the differential reachability of survey respondents. This is why we introduced controls for pre-treatment variables that could potentially be associated to the reachability of individuals in the regression models. As an additional check of the robustness of our results we add to our analyses information from ESS paradata about the number of times a given individual declined to reply to the questionnaire after having been contacted, which serves as a proxy for the respondent's reachability and motivation to participate in the survey process. The number of refusals ranges from 0 to 5, and across its distribution there are observations both in the treatment and the control group. The results of model 1 of table 4 show that introducing this control

variable in our models does not substantially alter the size of the coefficient of the treatment indicator. Hence, our results appear to be robust to a specification that explicitly takes into consideration the potential association between the time at which the interview was conducted and the reachability of individuals.

CONCLUSION

Although existing studies identify a negative association between corruption (or corruption scandals) and political support, most of them fail to isolate the causal effect of scandals on citizens' attitudes. In this paper we exploit the identification advantages generated by the fact that the uncovering of the Bárcenas scandal coincided with the fieldwork of the sixth round of the ESS in Spain. This quasi-experimental situation generated an exogenous source of variation in levels of corruption that allows us to rely on the day in which the ESS interview was conducted to assign individuals to the control and treatment conditions. This has provided a unique opportunity to estimate the causal impact of a real-world and large corruption scandal on citizens' trust in politicians with a great degree of internal and external validity.

Our results reveal that the corruption scandal has a corrosive effect on citizens' trust in politicians and that this effect is substantial. As a matter of fact, the impact of the Bárcenas case is larger than the difference in trust in politicians between election losers and winners. Our results contrast with those of Kumlin and Esaiasson (2012) who argue that political scandals involving only one party do not affect satisfaction with democracy. To provide further support for the causal interpretation of this effect we conducted a series of robustness checks that increase our confidence in the validity of our identification strategy and the results obtained. A further advantage of our research design is that it also allowed us to test how the effect of the scandal changed over time. While in the short-

term the scandal has a substantial effect, this effect weakens over time. Again, this result clearly contrasts with the arguments of Kumlin and Esaiasson (2012) who argue that the effects of political scandals are likely to be long lasting and detectable months after the political scandal occurred. Our results also indicate that the effect of the corruption scandal does not vary depending on whether the respondent supports the party involved in the scandal or not. As we argue above, it is possible that specific characteristics of the Bárcenas scandal, as well as the low initial levels of trust in politicians in Spain prior to the scandal contribute to this non-finding. Future research should analyze whether this is also the case for other types of scandals (e.g. less salient scandals), and also whether, as Anduiza et al. (2013) argue, the effect of the scandal and the moderating role of partisanship might also be moderated by political sophistication.

This paper has focused on the impact of a large and salient corruption scandal in the Spanish context. Are these results generalizable to other contexts and types of scandals? The Spanish context is characterized, first, by a high prevalence of corruption scandals and high levels of perceived corruption, as attested by the CPI and by the fact that corruption is considered one of the country's most important problems. Hence, it is possible that in contexts in which corruption is not considered an important problem citizens, as well as the media, pay less attention to corruption scandals. If this is the case, political scandals might not inform citizens' attitudes towards the political system to such a great extent. However, it is also possible that the magnitude of the effect we find in the Spanish context was mitigated by a floor effect, since the baseline levels of trust in politicians were already low before the scandal. There is reason to believe that in contexts where baseline levels of trust are higher (e.g. Scandinavian countries) the impact of corruption scandals could be accentuated. With regard to the nature of the scandal, other events not involving key members of the government party, or scandals having a regional

or local nature could have a more limited impact on citizens' attitudes towards the political system since, as Fernández-Vázquez et al. (2015) show in the case of the electoral consequences of scandals, the magnitude of the electoral punishment associated to scandals increases with the breadth of media coverage. Future studies should analyze the impact of different types of corruption scandals in different contexts.

This paper also has implications for future research focused on the impact of trust in politicians on other variables. This research is also usually threatened by endogeneity biases. Given that we have identified that exposure to the Bárcenas corruption scandal is an exogenous source of significant variation in political trust, this corruption scandal could be used as an instrument to analyze the impact of political trust on other third variables, such as trust in supranational institutions (e.g. EU institutions).

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FIGURE 1: GOOGLE TRENDS FOR THE SEARCH 'CASO BARCENAS' FOR JANUARY AND FEBRUARY 2013 IN SPAIN

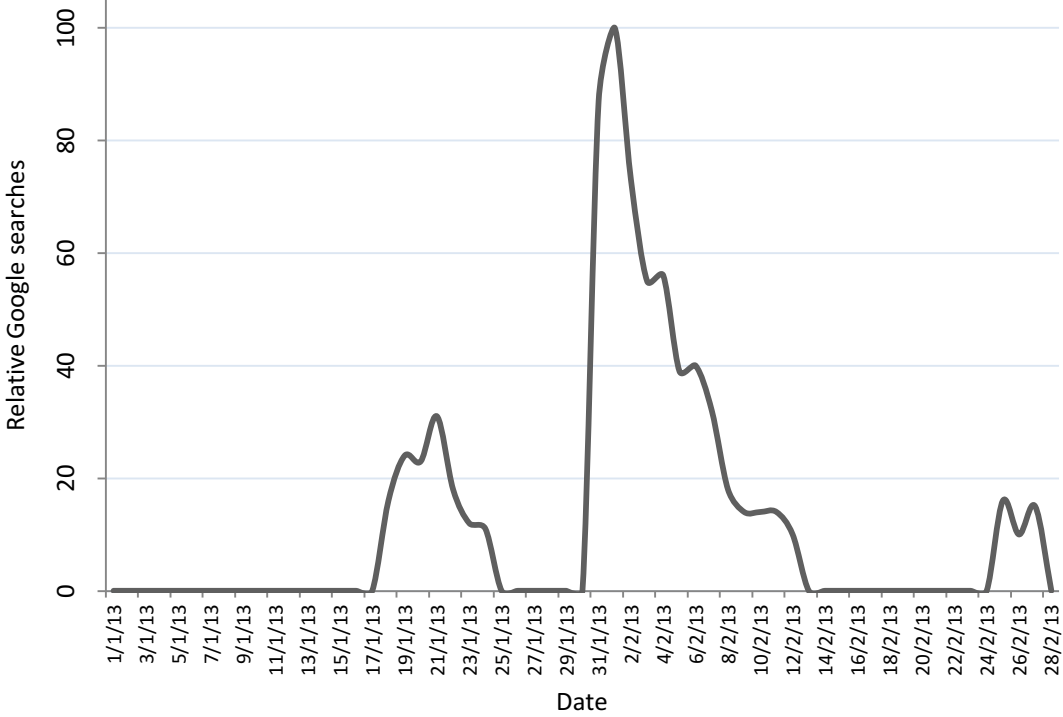


TABLE 1: TWO-SAMPLE T-TESTS

VARIABLES	Mean		P-value	Valid N
	Treatment	Control		
Years of education	12.5	13.07	0.262	1409
Gender	1.51	1.53	0.587	1428
Age	46.9	49.3	0.062	1428
Employment status				
In paid work	0.445	0.354	0.011	1422
In education	0.09	0.109	0.353	1422
Unemployed	0.158	0.14	0.475	1422
Out of the labor market	0.294	0.393	0.003	1422
Election winner	0.266	0.274	0.811	1428
Participation in national election	0.783	0.757	0.410	1336

FIGURE 2: CHANGE ON AVERAGE TRUST IN POLITICIANS AT THE TIME OF THE BÁRCENAS SCANDAL

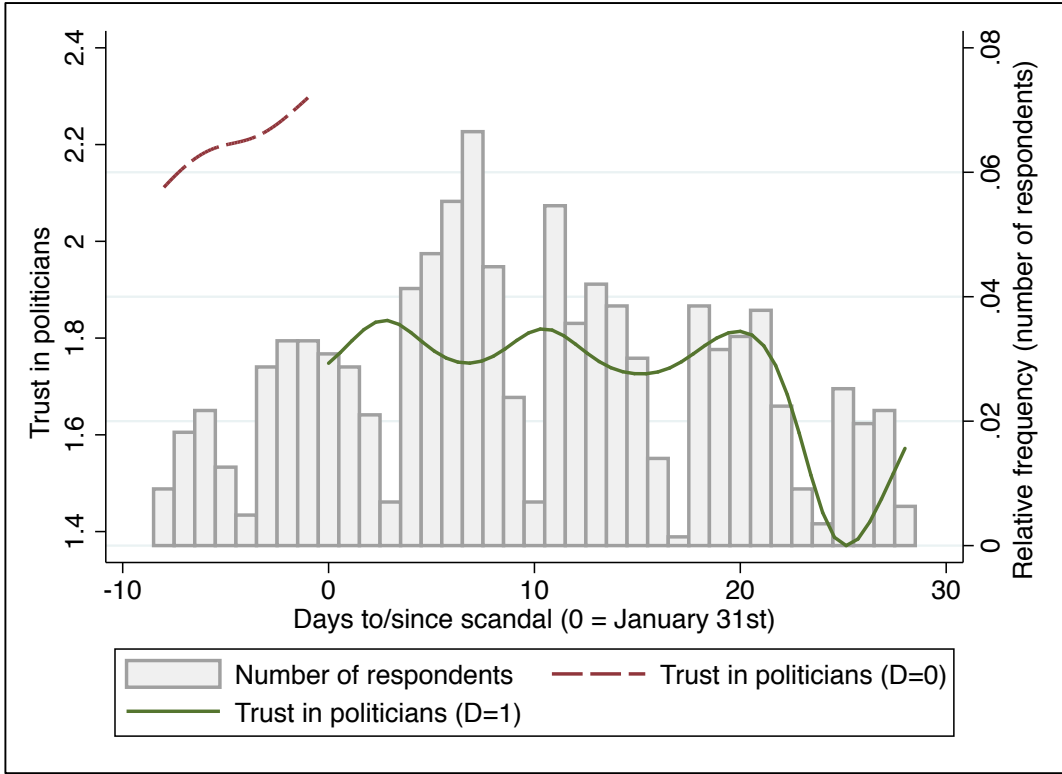


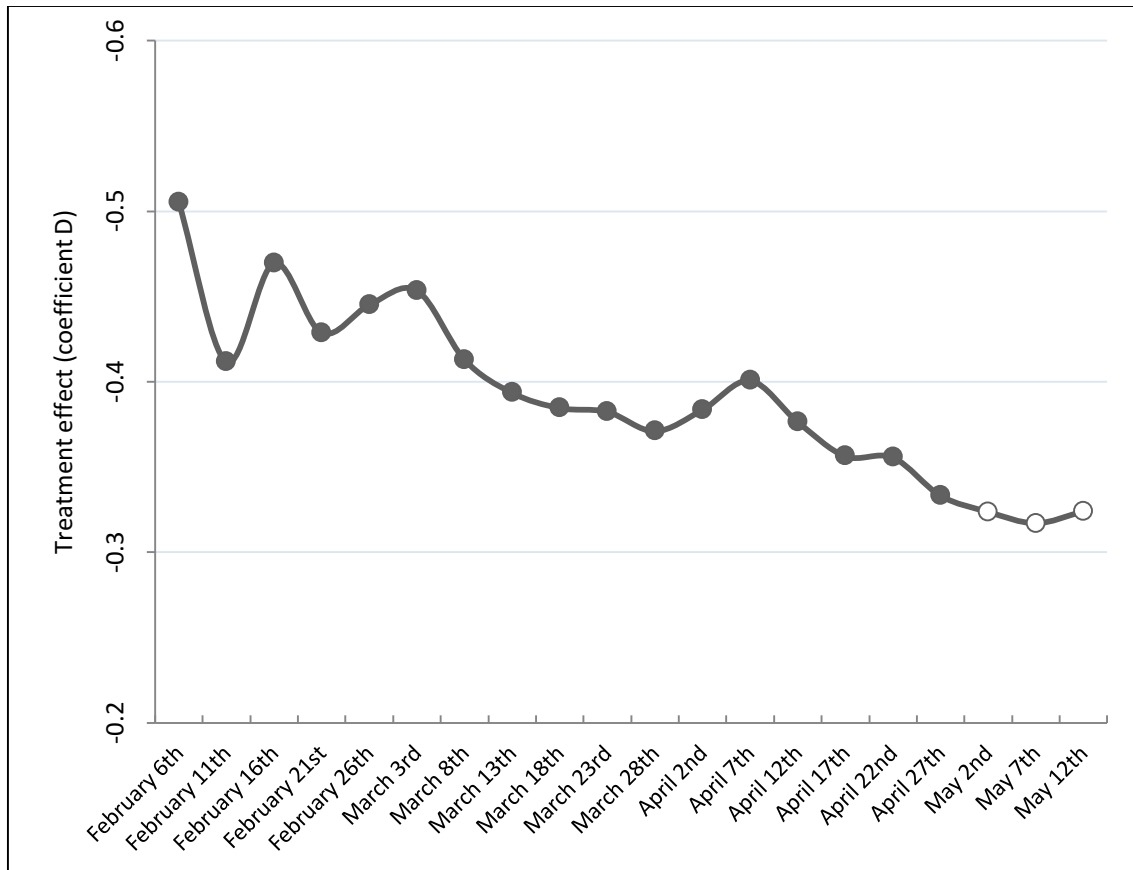
TABLE 2: THE IMPACT OF THE SCANDAL ON TRUST IN POLITICIANS

VARIABLES	Model 1	Model2	Model 3	Model 4
D (Exposure to the Bárcenas scandal)	-0.483** (0.151)	-0.390* (0.152)	-0.451** (0.168)	-0.410* (0.193)
Election winner		0.342** (0.126)	0.306* (0.130)	0.430 (0.311)
Election winner*D				-0.149 (0.339)
Female		-0.139 (0.112)	-0.158 (0.112)	-0.157 (0.112)
Years of education		0.023* (0.010)	0.027** (0.010)	0.027** (0.010)
Age		0.008 (0.005)	0.009 (0.005)	0.009 (0.005)
Employment status (Ref: In paid work)				
In education		0.713** (0.227)	0.698** (0.228)	0.699** (0.228)
Unemployed		-0.308 (0.166)	-0.360* (0.168)	-0.359* (0.169)
Out of the labor market		0.282 (0.174)	0.286 (0.175)	0.284 (0.175)
Other		-0.525 (0.523)	-0.565 (0.526)	-0.562 (0.526)
Region fixed-effects			YES	YES
Constant	2.224*** (0.138)	1.376*** (0.327)	1.155** (0.389)	1.113** (0.401)
Observations	1,414	1,392	1,392	1,392
R-squared	0.007	0.036	0.051	0.051

Standard errors in parentheses

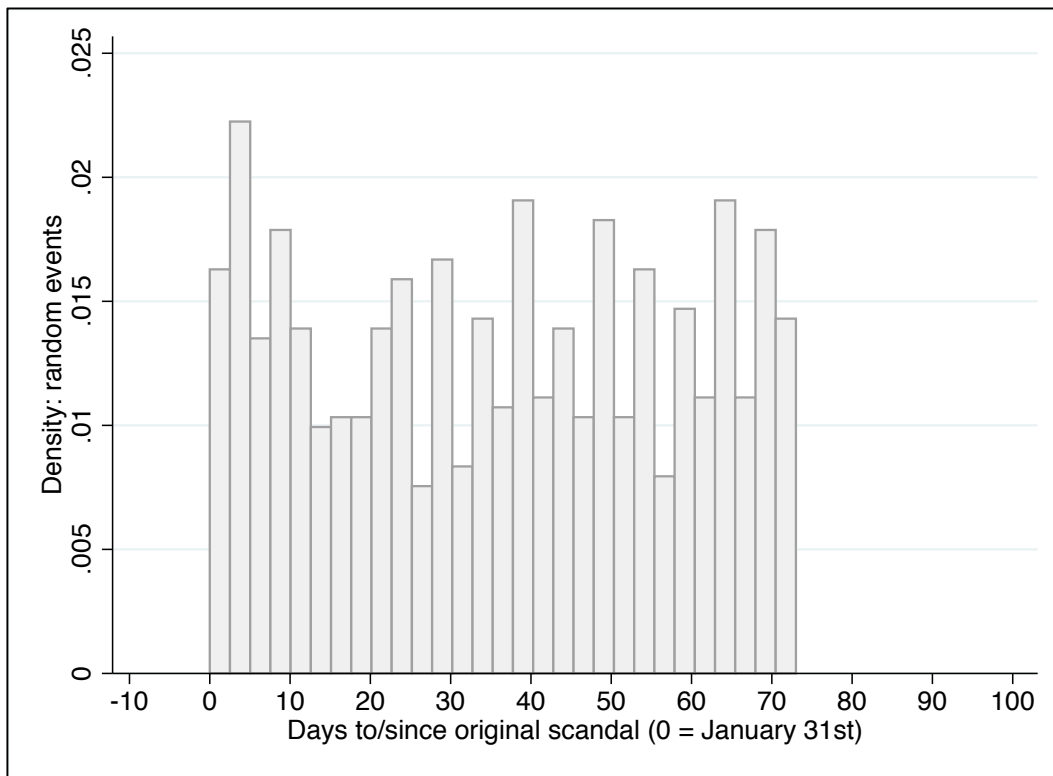
*** p<0.001, ** p<0.01, * p<0.05

FIGURE 3: CHANGE IN THE TREATMENT EFFECT OVER TIME



Note: The x-axis indicates the last day included in the treatment group. Round markers with filling denote coefficients that are statistically significant at least at $p < 0.05$. Round markers without filling denote coefficients that are not statistically significant.

FIGURE 4: RANDOM EVENTS DISTRIBUTION



Note: The x-axis ranges from the first (-10) till the last (101) of the days in the survey fieldwork

TABLE 3: RANDOM EVENTS SIMULATION RESULTS

Mean \tilde{D}	$\tilde{D} \leq D$	$\tilde{D} \leq D \text{ and } \tilde{t} \geq 1.96$	$\tilde{D} \leq D \text{ and } \tilde{t} \leq t$
-0.01	11.8 %	4.8 %	0 %

Note: The superscript \sim identifies simulated parameters (i.e. parameters without \sim correspond to the ones originally estimated). Since the estimated treatment effect is negative a smaller D indicates a stronger treatment effect. The same applies to t-values.

TABLE 4: ROBUSTNESS CHECKS

Dependent variable	Model 1 Trust in politicians	Model 2 Trust in the UN	Model 3 Gov't poverty reduction
D (Exposure to the Bárcenas scandal)	-0.438** (0.169)	-0.153 (0.215)	0.015 (0.107)
Refusals before completion	-0.055 (0.074)		
Female	-0.151 (0.113)	-0.116 (0.144)	0.024 (0.072)
Years of education	0.027** (0.010)	0.011 (0.013)	0.004 (0.006)
Age	0.008 (0.005)	-0.007 (0.006)	0.008* (0.003)
Employment status (Ref: In paid work)			
In education	0.686** (0.228)	1.000*** (0.287)	-0.312* (0.145)
Unemployed	-0.358** (0.169)	-0.556** (0.213)	0.109 (0.108)
Out of the labor market	0.285 (0.175)	0.129 (0.228)	-0.124 (0.112)
Other	-0.579 (0.526)	-0.359 (0.668)	-0.166 (0.345)
Election winner	0.304* (0.130)	0.512** (0.166)	-0.090 (0.083)
Region fixed-effects	YES	YES	YES
Constant	1.186** (0.392)	4.357*** (0.506)	8.890*** (0.249)
Observations	1,392	1,277	1,381
R-squared	0.052	0.055	0.042

Standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05

Appendix A: Question wording

Variable	Wording
Dependent variables	
Trust in politicians	"Using this card, please tell me on a score of 0-10 how much you personally trust each of the institutions I read out. 0 means you do not trust an institution at all, and 10 means you have complete trust. ... <i>Politicians?</i> "
Trust in the United Nations	"Using this card, please tell me on a score of 0-10 how much you personally trust each of the institutions I read out. 0 means you do not trust an institution at all, and 10 means you have complete trust. ... <i>the United Nations?</i> "
Government intervention to reduce poverty	"How important do you think it is for democracy in general that the government protects all citizens against poverty?" Response scale ranges from 0 (Not at all important for democracy in general) to 10 (Extremely important for democracy in general)
Independent variables	
D (treatment variable) Exposure to the Bárcenas scandal	Variable coded based on information on the day the respondent was interviewed. Coded as 0 if interviewed before January 31 st , and 1 if interviewed between January 31 st and February 28 th .
Election winner	Coded based on the question "Which party did you vote for in that election?" (the last national election). Coded as 1 if respondent reports having voted for the Popular Party (PP – Partido Popular), 0 otherwise.
Female	Gender of respondent, coded 0 for male and 1 for female.
Years of education	"About how many years of education have you completed, whether full-time or part-time? Please report this in full-time equivalents and include compulsory years of schooling"
Age	Age in years
Employment status	Categorical variable coded based on the question: "which of these descriptions best describes your situation (in the last seven days)? Please select only one." Response categories are: <ul style="list-style-type: none"> - In paid work (corresponds to employees, self-employed and working for the family business) - In education - Unemployed (both actively and not actively looking for a job) - Out of the labor market (includes permanently sick or disabled, retired, in community or military service, and doing housework, looking after children or other persons)
Region	Spanish Autonomous Communities
Days to/since the scandal	Number of days before/after January 31 st (January 31 st is coded as 0)
Participation in national election	"Did you vote in the last national election?"
Refusals before completion	Coded from the question in ESS paradata about "Results of n th visit" to the respondent. Number of refusals corresponds to the number of times

that the interviewer established contact with the respondent at the visit but without achieving to conduct the interview.
