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DOES CORRUPTION ERODE TRUST IN GOVERNMENT? EVIDENCE FROM A RECENT SURGE OF LOCAL SCANDALS IN SPAIN [*]

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ABSTRACT: We examine whether a corruption scandal in which the incumbent is implicated undermines trust in local government. We use a novel dataset containing information on local corruption scandals reported in Spain during the period 1999-2009, and data on the level of trust expressed in local politicians drawn from a new survey conducted in late 2009. We use matching methods to improve the identification of the effect of corruption scandals on trust, comparing municipalities affected by a scandal with those presenting similar traits but in which no scandal had been reported. We find that corruption scandals have a marked negative effect on trust in local politicians. This effect is even more marked in the case of individuals that have no ideological attachment to the party accused of corruption and/or who obtain their information from the media. Several falsification tests, based on a sample of corruption scandals reported after the survey had been conducted, confirm the causal interpretation of these results.

JEL Codes: P16, D72

Keywords: Corruption, trust

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1. INTRODUCTION

Widespread political corruption is commonly viewed as constituting a severe threat to public trust in political institutions. After all, one of the principles underpinning a democratic political system is the presumption that governments are accountable to citizens (Przeworski *et al*., 1999). As such, the abuse of the public power entrusted to elected government officials undermines these procedures of accountability (Bardhan, 1997) and corruption systematically erodes democratic principles and the faith of citizens in the political process. As a result, disaffected citizens are liable to withdraw from the electoral process (Chong *et al*., 2011), or they may even resort to less legitimate means of protest as they seek radical changes in the system (Mihsler and Rose, 1997; Kostadinova, 2009).

Several papers have shown that corruption does indeed have a negative effect on levels of trust in government (see, e.g., Anderson and Tverdova, 2003; Chang and Chu, 2006; Bowler and Karp, 2004; Kumlin and Esaiasson, 2012, among others). However, doubts remain as to whether these negative effects can actually be interpreted to be causal. Most of the empirical studies rely on measures of 'perception of corruption' and 'statements of trust' obtained form the same survey (e.g., Chang and Chu, 2006), and so it is likely that both responses reflect the same underlying individual characteristics. Even papers using more appropriate corruption measures (e.g., Anderson and Tverdova, 2003; Bowler and Karp, 2004) face serious identification issues, since they compare units (usually countries) that not only present different levels of corruption but differences with regard to many other dimensions. Here, the impaired ability of regression analysis to adjust for differences in observed covariates is well documented when the between-group differences in these covariates are substantial (e.g., Cochran, 1965; Rubin, 2001). Moreover, none of these studies addresses the possibility that corruption and trust might be affected at the same time by influences that are not observable to the researcher (see, e.g., Uslaner, 2004).

The main contribution of our paper therefore is the use of an improved identification strategy, based on the use of matching methods complemented with falsification tests. The use of this empirical strategy is possible thanks to the availability of a novel data set providing information on a recent wave of local corruption scandals in Spain and data drawn from a new survey of trust in local government. Thus, we know whether a municipality has experienced at least one corruption scandal (defined as a "public allegation of corruption brought to light by a newspaper") during the three

terms-of-office previous to our survey, while the trust survey contains information about the stated level of trust in local government among a sample of individuals in municipalities affected by a corruption scandal before the survey was carried out vs. municipalities in which no scandal was reported. The control group was selected using matching techniques and so it comprises corrupt-free municipalities that are identical in terms of observables to the corrupt-ridden municipalities. Interestingly, some of the municipalities that were corrupt-free before the survey was conducted were affected by corruption scandals in subsequent years. It is precisely this fact that allows us to design 'falsification' tests to confirm (or discard) the causal interpretation of our results.

This paper makes an additional contribution: it is the first, to the best of our knowledge, to analyse the effect of corruption scandals involving local incumbents on the level of trust in the local government. Earlier studies have focused on the determinants of trust in local government (Rahn and Rudolph, 2005), but they do not address the effect of corruption. Others have analysed the effect of local government performance and corruption on the level of trust in politicians in general, but they do not study the effect on trust in local politicians (Weitz-Shapiro, 2008). Some papers have studied the effect of corruption on local electoral turnout (Escaleras *et al*., 2012; Chong *et al*., 2011[1]), but none examines its impact on local trust. Clearly, focusing on trust in local government is interesting in its own right, given the implications of such findings for any evaluation of decentralization reforms[2], and also because local governments can be considered an essential part of any democratic system[3].

The results of our analysis suggest that, on average, corruption scandals involving local government do have a negative and non-negligible effect on trust in local politicians. This effect is even more marked in the case of individuals that have no ideological attachment to the party accused of corruption and/or who obtain their

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[1] Our paper is also related to that line in the literature which seeks to determine whether corrupt incumbents are punished at the polls (see, e.g., Peters and Welch, 1980; Dimock and Jacobson, 1995; and Welch and Hibbing, 1997, for the US; Chang *et al*., 2010, for Italy; Ferraz and Finan, 2005, for Brazil; Larcinese and Sircar, 2012, and Eggers, 2013, for the UK; and Costas *et al.,* 2012, and Barberá *et al*., 2013, for Spain).

[2] Local government is the tier closest to the citizens and any evaluation will depend largely on how accountable it is to local residents (Bardhan, 1997). Local corruption scandals might erode confidence in local government and generate demands for greater centralization.

[3] The establishment of elected local governments is a crucial step in the development of new democracies and, in fact, often precedes the democratization of the country at higher levels (see, e.g. Martínez-Bravo, 2014). The effect of scandals on trust in local government is often a prelude to what will happen at other tiers since local scandals have ramifications for politicians at higher layers.

information from the media (as opposed to on the grapevine or from local government itself). Several falsification tests confirm the causal interpretation of these results. We show, for example, that scandals reported after the survey was conducted had no influence on the stated levels of trust. We also show that scandals breaking out before the 2007 local election had an effect on turnout in that contest, but that scandals that occurred after that event did not have any effect on turnout. Overall, these results suggest that the effects of corruption on trust can be interpreted as causal.

The paper is organized as follows. The next section reviews the previous literature on corruption and trust, focusing first on the different empirical approaches used. The third section provides some background for the analysis by describing the recent surge in local corruption scandals in Spain and the current discussion regarding how this affects trust in government and the legitimacy of the country's political institutions. Section four describes the data (i.e., corruption database and trust survey) and the methods. Section five presents the results. The last section concludes.

2. EMPIRICAL LITERATURE

Perceptions of corruption. Most papers studying the effects of corruption on trust in government use individual responses to questions on 'perception of corruption' and 'statements of trust' in government (e.g., Seligson, 2002; Chang and Chu, 2006; Morris and Klesner, 2010)[4]. The results of most of these studies suggest that countries with higher levels of corruption do indeed show lower levels of trust in government. It is not clear, however, whether this result is indicative of a mere correlation between variables or whether it can be interpreted as a causal effect. The main concern with this approach is that it is affected by the 'chicken-and-egg' problem, with both variables being measured from survey data and, more often than not, the two are even taken from the same survey[5]. It is thus conceivable that individuals respond in the same way to two questions that they believe to be quite similar. Some authors deal with this problem by employing a simultaneous equation system (e.g., Chang and Chu, 2006), but then face the obvious difficulty of justifying the exogeneity of the instruments.

Contextual-level corruption. Only a few papers combine individual-level trust variables with contextual-level corruption measures. The most frequently cited paper is

[4] Other similar studies include Lavallée *et al*. (2000), Mishler and Rose (2001), Cho and Kirwin (2007), Lavallée *et al.* (2000) and Bratton (2007).

[5] Another problem with the use of 'perceptions of corruption' is that they are often biased (see Olken, 2004; and Donchev and Uhjely, 2011).

Anderson and Tverdova (2003), who also find a negative and statistically significant effect of corruption on trust in government. Their study draws on country-level information on corruption perceptions from Transparency International, whose surveys are conducted among experts and businessmen. As such the source is not the same as the one used for the trust variable and so they are able to overcome the aforementioned 'chicken-and-egg' problem. However, the aggregate nature of this index, which mixes the opinions of different agents on different kinds of corruption, means it is not that evident how these evaluations of corruption are linked to the citizens' statements of trust. Other papers use information on corruption scandals (e.g., Chanley *et al*., 2000; Bowler and Karp, 2004; Kumlin and Esaiasson, 2012), which is the kind of information we draw on here. Scandals, defined as 'accusations of corruption that have reached the general public', guarantee a closer link between the acts of corruption and citizens' evaluations of trust.

The paper most similar to ours is Bowler and Karp (2004), which examines corruption cases related to the famous U.S. House Bank Scandal. This is the only paper that links specific corruption scandals with measures of trust at the level of the electoral district of the politicians involved in the scandal. This is also our approach, as we seek to analyse the effect of a corruption scandal involving a local incumbent on the statements of trust made by residents in the same municipality with respect to local government politicians in general. Bowler and Karp (2004) claim that a design of this type helps isolate the impact of the scandal from other potentially confounding factors. They state, for example, that it would be important to see "if voters in those districts whose legislators have engaged in scandals have a higher awareness of the scandal and a lower regard for politicians and legislative institutions than voters who live in districts whose representatives have not been caught by scandals." Measuring both corruption and trust in small electoral districts is indeed an improvement on previous studies, although as we explain below further improvements are possible.

Matching. A problem presented by those papers that use contextual measures of corruption is that the corruption-ridden units do not necessarily have the same traits as the corruption-free units. Most papers attempt to deal with this issue by controlling for other contextual level factors in a regression framework. However, the ability of regression analysis to adjust for differences in observed covariates is well documented when the between-group differences in these covariates are substantial (e.g., Cochran, 1965; Rubin, 2001), as is probably the case in most of the aforementioned studies. In

such a situation, using matching methods to balance the distribution of covariates in the two subsamples helps reduce the bias of the estimates. Hence, the objective is to compare cases in which all the other causal variables are as similar as possible so that any difference between cases can be attributed to the treatment (see, e.g., Rosenbaum and Rubin, 1984, and Ho *et al*., 2007). Here, we use propensity score matching to construct our matched sample, a technique that is the observational analog to randomization in ideal experiments (see, Rosenbaum and Rubin, 1983, and Rubin and Thomas, 2000)[6].

A further advantage of matching is its complete transparency. The matching algorithm is applied before estimating the treatment so as to balance the covariates as far as possible in the two groups. This also ensures that the choices made at this stage by the researcher are not contaminated by the knowledge of the outcome variable or by how this choice impacts on the estimation results. As Ho *et al*. (2007) note, by using matching, researchers are forced to specify a priori the research design they are going to use. In our case, this effect is further enhanced by the fact that we use matching to select the municipalities in which to conduct our survey. Budget considerations mean that once the matching has been performed and the survey has been run, it is not feasible to go back and change the initial design. Thus, our design provides a full guarantee that the matched sample was selected before we obtained any information about the outcome variable (i.e., trust, obtained after conducting the survey), an ideal trait of a well-designed observational study (Rubin, 2001) and a task suited for the use of propensity score matching (Rosenbaum and Rubin, 1985).

One drawback of matching is that it is only able to balance the distribution of observed covariates, so the reliability of the results depends on the richness of the set of potential control variables. Thus, matching must always be applied in conjunction with a test that helps discard the possibility that the results are driven by omitted variables. When several years of data are available for the outcome variable (both before and after the treatment), the matching approach can be combined with either a 'difference-in-differences' method (Blundell and Costa-Dias, 2009) or with the 'unconfoundedness approach' (Imbens and Wooldridge, 2009), which involves including pre-treatment outcomes in the matching procedure. Then, when applying either method, there are

[6] This method has long been employed in medicine (e.g., Rubin, 2001) and economics (e.g., Dehejia and Waha, 1999), and more recently in political science (e.g., Ladd and Lenz, 2008; Gilligan and Sergenti, 2008).

ways of validating the 'conditional independence' or 'un-confoundedness assumption'. This can be achieved either by identifying differences in pre-trends in the outcome variable or by testing for the effect of the treatment on outcomes in a period of time before the treatment but after the initial period. When cross-sections of data for the outcome variable are not available (as is the case here), but information on future treatments that should not have an effect on the outcome are, a 'falsification' test can be implemented (see Heckman and Hotz, 1998; Rothstein, 2010) to rule out the possibility that the effects are driven by the omission of important variables.

3. CORRUPTION SCANDALS IN SPAIN

The recent surge of corruption scandals. In the first two decades following the restoration of Spain's democratic local governments (1979-99) not much concern was expressed in the media, among the political elite, or the population in general about the lack of accountability or possible cases of corruption (see Jimenez and Caínzos, 2003). This situation began to change after 1995, above all as a result of events in a booming housing market, but it did not really make itself manifest until 1999. Before that year, there had been just 46 corruption scandals, but this number was to jump to 211 during the 1999-2003 term (see next section for a discussion of the data sources). In the following term of 2003-07, a further 215 scandals were reported, while in the period that runs from the June 2007 elections to November 2009, 131 new cases appeared. Since that date, the collapse of the housing market has reduced opportunities for corruption. Yet, the public, the media, and the judiciary have been much more sensitive to corruption during the crisis, which means that a substantial number of scandals involving earlier corrupt acts have come to light in recent years. Although we do not have a comprehensive record of these latest cases, we exploit the fact that some previously non-corrupt municipalities became corrupt after 2009 to design several 'falsification' tests.

Corruption in land use regulations. Most of the local corruption scandals that broke out in Spain in the recent past have involved bribes received by local politicians in exchange for amendments to the land use plans. Land use regulations in Spain are governed by a highly interventionist and rigid system (Riera *et al.,* 1991) and most town planning responsibilities are in the hands of local governments. As such, municipalities draw up a 'General Plan', which provides a three-way land classification: *built-up land*, *developable land*, and *non-developable land*. The existence of a 'development border', a

line between plots of land on which developers are allowed to build and plots where development is prohibited, is a key feature of Spain's land regulation system. In periods of high demand this border creates a rent differential, which might fuel the bribes developers are willing to pay to local politicians in exchange for shifting this border to their advantage. Although land planning is subject to participatory and transparency requirements, in practice local incumbents readily find ways of circumventing them. It is this combination of discretionary decisions and the lack of transparency that fuelled the recent surge of corruption scandals in land use regulations in Spain. Most of these scandals involve local officials that wrongfully allowed huge tracts of land to be developed, that allowed building to go ahead in places where it had been previously prohibited, or that amended the land use plan so as to permit higher construction densities in already developed land (Fundación Alternativas, 2007).

Corruption, voting, and disaffection. In Spain, it is generally held that corrupt politicians are not punished at the polls. The press has provided intensive coverage of some highly prominent scandals and yet the incumbents accused of corruption have been re-elected. Several studies (see Fundación Alternativas, 2007, and Barberá *et al*., 2012) conclude that the average punishment is quite low (i.e., around 3-4% of the vote), although recent studies suggest this effect may, on occasions, be greater (Costas *et al*., 2012; Anduiza *et al*., 2012), depending on such factors as the quality of media information, the intervention of the judiciary, the existence of clientelistic networks, and the degree of ideological polarization. There has also been considerable debate about the possible adverse effects of corruption on *disaffection* (which we consider as being synonymous of trust in government). For example, in 2009 a prominent think-tank entitled its annual report *The erosion of confidence and well-being. Against citizens' disaffection* (see Fundación Alternativas, 2010). The report warned about the possible long-terms effects of corruption on trust in government and on legitimacy of democracy.

4. EMPIRICAL ANALYSIS

4.1 Measuring local corruption

We have had access to a database of corruption scandals compiled by the Spanish think-tank, Fundación Alternativas (2007). In 2007, shortly after the surge in corruption scandals of 2006, this organization commissioned a survey of local corruption in order to gauge the quantitative relevance of the phenomenon. They hired a journalist in each Spanish province with the task of compiling all corruption scandals involving the

municipalities in that province. The journalists looked at whether corruption-related news stories appeared in national, regional or local newspapers between 1 January 2000 and 1 February 2007. Overall, a total of 426 corruption scandals were reported during this period.

Since our survey was carried out in late 2009, we completed the database for the intervening period with internet-guided searches in MyNews, a paid digital information management service covering all national and many of the regional newspapers. We screened the period that runs from 1 February 2007 to 1 November 2009 (the day this search was performed). We conducted a search for news reports containing the word combination 'corrupción urbanística' (i.e. corruption related to land planning) and each of the more than 8,000 names of the Spanish municipalities. We found 131 additional scandals breaking out during this later period[7]. In the end, the total number of scandals in our database amounted to 557.

At a later juncture in this study we also use an additional set of corruption scandals that were reported after the survey was carried out (i.e., between 2010 and 2013). More specifically, we identified 42 additional corruption scandals by performing searches in MyNews (in January 2014) in the set of municipalities previously selected as control units in our matching procedure (see section 4.2). These corruption scandals are used in performing several falsification tests. Below we provide more details on the rationale underlying these tests.

A possible criticism of our corruption measure is that it does not take into account the seriousness of the case. The reason for this is that the original files that the Fundación Alternativas shared with us only contained the name of the municipality in which the scandal broke out and a brief description of the case. Only after the survey was carried out, did we have access to information on whether the judiciary decided to investigate or prosecute the politician involved in the case[8]. Similarly, after the survey was implemented, we repeated searches in MyNews for all the cases and now have information on the number of news stories published and on the type of newspaper publishing these stories (i.e., national, regional, or local). In the sensitivity analysis (see section 5.2) we replicate our results for subsamples of scandals considered to be more serious (e.g., with wide coverage, with judicial involvement, or with coverage by national

[7] As a robustness check, we also searched for news reports containing just the word "corrupción", but we did not find any additional cases.

[8] This information was also supplied by Fundación Alternativas but was included in a written report. The codification of these variables was very time consuming.

newspapers). The results are qualitatively similar to those obtained when using the whole sample although, as expected, the impact of scandals is somewhat higher for the more serious cases.

4.2. Measuring trust in local politicians

To obtain a measure of trust in local politicians at the municipal level, we designed a survey. We interviewed a sample of residents in a fraction of the municipalities in which a corruption scandal had been reported in the period 1999-2009 as well as in a number of municipalities with similar traits to those affected by corruption but which remained corruption free. The survey was undertaken in November 2009 and so the information gathered about trust in government is a reflection of the prevailing mood among Spanish citizens about politics at that time[9]. Below, we describe the *Questionnaire* used in the survey, the selection of *Treated municipalities,* and the construction of the *Matched sample* used as a control group.

Questionnaire. We asked respondents the following question: '*In the case of your city, do you think politicians on the local council can be trusted*?'. Interviewees could respond by selecting one of the following four alternatives: 1 ('*Local politicians can never be trusted*), 2 ('*Local politician can almost never be trusted*), 3 ('*Local politicians can be trusted most of the time*'), and 4 ('*Local politicians can always be trusted*')[10]. We used these four categories so individuals would have to indicate whether they had a high or low degree of trust, but they were then able to fine-tune their answer[11]. The proportions of individuals selecting each category were: 23.0%, 33.6%, 33.5% and 9.9%, for the categories 1 to 4, respectively. Respondents were also asked whether they believed

[9]This mood was becoming more critical because of the economic crisis, but had yet to hit the levels of discontent recorded today (i.e., according to the Spanish 'Centro de Investigaciones Sociológicas', the percentage of people saying that 'politicians are the country's main problem' reached 30% by the end of 2013, while it stood at 12% in November 2009, when the survey was carried out, and at 9% in September 2008, coinciding with the start of the financial crisis). Our impression is that the levels of discontent expressed at the beginning of the period are a response to the corruption scandals, while recent levels of dissatisfaction reflect the combined effect of corruption and the economic crisis.

[10] An additional category 5 (*Don't know – No answer*) was included, but following standard procedures we do not use these responses in our analysis.

[11] There is a trade-off between having too few and too many categories. Some surveys employ a dichotomous question, which has been criticized for forcing respondents to categorize themselves thus causing information loss. Other surveys employ an 11-point scale, but the outcome is often the concentration of individuals in the central categories, which are often chosen randomly (Uslaner, 2013). The four-category scale used here falls between these two extremes and has been used in many trust surveys (e.g., the trust in government question used in the World Values Survey).

corruption to be a serious problem in their municipality. They were also given four alternative replies[12].

The survey also included questions regarding political preferences (e.g., self-reported ideology), the degree of media exposure (e.g., whether the media is the main source of information regarding the activities of the local government), and information on a set of socio-economic controls (e.g., unemployed, type of job, marital status, etc.). Below, we provide full details for the variables used in the empirical analysis. The technical details of the survey are outlined in the Annex; the questionnaire used in the survey is available upon request.

Treated municipalities. Data limitations forced us to focus on municipalities with more than 1,000 residents[13]; 495 of the 557 municipalities affected by corruption scandals are in that size category. Because of budget constraints we had to select a subsample of these municipalities. We selected 160 municipalities in which corruption had been reported as our treatment group and 131 similar municipalities as our control group. The number of controls is smaller because some of these municipalities are used as controls for more than one treated municipality (see the justification for this below). In each of these municipalities, we interviewed between 20 and 50 residents, depending on population size (see also Box A.1 in the Annex). Our treated municipalities were selected on the basis of the proportions of corruption scandals that had broken out during each of the three terms-of-office (i.e., 1999-2003, 2004-2007, 2008-2009) as well as across different population sizes (i.e., less than 10,000, between 10,000 and 100,000, between 100,000 and 500,000 and larger than 500,000).

Matched sample. The control municipalities were selected using matching techniques. We constructed the matched sample using the 'propensity score'. We estimated a Probit model, using as a dependent variable a dummy equal to one if a corruption scandal had broken out in the municipality (and zero otherwise) and as regressors variables deemed to have an influence both on corruption and on the level of trust in local politician (see below)[14]. The 'propensity score' was then computed and

[12] This question was included at the very end of the interview so that the mention of the word corruption would not influence the respondents' answers to the trust question.

[13] We lacked information on the municipal-level variables needed to implement the matching for municipalities with fewer than 1,000 inhabitants. Spain has 8,114 municipalities, of which 3,252 have more than 1,000 inhabitants, the ones that belong to the control group. These municipalities include 85% of the overall population.

[14] The Probit equation was estimated with information for all 547 corrupt municipalities with more than 1,000 inhabitants plus all the non-corrupt municipalities of the same size. The

control municipalities were matched to the treatment units based on their having a similar 'propensity score'[15]. The method used was 'nearest neighbour matching with replacement'. This method allows a given control unit to match more than one treatment unit, which increases the average quality of matching and reduces the bias[16]. At the same time, the method has the additional benefit of allowing us to reduce the number of municipalities in the control group which, in turn (given budget restrictions), permitted us to increase the size of the sample of treated municipalities and/or the number of interviews conducted per municipality.

The matching strategy builds on the 'conditional independence assumption', requiring that the treatment variable (i.e., corruption) has to be independent of the error term conditional on the 'propensity score'. Thus, implementing the matching procedure requires that we choose a set of variables that credibly satisfy this condition (see Heckman *et al.*, 1997). Only variables that simultaneously influence the participation decision and the outcome variable should be included. More specifically, the municipal-level variables used to estimate the Probit equation are: % *Turnout* (i.e., historical average of turnout at local elections), % *Right voters* (also the historical average of right-wing voters), log(*Population*), % *Unemployed*, *Ethnic diversity*, *Income per capita*, % *Graduate studies*, % *Divorced.* The information for these variables is drawn from sources dated as close as possible to the first year in the surge of corruption scandals in our database, so that they can be considered as pre-determined (see Table A.1 in the Annex for the definition and sources of these variables). In line with Ho *et al*. (2007), we opt for a parsimonious specification in which all the variables are statistically significant and help predict the outcome. The use of this specification produced a good balance of covariates and good matches.

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random selection of corrupt municipalities (and of their matched pairs) for inclusion in the survey (stratified according to the term in which the corruption scandal was reported and population size) was performed afterwards. We checked that both the original sample and the randomly selected sample satisfied the balancing property.

[15] Just eight municipalities in the original sample fell outside the common support and were not included in the survey. The municipalities randomly selected for the survey satisfy this property.

[16] The main risk associated with this matching procedure is the generation of poor matches, i.e. the distance to the nearest neighbour is too large. This problem can be solved by specifying the caliper, i.e., the maximum propensity score distance allowed in any matching. In our case, however, the matching is quite good, with 95% of the matches having an absolute propensity score distance lower than 0.01 and all the matches having a distance lower than 0.03, which was the caliper finally used. We also tested other matching options (e.g., 'without replacement') but these did not work so well for the larger municipalities, so we opted not to use them.

The choice of variables was determined by a literature search focused on the determinants of corruption and trust and by data availability. First, we use the historical turnout at the local elections as the main proxy for structural trust. Corruption is known to be more prevalent in places with low levels of social capital and/or low trust in government (Nannicini *et al*., 2013). We also know that turnout is a good proxy for social capital and trust in government, hence places in which turnout has historically been high are considered to show lower corruption levels. We computed this variable as the average over the 1987, 1991 and 1995 local elections[17].

Second, we control for voter ideology by including the proportion of right-wing voters. Several surveys show that the level of support for democracy in Spain is lower among right-wing voters (although support remains very high), mainly because the national right-wing parties were filled with high-ranking officials from Franco's regime. A number of studies also suggest that right-wing voters are more tolerant of corruption and that right-wing politicians have stronger connections with private firms (Hessami, 2012). In fact, there is some evidence suggesting that right-wing governments in Spain are especially vulnerable to the influence of developers (see Solé-Ollé and Viladecans-Marsal, 2012 and 2013). Third, we control for the size of the municipality because some authors have documented that trust in government rises as the size of the polity falls (e.g., Rahn and Rudolph, 2005), while a prediction in the opposite direction has been made for political participation and accountability (e.g., Lassen and Serritzlew, 2011) and, hence, potentially for corruption. Fourth, there is also evidence that trust is negatively affected by belonging to a minority, living in a racially mixed community, having experienced a recent traumatic experience (e.g., divorce, unemployment), and being economically unsuccessful in terms of income or education (see, e.g., Alesina and La Ferrara, 2002; Gustavsson and Jordahl, 2008). Some studies also suggest that corruption is more prevalent in polities with low levels of education, and that corruption is related to income, unemployment and ethnic diversity (Glaeser and Saks, 2006).

Using the aforementioned variables we are able to balance the covariates in the two subsamples. We performed several tests to determine whether or not a good matching was achieved. First, we performed a comparison of means between treated and control units in the unmatched and matched samples (see Rosenbaum and Rubin, 1985). These tests are shown in Table A.2 in the Annex. In the unmatched sample, the

[17] We also computed the variable of the whole history of electoral turnout (i.e., turnout at the 1987, 1991 and 1995 local elections introduced separately), but the explanatory capacity of the model was not improved.

treated group (the corruption-ridden municipalities) presents lower levels of historical turnout, a higher share of divorced people and of individuals with graduate studies, greater ethnic diversity and larger population sizes than presented by the control group (the corruption-free municipalities). In the matched sample, none of the differences in means between the treated and the control group are statistically significant. Second, we also examined the percentage reduction in the standardized bias as a result of the matching procedure and found it to be substantial for all the variables that showed a statistically significant bias before the matching: % *Turnout* (79% fall), *Ethnic diversity* (86% fall), log(*Population*), % *Graduates* and % *Divorced* (98% fall each). Third, we also re-estimated the propensity score on the matched sample and compared the pseudo-R^2s before and after matching, which were 0.237 and 0.002, respectively. LR tests of joint significance of the regressors before and after matching presented values of 1871.77 and 2.32, with p-values of 0.000 and 0.941.

Finally, a number of further considerations are worth making. First, as explained, some variables that are plausibly correlated with corruption and with trust were finally excluded from the model. Although none of these variables was statistically significant, some of them presented the expected sign and had z-statistics close to one. These variables can be grouped into three categories: (i) additional measures of turnout (i.e., variation in turnout over the period 1987-1995, and average historical turnout measured at the provincial level), (ii) other plausible proxies of social capital (i.e., newspaper circulation or number of associations, both in per capita terms), and (iii) variables measuring corruption opportunities (i.e., population growth in the pre-treatment period and specialization in the tourism industry, proxied by the percentage of vacation homes). We use these variables for sensitivity checks, and include them as additional controls in the regression[18].

Second, note that our matching procedure does not deal explicitly with the fact that corruption scandals in Spain are spatially clustered. We know, for instance, that the proportion of corrupt-ridden municipalities differs across provinces. Failure to account for these spatial influences could bias the estimated effect of corruption on trust as the response of trust to corruption may not be due to a reaction to a local scandal but the effect of the accumulation of scandals at the aggregate level and/or the effect of spatially correlated omitted influences. Despite this, we opted not to match at the provincial level

[18] A number of other plausible confounders (e.g., past margin of victory of the local incumbent, number of terms in office, etc.) did not present the expected sign or offer any explanatory power in the Probit equation and were thus not considered.

so as to be able to find good matches for most of our corrupt municipalities. For example, matching at the provincial level would have made it impossible to find good matches for the largest cities and would have undermined the external validity of the survey (which was an explicit goal of our survey design). Instead, we decided to include a number of variables measured at the aggregate provincial level in the Probit equation, including, newspapers and associations per capita, and average historical turnout. These variables were negatively correlated with corruption but their explanatory capacity was low, so we decided not to include them in the final specification. Our interpretation of this is that ultimately many of the variables included are also spatially clustered (the case, for example, of unemployment, income and education), so they already capture the spatial effects. To corroborate the extent to which this is a problem we also include these aggregate variables together with a number of provincial fixed effects in some of the estimations. As we show in section five, the results do not change much after running these checks.

Third, note that our sample is also balanced with regard to the individual-level variables obtained from the survey (see Table A.3 for a comparison of means). This was not intentional, as we did not perform any additional matching at the individual level (as suggested, e.g., in Keele, 2005). The reason why we have achieved this balance is the combination of the stratification of the survey by age and gender and the fact that most individual variables are similar to the contextual ones. In any case, the fact that the sample is balanced at this level means that we compare the levels of trust of similar individuals (in terms of the individual characteristics included in the survey) living in similar municipalities (in terms of the observable contextual variables used in the matching procedure).

4.3. Estimation method

We follow the recommendation made by Ho *et al*. (2007) and estimate a parametric model with the data from our final matched sample. Other authors, such as Rubin (2001) and Crump *et al*. (2009), similarly recommend this procedure, suggesting that the propensity score should only be used for systematic sample selection as a precursor to regression estimation (or to more complex parametric methods). In most studies using matching techniques, the analysis performed to obtain the treatment effect is a simple difference in means (or the equivalent to a bivariate regression between the treatment indicator and the outcome, in the parametric case). However, it is well known that if the matching is not exact, this procedure can be improved by adjusting for covariates

(Abadie and Imbens, 2011). There are several ways in which this adjustment can be performed in a non-parametric way (Abadie and Imbens, 2011; Rubin, 2001; Dehejia and Wahba, 1999), but for the parametric case an obvious approach is simply to run a multivariate regression with the matched sample and the covariates used in the estimation of the propensity score. Ho *et al*. (2007) recommend this procedure and suggest treating the predetermined covariates as fixed, meaning that standard errors and confidence intervals should be computed as in a normal regression framework[19].

In our case, the multivariate regression has two additional advantages. First, it allows us to use the individual-level information extracted from the survey as additional covariates[20]. The individual variables we use as additional controls are *Income*, *Education*, *Age*, *Female*, *Divorced*, *Unemployed*, *Student*, *Retired*, and *Immigrant*. Controlling for individual-level variables is standard in the empirical analysis of trust (Alesina and La Ferrara, 2002; Anderson and Tverdova, 2003; Chang and Chu, 2006). In doing so we can purge the trust variable from a set of individual traits, which means that ultimately we can compare the level of trust of similar individuals living in similar municipalities with and without corruption, respectively (by virtue of the matching procedure). Second, we can also include in the equation additional controls that were discarded from the final specification of the Probit used in the matching procedure. Third, the use of a parametric framework allows us to choose the most appropriate estimation method. In our case, the fact that our dependent variable is categorical means that we should use an Ordered Logit model. An alternative would be to estimate a model by collapsing the four categories into two. Actually, the results of the Ordered Logit make it possible to test the feasibility of reducing the number of categories. In our case, it turns out that this option cannot in fact be accepted, so we have to use the Ordered Logit model. The problem with logistic models is that the quantitative interpretation of the coefficients is not straightforward, so we also provide information on the marginal probabilities.

[19] In some types of matching, the parametric analysis might require some adjustment. For instance, when using 'matching with replacement', weights must be used to ensure that the parametric analysis reflects the actual observations (see Ho *et al*., 2005; and Dehejia and Wahba, 1999). We take this into account in our estimation.

[20] We deal with the multilevel structure of the dataset, with individuals belonging to different municipalities, by clustering standard errors at the municipality level.

4.4. Validation of the results

Recall that matching methods assume 'conditional independence' or 'unconfoundedness' (see, e.g., Imbens and Wooldridge, 2009), which means that – after controlling for observables – the treatment variable should not be correlated with the error term. Therefore, the main challenge of matching is having a rich enough database to control for the many variables that might, at the same time, influence the treatment and the outcome. For this reason we examined a very broad set of possible covariates for inclusion in the first-stage of our matching procedure (see section 4.2). We show that a subset of these performed well in predicting the occurrence of corruption and we also undertake some robustness checks to show that the omission of the variables discarded at this stage does not affect the results. Nevertheless, in addition to these efforts, we also perform some 'falsification' tests to validate the matching results.

'Falsification' tests are common in economics (see, e.g., Heckman and Hotz, 2004; Rothstein, 2010) and are based on the idea that future treatments should not have an effect on past outcomes. The two outcomes we examine are the level of trust (and perception of corruption) in 2009 and the turnout at the 2007 elections (all as stated in the 2009 survey). The idea of using turnout as well is based on the fact that this variable works better as a measure of structural trust (as argued in the previous section). Thus, we expect corruption scandals breaking out after we conducted the trust survey not to have any effect on the stated levels of trust (or of perception of corruption). We also expect corruption scandals breaking out after the 2007 local elections but before we conducted the survey not to have any effect on the turnout in that contest[21]. A finding that municipalities experiencing a scandal after we conducted the survey also display lower levels of trust or perception of corruption (and/or that municipalities with post-election scandals also have lower turnout) would suggest that the cross-sectional matching estimates of the effects of scandals on trust are driven by unobservables. A finding that future scandals do not have any impact on trust or turnout would reinforce the confidence in the matching estimates. To design these falsification tests we need outcome data for units that experienced a scandal before the outcome was realized and

[21] In this case we face the additional worry that individuals might modify their answer to the turnout question after learning about a corruption scandal that occurred between the 2007 elections and the 2009 survey (that is, individuals that cast their vote might claim not to have voted after learning about the scandal). Note also that a finding that future corruption does not affect past turnout coupled with a finding that past corruption did have an effect on turnout (these results are presented below) would also discard this other source of bias.

also for units after that event. The length of the period over which corruption scandals occurred means that this is indeed the case here[22].

The first type of 'falsification' test uses trust and corruption perception data and relies on the fact that many more corruption scandals have broken out since our survey was conducted. Although we do not have a complete record of the scandals that broke out in the period 2010-2013, we know from the press that the phenomenon continued at a similar level of intensity[23]. This suggested that it would be worthwhile conducting an additional search in MyNews so as to try to find additional news reports related to corruption in the 131 municipalities selected as controls using our matching procedure (and which, therefore, had not experienced any corruption scandal before we conducted the survey). These searches were performed during January 2014 and we found a total of 42 municipalities in which a corruption scandal was reported during the period 2010-2013[24]. Following the identification of this group, we compared the level of trust (or the perception of corruption) in these municipalities with that reported in the municipalities that continued to be free of corruption. The problem here, however, is that these two groups cannot be considered comparable in terms of observables (i.e., the propensity score of the first group was higher than that of the second). In order to ensure that the treatment and control groups are comparable, we use the propensity score estimated earlier (see section 4.2) with the sample of 131 municipalities. We select matched pairs for each of the treated units using 'nearest neighbour matching with replacement' with a 0.03 caliper and dropping the observations that fall outside the common support (i.e., the procedure used to select our initial matched sample). We obtained a final sample of 75 municipalities, 38 in the treatment group and 37 in the control group[25,26], which we used to estimate an Ordered Logit model as before. Note

[22] Note that to design such tests we do not require data for several cross-sections of either trust or turnout. This is an advantage of this type of test given that our survey was conducted only once.

[23] For example, in an article published in *El País* in mid-2013, a total of around 800 corruption cases are mentioned ("Corruption reached 800 cases and nearly 2000 arrested during last decade", El País 17 June 2013). Recall that our database contains 557 corruption scandals that were reported before November 2009. Although, the number of cases in the two sources is perhaps not directly comparable, the numbers suggest that the trend in the emergence of corruption scandals persisted after our survey was conducted.

[24] This number might at first sight seem very high (representing as it does 32% of the total), but note that the predicted probability of becoming corrupt (i.e., the value of the propensity score) was also very high for this group (i.e., 0.37, compared to a value of 0.21 for the municipalities that remained corrupt-free both before and after we conducted the survey).

[25] Note that in this case we lost only four treated municipalities as they fell outside the common support. Also, each control is the matched pair of one treated municipality in all cases but one.

that these two samples are identical in terms of observables and if unobservables are unimportant we should not find any differences in levels of trust between them. Therefore, if we find that the corrupt municipalities continue to present lower levels of trust (or perception of corruption), this indicates that our main results are driven by omitted variables that are correlated both with the treatment and the outcome.

The second 'falsification' test is based on individual turnout data and is easier to design, because when we selected the random sample of treated units we had already included a number of scandals that we knew broke out after the 2007 local elections. So, in this case, we simply estimate the Probit for two different subsamples, with scandals breaking out before vs. after the 2007 local elections. Both subsamples include the treated municipalities and their respective matched pairs. The third 'falsification' test is based on turnout data aggregated at the municipal level. This test is usually referred to as an 'unconfoundedness' test and it has been proposed to assess the validity of this assumption when matching with pooled data (see Imbens and Wooldridge, 2009). To perform this test, several cross-sections of the outcome variable are needed. The idea is to use the history of the outcome as conditioning variables in the first-state Probit equation. Then, at least one of the cross-sections of data that precede the treatment should be reserved to implement the test. The test looks at the effect of the (future) treatment on the outcome in a pre-treatment period, having first controlled for the history of the outcome. In our case, we do not have any data on the history of trust or individual turnout decisions. However, we do have information on the aggregate levels of turnout at the municipal level for the elections prior to the outbreak of the corruption scandal epidemic (recall that our first scandal occurred in 2000). This means that we can control the history of the turnout (i.e., average turnout at the 1987, 1991 and 1995 elections) when looking at the effects of future corruption scandals (post 1999) on turnout at the 1999 elections. A finding that future corruption has an effect on past turnout (after controlling for the previous history of turnout) would suggest that our results are driven by some shocks that (during the period analysed) affected simultaneously the level of turnout (and hence possibly the level of trust) and the probability of a scandal breaking out.

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[26] As with the original matched sample, our procedure guarantees that this sample is balanced in terms of both contextual and individual variables. The tables comparing the means of treated and controls are not included for reasons of space but are available upon request.

5. RESULTS

5.1 Main results

The results of the estimation of the Ordered Logit model, using the matched sample selected as explained in the previous section, are presented in Table 1. The first four columns report the results of the effect of corruption on the level of trust in local politicians, while the dependent variable in the last two columns is the perception of corruption. The results suggest that the occurrence of a corruption scandal has a negative and statistically significant effect (at the 1% level) on trust in local politicians. This result holds whether or not we adjust for historical turnout (a proxy of the previous level of trust in the municipality) for all the contextual-level variables and for the individual characteristics[27]. This provides additional validation of the capacity of our matching procedure to balance the observable characteristics. The table also shows that corruption scandals have a statistically significant effect on the perception of corruption. Note that a precondition for declaring a lower level of trust after a scandal is that one is aware of a higher level of corruption. As such, the results regarding perceptions of corruption enhance our confidence in the fact that the fall in the level of trust can be attributed to the corruption scandal.

[Insert Tables 1 and 2]

The fact that the effects of corruption scandals on trust in government are negative and statistically significant does not necessarily mean that these effects are quantitatively meaningful. One drawback of the Ordered Logit model is that the size of the estimated coefficient cannot be directly interpreted. The interpretation requires the computation of the marginal effects of a corruption scandal in each of the four categories of trust. In Table 2 we present these marginal effects, computed as the difference in the predicted value of probability of choosing one of the trust categories as the corruption dummy changes from zero to one, while all other variables are held constant at their mean value[28].

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[27] Note that the explanatory capacity of the individual variables is much higher than that of the contextual variables (see F-test at the bottom of the table). At the contextual level, only historical turnout and percentage of graduate have an effect on trust. At the individual level, the rich and the old, as well as students and immigrants tend to express greater levels of trust in local politicians. The divorced tend to express lower levels of trust in local politicians. Unemployment and education have the expected negative and positive sign, but the coefficients are not statistically significant. Complete results are available upon request.

[28] The marginal effects are computed using the results from column (iv), Table 1.

Looking at the first row of Table 2, we see that when a corruption scandal breaks out, an additional 5.1% of the whole population state that '*Local politicians can never be trusted*' (category 1), and an additional 1.7% state that '*Local politicians can almost never be trusted*' (category 2). Conversely, after a corruption scandal the population stating that '*Local politicians can be trusted most of the time*' (category 3) falls by 4.5% and the population stating that '*Local politicians can always be trusted*' (category 4) falls by 2.4%. Overall, it seems that around 7% of the population shift from trusting to not trusting politicians as a result of a corruption scandal.

In order to appreciate the size of these numbers we need to put them in perspective. First, we need to consider the effect of corruption on the proportion of individuals in each category. Note for instance that in a corrupt-free municipality, around 10% of the individuals are grouped in category 4 (see row four in Table 2) while this proportion falls to around 8% in corrupt-ridden municipalities. The number of individuals in this category however falls by 23.5% after a case of corruption (see row two in the table). The number of individuals in category 1 rises by a similar percentage (24.1%) as a result of corruption, whereas the percentage changes in the number of individuals in categories 2 and 3 are not so great (+5.1% and -12.7%, respectively). If we add together the two high-trust and the two low-trust categories, we find that the number of individuals expressing trust in the government falls by 17% and that the number of individuals expressing a low level of trust in the government increases by 13%. Second, we need to compare the marginal effect with the standard deviation of the proportion of individuals in each category. This should give us a good idea of just how large the effect is vs. that of the other trust drivers. These numbers are shown in row three of Table 2 and the effects are striking. In the two extreme categories (1 and 4) the effects of corruption scandals on trust amount to around +45%, +16%, -37% and -31% of one standard deviation for categories 1 to 4, respectively. If again we add together the two high- and the two low-trust categories we find effects of +30% and -33% of one standard deviation for the low- and high-trust groups, respectively. These effects are quite sizeable, indicating that scandals seem capable of eroding much of the differences in trust levels across municipalities.

Finally, another way to judge the magnitude of these effects is to compare them with estimates of the average effects of corruption scandals on the vote for the incumbent and on turnout. The literature estimates small effects of scandals on the incumbent's vote, of around 2% in the US case (e.g., Peters and Welch, 1981) and of 3-4% in the Spanish case (Costas *et al*., 2012). As such, the effects of corruption on trust

found here (i.e., that 7% of people shift from high- to low-trust categories) seem relatively high. Yet, this effect is lower than the one we report in section 5.2 below when analysing the effect of corruption scandals on turnout, which is around 1.5%. One reason for this greater effect might be that the disaffected react very differently: some vote against the corrupt incumbent, others abstain, while others vote for radical parties or cast a blank vote or spoil their ballot paper.

5.2. Validation of the results

Falsification tests. Table 3 presents the results of the 'falsification' tests performed. In columns (i) and (ii) we present the test using our trust and corruption perception data, respectively. The results show that *future* corruption scandals (i.e., those that occurred during the period 2010-13, and hence after the trust survey was conducted) had no effect on either the level of trust or the perception of corruption as stated by the respondents in reply to the 2009 survey. In column (iii) we present a similar test based on turnout and find that corruption scandals happening after the 2007 local elections (i.e., between the local elections and November 2009) had no effect on turnout at these local elections. To enhance the interpretative value of this test, in column (iv) we show that corruption scandals breaking out before the 2007 local elections did have a negative and statistically significant effect (although at the 10% level) on turnout in 2007 (as stated in the answer to the 2009 survey). In columns (v) and (vi) we repeat the same test but with our turnout data now aggregated at the municipal level (using official electoral data). The results remain unchanged – only past corruption matters for turnout[29]. Finally, column (vii) shows the results of the 'unconfoundedness' test. We find that, after accounting for historical turnout levels, corruption scandals (breaking out after 1999) had no effect on turnout at the 1999 local elections.

[Insert Table 3]

The results of all these 'falsification' tests point in the same direction: corruption scandals occurring after the individual had answered the questionnaire (or voted at the local elections) had no effect on the stated level of trust or on the perception of

[29] Individual turnout data might be subject to certain biases as individuals tend to over-report their past turnout. In our survey, for example, turnout is around 82% while real turnout at the 2007 local elections was 74%. Also, our main control for historical turnout was built using aggregate data. For these reasons we present results using both individual and aggregate data. The results however are very similar. The size of the coefficients differs because when we use individual data we estimate a Probit, whereas when we use aggregate data we estimate a simple OLS.

corruption (or on the turnout decision). Only events happening before the survey was carried out (or before the local elections) matter. Overall, the results of these 'falsification' tests improve our confidence in the ability of the matching model to control for the most relevant variables that jointly influence corruption and trust.

Sensitivity checks. The main results presented in Table 1 are robust to the inclusion in the equation of additional controls and to the use of different definitions of corruption. All these sensitivity checks are presented in Table A.5 in the Annex. The first three columns replicate the results when adding to the equation the variables that were discarded in the estimation of the Probit equation but which had z-statistics close to one. The first column controls for additional measures of turnout (i.e., variation in turnout over the period 1987-1995, and average historical turnout measured at the provincial level), the second for other plausible proxies of social capital (i.e., newspaper circulation or number of associations, both in per capita terms), and the third for variables measuring corruption opportunities (i.e., population growth in the pre-treatment period and specialization in the tourism industry, proxied by the percentage of vacation homes). The fourth column of this table presents the results when adding regional fixed effects to the estimation to see whether the decision not to force the matching at the regional level had any influence on the results. The results do not change much in any of these cases. Finally, columns five to seven replicate the estimation using different measures of corruption that take into account the seriousness of the case. Column five focuses on scandals that received widest press coverage, column six on the cases in which the judiciary intervened, and column seven on the cases covered by national newspapers. The results are also qualitatively similar to those obtained when using the whole sample.

5.3 Heterogeneous effects

The results presented so far are a measure of the effect of a corruption scandal on the average citizen. It might be the case, however, that some citizens are more sensitive to corruption scandals than others. According to the literature the two main reasons accounting for greater sensitivity are the degree of ideological attachment to the party involved in the corruption scandal and the amount of information received about the case. Anderson and Tverdova's (2003) seminal paper has shown that the partisans' response to corruption tends not to be as marked as that of non-partisans. Anduiza *et al.* (2013) have recently replicated these results with an Internet experiment. Other papers have documented the effect of exposure to information about corruption on the

incumbent's share of the vote (e.g., Klansja, 2011) and on turnout (e.g., Chong *et al*., 2012).

[Insert Table 4]

Table 4 presents our results when we allow the effect of corruption scandals on trust to differ across these two dimensions: (i) ideological attachment between the voter and the corrupt incumbent and (ii) exposure to media information. To compute the first variable we rely on two sources of information. The first is a self-classification on an ideological scale undertaken by individual respondents (1=extreme left, 2=left, 3= centre, 4=right, 5=extreme right, and 6=none). The second is a classification of the ideology of the mayor's party employing the same scale[30]. With this information we computed two variables: *Ideology*(*Incumbent*) is a dummy equal to one if the voter and the mayor at the time of the survey shared the same ideology and zero otherwise, while *Ideology*(*Corrupt*) is a dummy equal to one if the voter shares the same ideology of the party of the mayor involved in the scandal[31]. A priori, our expectation is that the variable mediating the effect of corruption on trust is the ideological attachment to the party accused of corruption (i.e., *Ideology*(*Corrupt*)). However, we also include an interaction with the party of the actual mayor because this is the approach usually adopted in the literature and because some papers have shown that voters tend to trust the government more when they voted for the party in power (see Keele, 2005). The other interaction is with a dummy labelled *Media exposure* which is equal to one if the individual claims to obtain information about the activities of the local government only or mostly from the media, as opposed to only or mostly on the grapevine (e.g., family, friends) or from the local government itself (e.g., though pamphlets or government organized meetings).

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[30] Most mayors are members of a national or regional party and, as these are few in number and their ideological position is well known, they are readily classified. The main difficulty we encounter is that some mayors are members of local parties and it is not always easy to determine their ideology based solely on the party's name. In these cases we classified the party as belonging to the political centre. We then performed various sensitivity checks: we used just two major categories (left vs. right) as opposed to five, we included the centre parties in either of the two groups, and we either retained or discarded the individuals located at the extremes. Detailed information about the classifications used is available upon request.

[31] The survey also asked individuals to name the party they had voted for at the previous 2007 local elections. We decided not to use this variable because it is clearly endogenous to the occurrence of a corruption scandal. It might be argued that even the ideological self-placement is endogenous; note, however, that this variable is perfectly balanced between the treatment and control samples (see Table A.4 in the Annex).

The results are shown in Table 4. The first column replicates the average results for the new sample, since some individuals did not answer the questions required to compute these variables. The average effect of corruption on trust is the same as before. Columns (ii) and (iii) include the interaction with the two ideological attachment variables, separately, with the variable *Ideology*(*Incumbent*) also included as a control. We find that only the interaction between *Corruption* and *Ideology*(*Corrupt*) is statistically significant. This result is maintained when we include both interactions simultaneously in column (iv). The results suggest that corruption scandals have a smaller effect on trust in local government of voters who are ideologically attached to the party involved in the scandal (current or in the past). The results also show that voters that are ideologically close to the actual incumbent also display higher levels of trust in government. Column (v) presents the results when we include the interaction with *Media exposure*: these results suggest that corruption scandals have a more detrimental effect on the trust levels of individuals that claim to obtain their information mostly from media sources. Note also that this is compatible with the fact that individuals obtaining information from the media tend to show higher levels of trust in government. Column (vi) is our preferred specification, which includes simultaneous interactions with *Ideology*(*Corrupt*) and with *Media exposure*. The results of the previous columns do not change. Finally, column (vii) introduces a triple interaction, between *Corruption*, *Ideology*(*Corrupt*) and *Media exposure*. The coefficient of this interaction is not statistically significant but the overall results do not change.

[Insert Table 5]

All in all, the results shown in this table show that corruption scandals have a more marked effect on a subset of individuals: namely, the less ideological and the better informed. In order to gauge the magnitude of the effects for different groups of voters, Table 5 displays the marginal effects for the four possible groups: (i) ideological but informed *Ideology*=1 & *Media*=1, (ii) non-ideological and informed *Ideology*=0 & *Media*=1, (iii) ideological and un-informed *Ideology*=1 & *Media*=0, and (iv) non-ideological and un-informed *Ideology*=0 & *Media*=0. Several results are worth highlighting. First, the marginal effects are larger for the less ideological and the better informed individuals (group (ii)), the proportion of voters switching from high- to low-trust categories rises now to around 11% (vs. 7%). It is notable that 8.6% of voters are now categorised in the lowest trust category (compared to 5.1% previously, see Table 2). Note also that the increase (decrease) in the number of voters in the lowest (highest)

trust category (second row of panel (ii)) is now around 40% (having previously been around 24%). Likewise, the marginal effect now represents +77%, +24.5%, -64.2% and -45.0% of one standard deviation in categories 1 to 4, respectively (recall that these numbers were much lower in Table 2). Second, the effects are close to zero for the ideological and un-informed voters (category (iii)). Third, the effects for the other two categories (i.e., (i) ideological but informed, and (iv) non-ideological but un-informed) lie between those of categories (ii) and (iii), and the effects are slightly more marked for informed but ideological voters than they are for the un-informed and non-ideological. Although both ideological attachment and exposure to media information seem to mediate the effects of scandals on trust in local government, the impact of access to information is greater than that of ideological attachment between voters and corrupt politicians.

6. CONCLUSION

Local corruption scandals can have a highly detrimental effect on the level of trust shown by citizens in local politicians. On average, we find that around 7% of citizens shift from the high- to the low-trust categories following a corruption scandal. While this proportion might not seem great, it is quite large if we compare it with the actual number of people in the low-trust categories and/or with the standard deviation of this proportion across municipalities. The number also represents a larger proportion than the percentage share of votes lost by corrupt incumbents (see, e.g., Peters and Welch, 1980; and specifically for local incumbents in Spain, Barberá *et al.*, 2012, and Costas *et al*., 2012). We also find that the effect is much greater among individuals who present no ideological attachment to the incumbent implicated in the corruption scandal and/or who obtain their information from the media. In these instances, the proportion of citizens shifting from high- to low-trust categories in the relevant subpopulations is much higher at around 11%. Overall, our results suggest that corruption scandals are able to destroy a high proportion of differences in trust across municipalities. This ultimately might have real consequences for the decisions made by individuals, ranging from an increase in rates of abstention (we have also shown that turnout decreased after scandals) to a rise in the number of demonstrations and protests, or even a reduction in tax compliance and other indicators of civic behaviour. These aspects will be analysed in future work.

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Table 1: *Effects of corruption scandals on trust in local politicians. Ordered Logit results*.

	(*a*) *Trust in local politicians*				(*b*) *Corruption perception*	
Variables	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Corruption	-0.266***	-0.274***	-0.263***	-0.293***	0.361***	0.345***
	(0.069)	(0.070)	(0.072)	(0.076)	(0.092)	(0.091)
Historical turnout	NO	YES	YES	YES	NO	YES
Contextual variables	NO	NO	YES	YES	NO	YES
Individual variables	NO	NO	NO	YES	NO	YES
F-test. Contextual var.= 0	--.--	--.--	12.49	--.--	--.--	46.50
			[0.085]			[0.001]
F-test. Individual var.= 0	--.--	--.--	--.--	154.33	--.--	115.25
				[0.000]		[0.000]
Cut test: T1= T2	2951.63	2915.37	2981.76	2759.88	419.52	429.46
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Cut test: T2= T3	3527.11	3581.53	3541.21	3617.01	520.57	527.02
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Treated municipalities	160	160	160	160	160	160
Control municipalities	131	131	131	131	131	131
Observations	9967	9967	9967	9967	8002	8002

Notes: (1) Dependent variable: Trust in local politicians: 4=High trust. 3=Medium-High. 2=Medium-Low. 1=Low. (2) Standard errors clustered at the municipal level in parentheses, or Corruption perception: 4=High corruption, 3=Medium-High, 2=Medium-Low, 1=Low. (2) ***: $p<0.01$. **: $p<0.05$. *: $p<0.1$. (3) Estimation method: Maximum Likelihood. (4) *F-test. Contextual var.*=0 is an F-test for the joint significance of all contextual-level variables; *F-test. Individual var.*=0 is an F-test for the joint significance of all individual-level variables. (5) *Thresholds tests:* test indicating whether the cuts delimiting two contiguous categories are equal, meaning it is possible to reduce the number of categories. p-values in brackets.

Table 2: *Marginal effects.*

	Trust Category:			
	(1)	(2)	(3)	(4)
Marginal effect (a)	0.051***	0.017***	-0.045***	-0.024***
Effect in % = (a) / (b)	24.1	5.1	-12.7	-23.5
Effect in % = (a) / (c)	45.5	16.0	-37.5	-31.2
Prob.(*Trust* = *j* / *Corruption* = 0) (b)	0.211	0.331	0.355	0.102
St. Dev. (*Prob.*(*Trust* = *j*)) (c)	0.112	0.106	0.120	0.077

Notes: (1) See Table 1; (2) Marginal effect = difference in the predicted probability of choosing one of the trust categories as the corruption variable changes from zero to one. (3) Computed using the results of column (vi), Table 1. (4) *Prob.*(*Trust* = *j* / *Corruption* = 0) is the proportion of individuals selecting each of the categories in municipalities without corruption scandals. (5) *St. Dev.* (*Prob.*(*Trust* = *j*)) is the standard deviation of the percentage of individuals selecting each trust category in the whole sample.

Table 3: *Falsification tests.*

Variables	*Trust in* 2009 (i)	*Corruption Perception in* 2009 (ii)	*Turnout in* 2007 *Dummy Turnout*=1 (iii)	(iv)	*Turnout in* 2007 % *Turnout* (v)	(vi)	*Turnout in* 1999 (%) (vii)
Corruption after 2009	0.063 (0.166)	-0.014 (0.155)	--.--	--.--	--.--	--.--	--.--
Corruption after 2007	--.--	--.--	0.045 (0.166)	--.--	0.030 (0.021)	--.--	--.--
Corruption before 2007	--.--	--.--	--.--	-0.146* (0.075)	--.--	-0.015* (0.008)	-0.005 (0.006)
Historical turnout	YES	YES	YES	YES	YES	YES	YES
Contextual variables	YES	YES	YES	YES	YES	YES	YES
Individual variables	YES	YES	YES	YES	NO	NO	NO
Treated municipalities	40	40	38	122	38	122	122
Control municipalities	31	31	37	97	37	97	97
Observations	2301	1853	1757	219	1757	219	219

Notes: (1) See Table 1. (2). In columns (i) and (ii) the dependent variable is the four-category trust (or corruption perception) indicator and the estimation method used in an Ordered Logit (as in Table 1). (3) In columns (iii) and (iv) the dependent variable is a dummy coded one if the individual turned out to vote at the 2007 local elections, the information comes from the survey, and the estimation method is a Probit. (4) In columns (v) to (vii), the dependent variable is the % Turnout measured at the municipal level using aggregate electoral data. (5) The estimation method in columns (vii) is OLS.

Table 4: *Effects of ideology and information. Ordered Logit results*.

	Trust in local politicians						
Variables	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Corruption	-0.280***	-0.301***	-0.342***	-0.318***	-0.124	-0.190**	-0.189**
	(0.077)	(0.097)	(0.080)	(0.097)	(0.083)	(0.086)	(0.087)
Corr. × *Ideology* (*Incumbent*)	--.--	0.057	--.--	-0.085	--.--	--.--	--.--
		(0.105)		(0.114)			
Corr. × *Ideology* (*Corrupt*)	--.--	--.--	0.214***	0.249***	--.--	0.216***	0.214**
			(0.071)	(0.074)		(0.070)	(0.107)
Corr. × *Media exposure*	--.--	--.--	--.--	--.--	-0.303***	-0.297***	-0.298***
					(0.091)	(0.096)	(0.108)
Corr. × *Ide.*(*Corrupt*) × *Media exp.*	--.--	--.--	--.--	--.--	--.--	--.--	0.005
							(0.146)
Ideology (*Incumbent*)	--.--	0.639***	0.609***	0.640***	0.609***	0.607***	0.604***
		(0.086)	(0.060)	(0.086)	(0.060)	(0.059)	(0.076)
Media exposure	--.--	--.--	--.--	--.--	0.264***	0.261***	0.259***
					(0.061)	(0.067)	(0.083)
Ide.(*Incumbent*) × *Media exp.*	--.--	--.--	--.--	--.--	--.--	--.--	0.006
							(0.096)
Historical turnout	YES	YES	YES	YES	YES	YES	YES
Contextual variables	YES	YES	YES	YES	YES	YES	YES
Individual variables	YES	YES	YES	YES	YES	YES	YES
Treated municipalities	160	160	160	160	160	160	160
Control municipalities	131	131	131	131	131	131	131
Observations	8853	8853	8853	8853	8853	8853	8853

Notes: (1) See Table 1. (2) Ideology (Incumbent)= dummy equal to one if the individual is ideologically close to the actual incumbent; Ideology (Corrupt) = dummy equal to one if the individual is ideologically close to the incumbent involved in a corruption scandal (that might or might not coincide with the actual one); Media exposure = dummy equal to one if the individual obtains the information regarding the activity of the local government mostly from the media (instead of on the grapevine or from the local government itself).

Table 5: *Marginal effects. Ideology and media groups.*

	Trust Category:			
	(1)	(2)	(3)	(4)
(i) *Ideology* =1 & *Media=1*				
Marginal effect (a)	0.047**	0.013***	-0.041***	-0.020**
Effect in % = (a) / (b)	18.5	3.8	-12.8	-24.7
Effect in % = (a) / (c)	42.0	12.3	-34.2	-25.9
(ii) *Ideology*=0 & *Media* =1				
Marginal effect (a)	0.086***	0.026***	-0.077***	-0.035***
Effect in % = (a) / (b)	42.2	7.7	-21.3	-36.8
Effect in % = (a) / (c)	76.7	24.5	-64.2	-45.0
(iii) *Ideology* =1 & *Media*=0				
Marginal effect (a)	-0.004	-0.001	0.004	0.002
Effect in % = (a) / (b)	-1.6	-0.3	1.2	2.6
Effect in % = (a) / (c)	-3.6	-0.9	3.3	2.6
(iv) *Ideology*=0 & *Media*=0				
Marginal effect (a)	0.030**	0.010**	-0.030**	-0.013**
Effect in % = (a) / (b)	13.4	2.9	-8.7	-14.9
Effect in % = (a) / (c)	26.8	9.4	-25.0	-16.9
Prob.(*Trust* = *j* / *Corruption* = 0) (b)	0.211	0.331	0.355	0.102
St. Dev. (*Prob.*(*Trust* = *j*)) (c)	0.112	0.106	0.120	0.077

Notes: (1) Marginal effects = difference in the predicted probability of choosing one of the trust categories as we change from having no corruption to each of the four mutually exclusive cases. (2) Computed using the results of column (vi), Table 4. (3) (b) = *Prob.*(*Trust* = *j* / *Corruption* = 0) & (c) *St. Dev.* (*Prob.*(*Trust* = *j*)); (b) & (c) computed using the whole sample.

Online Annex:

Box A.1: *Description of the survey*

The survey was conducted by "*Treball de Camp*", a firm specialising in the design and implementation of surveys. The interviews where conducted by telephone between December 2009 and February 2010. Due to budget constraints, it was not possible to include all the municipalities in which at least one corruption scandal had been reported during the period 1999-2009 and their matched pair. Thus, a representative sample of municipalities was selected, composed of 160 corrupt and 131 non-corrupt municipalities. The sample is representative with regard to three specific dimensions: i) the timing of the corruption scandal; ii) municipality size (in terms of population); iii) and geographical location of the municipalities (by province). The number of individuals interviewed varied according to municipality size: 20 individuals were interviewed in municipalities with fewer than 10,000 inhabitants; 40 if $10.000 < \text{Population} \leq 100.000$; 50 if $100.000 < \text{Population} \leq 500.000$; and 100 if Population>500.000. The final sample included 9060 interviews. The sample was also representative in terms of individual characteristics (gender and age) for the whole Spanish population and by municipality size.

To guarantee a high response rate, the survey was designed to be completed in five minutes. To avoid any conditioning of responses the answer, the survey was organized as follows: First, a set of basic filter questions (gender, age, nationality and municipality in which the individual is registered) were used to obtain a representative sample; second, the question regarding trust was asked; third, a bloc of questions concerning voting decisions and information about the individual were asked; finally, several socio-demographic characteristics where ascertained.

Table A.1:
Definition of the variables and descriptive statistics

Variable	*Definition*	*Mean*	*St.Dev.*
	Individual-level variables		
Trust	Question (1-4); 1: Never; 2: Rarely; 3: Most of the time; 4: Always	2.259	0.925
Income	Self-reported socio-economic classification (1-5): 1: Low; 2: Medium-low; 3: Medium; 4: Medium-High; 5: High	2.754	0.799
Education	Highest level of education completed (1-5) 1: any studies; 2: primary; 3: secondary; 4: graduate	3.232	1.275
Age	Age in years	45.46	17.220
Female	Dummy variable coded 1 for females	0.499	0.500
Divorced	Dummy variable coded 1 for people who are divorced or separated	0.042	0.200
Unemployed	Dummy variable coded 1 for people who are unemployed	0.135	0.342
Student	Dummy variable coded 1 for students (do not work)	0.084	0.278
Retired	Dummy variable coded 1 for people who are retired	0.205	0.404
Immigrant	Dummy variable coded 1 for people who are not born in Spain	0.043	0.202
Ideology (*incumbent*)	Dummy variable coded 1 for people with an ideology closer to incumbent at the time the survey was carried out	0.358	0.412
Media exposure	Dummy variable coded 1 for people informed only by media sources (of kinds of national. regional. and local media)	0.498	0.500
	Contextual-level variables (*used in the matching procedure*)		
Corruption	Dummy variable coded 1 for municipalities with at least one corruption scandal in the period 1999-2009	0.584	0.493
% Turnout	Average vote turnout at the 1987, 1991 and 1995 local elections	0.707	0.091
Income p.c.	Average socio-economic condition. Arithmetic average of the socio-economic condition according to their employment status	0.941	0.146
% Divorced	Percentage of divorced and separated among all population	0.020	0.011
% Graduate	Percentage of population with third level studies (diploma, degree and doctorate) among population 16 years and older	0.082	0.048
% Unemployed	Percentage of unemployed among individuals aged 20-59	0.144	0.105
Ethnic diversity	1- Σ_k(*Pop*$_k$/*Population*)^2 where *Pop_cont*$_k$ is population whose nationality is from continent *k*, and *k* refers to Europe, Africa,	0.039	0.048
% Right voters	Average historical vote share that the right wing parties obtained in 1979, 1982, 1986 and 1989 local elections	0.406	0.096
Log(*Population*)	Log of the registered population	8.428	1.190
	Contextual-level variables (*used in the robustness checks*)		
% Provincial turnout	Average vote turnout at the 1987, 1991 and 1995 local elections in the respective province	0.702	0.085
%ΔTurnout	Change in the local turnout between the 1987 and 1995 elections	-0.055	0.087
Newspapers p.c.	Per day average number of newspapers sell at a province per capita	0.094	0.029
Associations p.c.	Number of associations at a province per capita	0.005	0.001
% ΔPopulation	Percentage of 2001 population that in 1991 lived in another municipality	0.148	0.116
% Second homes	Percentage of houses that are second residences	0.199	0.175

Notes: (1) Source of the individual-level variables: own-designed survey (see Box A.1). (2) Sources of the contextual-level variables: (i) 2001 Census of Population (National Institute of Statistics, www.ine.es), for *Income p.c.*, *% Divorced*, *% %Graduate*, *% Unemployed*, population by continent used to construct the *Ethnic diversity* index, and *Population*. (ii) Database on corruption scandals, constructed form an initial list of scandals compiled by Fundación Alternativas and own Internet searches (see section 3 for more details). (iii) Voting data from the Ministry of the Interior, used for the construction of the *% Right voters* and *% Turnout* variables.

Table A.2:
Determinants of corruption. Probit estimation.

	Coef.	z-stat.
% Turnout	0.653	(20.31)***
Income p.c.	-1.451	(-3.53)***
% Divorced	12.502	(3.18)***
% Graduate	2.337	(3.03)***
% Unemployed	0.809	(1.73)*
Ethnic diversity	2.620	(3.72)***
% Right voters	1.728	(4.76)***
Log(*population)*	0.495	(16.12)***
Constant	-4.980	(-9.78)***
Pseudo-R^2	0.237	

Note: (1) Dependent variable is *Corruption* (dummy equal to one if a corruption scandal broke out in the municipality between 1999 and end of 2009). (2) Final specification: only variables statistically significant at the 90% level are kept.

Table A.3:
Differences in means between Treated and Control groups. Contextual variables used in the matching procedure.

	Mean		*t-test*
	Treated	*Control*	[p-*value*]
Unmatched sample			
% Turnout	0.541	0.654	4.25 [0.000]
Income p.c.	0.947	0.939	1.09 [0.282]
% Divorced	0.026	0.018	14.09 [0.000]
% Graduate	0.106	0.077	12.57 [0.000]
% Unemployment	0.147	0.143	0.88 [0.381]
Ethnic diversity	0.060	0.035	10.83 [0.002]
% Right voters	0.507	0.505	0.36 [0.724]
log(*Population*)	96.10	81.82	27.31 [0.003]
Matched sample			
% Turnout	0.541	0.537	0.54 [0.683]
Income p.c.	0.946	0.942	0.47 [0.642]
% Divorced	0.026	0.026	-0.12 [0.915]
% Graduate	0.105	0.105	0.19 [0.853]
% Unemployment	0.147	0.150	-0.40 [0.694]
Ethnic diversity	0.060	0.057	0.86 [0.390]
% Right voters	0.507	0.509	-0.29 [0.772]
log(*Population*)	96.09	95.82	0.31 [0.753]

Note: Treated group = municipalities where at least one corruption scandal broke out during the period 1999-2009; Control group = municipalities where no corruption scandal broke out during the same period.

Table A.4:
Differences in means between Treated and Control groups. Individual variables obtained from the survey.

	Mean		*t-test*
	Treated	*Control*	[p-*value*]
	Matched sample		
Income	2.768	2.786	-0.671 [0.489]
Schooling	3.292	3.315	-0.373 [0.712]
Age	45.525	44.833	1.050 [0.220]
Female	0.502	0.491	0.495 [0.630]
Divorced	0.618	0.609	0.514 [0.604]
Unemployed	0.136	0.140	-0.608 [0.542]
Student	0.081	0.086	-0.679 [0.489]
Retired	0.204	0.198	0.392 [0.705]
Immigrant	0.046	0.043	0.851 [0.358]
Ideology (*incumbent*)	0.509	0.494	0.951 [0.304]
Media exposure	0.374	0.367	-0.675 [0.499]

Note: Treated group = municipalities where at least one corruption scandal broke out during the period 1999-2009; Control group = municipalities where no corruption scandal broke out during the same period.

Table A.5: *Effects of corruption scandals on trust in local politicians. Sensitivity checks*

		(a) *Adding controls*			(b) *Corruption definitions*		
	Turnout	*Social capital*	*Corruption opportunities*	*Provincial effects*	*Wide coverage*	*Judicial intervention*	*National press*
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Corruption	-0.268*** (0.077)	-0.271*** (0.075)	-0.282*** (0.081)	-0.252*** (0.079)	-0.296*** (0.095)	-0.299*** (0.081)	-0.288*** (0.090)
% ΔTurnout	YES	NO	NO	NO	NO	NO	NO
% Provincial turnout	YES	NO	NO	NO	NO	NO	NO
Newspapers p.c.	NO	YES	NO	NO	NO	NO	NO
Associations p.c.	NO	YES	NO	NO	NO	NO	NO
% ΔPopulation	NO	NO	YES	NO	NO	NO	NO
% Second houses	NO	NO	YES	NO	NO	NO	NO
Provincial effects	NO	NO	NO	YES	NO	NO	NO

Notes: (1) See Table 1. (2) All the regressions include *Historical turnout*, *Contextual variables* and *Individual variables.* (3) Provincial effects: one dummy for each of the 50 Spanish provinces. (3) Wide coverage = dummy equal to one if number of news stories higher than the one predicted by population size, Judicial intervention = dummy equal to one if the judiciary has decided to investigate of prosecute the politician involved in the corruption scandal, National press = dummy equal to one if the scandal has been covered by a newspaper with national coverage. (4) When using the different corruption definitions the sample included the municipalities treated by the narrow definition of corruption plus their corresponding matched pairs.