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ABSTRACT: Corruption may affect voter turnout either by mobilizing citizens to go to the polls or by promoting voter disaffection. Using Spanish local and survey data, we study whether these effects depend on partisan leanings or the timing of scandals. Our results show that repeated episodes of corruption increase the boost abstentionism. Independent voters – those with no political attachments – are the only group that that abstains in response to corruption. The incumbent's core supporters fail to recognise corruption within their party, while both independent voters and the opposition's core supporters report higher corruption perceptions in response to a scandal.

JEL Codes: P16, D72, D73

Keywords: Electoral turnout, accountability, corruption

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1. Introduction

In democratic systems elections serve as an instrument with which citizens, by withholding their vote, can punish dishonest politicians and so bring down corrupt governments. However, studies examining the effect of scandals on electoral outcomes tend not to find any significant evidence for this, implying that in most cases malfeasant politicians will be re-elected[1]. These results have, on occasions, been interpreted to indicate a cultural acceptance of corruption, a hypothesis that considers some societies to be tolerant of such scandals.

Yet, electoral outcomes, it should be recalled, are not only a reflection of who receives the votes but also of who turns out to vote. Once an election is called, individuals face the choice of a participation decision. Some citizens may react to corruption by abstaining, rather than by specifically withholding their electoral support for the accused incumbent. In such instances, scandals affect electoral outcomes in a much broader manner than just their direct impact on the incumbent's vote share. Therefore, to identify the impact of the effects of corruption, voter turnout must also be taken into consideration.

Corruption scandals can affect individuals' participation decision either by mobilizing the electorate to go to the polls to punish/support the malfeasant politician – henceforth, the "mobilization effect", or by dissuading them from voting given their disaffection with the democratic process – henceforth, the "disaffection effect". Most studies to date focus on the aggregated impact of scandals on voter turnout without distinguishing between the mobilization and disaffection effects (Dominguez and McCann, 1998). Indeed, the extant literature presents *ad hoc* explanations of variations in turnout that do not reflect a prior strategy to identify these different effects. Hence, once the analysis is complete, the impact of either the mobilization or the disaffection effects cannot be differentiated. Moreover, the aggregate level data used in some studies (e.g., Stockemer, 2013; Stockemer *et al*., 2013) represent an additional obstacle to determining the relative power of the two effects.

The aim of this paper is to determine whether the participation of individuals at local elections is affected by a corruption scandal involving the incumbent. Drawing on data at the individual level, we are able to identify the mobilization and disaffection effects

[1] Peters and Welch (1980) and Welch and Hibbing (1997) show that US House of Representative members involved in scandals are more likely to be re-elected than not. For Brazilian mayors, Ferraz and Finan (2008) report a 10% fall in their probability of re-election, if municipalities are involved in random federal audits. In Spain, Rivero and Fernandez-Vazquez (2011) find that corruption has no effect on local election outcomes and Costas-Pérez et al. (2012) estimate a 3% vote loss after a corruption scandal is revealed. Chong et al. (2012) use an experimental setting in Mexico to show that information on corruption does not significantly affect an incumbent's vote share, a similar result to that found for Brazil (Winters and Weitz-Shapiro, 2013).

that corruption may generate taking into consideration both the individual's partisan leanings and the timing of the scandals.

The likelihood of a citizen being mobilized to vote following an episode of scandal may depend on his or her partisan leanings. Those with strong political leanings tend to turn out at the polls to support their politicians regardless. Similarly, high degrees of partisanship appear to be a factor influencing the way in which some individuals evaluate corruption scandals (Rundquist *et al*., 1977; Anduiza *et al*., 2012). By contrast, individuals who vote only occasionally do not exhibit strong partisan attachments and are likely to be more sensitive to scandals (Chong *et al*., 2012). Thus, the degree of partisanship will play a crucial role in determining how citizens perceive accusations of corruption of the incumbent, and how they translate their perceptions into voter behaviour.

The timing of scandals may also affect participation decisions differently. Recent cases of corruption are perhaps quite likely to mobilize voters, while more distant scandals may already be forgotten (Fair, 1978; Kramer, 1971). Additionally, the timing of the episode may also play a fundamental role in shaping faith in the democratic system. Repeated cases of corruption can generate wide scale disaffection with the electoral system among the politically alienated, who eventually stop participating in elections (Kostadinova, 2009).

Here we examine the impact of corruption scandals at the local level in Spain, which constitutes a good setting in which to test their effects on electoral outcomes. The Spanish case combines a recent wave of scandals in local government, concentrated above all in the 2003 to 2007 term of office, with a significant number of municipalities that experienced recurring cases of corruption in two consecutive terms (1999 to 2003 and 2003 to 2007). Our database of local corruption scandals allows us to verify if past, recent or repeated corruption cases have different impacts on voter turnout. In addition, we draw on a survey conducted in several Spanish municipalities (affected or otherwise by corruption scandals) that collects information on, among others, individual voting behaviour in the 2007 local elections, degree of partisanship, ideology, and perception of corruption[2]. The use of these survey data collected at the individual level permits us to analyse the mobilization and disaffection effects of corruption on turnout depending on the voters' partisan leanings.

The adoption of a matching strategy allows us to identify a valid control group for those municipalities affected by corruption. As well as selecting 'twin' municipalities that did not experience scandals in the period analysed, the strategy purports further advantages.

[2] We refer specifically to a matching sample and a survey designed by Solé-Ollé, A. and Sorribas-Navarro, P. (2014) for the study "Does corruption erode trust in government? Evidence from a recent surge of local scandals in Spain".

First, the use of a matched sample improves the identification of the effect of corruption scandals on voter turnout, balancing the distribution of the covariates in the two subsamples. Matching also increases the transparency of our research design, as described in more detail in section 3.2. We report a series of placebo tests that further confirm the validity of our identification strategy.

Overall our results show that an episode of local corruption means citizens are, on average, 1.5% less likely to vote. However, not all individuals modify their electoral participation in response to a case of corruption in the same way. This study shows that partisan leanings play a key role in shaping individuals' responses to corruption. We find that independent voters are 4.4% less likely to vote if a corruption scandal occurs in their municipalities. Core supporters, defined as those citizens who always vote for the same party, do not seem to react to corruption scandals, irrespective of whether they support the incumbent party or the opposition.

We also find that when cases of corruption recur in a municipality over various periods, citizens are more likely to abstain. However, this effect disappears when the episodes are distant in time or in the period in the run-up to the election analysed.

Taking both individuals' partisan leanings and the timing of scandals into consideration, our results show that corruption only impacts the participation of independent voters. These individuals are also less likely to vote in municipalities that have experienced past or repeated cases of corruption, the latter being responsible for a 6.3% fall in their likelihood of voting. This implies, all in all, that the disaffection effect of scandals predominates over that of mobilization.

We find that independent voters report higher perceptions of corruption after revelations of a scandal. By contrast, core supporters of an incumbent involved in a case of corruption seem to turn a blind eye to the scandal, while the corruption perception of core supporters of the opposition parties increases. We also find that in the short term local scandals affect corruption perceptions, but that they do not immediately affect individuals' participation decisions.

Our paper makes three significant contributions to the extant literature. First, our research strategy draws a necessary distinction between the 'mobilization' and 'disaffection' effects of corruption on voter turnout. Second, this is the first paper, to the best of our knowledge, that empirically analyses how these two effects are influenced by partisan leanings. Thus, we identify the individuals that are potentially mobilized to vote, and those who withdraw from elections as a consequence of corruption scandals. Third, by testing the impact of episodes of corruption occurring at different times on voter turnout we are able to untangle conclusions drawn in earlier studies that did not differentiate voter responses on the basis of the timing of scandals.

Importantly, our study provides empirical evidence regarding the effects of actual corruption cases. Most empirical studies of the determinants and consequences of corruption rely on perceptions, which while being easier to obtain raise concerns as to both their bias and their accuracy. Moreover, we do not consider corruption at the aggregate level, but rather only at the local municipal level. Measuring corruption at this level provides us with a more extensive sample of cases. It also has the additional advantage that cases of corruption at this level are more easily linked in the minds of the citizens with the corresponding local politician, which potentially increases the accountability of municipal elections.

The rest of the paper is organized as follows. The next section explores the reasons why the timing of corruption and partisan leanings can affect turnout once a scandal becomes public, and it outlines the main hypotheses tested. Section 3 defines the empirical analysis undertaken here and the database used. Section 4 discusses the estimation strategy. Section 5 presents the results and, lastly, section 6 concludes.

2. The mobilization and disaffection effects of corruption

2.1. Previous literature

Neither the theoretical nor the empirical literature agrees on the exact relationship between corruption and voter turnout[3]. A major shortcoming of this existing literature is the lack of an empirical strategy for identifying the effects of a scandal, be it an increase in electoral participation through the mobilization of voters or a decrease in electoral participation as a result of voter disaffection with the democratic process. Kostadinova's (2009) study of post-communist transitional countries seeks to identify both a direct (mobilization) and an indirect (disaffection) effect of corruption on turnout. However, she considers corruption perceptions, which given their possible correlation with voting decisions, casts some doubt on the model's overall exogeneity.

It has been shown that good governance is related to the citizens' capacity to hold their politicians accountable (Adsera, *et al*., 2003). Thus, if we understand elections to be an effective tool for accountability, individuals who feel betrayed corrupt politicians may cast their vote to remove them from power. In this instance, corruption can be said to act as a mobilization factor – some citizens, who might otherwise have abstained, go to the polls in order to punish the politician embroiled in the scandal. Likewise, following reports of a scandal, party members and sympathisers may also be mobilized to give

[3] In most cases the empirical evidence confirms the negative relationship between corruption and turnout (Domínguez and McCann, 1998; Kostadinova, 2009; Birch, 2010; Chong *et al*., 2012). By contrast, a few studies (Karahan et al., 2006; Escaleras et al., 2012) attribute corruption with a positive effect on turnout. Finally, some studies find no relationship between corruption scandals and voter turnout (Stockemer, 2013).

their support to the politician accused of being corrupt, either in the belief that the allegations are false or out of a need to offer their unconditional support. Alternatively, in highly corrupt contexts characterised by clientelistic networks, such scandals could stimulate turnout as corrupt politicians seek to buy voters so as to retain their power (Karahan *et al*., 2006).

Conversely, corruption can also undermine voter turnout (Putnam, 1993; Warren, 2004; Chang and Chu, 2006). Corruption is detrimental to levels of citizen trust in local and national politicians (Solé-Ollé and Sorribas-Navarro, 2014), and this can result in cynicism and voter apathy. Disaffection means individuals are less likely to vote for what they see as corrupt political parties (Warren, 2004; Wagner *et al*., 2009). If episodes of corruption recur, disaffected individuals may in the long run decide to disengage from the electoral system (Chong *et al*., 2012) in the belief that given the widespread nature of corruption, replacing the corrupt incumbent with a new one will do nothing to remedy the situation. In the most extreme cases, widespread corruption might even result in doubts being cast on the sustainability of the whole democratic system (Kostadinova, 2009). Empirical evidence confirms this negative relationship between corruption and turnout (Domínguez and McCann, 1998; Andersen and Tverdova, 2003; Kostadinova, 2009; Stockemer, 2013; and Stockemer *et al*., 2013).

2.2. Who is affected by corruption and why?

The literature therefore fails to differentiate between the relative importance of the mobilization and disaffection effects resulting from corruption scandals. It is our contention that this is because two crucial factors tend to be excluded from such analyses: the role of individual partisan leanings and the timing of corruption.

First, we consider that the mobilization and disaffection effects of corruption will differ depending on the strength of the voters' partisan attachment with the incumbent implicated in the scandal. Adopting a similar strategy to the one we adopt here, Chong *et al*. (2012) take the analysis one step further, and show with experimental evidence that exposing Mexican citizens to information on corruption not only reduces voter turnout, but also negatively affects voters' identification with the corrupt incumbent's party. They find that providing information about high levels of corruption has a more marked impact on the challenger's vote than it does on the incumbent's; however, their data do not allow them to determine whether voters with different degrees of partisanship respond differently to corruption.

Independent voters, i.e., those that only vote occasionally or who do not always vote for the same party, do not present such strong partisan attachments, and tend to be more deeply affected by shocks such as occasional episodes of corruption (Rundquist *et al*., 1977; Feddersen and Pesendorfer, 1996; Sobbrio and Navarra, 2010; Stockemer, 2013).

As such, the disaffection effect is expected to be more marked for them and so we would expect to observe independent voters withdrawing from the elections if corruption scandals occur. Hence, the first hypothesis regarding partisan attachment is:

H1.a: Independent voters (i.e., those who do not always vote for the same party) are more likely to abstain if a corruption scandal becomes public.

By contrast, core supporters have stronger partisan leanings and are unlikely to defect (Chong *et al*., 2012). If partisan leanings are strong, citizens may disregard corruption as a determining factor in their decision and continue to vote for the party to which they are ideologically aligned (Peters and Welch, 1980; and Anderson and Tverdova, 2003). Ideology or party allegiance can also modify the way in which voters evaluate corruption, depending on the party to which the corrupt incumbent belongs. Anduiza *et al*. (2012) find, in support of this hypothesis, that individuals tend to present a partisan bias and are more tolerant of corruption if the politician involved is a member of their own party. If this were to be true, the core supporters of corrupt incumbents should be unaffected by corruption scandals when deciding to vote. Thus, the hypothesis we test is:

H1.b: The incumbent's core supporters do not modify their electoral participation decision as a result of a corruption scandal involving the incumbent.

In the case of the core supporters of the opposition parties, our expectation is that, even though they are aware of corruption scandals, they continue to vote for their party. It may also be the case that, in the presence of corruption, some opposition core supporters who would otherwise have abstained go to the polls to oust the corrupt incumbent. However, those who identify closely with a political party are more likely to vote (Norris, 2004), so we are unlikely to observe this mobilization effect. Hence our final hypothesis regarding partisan attachment is:

H1.c: Opposition core supporters do not modify their electoral participation decision as a result of a corruption scandal involving the incumbent.

Second, we consider that the timing of a scandal may modify the influence of the mobilization or disaffection effects of corruption on voters' participation decisions. There is evidence that voters tend to give greater weight to information received close to an upcoming election, as recent events have a stronger influence on their evaluations of the incumbent's performance (Fair 1978; Kramer 1971). A further explanation for the stronger effects of recent scandals on turnout is that voters are more attentive to the indicators of the incumbent's performance as an election approaches (Valentino and Sears 1998). Moreover, voters are not likely to recall so readily the details of the

incumbent's earlier performance (i.e., past corruption cases) (Huber *et al*. 2012) and old episodes of corruption are more easily forgotten.

Thus, while in the short term corruption can mobilize voters to oust corrupt governments, the relationship may not hold if corruption is persistent over time. This hypothesis is based on the fact that the persistence of corruption can affect citizens' trust in the political system in different ways. Repeated cases of corruption over a period of years can result in citizens doubting the democratic system's capacity to make politicians accountable (Kostadinova, 2009). Solé-Ollé and Sorribas-Navarro's (2014) study shows that, for the Spanish case, corruption scandals have an impact on levels of citizen trust in local politicians. A lack of trust in the political system leads to disaffection and alienation from politics, which may result in voters withdrawing from the electoral process, i.e., increased rates of abstention. In such a scenario, repeated cases of corruption will set in motion a mechanism of disaffection via which corruption scandals reduce voter turnout.

Hence, the following hypothesis regarding the effects of the timing of corruption cases on voter turnout can be formulated:

H2: Past corruption cases do not affect turnout, while recent scandals either have no effect or mobilize people to vote. Repeated cases of corruption over time reduce voter turnout as a result of the disaffection effect.

In conducting our study, we analyse these hypotheses, both independently and together. We predict that independent voters – those more susceptible to defection – will be more sensitive to repeated episodes of corruption, which will further erode their trust in the system. It is difficult to know how core supporters will react to the timing of corruption, as we predict that they are less likely to modify their electoral participation as a consequence of corruption.

3. Data and Empirical Analysis

3.1. Data and typology of corruption scandals

In order to carry out our analysis, we use a novel database that includes information on local corruption scandals in Spain, in addition to survey data from Spanish municipalities. We define a local corruption scandal as the "public allegation of corruption brought to light by a newspaper". Our data on these corruption cases is based on a report compiled by the *Fundación Alternativas* (2007). Following a wave of local corruption scandals in the early 2000s, this Spanish think-tank hired several journalists to compile all the corruption-related stories published in the national, regional and local media between January 2000 and January 2007. However, the time period we are

interested in runs from the local elections in July 1999 to those held in May 2007. For this reason, we had to complete the *Fundación Alternativas* information with an internet-guided search[4] for news on corruption scandals. Overall, we found that a total of 565 municipalities had been affected by corruption during this period[5]. We also verified that our data did not present a partisan bias by comparing our information with other corruption maps compiled by the media outlets of different political ideologies. The percentage of corruption cases by political party did not differ significantly across these databases, verifying that our compilation of cases was not ideologically biased.

It should be stressed that during the early years of democracy (1979-1999), few local corruption cases were reported in the Spanish media (Jiménez and Caínzos, 2006). The cases we study here concern land use regulations, an area especially prone to corruption in Spain during the years of the housing boom. The cases involved local politicians taking bribes in return for introducing changes in the municipal land use plan (i.e., reclassifying public land). As Spain's municipal governments are responsible for land use regulation, this makes it easier to detect the effect of these scandals on electoral outcomes. In such cases, voters can clearly identify the incumbent as being guilty of land-use related corruption.

The number of corruption cases rose significantly in the late nineties, when the Spanish media started highlighting this form of corruption and the judiciary began their investigations. Indeed, the number of cases shot up after 1999 (Costas-Pérez *et al*., 2012), peaking before the 2007 local elections. This distribution of corruption scandals makes the Spanish situation an optimal context in which to test our hypothesis that the exact timing of a corruption has a different effect on citizens' voting behaviour.

Our database includes 122 municipalities affected by corruption in the period from June 1999 to May 2007. We classified them into three sub-categories (used throughout our analysis) according to the *persistence* of corruption. First, 32 municipalities experienced at least one corruption scandal in the term 1999-2003, but no subsequent cases were reported after that date. We refer to these as '*past corruption cases'*, since by the 2007 elections voters can be expected to have only a distant memory of these scandals. Second, 58 municipalities experienced at least one corruption scandal in the term 2003-2007, but no cases of corruption had been reported in the previous term. We refer to these as '*recent corruption cases'* from the perspective of an individual deciding how (or whether) to vote at the 2007 local election. Third, we also considered those municipalities that have experienced repeated episodes of corruption, at least one case

[4] We used a paid digital information management service, MyNews, covering all national and many of the regional newspapers until November 2009. Thus, we have an additional sample of corruption cases occurring between the local elections of 2007 and November 2009 that we use to perform a placebo test (see section 4.6).

[5] See Costas-Pérez *et al*. (2012) for more information on the construction of the corruption database.

in both the 1999-2003 and in the 2003-2007 terms of office. A total of 32 municipalities are classified in the category of *'repeated corruption cases'*.

Hence, overall our corruption database indicates whether at least one corruption scandal was made public between June 1999 and May 2007 (the two terms of office analysed). Since our objective is to measure the impact of corruption, we need a sample of individuals from corruption-free municipalities that can be compared with those from municipalities affected by scandals. The fact of using a matched set of municipalities allows us to balance the distribution of covariates between *corruption-ridden* and *corruption-free* municipalities and so avoid biased estimations.

3.2. The matching strategy

In order to construct the sample for analysis we use a matched database that identifies a valid control group for each of the municipalities affected by corruption. Hence, a matching procedure was followed to select the corruption-free municipalities that could be compared with the corruption-ridden municipalities (our control and treatment groups, respectively). Implementation of the matching strategy allowed us to balance the covariates in the two subsamples. We ended up with a treatment group of 122 municipalities that had experienced at least one corruption scandal between 1999 and 2007. These, plus 97 control municipalities that did not experience any corruption in those years, constitute the 219 municipalities included in our database[6].

To confirm the quality of the match between the sample of treatment and control municipalities used in this paper we conducted various tests. We first analysed the percentage reduction in the standardised bias as the result of the matching procedure, finding a considerable decrease that showed a statistically significant bias before the matching. Second, we performed a comparison of means between the treatment and control units in the unmatched and matched samples (see Rosenbaum and Rubin, 1985). Table A.2. in the Appendix shows the means of each group for all variables considered when performing the matching. The last column of the table reports the test and p-values of the differences in means between the treatment and the control groups. On matching the sample, these differences are no longer statistically significant. Third, we re-calculated the propensity score on the matched sample and compared the pseudo-R^2 before and after matching[7].

[6] Solé-Ollé and Sorribas-Navarro (2013) originally selected a sample of 160 treatment municipalities and 131 controls for scandals between 1999 and 2009. For the specific purposes of this paper we restricted the cases to those occurring before the 2007 local elections, and these are the matched sub-sample data that we use. All tests to verify that a good matching was achieved were conducted with both samples.

[7] They were 0.237 and 0.002, respectively. LR tests of joint significance of the regressors before and after the matching have values of 1871.77 and 2.32, with p-values of 0.000 and 0.941.

We also performed a difference in means test for the individual level variables used in our analysis, using the survey observations as the treatment and control groups. The results of this test verify that interviewees from our treatment and control groups not only live in very similar municipalities, but also share the same individual traits. Table A.3. shows that the same conclusions regarding matching quality are drawn when we use our individual sample data. For this reason we consider that matching at the individual level is not necessary in our case since the citizens interviewed in the treatment and control municipalities are already very similar.

After performing all these tests we can confirm that the sample of treatment and control municipalities selected for this paper were successfully balanced in their baseline characteristics across the groups, both for the municipalities and the individuals analysed. An additional advantage of the matching procedure is that it ensures complete transparency and guarantees the predetermined nature of our research design. Since the matching algorithm has to be applied before the estimation of the treatment, the decisions taken at this stage are not influenced by any information from the estimation results (Ho *et al*., 2007).

3.3. Data on individual turnout and corruption perceptions

This paper draws on the results of a survey specially designed for use in the selected matched municipalities[8]. The survey was conducted in November 2009 and specifically asked if the interviewee had voted in the 2007 local elections or not. Here, we use this information to obtain an indicator of individual electoral turnout[9].

As Table A.1 shows, the average turnout of our sample is slightly higher than the figure reported in the official statistics[10]. Previous papers have also suffered from this "overreporting" bias, explained by both the misreporting of non-voters among the survey's respondents and the overrepresentation of actual voters (Traugott, 1989). However, several studies have shown that the overreporting problem has no real impact on the implications of the models' estimations, which seek to shed light on the factors that may influence voting or abstention (Hillygus, 2003). Further, recent research shows that participation in surveys does not increase the probability of turning out to vote (Mann, 2005). Thus, we are confident of the implications of our results.

A second question included in the survey concerned the respondents' corruption

[8] The questionnaire used by Solé-Ollé and Sorribas-Navarro(2013) is available upon request.
[9] The specific question asked in the survey was: 'Did you vote at the last local elections held in May 2007?' Our main dependent variable considers those individuals that reported having voted at that election, excluding from the analysis those who were either too young to vote or who were not registered in that municipality at that time.
[10] The actual voter turnout at the 2007 Spanish local elections for the municipalities analysed was 68.9%. More information on Spanish electoral outcomes can be found at: http://www.infoelectoral.mir.es/min/

perceptions: 'What level of corruption is there in local government, do you think?' The interviewees were asked to indicate one of following five levels: 5 "very high", 4 "high", 3 "medium", 2 "low", and 1 "none"[11].

Among other socioeconomic characteristics, the survey included questions about the respondents' political preferences (e.g., partisan attachment and ideology), and information on a series of socio-economic controls (e.g., unemployed, type of job, marital status, etc.).

3.4. Data on Individuals' Partisan Leanings

In order to test our hypothesis regarding the effects of partisan leanings on voter turnout, we classified interviewees according to the degree of partisan attachment shown and their self-reported ideology. To build our variables of partisan attachment we used the following survey question: 'Do you usually vote for the same party at municipal elections?' Interviewees who said they always vote for the same party were classified as *'core supporters'*; all others were classified as *'independent voters'*[12]. Note, independent voters include those that switch their vote from one election to another as well as those who do not always vote. As Table A.1 shows, *'independent voters'* represent slightly more than half of our sample of individuals[13].

In the survey, interviewees were also asked to place themselves on the left-right spectrum using a seven-point scale, where 1 represents 'extreme left' and 7 represents 'extreme right'[14]. We also classified Spain's political parties based on a combination of the party's own ideological statements (where available), online rankings, as well as *ad hoc* rules for specifying party ideology. Our specification is necessarily arbitrary, but we consider it to account for the complex reality of the Spanish state[15].

To avoid any errors in our classifications we normalised both databases – interviewees' self-reported ideology and party ideology – into a three-point scale, where 1 represents 'left', 2 'centre' and 3 'right'. We then combined the information in these two databases to determine whether each individual's ideology matched that of the incumbent party. With this information we were then able to define the *'incumbent core supporters'* as those core supporters who shared the ideology – left, centre or right – of the party

[11] Two additional categories (*Don't know* and *No answer*) were used but they are not included in our analysis.
[12] Individuals who answered *Don't know*" or *No answer* were considered as 'independent voters'. Interviewees who were too young to vote in the 2007 local elections were excluded from our analysis.
[13] This value is in line with that reported in other countries. For example, the 2013 Gallup Poll result estimated that, on average, forty-two per cent of Americans identified themselves as political independents.
[14] Interviewees who did not select any ideological position as an answer were classified as neither leaning to the left nor to the right, but as centre voters.
[15] The classification (on the left-right spectrum) of the more than 200 Spanish political parties in office for the 2003-2007 term is available on request from the authors.

forming the government in the term prior to the 2007 local elections (2003-2007). We classified the remaining core supporters as *'opposition core supporters'*.

4. Estimation Strategy

In line with Ho *et al's* (2007) proposal, we use the same parametric analysis on the matched sample as we would have used to analyse the original raw data. Derived for the dichotomous behaviour of our dependent variable – turnout – we use a Logit model. We need to bear in mind that when the matching is not exact the matching estimator will be biased in finite samples (Abadie and Imbens, 2002). Therefore, to reduce the biased term that remains after matching, we perform an additional bias correction, adjusting for covariates (Rubin, 1979; Dehejia and Wahba, 1999). Specifically, we run a Logit model with the matched sample and the covariates used in the estimation of the propensity score (Ho *et al*., 2007).

Since matching with replacement was used to select the survey sample, we need to perform adjustments when implementing our analysis. We use weights in all estimations to ensure that the parametric analysis reflects the actual observations (Ho *et al.*, 2007; Dehejia and Wahba, 1999). Thus, we weight the control municipalities by the number of times they are matched to a municipality affected by corruption.

In order to measure the effect of corruption scandals on the voter turnout of an individual, we estimate the following general specification:

$$Pr(Vote_{ij}=1) = \alpha + \beta_1 Corruption_j + \delta X' + \varepsilon_{ij} \quad (1)$$

where $Vote_{ij}$ is a dummy variable equal to one if the individual *i* voted in municipality *j* in the 2007 local elections; $Corruption_j$ is a dummy variable equal to one if at least one corruption scandal was made public in municipality *j* between June 1999 and May 2007 (two terms of office); X' is a vector that includes the covariates used in the propensity score estimation and additional individual-level information from the survey[16]; and ε_{ij} is the error term.

The sign of β_1 in Equation (1) indicates which effect – that is, mobilization (positive sign) or disaffection (negative sign) – predominates after a scandal is made public.

[16] We consider the following individual-level survey variables: income, education, gender, age, divorced, unemployed, student, retired, and immigrant.

In order to capture the effects of partisan leanings on the voter turnout of an individual, and to test our hypotheses that certain individuals are more (or less) likely to vote as a result of corruption, we estimate alternative specifications of our model. We include in Equation (1) interactions between corruption scandals and the variables indicating if the individual is an *'independent voter'*, an *'incumbent core supporter'* or an *'opposition core supporter'*.

$$Pr(Vote_{ij}=1) = \alpha + \beta_1 Corruption_j + \beta_2 Incumbent\ Core_i + \beta_3 Opposition\ Core_i + \beta_4 Corruption_j\ x\ Incumbent\ Core_i + \beta_5 Corruption_j\ x\ Opposition\ Core_i + \delta X' + \varepsilon_{ij} \qquad (2)$$

In Equation (2) we include interaction terms between the dummy variable that indicates if a corruption scandal has been made public in municipality *j* for the period analysed, and two dummy variables that indicate whether the individual is an incumbent or an opposition core supporter[17]. Hence, for independent voters the effect of corruption scandals on their participation decision is identified by the estimation of the coefficient β_1 (where *'independent voter'* is the base category of '*incumbent core supporter'* and '*opposition core supporter'*).

In the interaction model of Equation (2) the coefficients estimated for '*incumbent core supporter'* and '*opposition core supporter'* (β_2 and β_3, respectively) are no longer interpretable as the unqualified turnout difference between the incumbent and the opposition's core supporters with and without corruption scandals in their municipalities. Once these interactions are included in the model, these coefficients no longer represent a meaningful partial effect.

The impact of corruption scandals on core supporters is represented by the linear combination of the estimated coefficients of *'corruption'* (β_1) and those of the '*incumbent core supporter'* or '*opposition core supporter'* (β_4 or β_5, respectively). These effects do not appear directly in the model, and the significance of their linear combination must be tested after the estimation.

All the corruption coefficients in Equations (1) and (2) estimate the overall effect of scandals on turnout, regardless of the timing of the corruption cases. To capture the different effects of corruption occurring at different points in time, we first estimate Equation (1) defining three different subsamples of corruption scandals: '*past corruption cases'* (municipalities that experienced scandals in the 1999-2003 term, but none thereafter); '*recent corruption cases'* (municipalities that experienced scandals in the 2003-2007 term, but none previously); and *'repeated corruption cases'* (municipalities that experienced scandals in both the 1999-2003 and 2003-2007 terms).

[17] A detailed description of how the partisan leaning variables are built is included in section 3.4.

In order to estimate Equation (1) for each of these three corruption subsamples we adjusted the matching data to include just those municipalities affected by the specific corruption type under analysis and their pertinent controls.

When the matching techniques were first applied to select the control municipalities in which to run the survey, the three subsamples – *past*, *recent*, and *repeated corruption cases* – were not considered separately. Taking the specific subsamples of the corruption-ridden municipalities under analysis and then applying a new matching procedure to each of them would have required a two-stage matching procedure. Unfortunately, this procedure was not adopted at that juncture. However, for all the estimations that consider the timing of the episode of corruption, the sample was adjusted to the specific group of treated municipalities, as well as their respective controls assigned during the full matching procedure. Thus, while we need to exercise some caution when interpreting the results from the matched data for these subsamples, we achieved a good balance between the corresponding treatment and control groups[18].

As for the effects of corruption scandals on voter turnout, we also consider the combined cases of different corruption timings and individual partisan leanings. This interaction model follows the Equation (2) specification for each of the three subsamples of corruption scandals: *past, recent,* and *repeated corruption cases*. The interpretation of the coefficient is the same as in Equation (2).

Finally, to account for the possibility that our results are driven by the fact that citizens are unaware of the corruption scandals, we analyse the effect of these scandals on individuals' corruption perceptions. Specifically, we estimate the following specification:

$$Perceptions_{ij} = \alpha + \beta_1 Corruption_j + \beta_2 Incumbent\ Core_i + \beta_3 Opposition\ Core_i + \beta_4 Corruption_j\ x\ Incumbent\ Core_i + \beta_5 Corruption_j\ x\ Opposition\ Core_i + \delta X' + \varepsilon_{ij} \qquad (3)$$

where $Perceptions_{ij}$ are the local corruption perceptions of the interviewees. The same covariates used in the previous estimations are included. The interpretation of the interaction effects is the same as in Equation (2) and Table 3. However, to account for the fact that our dependent variable is now categorical we estimate an Ordered Logit model.

To deal with the multilevel structure of the dataset, with individuals belonging to different municipalities, in all the estimations we cluster standard errors at the municipality level. There are 219 municipalities in our general estimation.

[18]Full results are available from the authors upon request.

5. Results

Our empirical study is structured in five different phases. First, we analyse how individuals modify their participation at local elections if a scandal implicating the incumbent has been reported in either of the two previous terms of office. Next, we compare the individual turnout of citizens at these elections in terms of their degree of partisan attachment. Third, we evaluate how the timing of corruption affects the participation decision. Fourth, we combine the different timings of corruption scandals and an individual's partisan leanings. Finally, we observe how scandals modify corruption perceptions so as to better understand the electoral behaviour of the individuals analysed.

5.1. General Results

The results of the Logit estimation of Equation (1) using our matched sample are presented in Table 1[19].

[Table 1 about here]

We find that the revelation of a corruption scandal implicating the incumbent makes it less likely for an individual to vote at the local elections. This negative and statistically significant effect on voter turnout holds when we adjust both for the contextual-level variables and the individual characteristics (columns 2 to 4). As explained above, the adjustment for covariates in our model seeks to reduce the potential bias that remains after matching. However, the fact that our results are robust to the inclusion and exclusion of these covariates indicates that this bias is not a relevant issue in our estimation.

Our negative estimation of the corruption coefficient indicates that, overall, the disaffection effect of corruption prevails over the mobilization factor. However, the interpretation of the logistic coefficients is not straightforward. To measure the substantive effects of the significant factors and to better understand the consequences of these findings we compute a simulation of the impact of a corruption scandal being made public. For this reason we use our estimates to perform a series of first difference calculations for an average voter[20]. We focus on the estimated changes in the probability of voting that result from the occurrence or otherwise of a corruption scandal in that municipality, holding all other variables constant. Thus, the predicted turnout probabilities were simulated using the significant coefficients from the Logit estimation

[19] Complete results for the covariates have been omitted for reasons of space but are available upon request.

[20] These and all subsequent predicted coefficients were simulated using the CLARIFY program in Stata (Tomz *et al*. 2000), available at http://gking.harvard.edu/

of Equation (1). Predicted probabilities in Table 5 indicate that, on average, the revelation of a corruption scandal reduces an individual's probability of voting by 2.1 percentage points. This implies a 1.5% decrease in the likelihood that an individual from a corrupt municipality will vote as a consequence of the scandal being made public. Given that turnout at the 2007 local elections for the municipalities analysed is 68.9%, with a standard deviation of 0.10, our results indicate that corruption scandals account for a significant – albeit not huge – fall in aggregate turnout levels.

The estimations in Table 1 show how all citizens considered as an aggregate group react to corruption scandals. However, as discussed, depending on their partisan leanings, some individuals might be mobilized to vote, while others might decide to withdraw from the elections as a consequence of the scandals.

5.2. Partisan Leanings

Table 2 shows the Logit estimation of Equation (2), as well as the linear combinations of the different coefficients.

[Table 2 about here]

The estimations in Table 2 are controlled by the fact that the individual can be either an independent voter or a core supporter of the incumbent or opposition, while they also include interaction terms between corruption and an individual's partisan leanings. Following the same strategy as in Table 1, column (1) estimates the model without adjusting for covariates, column (2) includes contextual-level variables, column (3) includes individual-level variables, and column (4) accounts for both groups of controls. The corruption coefficient is not statistically significant when the covariates are not included in the specification. Once they are included in the model, all estimations indicate a negative and statistically significant effect of corruption. Adjusting for covariates reduces the bias that remains after matching, and the estimation of the coefficient is stable in all specifications.

Table 2 also reports the linear combinations for the statistically significant interactions between corruption and an individual's degree of partisanship. With this information we can verify the hypotheses formulated in Section 2.1 regarding partisan leanings. Our first hypothesis (*H1.a*) states that independent voters are more likely to abstain if a corruption scandal is made public. Our results show that this holds once we adjust for covariates. Table 5 indicates that the difference in the predicted probability of voting in the case of the independent voter, as the corruption variable (*'all corruption cases'* category) changes from zero to one, is 3.6 percentage points, revealing a 4.4% reduction in the independent voter's likelihood of voting.

To consider the reaction of the core supporters we need to observe (see Table 2) the linear combinations of coefficients associated with *'corruption'* and its interaction with '*incumbent'* or *'opposition core supporter'* in Equation (2). Hypothesis *H1.b* considers those regular voters that share the same ideology as the party holding office in local government. We predict that partisan leanings make individuals more tolerant of their own party's corruption and so they do not change their participation decision in response to a scandal. Our results confirm this, suggesting that voters tend to accept evidence of the incumbent's corruption if they belong to the same party (Anduiza *et al*., 2012).

Finally, in order to verify *H1.c*, which hypothesizes about the response of the core supporters of the opposition parties, we analysed the linear combinations of coefficients associated with *'corruption'* and its interaction with '*opposition core supporter'* in Equation (2). Our test results verify that opposition core supporters do not change their electoral participation.

It should be borne in mind that the estimations in Tables 1 and 2 measure the overall impact of any scandal that broke out between June 1999 and May 2007. For this reason in the following sub-section the persistence of corruption scandals is taken into account.

5.3. Timing of Corruption

To test our hypothesis that cases of corruption occurring at different points in time may affect an individual's participation decision, we considered the following three subsamples: *'past corruption cases', 'recent corruption cases'* and *'repeated corruption cases'*. The Logit results of these estimations are presented in Table 3.

[Table 3 about here]

In these estimations the matching sample has been adjusted to include the municipalities affected by the specific corruption cases analysed, as well as their pertinent controlled pairs.

Panels A and B in Table 3 show the results of the estimation of Equation (1) considering '*past corruption cases'* and '*recent corruption cases'*, respectively. None of the estimations are statistically significant, indicating that neither corruption cases that occurred some years previously nor cases coming to light in the period immediately preceding the elections modify the individual's electoral participation decision. Citizens appear not to remember past scandals, and our results fail to support the idea that individuals attach greater importance to more recent cases of corruption (Fair 1978; Kramer 1971). This might be because citizens perceive information received in the period leading up to the elections as electoral noise and are unable to distinguish between real episodes of corruption and the electoral strategies of the opposition.

By contrast, Panel C shows that repeated episodes of corruption do have an impact on voter turnout, with individuals resident in these municipalities more likely to abstain in the local elections. After experiencing corruption scandals in consecutive terms of office, citizens may begin to call into question the efficiency of elections as a tool for ensuring the accountability of their representatives. This will lead to an increase in disaffection, resulting in some individuals withdrawing from the electoral process. Thus, persistent evidence of corruption means the disaffection effect becomes more predominant than the mobilization factor, reducing voter turnout.

Table 5 shows a fall of 4.5 percentage points in the predicted individual turnout for individuals from municipalities that have experienced at least one corruption scandal in both terms of office analysed (1999-2003 and 2003-2007). This reduction represents a 4.8% fall in the probability of their voting, a much higher value than the 1.4% decrease obtained when we considered all corruption cases.

These results confirm our hypothesis regarding the timing of corruption (*H2*): occasional episodes of corruption (either past or recent) have no effect on voter turnout, while repeated scandals make an individual less likely to vote. Following this analysis of individual reactions to corruption (depending on partisan leanings) and how scandals occurring at different times modify voter turnout, we proceed to estimate the combined effect of both factors.

5.4. Timing of Corruption and Partisan Leanings

Table 4 shows the Logit estimation of Equation (2) considering all corruption cases and the three subcategories of scandals: *'past corruption cases'*, *'recent corruption cases'* and *'repeated corruption cases'*. It also reports the results of the linear combination tests for the different coefficients[21].

[Table 4 about here]

Column (1) in Table 4 measures the impact of all corruption cases (*'corruption'*) occurring between 1999 and 2007 on voter turnout, controlling for the fact that the individual might be an independent voter or an incumbent or opposition core supporter. Column (2) considers the same equation and category of corruption cases, also including the interactions terms between corruption types and an individual's partisan leanings. Columns (3) and (4) follow the same specification for the subsample '*past corruption cases'*, (5) and (6) for '*recent corruption cases'* and finally, (7) and (8) for *'repeated corruption cases'*. Even with the introduction of partisan leanings, scandals

[21] Tables 4 and 5 show the estimation adjusted for both the contextual-level variables and the individual characteristics. Alternative estimations not adjusting for those covariates lead to the same results, but they have been omitted for reasons of space. Complete results are available upon request.

are significant for all corruption cases and repeated corruption (columns (1) and (7), respectively).

Our results show that hypothesis *H1.a* - that independent voters are more likely to abstain if a scandal is made public - holds when repeated corruption cases are taken into account (column (8)). As Table 2 shows, *HI.a* is also true when we analyse all corruption cases (column (2)). Past and recent corruption cases do not affect the participation decision of independent voters. When considering independent voters only, Table 5 shows that the overall effect of corruption reduces the estimated probability of voting by 3.6 percentage points. As a result, independent voters are 4.4% less likely to vote if at least one corruption scandal has been revealed in their municipalities between 1999 and 2007. Repeated cases of corruption account for a 6.3% reduction in their predicted probability of voting, implying a decrease of 5.3 points as the '*repeated corruption cases'* variable changes from zero to one (Table 5). Hence, we can affirm that in this instance corruption cases do have an effect on independent voters, who will tend to withdraw from the elections as a consequence of increasing disaffection.

Hypothesis *H1.b* claims that higher degrees of partisanship make individuals more tolerant of corruption within their own party. Hence, we expect incumbent core supporters not to modify their participation in the elections as a consequence of corruption. As seen in Table 2, the hypothesis is found to hold even when the different sub-categories of corruption are considered.

If we analyse the behaviour of the opposition core supporters we can see, from the linear combinations shown in Table 4, that they do not modify their electoral participation. This holds for all the corruption categories analysed. Our results verify that the turnout of opposition core supporters does not increase if a scandal is revealed but remains unchanged.

The results in Table 4 show that neither the incumbent's core supporters nor those of the opposition modify their voting behaviour as a consequence of corruption scandals. We are therefore unable to identify any effect of corruption on the core supporters' level of electoral participation. This could indicate that the mobilization and the disaffection effects cancel each other out for individuals with a strong attachment to a political party. To verify this, and also to determine whether core supporters fail to see their party malfeasance as corruption (or if they do yet nevertheless opt to vote for them), we estimate the effect of corruption scandals on individuals' corruption perceptions.

5.5. Corruption Perceptions

Table 6 shows the Ordered Logit estimation of Equation (3), as well as the linear combinations of the interaction coefficients[22].

[Table 6 about here]

Our results show that, except for past corruption cases, scandals always increase independent voters' perceptions of corruption, even controlling for individuals' partisan leanings. However, after including the interaction effects between scandals and ideology in our model, our results are no longer homogeneous across all groups of individuals.

The corruption perceptions of the incumbent's core supporters do not increase for any of the corruption types. Indeed, these citizens do not seem to be aware that their party is involved in a scandal, either because they fail to attach any value to the importance of the scandals attributed to the incumbent or they simply fail to believe the accusations of corruption. Or, it might be that the incumbent's core supporters are more tolerant when judging scandals affecting their own party, even while recognising the potential existence of corruption (Anduiza *et al*., 2012).

Considering all corruption cases (*'corruption'*), the corruption perceptions of both independent voters and the opposition's core supporters rise after a scandal is made public. We have already shown that the opposition's core supporters do not modify their participation decision as a consequence of these scandals, but they do report higher levels of corruption perception. This supports our suspicion that if corruption is present, core supporters of opposition parties maintain their support for their party in an attempt at ousting the corrupt incumbent. We have no evidence of a mobilization effect for these individuals, which is not unexpected given that core supporters by definition tend to vote. However, we can conclude that the disaffection effect is not relevant in the case of opposition core supporters.

For '*past corruption cases'*, the corruption perceptions of all groups of individuals remain unchanged. This can be attributed to the fact that voters seem to have short memories in relation to old scandals. While independent voters are less likely to vote in municipalities marked by past corruption cases, they do not report higher levels of corruption perceptions. It would seem that voters in municipalities that reported corruption scandals in the past had already modified their perceptions by the time our

[22] The number of observations for the results in Table 6 is not as high as for the other results as several interviewees did not answer the question about corruption perceptions. Since the effect of scandals on individuals' perceptions is not the principle objective of our paper we decided not to include all these observations in our main results. We also performed the thresholds tests of all the specifications of our Ordered Logit, concluding that it is not possible to reduce the number of cut-offs delimiting a category.

survey was conducted; hence, they did not subsequently report an increased awareness of corruption.

For '*recent corruption cases'*, the corruption perceptions of both independent voters and the opposition's core supporters increase after a scandal is made public. However, we did not find any effect on the turnout in either group. Hence, in the short term, local scandals do affect the corruption perceptions of independent voters and the opposition's core supporters, but this does not immediately affect individuals' electoral participation decisions.

Finally, '*repeated corruption cases'* also increase the corruption perceptions of both independent voters and the opposition's core supporters. However, only the turnout of independent voters is affected by the persistence of corruption.

As in the previous estimations, considering that we have a non-linear model with multiple interaction terms, the simulation-based approach is the most practical alternative for estimating the actual impact of corruption scandals on individuals' perceptions (Tomz *et al*., 2000). Table 7 reports the simulations of the predicted changes in probability of voting in the corruption perceptions using the significant coefficients obtained from the Ordered Logit estimation of Equation (3).

[Table 7 about here]

The model simulations included all the contextual and individual covariates, as well as the whole set of interactions (columns (2), (4), (6) and (8) in Table 6). We see that when a corruption scandal breaks out independent voters are 3.2 percentage points more likely to perceive their municipality as having "very high" levels of corruption. This value rises to 4 points for recent corruption cases. However, as we saw in Table 4 this does not imply that independent voters withdraw from the elections if a recent corruption case has been revealed in their municipality. A similar effect is seen for the opposition's core supporters, which are 3 percentage points more likely to perceive "very high" levels of corruption if a scandal breaks out, while for repeated cases of corruption, they are 3.9 points less likely to consider there to be no corruption ("none") in their municipality. This value for independent voters is 3.5 points. However, as we see in Table 6, modifications in perceptions following the reporting of a scandal depend to a large extent on the individual's baseline corruption perceptions. That is, if independent voters already have a very low opinion of their local politicians' integrity, they are less likely to report an increase in their corruption perceptions once a scandal is made public. In any case, the information included in Tables 4-7 indicates that corruption perceptions are not immediately translated into electoral actions for all individual types.

5.6. Placebo tests

The 'conditional independence' or 'unconfoundedness' assumption is based on the presumption that the treatment satisfies some type of exogeneity (Rosenbaum and Rubin, 1983; Imbens and Wooldridge, 2008). As explained above, this requires turnout to be independent of corruption scandals. If this were the case, systematic differences in individual turnout between the treated and control municipalities, with the same individual and contextual traits, can be attributed to the treatment – corruption scandals. We check for the exogeneity of our treatment by testing that our results are not driven by either spurious correlations or an omitted trend that affects both the corruption cases and electoral turnout. Hence, we conduct two placebo tests to verify that levels of voter turnout are not explained by future corruption

Our first placebo test considers our general sample of observations at the municipal level, and verifies that corruption occurring after 1999 does not explain levels of turnout in that year:

$$\underset{}{Turnout_{99}} = \underset{(0.007)}{-0.005\ Corruption_{>99}} + \underset{(0.038)^{***}}{0.932 Turnout_{87\text{-}95}} \qquad (4)$$

where $Turnout_{99}$ is the local electoral turnout in 1999 for the municipalities in our sample, $Corruption_{>99}$ is a dummy variable equal to one if at least one corruption scandal broke out after 1999, and $Turnout_{87\text{-}95}$ is the average level of electoral turnout between 1987 and 1995. Standard errors are in brackets and *** indicates a 1% level of significance. Thus, we find that corruption scandals had no significant effect on previous levels of turnout, confirming the assumption that there is no omitted variable bias.

Since the original sample was made up of 160 treatment municipalities, including those that experienced corruption after the local elections of 2007, there are 38 additional treatment municipalities that are not used in our analysis. These municipalities reported at least one corruption scandal between May 2007 and November 2009, when the survey was carried out, but not at any time before. Thus, we can also perform a placebo test with the corruption cases that were reported after the 2007 local elections. This placebo test follows the specification:

$$\underset{}{Turnout_{07}} = \underset{(0.012)}{0.008\ Corruption_{07\text{-}09}} + \underset{(0.044)^{***}}{0.828 Turnout_{87\text{-}95}} \qquad (5)$$

where $Turnout_{07}$ is the local electoral turnout in 2007 for the municipalities in our sample, $Corruption_{07\text{-}09}$ is a dummy variable equal to one if at least one corruption scandal broke out after 2007, and $Turnout_{87\text{-}95}$ is the average level of electoral turnout

between 1987 and 1995, which was used to conduct our matching. Standard errors are in brackets and *** indicates a 1% level of significance. Hence, our second placebo test confirms the previous results, verifying that the 'conditional independence assumption' holds.

6. Conclusions

This paper has examined the effects of local corruption scandals on voter turnout and finds that corruption generates disaffection with the electoral system among the politically alienated. Consequently, some individuals abstain, withdrawing from the electoral process, thus helping corrupt incumbents retain office.

Drawing on data identifying cases of corruption at the local level in Spain between 1999 and 2007, and survey information on turnout at the individual level, we devise a balanced matched sample of corruption-ridden and corruption-free municipalities. Overall, our results support the hypothesis that the disaffection attributable to scandals predominates over the impact of such episodes on voter mobilization. On average, an episode of corruption in a municipality makes individuals 1.5% less likely to vote. This result, however, is lower than that reported by Chong et al. (2012), who found that exposing Mexican citizens to information on corruption reduces voter turnout by 3%.

The paper's main contribution is that we have been able to identify both individual partisan leanings and the timing of the scandals as being key determinants of the way in which potential voters react to corruption. We show, first, that scandals only affect the turnout of those citizens that do not manifest any strong political attachments. These independent voters are 4.4% less likely to vote if a corruption scandal becomes public. By contrast, those with strong partisan leanings (core supporters) do not react to cases of corruption, irrespective of whether they support the incumbent party or the opposition. Second, we show that, while neither past nor recent corruption scandals have an impact on the voting participation decisions of individuals, repeated instances of corruption result in higher rates of abstention. Thus, for those municipalities that experienced at least one case of corruption in both terms of office analysed (1999-2003 and 2003-2007), individuals were 4.8% less likely to vote, a much higher probability than that found when all corruption scandals are considered together. Hence, persistent corruption causes feelings of disaffection to predominate over the mobilization factor, thus reducing voter turnout.

When considering the combined effect of these two determinants, we find that independent voters are even less likely to vote when faced by repeated cases of corruption. In this instance, the likelihood of voting falls by 6.3%. Past and recent corruption cases do not seem to have an impact on any group of individuals as neither

the incumbent's nor the opposition's core supporters change their electoral participation decision if a scandal is revealed, irrespective of the timing of corruption analysed. Thus, our results show that, with the exception of recent scandals, cases of corruption only have an effect on independent voters, who may abstain as a result of their feelings of disaffection.

Our analysis of the corruption perceptions of individuals suggests that the incumbent's core supporters are unaware of such episodes. Their failure to report higher levels of corruption perception when a scandal is made public supports Anduiza *et al's* (2012) findings that citizens are more tolerant of an incumbent's malfeasance if they share the same ideology. The corruption perceptions of both the core supporters of opposition parties and independent voters are raised when a scandal is revealed; however, the former do not modify their electoral participation. We surmise that they continue to vote for the candidate of the party with which they share a close ideological affinity with the aim of defeating the party embroiled in the scandal.

Voter turnout is one of the key indicators of a political system's democratic health. We have found that local corruption scandals affect voter turnout by dissuading citizens from voting. These results allow us to reinterpret earlier conclusions reported in the literature that attribute the absence of any notable electoral punishment of corruption to cultural explanations. Considering the non-trivial effect reported here of scandals on voter turnout, we are able to confirm that some individuals react to corruption by withdrawing from elections. It is difficult to speculate as to what would happen if independent voters did not withdraw from the elections as a consequence of corruption. However, since a fall in turnout tends to have a more marked effect on minority parties[23], it seems highly likely that the votes cast by disaffected citizens would make it much more difficult for corrupt politicians to retain power. Considering both the incumbent's vote loss and the fall in voter turnout, the actual impact of corruption on electoral outcomes may be much higher than is currently reported in the literature.

[23] Hansford and Gomez, (2010).

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Table 1: *Effects of corruption scandals on voter turnout: all corruption cases.*

Variables	(1)	(2)	(3)	(4)
Corruption	-0.146	-0.165	-0.164	-0.164
	(0.088)*	(0.071)**	(0.087)*	(0.074)**
Contextual-level variables	NO	YES	NO	YES
Individual-level variables	NO	NO	YES	YES
Observations	8,014	8,014	8,014	8,014

Notes: (1) Dependent variable: whether the individual has voted (=1) or not (=0). (2) Standard errors clustered at the municipal level in parentheses; ***: p<0.01. **: p<0.05. *: p<0.1. (3) Estimation method: Maximum Likelihood; (4) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation.

Table 2: *Effects of corruption scandals and individual's partisan leanings on voter turnout. Logit results.*

Variables	(1)	(2)	(3)	(4)
Corruption	-0.189	-0.213	-0.237	-0.244
	(0.145)	(0.123)*	(0.140)*	(0.133)*
Corruption x *Inc. core sup.*	-0.035	0.002	0.043	0.088
	(0.230)	(0.218)	(0.236)	(0.226)
Corruption x *Opp. core sup.*	0.140	0.154	0.218	0.223
	(0.265)	(0.270)	(0.272)	(0.279)
Incumbent core sup.	-0.257	-0.224	-0.221	-0.194
Test: $\beta_1+\beta_4 \neq 0$	[0.249]	[0.294]	[0.392]	[0.390]
Opposition core sup.	-0.034	-0.049	-0.059	-0.019
Test: $\beta_1+\beta_5 \neq 0$	[0.826]	[0.778]	[0.756]	[0.917]
Contextual-level variables	NO	YES	NO	YES
Individual-level variables	NO	NO	YES	YES
Observations	8,014	8,014	8,014	8,014

Notes: (1) Dependent variable: whether the individual has voted (=1) or not (=0). (2) Standard errors clustered at the municipal level in parentheses; ***: p<0.01. **: p<0.05. *: p<0.1. (3) Estimation method: Maximum Likelihood; (4) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation; (5) Linear combinations of coefficients $\beta_1+\beta_{4/5}\neq 0$, measuring the interaction's total effect. p-values in brackets.

Table 3: *Effects of corruption scandals on voter turnout: timing of corruption. Logit results.*

Panel A	(1)	(2)	(3)	(4)
Past corruption cases	-0.143	-0.139	-0.151	-0.124
	(0.162)	(0.135)	(0.151)	(0.130)
Contextual-level variables	NO	YES	NO	YES
Individual-level variables	NO	NO	YES	YES
Observations	1,796	1,796	1,796	1,796
Panel B	(1)	(2)	(3)	(4)
Recent corruption cases	0.046	-0.000	0.065	0.033
	(0.112)	(0.112)	(0.112)	(0.112)
Contextual-level variables	NO	YES	NO	YES
Individual-level variables	NO	NO	YES	YES
Observations	3,129	3,129	3,129	3,129
Panel C	(1)	(2)	(3)	(4)
Repeated corruption cases	-0.329	-0.276	-0.384	-0.324
	(0.125)***	(0.116)**	(0.140)***	(0.118)***
Contextual-level variables	NO	YES	NO	YES
Individual-level variables	NO	NO	YES	YES
Observations	3,089	3,089	3,089	3,089

Notes: (1) Dependent variable: whether the individual has voted (=1) or not (=0). (2) Standard errors clustered at the municipal level in parentheses; ***: p<0.01. **: p<0.05. *: p<0.1. (3) Estimation method: Maximum Likelihood; (4) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation.

Table 4: *Effects of timing of corruption scandals and individual's partisan leanings on voter turnout. Logit results.*

Variables	*All corruption cases*		*Past corruption cases*		*Recent corruption cases*		*Repeated corruption cases*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption	-0.180	-0.244	-0.166	-0.268	0.004	-0.036	-0.309	-0.368
	(0.078)**	(0.133)*	(0.137)	(0.174)	(0.119)	(0.155)	(0.127)**	(0.190)*
Corruption x *Inc. core sup.*	--.--	0.088	--.--	0.007	--.--	0.203	--.--	-0.053
		(0.226)		(0.382)		(0.306)		(0.389)
Corruption x *Opp. core sup.*	--.--	0.223	--.--	0.529	--.--	0.043	--.--	0.259
		(0.279)		(0.305)*		(0.287)		(0.382)
Incumbent core sup.	--.--	-0.156	--.--	-0.261	--.--	0.167	--.--	-0.421
Test: $\beta_1+\beta_4 \neq 0$		[0.440]		[0.439]		[0.552]		[0.194]
Opposition core sup.	--.--	-0.021	--.--	0.261	--.--	0.007	--.--	-0.109
Test: $\beta_1+\beta_5 \neq 0$		[0.912]		[0.338]		[0.976]		[0.708]
Contextual-level variables	YES	YES	YES	YES	YES	YES	YES	YES
Individual-level variables	YES	YES	YES	YES	YES	YES	YES	YES
Observations	8,014	8,014	1,796	1,796	3,129	3,129	3,089	3,089

Notes: (1) Dependent variable: whether the individual has voted (=1) or not (=0). (2) Standard errors clustered at the municipal level in parentheses; ***: p<0.01. **: p<0.05. *: p<0.1. (3) Estimation method: Maximum Likelihood; (4) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation; (5) Linear combinations of coefficients $\beta_1+\beta_{4/5}\neq 0$, measuring the interaction's total effect. p-values in brackets.

Table 5: *Effects of corruption scandals on voter turnout. Difference in the predicted probability of voting.*

Variables	*All corruption cases*	*Repeated corruption cases*
All citizens	-0.021	-0.042
Independent Voters	-0.036	-0.054
Contextual-level variables	YES	YES
Individual -level variables	YES	YES

Notes: (1) Estimates generated by running post-estimation simulations of the significant coefficients from Tables 1-4 using the Clarify routine in Stata as described by Tomz et al. (2000), setting each explanatory variable at its mean value. They indicate the difference in the predicted probability of voting as the corruption variable changes from zero to one; (2) Dependent variable: whether the individual has voted (=1) or not (=0); (3) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation.

Table 6: *Effects of corruption scandals on individual's corruption perceptions. Ordered Logit results.*

	All corruption cases		*Past corruption cases*		*Recent corruption cases*		*Repeated corruption cases*	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Corruption	0.327	0.358	0.194	0.261	0.356	0.417	0.391	0.360
	(0.108)***	(0.117)***	(0.163)	(0.197)	(0.131)***	(0.161)***	(0.1710)**	(0.193)*
Corruption x *Inc. core sup.*	--.--	-0.325	--.--	-0.262	--.--	-0.397	--.--	-0.183
		(0.146)**		(0.277)		(0.216)*		(0.227)
Corruption x *Opp. core sup.*	--.--	0.146	--.--	-0.045	--.--	0.105	--.--	0.237
		(0.130)		(0.231)		(0.176)		(0.182)
Incumbent core sup.	--.--	0.032	--.--	-0.001	--.--	0.020	--.--	0.177
Test: $\beta_1+\beta_4 \neq 0$		[0.796]		[0.997]		[0.914]		[0.384]
Opposition core sup.	--.--	0.505	--.--	0.216	--.--	0.521	--.--	0.597
Test: $\beta_1+\beta_5 \neq 0$		[0.002]		[0.339]		[0.004]		[0.014]
Contextual-level variables	YES	YES	YES	YES	YES	YES	YES	YES
Individual -level variables	YES	YES	YES	YES	YES	YES	YES	YES
Observations	6,259	6,259	1,387	1,387	2,394	2,394	2,478	2,478

Notes: (1) Dependent variable: Perceptions of local political corruption: 5=Very High Corruption. 4=High. 3=Medium. 2=Low. 1=None. (2) Standard errors clustered at the municipal level in parentheses; ***: p<0.01. **: p<0.05. *: p<0.1. (3) Estimation method: Maximum Likelihood; (4) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation; (5) Linear combinations of coefficients $\beta_1+\beta_{4/5}\neq 0$, measuring the interaction's total effect. p-values in brackets.

Table 7: *Effects of corruption scandals on individual's corruption perceptions. Simulations.*

Variables	*Perceptions of local political corruption category*				
	None	*Low*	*Medium*	*High*	*Very High*
All Corruption Cases					
Independent Voters	-0,053	-0,035	0,007	0,049	0,032
Opposition core supporter	-0,056	-0,031	0,009	0,049	0,030
Recent corruption cases					
Independent Voters	-0,070	-0,044	0,008	0,066	0,040
Opposition core supporter	-0,075	-0,038	0,010	0,066	0,037
Repeated corruption cases					
Independent Voters	-0,035	-0,047	0,003	0,048	0,031
Opposition core supporter	-0,039	-0,042	0,006	0,049	0,027

Notes: (1) Estimates generated by running post-estimation simulations of the significant coefficients from Table 6 using the Clarify routine in Stata as described by Tomz et al. (2000), setting each explanatory variable at its mean value. They indicate the difference in the predicted probability of voting as the corruption variable changes from zero to one; (2) Dependent variable: Perceptions of local political corruption: 5=Very High Corruption. 4=High. 3=Medium. 2=Low. 1=None; (3) Treated observations weighted as 1, and control observations weighted by the number of times they are matched to a treatment observation; (4) All estimations include contextual and individual level variables.

Appendix.

Table A.1: *Definition of the variables and Summary Statistics*

Variable	Definition	Mean	St.Dev.	Min	Max
	Individual-level variables				
Turnout	Dummy variable coded 1 if the individual voted in the 2007 local elections	0,775	0,418	0	1
Income	Self-reported socio-economic classification (1-5): 1: Low; 2: Medium-low; 3: Medium; 4: Medium-High; 5: High	3,357	1,308	1	5
Schooling	Highest level of education completed (1-5): 1: any studies; 2: primary; 3: secondary; 4: graduate	46,323	16,697	1	5
Age	Age in years	0,499	0,500	18	99
Female	Dummy variable coded 1 for females			0	1
Divorced	Dummy variable coded 1 for people who are divorced or separated	0,046	0,209	0	1
Unemployed	Dummy variable coded 1 for people who are unemployed	0,142	0,349	0	1
Student	Dummy variable coded 1 for students (do not work)	0,058	0,233	0	1
Retired	Dummy variable coded 1 for people who are retired	0,208	0,406	0	1
Immigrant	Dummy variable coded 1 for people who are not born in Spain	0,035	0,183	0	1
Corruption Perceptions	Self-reported perceptions of local political corruption (1-5): 1: None; 2: Low; 3: Medium; 4: High; 5: Very High Corruption	2,719	1,313	1	5
Ideology	Self-reported ideology (1-3): 1: Left; 2: Centre; 3: Right	1,831	0,711	1	3
Independent	Dummy variable coded 1 for people who do not always vote for the same party	0,512	0,500	0	1
Incumbent core Supporter	Dummy variable coded 1 for people who always vote for the same party, and their ideology is the same as the incumbent's	0,215	0,411	0	1
Opposition core Supporter	Dummy variable coded 1 for people who always vote for the same party, and their ideology is the same as the opposition' party	0,272	0,445	0	1
	Contextual-level variables				
Corruption	Dummy variable coded 1 for municipalities with at least one corruption scandal in the period 1999-2007	0,482	0,500	0	1
Past corruption cases	Dummy variable coded 1 for municipalities with at least one corruption scandal in the period 1999-2003, but were not corruption has broke out afterwards	0,110	0,313	0	1
Recent corruption cases	Dummy variable coded 1 for municipalities with at least one corruption scandal in the period 2003-2007, but were not corruption has broke out before	0,188	0,390	0	1
Repeated corruption cases	Dummy variable coded 1 for municipalities with at corruption scandal in both periods: 1999-2003 and 2007-2009.	0,185	0,388	0	1
Voter turnout	Average voter turnout at the 1987, 1991 and 1995 local elections	0,687	0,084	0,508	0,922
Income p.c.	Average socio-economic condition. Arithmetic average of the socio-economic condition according to their employment status	0,951	0,118	0,610	1,200
Divorced	Percentage of divorced and separated among all population	0,029	0,012	0,002	0,074
Graduate	Percentage of population with third level studies (diploma, degree and doctorate) among population 16 years and older	0,128	0,069	0,016	0,434
Unemployment	Percentage of unemployed among individuals aged 20-59	0,157	0,079	0,048	0,675
Ethnic diversity	1- Σk(Popk/Population)2 where Pop_contk is population whose nationality is from continent k, and k refers to Europe, Africa, America and others	0,058	0,058	0	0,279
Right voters	Average historical vote share that the right wing parties obtained in 1979, 1982, 1986 and 1989 local elections	0,226	0,093	0,037	0,483
log(*Population*)	Log of the registered population	10,545	1,778	6,973	14,957

Notes: (1) Source of the individual-level variables: own-designed survey (see Box A.1). (2) the contextual-level variables: (i) 2001 Census of Population (National Institute of 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 % Vote turnout variables.

Table A.2: *Differences in means between Treated and Control groups.*

	Mean		t-test
	Treated	Control	[p-value]
Unmatched sample			
% Vote 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*)	9.610	8.182	27.31 [0.003]
Matched sample			
% Vote turnout	0.708	0.692	-1.28 [0.202]
Income p.c.	0.947	0.934	-0.79 [0.428]
% Divorced	0.026	0.026	0.03 [0.976]
% Graduate	0.114	0.104	-1.09 [0.278]
% Unemployment	0.151	0.166	1.23 [0.220]
Ethnic diversity	0.053	0.054	0.08 [0.932]
% Right voters	0.219	0.218	-0.12 [0.908]
log(*Population*)	9.788	9.678	-0.51 [0.610]
Observations	122	97	

Note: (1) Treated group = municipalities where at least one corruption scandal was made public during the period 1999-2007; Control group=municipalities where no corruption scandals were made public during the same period.

Table A.3.*: Differences in means (survey observations).*

	Mean		t-test
	Treated	Control	[p-value]
Turnout	0.833	0.846	-1.01 [0.220]
Income	2.702	2.721	0.02 [0.479]
Schooling	3.184	3.159	0.03 [0.700]
Age	47.968	47.243	0.72 [0.260]
Female	0.522	0.504	0.02 [0.225]
Divorced	0.044	0.039	0.00 [0.424]
Unemployed	0.130	0.140	-0.01 [0.327]
Students	0.045	0.052	-0.01 [0.222]
Retired	0.241	0.226	0.01 [0.283]
Immigrants	0.039	0.031	0.01 [0.168]
Ideology	1.841	1.817	0.02 [0.324]
Independent	0.510	0.520	-0.01 [0.463]
Incumbent core supporter	0.224	0.203	1.28 [0.200]
Opposition core supporter	0.265	0.275	-0.01 [0.507]
Interviewees per municipality	34.779	38.016	-1.18 [0.241]
Number of municipalities	122	97	

Note: (1) Treated group = municipalities where at least one corruption scandal was made public during the period 1999-2007; Control group=municipalities where no corruption scandals were made public during the same period.

2011

2011/1, Oppedisano, V; Turati, G.: "What are the causes of educational inequalities and of their evolution over time in Europe? Evidence from PISA"
2011/2, Dahlberg, M; Edmark, K; Lundqvist, H.: "Ethnic diversity and preferences for redistribution"
2011/3, Canova, L.; Vaglio, A.: "Why do educated mothers matter? A model of parental help"
2011/4, Delgado, F.J.; Lago-Peñas, S.; Mayor, M.: "On the determinants of local tax rates: new evidence from Spain"
2011/5, Piolatto, A.; Schuett, F.: "A model of music piracy with popularity-dependent copying costs"
2011/6, Duch, N.; García-Estévez, J.; Parellada, M.: "Universities and regional economic growth in Spanish regions"
2011/7, Duch, N.; García-Estévez, J.: "Do universities affect firms' location decisions? Evidence from Spain"
2011/8, Dahlberg, M.; Mörk, E.: "Is there an election cycle in public employment? Separating time effects from election year effects"
2011/9, Costas-Pérez, E.; Solé-Ollé, A.; Sorribas-Navarro, P.: "Corruption scandals, press reporting, and accountability. Evidence from Spanish mayors"
2011/10, Choi, A.; Calero, J.; Escardíbul, J.O.: "Hell to touch the sky? Private tutoring and academic achievement in Korea"
2011/11, Mira Godinho, M.; Cartaxo, R.: "University patenting, licensing and technology transfer: how organizational context and available resources determine performance"
2011/12, Duch-Brown, N.; García-Quevedo, J.; Montolio, D.: "The link between public support and private R&D effort: What is the optimal subsidy?"
2011/13, Breuillé, M.L.; Duran-Vigneron, P.; Samson, A.L.: "To assemble to resemble? A study of tax disparities among French municipalities"
2011/14, McCann, P.; Ortega-Argilés, R.: "Smart specialisation, regional growth and applications to EU cohesion policy"
2011/15, Montolio, D.; Trillas, F.: "Regulatory federalism and industrial policy in broadband telecommunications"
2011/16, Pelegrín, A.; Bolancé, C.: "Offshoring and company characteristics: some evidence from the analysis of Spanish firm data"
2011/17, Lin, C.: "Give me your wired and your highly skilled: measuring the impact of immigration policy on employers and shareholders"
2011/18, Bianchini, L.; Revelli, F.: "Green polities: urban environmental performance and government popularity"
2011/19, López Real, J.: "Family reunification or point-based immigration system? The case of the U.S. and Mexico"
2011/20, Bogliacino, F.; Piva, M.; Vivarelli, M.: "The impact of R&D on employment in Europe: a firm-level analysis"
2011/21, Tonello, M.: "Mechanisms of peer interactions between native and non-native students: rejection or integration?"
2011/22, García-Quevedo, J.; Mas-Verdú, F.; Montolio, D.: "What type of innovative firms acquire knowledge intensive services and from which suppliers?"
2011/23, Banal-Estañol, A.; Macho-Stadler, I.; Pérez-Castrillo, D.: "Research output from university-industry collaborative projects"
2011/24, Ligthart, J.E.; Van Oudheusden, P.: "In government we trust: the role of fiscal decentralization"
2011/25, Mongrain, S.; Wilson, J.D.: "Tax competition with heterogeneous capital mobility"
2011/26, Caruso, R.; Costa, J.; Ricciuti, R.: "The probability of military rule in Africa, 1970-2007"
2011/27, Solé-Ollé, A.; Viladecans-Marsal, E.: "Local spending and the housing boom"
2011/28, Simón, H.; Ramos, R.; Sanromá, E.: "Occupational mobility of immigrants in a low skilled economy. The Spanish case"
2011/29, Piolatto, A.; Trotin, G.: "Optimal tax enforcement under prospect theory"
2011/30, Montolio, D; Piolatto, A.: "Financing public education when altruistic agents have retirement concerns"
2011/31, García-Quevedo, J.; Pellegrino, G.; Vivarelli, M.: "The determinants of YICs' R&D activity"
2011/32, Goodspeed, T.J.: "Corruption, accountability, and decentralization: theory and evidence from Mexico"
2011/33, Pedraja, F.; Cordero, J.M.: "Analysis of alternative proposals to reform the Spanish intergovernmental transfer system for municipalities"
2011/34, Jofre-Monseny, J.; Sorribas-Navarro, P.; Vázquez-Grenno, J.: "Welfare spending and ethnic heterogeneity: evidence from a massive immigration wave"
2011/35, Lyytikäinen, T.: "Tax competition among local governments: evidence from a property tax reform in Finland"
2011/36, Brülhart, M.; Schmidheiny, K.: "Estimating the Rivalness of State-Level Inward FDI"
2011/37, García-Pérez, J.I.; Hidalgo-Hidalgo, M.; Robles-Zurita, J.A.: "Does grade retention affect achievement? Some evidence from Pisa"
2011/38, Boffa, f.; Panzar. J.: "Bottleneck co-ownership as a regulatory alternative"

2011/39, González-Val, R.; Olmo, J.: "Growth in a cross-section of cities: location, increasing returns or random growth?"
2011/40, Anesi, V.; De Donder, P.: "Voting under the threat of secession: accommodation vs. repression"
2011/41, Di Pietro, G.; Mora, T.: "The effect of the l'Aquila earthquake on labour market outcomes"
2011/42, Brueckner, J.K.; Neumark, D.: "Beaches, sunshine, and public-sector pay: theory and evidence on amenities and rent extraction by government workers"
2011/43, Cortés, D.: "Decentralization of government and contracting with the private sector"
2011/44, Turati, G.; Montolio, D.; Piacenza, M.: "Fiscal decentralisation, private school funding, and students' achievements. A tale from two Roman catholic countries"

2012

2012/1, Montolio, D.; Trujillo, E.: "What drives investment in telecommunications? The role of regulation, firms' internationalization and market knowledge"
2012/2, Giesen, K.; Suedekum, J.: "The size distribution across all "cities": a unifying approach"
2012/3, Foremny, D.; Riedel, N.: "Business taxes and the electoral cycle"
2012/4, García-Estévez, J.; Duch-Brown, N.: "Student graduation: to what extent does university expenditure matter?"
2012/5, Durán-Cabré, J.M.; Esteller-Moré, A.; Salvadori, L.: "Empirical evidence on horizontal competition in tax enforcement"
2012/6, Pickering, A.C.; Rockey, J.: "Ideology and the growth of US state government"
2012/7, Vergolini, L.; Zanini, N.: "How does aid matter? The effect of financial aid on university enrolment decisions"
2012/8, Backus, P.: "Gibrat's law and legacy for non-profit organisations: a non-parametric analysis"
2012/9, Jofre-Monseny, J.; Marín-López, R.; Viladecans-Marsal, E.: "What underlies localization and urbanization economies? Evidence from the location of new firms"
2012/10, Mantovani, A.; Vandekerckhove, J.: "The strategic interplay between bundling and merging in complementary markets"
2012/11, Garcia-López, M.A.: "Urban spatial structure, suburbanization and transportation in Barcelona"
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