The Effects of Revealed Corruption on Local Finances[1]

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Abstract

This paper analyzes the financial implications of disseminating information about corruption. In particular, we study how the revelation of local corruption affects public finances. We use data from Spain during the period 2003-2010 and match municipalities that suffer a corruption scandal with a control sample of similar municipalities. We use two identification strategies. The first one matches each municipality in which a scandal has been revealed to a corrupt municipality in which the scandal has not yet been revealed. The second strategy matches municipalities with corruption scandals to similar municipalities without corruption scandals and then implements a differences-in-differences regression to isolate the causal effect. We find that after corruption is revealed, both revenues and expenditures decrease significantly (approximately by 8% and 7%, respectively) in corruption-ridden municipalities compared to the counterfactual group. The effect comes mostly from other economic agents' unwillingness to fund or start new infrastructure projects in municipalities where corruption has been revealed.

Keywords: Revealed Corruption, Public Expenditures, Public Revenues, Differences in Differences, Propensity Score Matching

JEL Codes: D72, D78.

1. Introduction

Corruption decreases economic growth and affects the political process by diminishing people's trust in the system (e.g. Mauro, 1995, Bowler and Karp, 2004). Therefore, a fundamental pillar of well-functioning economies and political institutions is the ability to detect and penalize corrupt behavior, which requires an appropriate dissemination of information about corruption.

While several studies have pointed out that information has a crucial role in shaping people's reaction to corruption (e.g. Reinikka and Svensson, 2004, 2011), the full array of effects triggered by the dissemination of information about political corruption are, to date, not well understood. In fact, the only well-studied reaction to the revelation of corruption scandals is that of voters (e.g. Ferraz and Finan, 2008; Costas-Pérez et al., 2012).

In this paper we show that the dissemination of information triggers a reaction by other political and economic agents as well and, as a result, the revelation of the scandal causes a significant effect on the revenues and expenditures of the municipalities affected by corruption scandals. Enhancing our knowledge of how information on corruption scandals affects agents is key to the implementation of adequate policy responses to deal with corruption because the design and the effectiveness of such policies relies on us having a thorough understanding of how economic and political agents behave when information about corruption is released.

We focus on the reaction to local corruption scandals from politicians, firms, and taxpayers. The basic hypothesis is that all these agents are likely to react to the public release of information about corruption in a way that will have a negative impact in the public finances of the municipality affected by the scandal. The rationale that explains this behavior is different for each agent.

In the case of politicians we should distinguish between the incumbent politicians charged with corruption (who usually remain in office while the investigation takes place) and politicians from higher levels of government. The accused incumbents may react to the public release of the scandal manipulating public finances strategically to decrease the electoral impact of the scandal, which would imply reducing visible taxes and increasing visible expenditures. On the other hand, politicians from higher levels of government may react to news about corrupt peers by trying to distance themselves from the scandal which may imply cutting funds to projects promoted by politicians charged with corruption. Similarly, other economic agents such as firms may also react by choosing to start new projects in areas other than those affected by a corruption scandal. Finally, if corruption decreases the level of trust in the corrupt incumbent, taxpayers may feel more compelled to evade taxes, or may choose to relocate to pay taxes in a different location.

Despite their potentially large impact on public finances, these effects have, thus far, been scarcely studied. This paper contributes to the literature by being the first to study how the revelation of corruption affects the finances of corruption-ridden municipalities.

The lack of attention in the literature to the non-electoral effects of revealed corruption may be due to the inherent difficulties of distinguishing the causal impact of the *revelation* of the corruption scandal from the effects of corruption itself. The identification problem arises from the fact that politicians involved in a corruption scandal are likely different from other politicians in aspects other than their tendency towards corrupt acts. For example, corrupt politicians may exhibit different ability levels, or biases towards deficits or surpluses, or biases towards certain types of spending. Similarly, municipalities in which corruption scandals occur may have different characteristics than those in which corruption is absent (e.g. location, population, socio-economic situation, etc). This implies that fiscal outcomes are endogenous to corruption, rendering strategies that do not take this into account inappropriate.

We are able to avoid this problem by using a quasi-experimental design and taking advantage of a unique data set that includes all corruption scandals that took place at the municipality level in Spain between 2003 and 2010. Spain provides an ideal setting to study corruption because it provides a relatively large number of corruption cases at the municipal level during the period of study (see Costas-Pérez et al., 2012; Jiménez, 2013), while holding many other institutional aspects constant across units of study. We define corruption scandals as a local politician being prosecuted due to corruption during the electoral term. As we have the exact date in which the politician was first accused, we observe the behavior of the municipalities before and after the corrupt behavior was known, which allows us to follow a quasi-experimental design.

We use two different identification strategies based on matching. Our first approach uses only municipalities that are revealed as corruption-ridden during our period of study. We match each observation corresponding to a municipality in which corruption has already been discovered to an observation from the same year corresponding to a similar municipality in which corruption has not been revealed yet. Our second method combines a propensity matching estimator with a differences-in differences-regression. We use propensity score matching to select a control group of municipalities that were not subject to corruption scandals during our period of study, but that were very similar to exposed municipalities at the beginning of the period. Then, to eliminate further biases and to control for unobserved differences and time effects, we estimate a differences-in-differences regression on the matched sub-sample.

We find that local finances are greatly affected by exposing the corruption of politicians, both on the revenue and on the expenditure side. During the years right after the corruption scandal is revealed, both revenues and expenditures decrease by approximately 8 per cent. We also find that the decrease in revenues is due mostly to a reduction in "non-autonomous" revenues, which are those that depend on the willingness of other economic agents to fund new projects in the municipality, such as revenues obtained to fund new infrastructure projects from either other levels of government (e.g. discretionary grants), or the private sector (e.g. license fees, real estate purchases). On the expenditures side, the reduction is concentrated mostly in infrastructure projects, for which funds are no longer available. Combined, the results show that after the corruption scandal is revealed, public administrations and private economic agents are reluctant to participate in economic transactions with the corruption-ridden municipality, particularly in the areas more prone to corruption such as construction and infrastructure projects. To our knowledge this paper is the first to document this effect empirically.

The rest of the paper is structured as follows. Section 2 is devoted to providing background and relating the paper to the previous literature. Section 3 provides a brief overview of the Spanish case. Section 4 explains the identification strategy and the econometric model. Section 5 presents the data. Section 6 presents and discusses the results. Finally, Section 7 is devoted to concluding remarks.

2. Literature and Hypotheses

While there is a lack of research on how the revelation of corruption scandals affects economic agents, there is a large literature that has studied the relationship between country level measures of corruption and macroeconomic variables. According to this research, corruption negatively affects growth by detracting resources from their most efficient uses (Mauro, 1995). Corruption has also been found to be correlated with an increase in public expenditure (Tanzi and Davoodi, 1997, 2000) and deficits (Arin et al., 2011); and to lead to a reallocation of resources from items such as education or health to infrastructure construction in which it is easier to extract funds (Mauro, 1998). Most of this literature has used cross-country data and perceived corruption as a measure of corruption. However, recent research such as Liu and Mikesell (2014) using state level data from within the U.S. and an objective measure of corruption such as the number of convictions, reaches a similar conclusion: corruption increases public spending, and changes the allocation of public money favoring construction projects and capital spending.

One way to reduce these negative effects of corruption is increasing public awareness of corruption cases and of the negative consequences of such behavior (Reinikka and Svensson, 2004, 2011). In this context, releasing information about corruption can be seen as a tool to increase political accountability and to avoid the inefficiencies in the public policy decision process and in the allocation of public goods (Reinikka and Svensson, 2011). The effects of the revelation of information about corruption, however, have only been tested extensively in the context of voting behavior, the hypothesis being that revealed corruption reduces the vote share of incumbents involved in corruption scandals. Earlier papers find that the electoral effects of revealing corruption are modest (Peters and Welch, 1980). More recent works, using stronger identification strategies, have found that the electoral effects of corruption scandals are quite large. For example, Ferraz and Finan (2008) use the publicly released results of random audits to Brazilian municipalities to identify corrupt municipalities and find that incumbents in corrupt municipalities lose between 10% and 30% of their vote share and that their election probability decreases by 17%.[2] Similarly, Costas-Pérez et al. (2012) use a matching procedure and find that, in the case of Spain, incumbents pressed with corruption charges lose around 12% of the vote share. They also find that both the intensity of news covering and the type of scandal covered (charges pressed or not) affect electoral outcomes.

Recent literature has moved to study the mechanisms through which these electoral effects happen. Some papers argue that voters punish corrupt incumbents because corruption reduces trust in government (e.g. Bowler and Karp, 2004 and Solé-Ollé and Sorribas-Navarro, 2014). Other papers argue that voters only punish corrupt incumbents when they are unable to appropriate part of the corruption rents but that they forgive corrupt incumbents if they also obtain a short-run benefit from corruption. For example, Fernández-Vázquez et al. (2015) study the case of Spain and find that in those cases in which corruption leads to a short run increase of economic activity and in the value of voters' real estate assets, the effect of the electoral punishment is much smaller. Similarly, in the case of Brazil, Brollo (2011) shows that voters punish corrupt politicians more when the revelation of corrupt behavior results in lower resources to the municipality due to the punishment of the central government authorities in terms if intergovernmental transfers.

In this paper we focus on the non-electoral effects of the dissemination of information about corruption scandals. Similar to voters taking their vote away from corrupt

[2] See also Ferraz and Finan (2011).

incumbents, other economic agents such as firms or other levels of government will be more reluctant to deal with corruption-ridden municipalities once the scandal is out on the news. The main hypothesis tested in the paper is that this will likely have a negative impact on local finances.

There are several mechanisms that could explain why local finances could be affected by the release of the corruption scandal. Each of the mechanisms is explained by the likely behavior of economic and political agents post-revelation of the scandal. On the one hand, if corrupt politicians were using fiscal policy strategically to obtain rents or to distract voters with visible expenditure, as previous literature suggest (e.g. Mauro, 1998), they will have no incentives to do so after the scandal is out due to the higher scrutiny. This is likely to lead to less activity in those areas of policy more prone to corruption (e.g. construction and infrastructure) and therefore less revenues and expenditures in the corresponding categories of the municipal budget.

Secondly, previous literature has repeatedly shown that the allocation of transfers from higher levels of government is influenced by political variables (Solé-Ollé and Sorribas-Navarro, 2008; Brollo and Nannicini, 2012). If higher levels of government worry that the scandal at a lower level may affect their political fortunes, they may prefer to send funds to municipalities not tainted with corruption, so as to distance themselves from the scandal. We should then observe, as suggested in Brollo (2011) for the case of Brazil, lower discretionary transfers to a corrupt municipality –e.g. capital grants-, right after the revelation of the scandal, which reduces overall revenues, and prevents municipalities from being able to fund new projects.

Thirdly, private firms may be less likely to start new projects in the municipality either because they will not be able to obtain extra rents from the corrupt politician anymore, or because they do not want the name of the firm to be associated with corruption. In the case of firms that were part of the corruption scheme (e.g. by paying bribes to corrupt incumbents), the judicial investigation and the dissemination of information about corruption brings higher awareness to the public and makes it harder for the firm to extract further rents from corruption, which will lead these firms to stop their practices or to focus on other municipalities where they can still extract rents because corruption has not been revealed yet. On the other hand, if a firm was not part of a corruption scheme prior to the revelation of the scandal, the new information about the corruption scandal in a given municipality may lead the firm to choose a location in which to operate their business away from corruption-ridden municipalities. Therefore, revenues from new businesses or projects being developed in the municipality, such as licenses and fees, will likely decrease.

Finally, as suggested by Timmons and Garfias (2015), the corruption scandal may also affect tax compliance if trust in government erodes sufficiently and fraud is possible, which will decrease revenues on certain types of local taxes.

If these effects are present we should observe an overall negative shock in municipalities' finances post-revelation of the scandal but before the next election takes place, and a shock in those categories of revenues associated with each of the potential channels that explain the overall effect. Therefore, by studying the effects of revealed corruption on overall levels of revenues and expenditures and on each category of revenues and expenditures separately, we are able to shed light on how information about corruption affects economic and political agents, on how it affects the allocation of public goods at the local level, on how the revelation of corruption may cause short run welfare losses in municipalities affected by scandals, and on how these effects may mediate the electoral response observed once elections take place.

3. Institutional framework

In this section we describe the characteristics of the Spanish system necessary to understand the construction of the database and interpretation of the results.

Electoral system

Spanish local elections take place in May every four years. Municipalities are the lowest of the three levels of government in Spain (the other two are the national government and the seventeen regional governments). The municipality council is elected through a proportional representation system that uses D'Hondt rule. The municipality council consists of a different number of councilors depending on the size of the municipality. Parties with less than 5% of the vote share are excluded from being part of the council. The mayor of the municipality is then elected by the municipality council.

The party system is similar at the national and at the local level. There are two main parties that dominate each side of the left-right scale. The Popular Party (*PP*) is the main rightwing party while the socialist party (PSOE) is the main leftwing party. The Popular Party does not face much competition on the right while the socialist party faces competition from the left, mostly from the United Left (IU). In addition, in several regions and particularly in Catalonia and Basque Country there are several nationalist parties that receive large support in their regions of influence.

Table 1 presents a summary of electoral results in local elections during the two elections that we analyze in this paper (2003 and 2007), the two elections before (1995 and 1999) and the most recent election of 2011. According to this table, the combined support to both PSOE and PP has remained fairly constant at around 70% during the last few elections.[3]

Local Public Finances

Municipalities have power over different policy areas depending on their size. Generally, they must manage services such as waste collection, water supply or pavement repair.[4] In addition, larger municipalities must provide services in other areas of policy such as social care, security and environmental protection. Most importantly for our purposes, all municipalities have powers over land use regulations. This means that they decide about the urbanization of the land, which implies that they can decide which areas are devoted to agriculture, which ones are devoted to industrial use and which ones to devote to housing. There is some supervision by regional governments over the urbanization plan but in general municipalities have substantial freedom to pass a plan or to amend it later. As we explain below, this is important to understand the type of corruption most frequently observed in Spanish local politics: bribes in exchange for land use amendments.

On the revenue side, municipalities have some taxation powers in areas such as economic activity, real estate assets, and vehicle taxes. One of the main sources of revenues is the real estate tax (known as "I.B.I."), which is paid by property owners. Municipalities have freedom in deciding the rate of the real estate tax within a certain range.[5] They can also obtain additional funds through transfers from other levels of

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[3] This may change in future elections, as newly created parties such as *Podemos* or *Ciudadanos* are expected to do well according to recent CIS polls (Center of Sociological Research, a public institute that conducts sociological and political analyses).

[4] According to the Spanish Law the local governments must provide certain services, which differ depending on their population. The main law that regulates Spanish municipalities is the *Ley 7/1985 Reguladora de las Bases del Régimen Local*.

[5] The range is between 0.4% and 1.1% of the value of the house, according to article 72 of the Local Finances Statute (*Texto Refundido de la Ley de Haciendas Locales*).

governments, through fees and licenses, and from the selling of their own real estate. Some of these transfers are non-discretionary (mostly current transfers) and they depend on municipalities' size. Other transfers are discretionary and are used mostly to finance infrastructure projects. Usually municipalities present a project to an open call published by the higher level of government and then the regional or federal governments decide which projects receive the transfer. As there is a fair amount of discretion in these transfers (see Solé-Ollé and Sorribas-Navarro, 2008), we would expect them to be one of the sources of revenues to be affected by the revelation of corruption.

During the period of analysis, there were no balanced budget rules on local finances. Municipalities were able to borrow both long and short-term credits. This, combined with the expenditures and revenue regulations means that municipalities had a relatively high level of autonomy to increase or decrease their taxes and expenditures.

Finally, Table 2 shows the distribution of different types of revenues and expenditures during the period of analysis used in this paper. On the revenue side, the larger source of revenues is current transfers from other levels of government (27.73%), tax revenues (indirect and direct taxes add up to 27.03%) and revenues from capital transfers (17.19%). On the expenditure side, current spending (Wages+Goods and Services+Financial Expenses+Current Transfers) represents 64.1%, while investment and other capital spending amounts 32.1%.

Local Corruption in Spain

Several research papers and databases have reported a significant increase in the number of corruption cases at the local level in Spain (Fundación Alternativas, 2007; Jerez et al., 2012). For example, Costas-Pérez et al. (2012), using data from scandals reported on the news collected by the *Fundación Alternativas* finds that before 1999 only 46 cases of corruption scandals were reported on the news, while the numbers skyrocketed to 288 scandals during the 1999-2002 electoral term and to 408 during the 2003-2006.

Parallel to this increase in cases reported on the news, corruption has also become one of the most relevant problems in Spain according to sentiment surveys. For example, in the 2012 CIS Barometer corruption appeared as the fourth most relevant problem. This increase in perceived corruption is not just due to scandals at the local level, but to scandals involving also institutions such as the monarchy, the political parties, the judiciary, and the workers' and the employers' union representatives.

The increase in corruption scandals, their coverage in the news and the widespread opinion of this being a very relevant problem has given rise over the last few years to several legal initiatives to make it easier for judges to investigate and prosecute corrupt behavior, and, as a consequence, the number of corruption cases investigated by the courts and the number of cases that resulted in politicians being formally prosecuted has also increased.

The most frequent type of corruption found in Spanish local politics is bribing related to urban planning or to the adjudication of contracts to manage certain public services.[6] A paramount example is the "Malaya Case", which involved the municipality of Marbella. The mayor and several members of the municipality council of Marbella were accused and found guilty of accepting bribes in exchange of authorizing a variety of construction projects. Some of them were also found guilty of authorizing the sale of municipality real estate to private firms at discount prices in exchange for bribes. Similarly, in a recent scandal involving the city of Alicante, the mayor of the city was charged with

[6] According to Jerez et al. (2012) or Jiménez (2013), approximately 70% of corruption cases at the local level are related to urban planning.

corruption after making several amendments to the urban plan to favor a local construction company. The police found evidence that the mayor accepted gifts such as a boat, a car, and several vacations in exchange for re-zoning several areas of land at a construction company's request.

Although our database includes several other types of corruption, these cases provide a good account of the type of corruption usually found in Spanish local politics.[7]

4. Empirical strategy

As mentioned above, our purpose is to identify the causal effect of revealed corruption on fiscal outcomes at the municipality level. The identification challenge is that although we observe the outcome (fiscal behavior) of corrupt municipalities once corruption is discovered, the counterfactual is not observed. We do not know what the outcome would have been had corruption not been revealed. Therefore, in order to be able to make causal inferences we need to find a good counterfactual.

Past behavior of municipalities in which corruption has been revealed is not a good counterfactual because there may be changes in observable or unobservable variables that may had led those municipalities to change their fiscal behavior regardless of the revelation of corruption. Present behavior of non-corrupt municipalities is not a good counterfactual either, because non-corrupt politicians may be fundamentally different to corrupt ones in their inherent ability levels, managerial skills, or biases towards certain types of spending.

Our strategy consists instead on using a matching method to select a counterfactual group of municipalities similar to those affected by corruption. As we have panel data,

[7] A brief description of several other corruption cases can be found in the Appendix tables reported in Fundación Alternativas (2007).

we use two different matching alternatives to choose the counterfactual and to estimate the treatment effect.

First, we use only municipalities that at some point during our period of study were subject to a scandal, and by year, match municipalities that are already revealed to be corrupt to a similar municipality that has not yet been revealed to be corrupt. This way, the counterfactual group for the behavior of municipalities already found to be corruption-ridden is the group of municipalities that are revealed to be corrupt at some point in our period of analysis, but have not been discovered in the year in which they serve as a control in our matching model.

Our second strategy involves using all municipalities (those found to be corrupt at some point and those never found to be corrupt), and match municipalities that at some point are revealed as corrupt to those that are not revealed to be corrupt in our panel. Matching in this case is done in the first year of the data and followed throughout in subsequent years.

We estimate our first identification strategy using a non-parametric matching model, with the sample restricted to only municipalities that at some point during the period of analysis are subject to a scandal. Analytically, this requires a non-parametric matching instead of a propensity score matching so that we can perform an exact match on year and pair each treatment observation (municipalities in which corruption has been revealed already) with a control observation of the same year but for which corruption has not been revealed yet.[8] This is equivalent to including fixed year effects. This non-parametric approach is preferable to a regression restricting the sample to ever corrupt municipalities and including year and municipality effects, because in this case we

[8] See Abadie and Imbens (2011). This procedure is implemented using the *teffects nnmatch* Stata command.

would still be using within municipality variation, which, as we describe above, is not ideal for identification of a causal effect.[9]

If we denote *Y* as the fiscal outcome of interest (e.g. expenditures or revenues per capita), *X* as the vector of covariates, *d(.)* as the distance function, and *t* as the year, our estimator of the average treatment effect can be summarized as:

$$ATE_t = E\left[Y_1 \mid Tr = 1, d(X), t\right] - E\left[Y_0 \mid Tr = 0, d(X), t\right] = E\left[Y_1 - Y_0 \mid d(X), t\right] \qquad [1]$$

We estimate this effect using nearest neighbor non-parametric matching and the Mahalanobis distance to determine which control observation is closest to each treatment observation in terms of *X*. The distance is calculated using as covariates measures of economic activity at the local level such as unemployment and previous debt, political controls such as ideology, and other socio-demographic covariates.

Unemployment is defined as the percentage of people registered as unemployed in the municipality's unemployment office. Monetary variables such as debt are measured in real euros per capita. Ideology is captured by the vote share of rightwing and leftwing parties in the electoral term corresponding to the year in which observations are matched and by the vote share of the two main parties (PSOE and PP), which captures the peculiarities of party competition in the municipality.[10] Finally, the additional socioeconomic controls are the population size of the municipality, the percentage of population between 15 and 65 (which captures the need for schooling and health services), and the population density, which is a proxy for urbanization. We included linear and quadratic terms to assure that a good balance was achieved. As the matching

[9] This within strategy, however, leads to results that are qualitatively similar to the ones obtained in our preferred specifications. The results of this set of regressions are available from the authors.
[10] In some regions, and particularly in the Basque Country and Catalonia, the two main parties at the national level are not the main parties.

method is not consistent when matching on two or more covariates, we use the bias correction algorithm suggested in Abadie and Imbens (2011).[11]

Overall this non-parametric approach has several advantages. First, as we are using only corrupt municipalities in both the treatment and the control group, we do not have to worry about unobserved differences between corrupt and non-corrupt municipalities driving the results. Second, as we match each treatment observation to a corrupt municipality in which corruption has not been revealed yet, our model allows us to isolate the effects of the *revelation* of corruption, which is our treatment of interest, instead of capturing the effects of corruption itself. Third, as we perform an exact match on year, common shocks to all municipalities such election cycle effects or other economic shocks, are accounted for. Fourth, as this approach is non-parametric, it does not require us to specify a formal model for either the treatment status or the calculation of the treatment effects (Abadie and Imbens, 2011).

A potential drawback of this approach, however, is that as we restrict the universe of potential controls to corruption-only municipalities, it is harder to find controls that are a good match in terms of observables than if our potential controls included also municipalities with no corruption scandals over the period. This is particularly true for observations in the later years of our sample, in which most corruption-ridden municipalities have already been revealed as corrupt. While our definition of corruption allows us to find a match among municipalities that were corrupt on the first electoral term but not on the second, the number of potential controls is limited. To check to what extent this could be a problem, we performed caliper matching restricting the estimation to pairs that lie within a variety of caliper distances with no significant change in the

[11] This estimator is implemented in Stata using *teffects nnmatch* command and the bias correction option.

results. In addition, several balancing tests show a reasonably good balance of observable characteristics across treatment and control groups.

Our second approach complements the previous one by using a different identification strategy. This approach allows us to use as potential controls the whole sample of municipalities that were not found to be corrupt at any time during our period of analysis, which includes more than 3,000 municipalities. We match each of the corrupt municipalities in the treatment group to one of the control groups of non-corrupt municipalities. As mentioned, we perform the matching at the beginning of the period (in 2003). That is, we match observations before corruption has been revealed in any of the corruption-ridden municipalities, and then we follow them and their controls throughout the whole period of study.

In this case, we match using a parametric propensity score (Rosenbaum and Rubin, 1983). We use a logit model to estimate the propensity score. The covariates in the logit model are the same as in the non-parametric model, except that we include a one year lag of the variables that capture ideology so that we incorporate the political structure of the previous electoral period.[12] After the estimation of the logit model, we define the missing counterfactual of each treatment observation using nearest-neighbor matching.

As this matching strategy only allows us to balance covariates on observables, and municipalities that did not suffer a corruption scandal may have different unobserved characteristics, we complement matching with the following differences in differences regression performed on the matched sample.[13]

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[12] In the non-parametric model this is not needed because the ideology variables are constant within electoral term.
[13] For other papers combining matching and differences-in-differences see for example Abadie (2005), Blundell and Costa Dias (2000, 2009), or Girma and Görg (2007).

$$Y_{it} = \alpha_0 + \alpha_1 Corrupt_{it} + \alpha_2 After_{it} + \alpha_3 Corrupt_{it} \cdot After_{it} + \beta X_{it} + \theta_{cycle} + \gamma_t + \lambda_i + \varepsilon_{it}$$

[2]

The dependent variable in this model, *Y,* is the fiscal outcome of interest (revenues, expenditures or deficit) of municipality *i* in year *t*. The coefficient of interest is α_3, which captures the causal effect of revealing corruption in a corruption-ridden municipality on the outcome variable. The coefficient α_3 identifies a causal effect because we control for unobserved differences between treatment and controls and for common shocks through the variables *Corrupt* and *After*. The variable *Corrupt* takes value 1 for municipalities that are corruption-ridden in a given electoral term and 0 otherwise. *After* takes value 1 in the years after corruption is revealed and 0 otherwise.

For each control observation, the "after" years are determined by the treatment municipality to which they are matched. Therefore, *After* controls for shocks that are common to treatment and control municipalities. θ_{cycle}, γ_t and λ_i are electoral cycle, time and individual fixed effects that allow to capture the effects of each year of the electoral cycle, each year overall and remaining unobserved idiosyncratic differences across municipalities.

The vector of controls includes the variables included in the matching model (unemployment, size, population density, percentage of population between 15 and 65 and the ideology variables). Strictly speaking these controls are not needed because they are balanced across treatment and controls. Including them, however, improves precision and removes potential remaining biases.

Note that in equation [2], identification arises from two sources. First from the fact that due to matching our group of controls is very similar on observables to corruption-ridden municipalities. Second, due to the differences-in-differences regression, we are removing unobserved differences and common shocks. The model, therefore, is likely to estimate the causal effect of revealed corruption on fiscal outcomes with no bias. However, as we are still discarding the observations that did not serve as controls, unbiasedness comes at the price of losing precision. For comparison purposes, we will also show the results of a similar differences-in-differences model that has the opposite strength (e.g. more bias but more precision). To this aim, we estimate the following regression on the whole sample of municipalities and not just on the matched sample:

$$Y_{it} = a_0 + a_1 Corrupt_{it} + a_3 Corrupt_{it} \times After_{it} + bX_{it} + \theta_{cycle} + \gamma_t + \lambda_i + \varepsilon_{it}$$

[3]

The difference between equations [2] and [3] is that in this model, the variable *After* is not included separately. This is because we use the whole sample and each treatment observation is not paired to a specific municipality in the treatment group –i.e. some observations do not have an "After". Common shocks to treatment and control municipalities for each period after corruption, are captured by the time fixed effects.

If the set of observables included in the model is adequate, and given that we have individual and time fixed effects and the variable *Corrupt* to control for unobservables, the coefficient α_3 should also identify the causal effect of interest with no bias and with more precision than in the previous strategy, as we are using the whole sample. Otherwise, we would have a precise but more biased estimate. In the results section we present the results of this model to show that results hold when using the whole sample as well.

5. Data and measurement issues

Data

We test our hypothesis using a database of Spanish municipalities from 2003 to 2010. This period includes two complete electoral terms (2003 to 2006 and 2007 to 2010). We use these two electoral terms because public finance data at the municipal level are only available from 2001 onward and because these are the only two electoral terms completely covered in our corruption database.

We obtain financial data for each Spanish municipality from the *Ministerio de Administraciones Publicas*. The database includes not only aggregate expenditures and revenues in each municipality but also the composition of revenues and expenditures according to standard accounting categories.

We collected data on political results from the *Ministerio del Interior*, and socio-economic variables from *La Caixa* database. Political variables included in the database are the vote count of each party obtaining representation in each municipality, the size in terms of population, and the seats obtained by each party. *La Caixa* database provides us with information on unemployment levels. The percentage of population between 15 and 65 years old, and population density, which we use as a proxy for urbanization, were obtained from Spanish National Statistic Institute. This database includes information on municipalities larger than 1,000 inhabitants (more than 95% of Spanish population), so those are the ones used in the analysis.

Corruption data were compiled by the authors using published information about corruption scandals. The data include all corruption scandals in Spain affecting local politicians from 2003 to 2010 (it does not include those affecting the regional or national government). We define a municipality as corruption-ridden if either the mayor or a member of the municipality was formally charged with corruption during the electoral term.

This is different from other measures of corruption typically used, such as perceived corruption or the number of news counts about a given scandal. Both of these alternative measures have the advantage of providing a measure, even if subjective, of the coverage of the scandal, but they are subject to strategic manipulation. For example, a newspaper may inflate the number of news affecting a politician of the opposite ideological wing, or may not cover stories affecting politicians they favor.[14] In addition, news counts do not filter scandals by their relative importance (e.g. formal criminal accusations versus mere administrative infractions or even rumors). Our variable takes into account only objective facts (formal accusation) and in addition, it selects only relatively important cases, since, in Spain, to be formally accused of corrupt behavior by the judiciary there must be a preliminary investigation confirming that initial evidence is strong enough to support the presumption that there may have been an unlawful behavior.

The corruption variables were compiled using information from a variety of sources including published reports about corruption, information available in different corruption blogs and, mostly, a thorough online search of corruption cases reported on the news. We gathered information on the date in which the courts officially pressed charges, what decision was finally made (if any), when was the decision made, the partisanship of the politician involved, the type of corrupt behavior and the source from which we obtained the information. We identified 274 corruption cases in which the mayor or a member of the municipality council was formally accused of corruption by

[14] See for example Larcinese et al. (2011) for US politics.

the judiciary, and for which we could reliably identify the date in which the formal accusation took place.[15]

Measurement issues

After merging and cleaning the data of missing and implausible values, our final sample is a panel containing 22,142 observations corresponding to 3,151 municipalities observed during the period 2003 to 2010[16]. Table 3 presents the summary statistics for the whole sample, the sub-sample of municipalities that suffered a corruption scandal during the period and the sub-sample of municipalities that were not subject to a corruption scandal.

Table 3 includes two corruption variables: *Corruption over the period* and *Corrupt*. *Corruption over the period* is defined as 1 for observations that correspond to a municipality that faced a corruption scandal at some point during the period. It is the variable used to limit the sample in the non-parametric estimation (equation [1]) and is also used to calculate the propensity score in the parametric matching (equation [2]). According to this variable, approximately 10% of the observations correspond to municipalities that suffered a corruption scandal during the period. *Corrupt* accounts for whether corruption occurred in the first or in the second electoral term. This variable is

[15] Our corruption database is similar in content but different to other databases compiled in other independent research efforts (Fundación Alternativas, 2007, Costas-Pérez et al., 2012, Fernández-Díaz, 2015, and Jerez et al., 2012). These databases also use online searches as the main source of information but differ in the type of cases included and in the periods and regions covered. The most complete of them is the one by Fundación Alternativas (2007), extended later in Costas-Pérez et al. (2012) and Solé-Ollé and Sorribas-Navarro (2014). We differ from them in the coverage (2003-2010 vs. 1999-2009) and in that we only consider corruption cases those in which the politician was formally accused with corruption charges after a criminal investigation was performed by the judiciary. Our inclusion criteria is similar to the one used in Fernandez-Díaz et al. (2015). Their database, however, covers only one electoral term instead of two (which is important to control for economic and electoral cycle effects), two regions instead of all, and has no information on accusation dates. Finally, Jerez et al (2012) differs from ours in that it includes a different study period (2000 to 2008), focuses only on scandals related to urban planning, includes all cases reported on the news and not only formal accusations, and has no information about dates in which charges were pressed.

[16] Our panel is unbalanced because we eliminated observations with implausible values in some of the fiscal variables and because there are some missing values for some municipalities in some of the years.

a time-varying measure that takes value 1 if an observation corresponds to municipalities that suffered a corruption scandal in a given electoral term and 0 otherwise. *Corrupt* is thus the variable used in the estimation of the treatment effect in the non-parametric matching (which is restricted to municipalities for which *Corruption over the period* takes value 1) and in the differences-in-differences model. Approximately 5% of all observations in the database are considered as corrupt according to this definition. Note that on the second period of our study we consider that municipalities that suffered corruption in the previous electoral term start clean. An alternative is to consider that only those municipalities in which the incumbent party changes do have a clean start on the second period. Estimation using this alternative definition yields similar conclusions as found with the first measure.

Second, the variable *After* has a value of 0.3632, which implies that approximately 36% of the observations corresponding to corrupt municipalities are from years strictly after the corruption is revealed. It is arguable whether we should define the after years including also the year in which corruption occurs, because many effects may already occur during that year, particularly if the scandal is revealed at the beginning of the year. In our preferred specifications we use the more restrictive definition because many budgeting decisions have already been taken for the year in which the revelation occurs (as budgets are completed for each year at the end of the previous year) and because it is a more conservative approach. If the effects of the revelation start happening already in the year in which the scandal is out, our control group would include observations in which some effects are already happening, which would go against finding an effect. Consequently, the estimation of the model using the alternative, more inclusive definition does not change the results.

Finally, it is worth explaining why the leftwing and rightwing vote share variables in Table 3 do not add up to 1. There are some parties that are difficult to classify as left or right either because they are center parties, or because they are local parties for which we do not have enough knowledge to classify them into the standard left-right scale. These parties are approximately 20% of the sample. They constitute the excluded category in the regressions that control for ideology.

6. Results[17]

We start this section by describing the results of the non-parametric matching model. Then we describe the results of the parametric matched differences-in-differences model. We then show the robustness of results by presenting several falsification exercises. Finally, we investigate the mechanisms by analyzing the effects on different types of revenues and expenditures.

6.1 Non-parametric Matching

To evaluate the quality of the matching, the results of the non-parametric matching are summarized in Table 4. Recall that in this model we only use observations from municipalities that suffer corruption scandals. We match observations corresponding to municipalities in which corruption has been revealed during the electoral term to municipalities that are subject to corruption scandals during the period of analysis but for which corruption has not been revealed yet on the year for which the observation serves as a control.

[17] Although not shown to save space, the robustness of the results discussed in this section was tested through a battery of tests. In particular the results are generally robust to 1) including different subsets of the control variables, 2) Using a different definition of the variable *Corrupt* in which we did account for corrupt incumbents being re-elected, 3) using a different definition of *After* in which the year in which corruption is revealed is considered the first year of the treatment instead of excluded from the treatment; 4) constructing the matched sample using different polynomials of the control variables; 5) estimating the model eliminating the observations with values in the top and bottom of the distribution of the dependent variables; and 6) using lags of some of the control variables that one may be concerned that may be affected by corruption, such as unemployment.

We perform an exact matching by year using nearest neighbor and the Mahalanobis distance to calculate similarities between treatment and controls on a set of observables. The covariates used to calculate the distance are second order polynomials of debt at the beginning of the year, unemployment, population, population density, the percentage of people between 15 and 65 and the variables that measure ideology (to capture electoral results in the electoral period).

Panel A in Table 4 shows a t-test of the differences of means between treatment and control groups after the matching. Panel B shows the t-tests results before the matching. This table shows that the matching model allows to achieve a good balance on observable characteristics, as only one of the differences of means performed on the matched sample is significant at the 5% level (PSOE share). In addition, the magnitude of the difference is small for all variables (including PSOE share), confirming that treatment and control groups share similar observable characteristics. Panel B shows that matching is needed to achieve a balanced on observables, as in this case both the t-tests and the magnitudes show significant differences between the treatment and the unmatched sample of controls.

Table 5 shows the estimation of the average treatment effect using the nearest-neighbour method. For robustness, we show results using 1, 2, 3 and 4 neighbours (M is the number of neighbours used). This table shows that the revelation of corruption has a significant causal effect on municipalities' finances. According to this table the estimated effect of revealing corruption on revenues per capita is a reduction of revenues by approximately 100 euros per capita (the estimates range between -98.14 and -107.58). As the sample mean of revenues per capita is 1245.26 euros (see Table 3), the estimated reduction in revenues after the revelation of corruption is approximately 8% of total revenues. In the case of expenditures the effect of revealing corruption is a reduction of expenditures of approximately 70 euro per capita (estimates range between -58.9 and -75.8), or approximately 6% of total expenditures. The combined effect is an increase in deficit, although in this case the effect is not statistically significant.

6.2 Parametric matched Differences in Differences

The results of the previous model are confirmed by estimates from the parametric model. We start the discussion of the estimates from this model by describing first the results of the parametric matching and then the estimates of the treatment effects from the differences-in-differences model.

Matching model

Table 6 shows the results of the logit model used to calculate the propensity scores. This model estimates the probability of being corrupt at the beginning of the period of analysis (in 2003). The dependent variable is *Corruption over the period*, which is a dummy that takes value 1 if the municipality suffers a corruption scandal between 2003 and 2010. The covariates in the model are second order polynomials of the debt, unemployment, population, population density, and the percentage of people between 15 and 65; and second order polynomials and a one year lag of the variables that measure ideology (to capture electoral results in the prior election). Although many coefficients in this regression are non-significant, a Chi-Square test of the joint significance reveals that each of groups of variables included in the regression are jointly significant.

The model of Table 6 was then used to match each corruption-ridden municipality to the most similar control, according to their propensity scores. To evaluate the quality of the match, the results of the matching model are summarized in Table 7. In this table we present a t-test of the differences in means of both the control variables used in the matching model and the outcome variables that we use in the differences-in-differences.

This table shows that before the matching (lower panel) both the control variables and the outcome variables differ significantly across treatment and control groups. After the matching (upper panel), the set of controls is balanced across treatment and control groups and none of the differences of means are now statistically significant. Most importantly, the outcome variables, which were not used in the matching model, are also balanced. This implies that our matching model identifies a sample of controls that were alike in terms of socio-economic situation, were ideologically similar, and had an undifferentiated fiscal behavior before corruption was revealed.

Differences-in-Differences model

Table 8 presents the results of the differences in differences model. The first three columns display the results of the model of equation [2], while columns 4 to 6 estimate the model of equation [3], which provides a check for robustness.

The regressions of the first three columns are estimated on the matched sub-sample. Each column estimates the effect of corruption on a different dependent variable. Column 1 estimates the effects on Revenues per capita. Column 2 estimates the effects on Expenditures per capita. Column 3 estimates the overall effect on fiscal deficits/surpluses as a percentage of revenues (the variable is defined as (Revenues – Expenditures)/ Revenues). The coefficient of interest is the interaction between *Corrupt* and *After*, which measures the causal effect of the revelation of the scandal in the municipality. The coefficient has a value of -98.7321 in Column 1 and is significant at the 1%. This number implies that after a corruption scandal is revealed, revenues decrease in the municipality by 98.73 euros per capita, which is approximately an 8%

decrease compared to what would have occurred had corruption not been revealed. Column 2 shows that expenditures also decrease significantly as a consequence of the revelation of corruption. The decrease in expenditures is smaller (75.57 euros per capita) and amounts approximately 7%. Again, the combination of the decrease in both revenues and expenditures produces no significant effects on deficit.

Columns 4 to 6 in Table 5 estimate the model using the whole sample. These regressions confirm the results of the first three columns. The revelation of corruption has a significant effect on revenues and expenditures. The magnitude of the coefficients shows that the effect on revenues and expenditures is slightly larger. As the effects on revenues are larger than those on expenditures the model finds a negative, although small and non-significant, effect on surpluses.

Overall, Table 8 confirms the results of the non-parametric model and show that the revelation of corruption has a quantitatively large effect on municipalities' finances both on the revenue and on the expenditure side. As a result, the revelation of corruption likely may have a negative effect on surpluses, although this effect is not very robust, and we cannot discard the possibility that the combined effects on revenues and expenditures are neutral to surpluses.

6.3. Falsification tests

We now discuss two falsification exercises that provide support to the causal interpretation of our results.

The first falsification exercise is presented in Table 9. In this table we estimate the same non-parametric model as in Table 5 but using a fake date for the revelation of corruption variable. To construct the fake revelation of corruption date we substract a random number between 3 and 5 from the true revelation year. The idea of this test is that if our model truly captures the effects of revealed corruption, we should not find an effect when we use a date a few years earlier than the true date. In this test, our treatment and control samples are both formed by corrupt municipalities before corruption is revealed, so there should be no differences between the two groups if the model is correctly specified. We construct the fake revelation date using a random number instead of arbitrarily fixing the lag at a given number to avoid the possibility that results may be influenced by anticipation effects or something else that may affect all corrupt municipalities before the revelation of corruption.

Anticipation effects may occur, for example, if before the formal prosecution of the politician there are some other initial judiciary actions such as preliminary investigations or interrogations that are leaked to the public. By creating the fake revelation date sufficiently earlier than the true date (e.g. between 3 and 5 years) and by randomly choosing the lag from the true date, we avoid these effects. Consistent with the model of Table 5 correctly identifying a causal effect, Table 9 shows that our non-parametric model finds no effect of the fake treatment on public finances. As one would expect, the estimates of the "placebo" treatment are now small in magnitude, non-significant, and change in signs from one specification to another.

Table 10 provides an additional falsification test that is in the same spirit of the one just described but is implemented using the parametric differences-in-differences model. In this case we use the same fake revelation of corruption date as in the previous falsification test, and, in addition, we eliminate from the estimation of the differences-in-differences model all truly treated observations (e.g. corrupt municipalities after the true revelation date). Note that in this case we do not change the matched sample. Instead, we artificially create a random date of assignment to the treatment and we remove truly treated observations from the estimation. As we do not have truly treated observations in this sample, we should not find any treatment effects. Table 10 shows that this is the case. The relevant interaction term of columns 1 to 6 is again small in magnitude and far from being significant, providing support to the interpretation of our results as causal.

6.4. Mechanisms

After providing evidence that revealed corruption has a significant causal effect on the finances of municipalities, we now explore the mechanisms through which the effects happen. Table 11 shows the results on different types of revenues, while Table 12 explores the effects on different types of expenditures. To save space, we show the results using the non-parametric model.[18]

The rows of Table 11 show the estimates of the effects on each revenue category. We find that the categories of revenues in which revealed corruption has a statistically significant effect are: licenses and fees, current transfers, alienation of property and capital transfers. These results are consistent with corruption related to urban planning being the most common type in our database. In addition, these results show which agents are reacting to the revelation of corruption.

The reduction on transfers, particularly in capital transfers, shows that other levels of government reduce their discretionary transfers to corrupt municipalities. In this regard, note that as mentioned before, many capital transfers are assigned to municipalities through open calls to fund infrastructure projects, and, as a consequence, there is a large amount of discretion on the side of the regional governments to decide what projects to fund. We find that, *ceteris paribus*, immediately after the scandal is exposed,

[18] Results of the parametric model are overall similar to results of the parametric model, although less robust for some of the types of revenues and expenditures.

municipalities involved in the scandal receive less of these funds (around a 12% reduction), which is line with the results of Brollo (2011) for the case of Brazil.

Similarly, municipalities receive less revenue from the selling of real estate (more than a 50% reduction). This is consistent with either the municipality coming less willing to initiate new construction projects after corruption is revealed, or private firms coming less willing to buy public property from corruption-ridden municipalities. This could be due to a reduction in trust in the local government, the inability to extract funds from the corrupt mayors after the revelation (if for example buying at discount prices was part of the scheme) or to the increased opportunity cost of investing in a corrupt municipality, as firms do not want their names to be associated with corruption.

The decrease in revenues from licenses and fees (around a 15% reduction) is consistent with less construction happening in the municipality due to either fewer people interested in investing in places tainted with corruption, or with the municipality authorizing fewer projects. Although less likely, this result could also arise from the reduction of the license fees as an electoral strategy to mitigate the effects of corruption.

Additionally, we find no effect of revealed corruption on direct or indirect taxes (see columns 1 and 2). This result is different than the one found for Brazil in Timmons and Garfias (2015), which hypothesize that revealed corruption would reduce revenues from property tax due to reduced compliance by taxpayers. The differences between our finding and theirs are likely explained by the small incentives for tax fraud in the property tax in Spain, which has a very strict process to record property data.

Finally, Table 12 completes the picture by looking at the expenditure side. In this case, the effects occur mostly through a reduction of investment expenditures (e.g. infrastructure building). The coefficient of investment expenditures has a magnitude of

61.14 euros per capita in column 1, which represents approximately 80% of the total estimated reduction in expenditures (which was 77.39 in column 1 of Table 5). The implied reduction in investment is therefore quantitatively large and it amounts approximately a 12% decrease. This again confirms the pattern detected in the revenue side: the revelation of corruption has a significant effect on the areas of the public budget more related with construction activities.

7. Conclusion

This paper finds that revealed corruption has a strong negative causal effect on public expenditures and revenues at the municipality level. Revenues and expenditures decrease by approximately 8%. We also find that the reduction is concentrated in the areas of revenue and expenditure most related to construction activity. This is due to both a reduction of publicly funded projects (e.g. less capital grants to fund infrastructure projects) and privately funded projects (e.g. reduction in revenues from construction taxes). Overall, the revelation of corruption leads other agents to reduce their economic transactions with the municipality and likely reduces corrupt behavior.

These results contribute to expanding our knowledge about the effects of corruption in several ways. On the one hand, previous literature has shown that, at the macroeconomic level, corruption affects growth and changes the allocation of public resources to favor certain areas such as construction projects and capital spending (Mauro, 1998; Liu and Mikesell, 2014). Our results show that that the dissemination of information about the corrupt behavior of specific politicians is likely to change that pattern of behavior and that spending in those areas is reduced after the revelation.

Secondly, our results contribute to increase our overall knowledge about the effects of revealed corruption. Thus far, researchers working on the effects of revealed corruption have shown that the revelation of corruption has significant electoral effects and that corrupt incumbents obtain less electoral support once voters know about their corrupt behavior. This paper shows that the revelation of corrupt behavior has consequences that go beyond electoral effects and that directly affect municipalities finances' (and therefore public policies) even before elections take place.

Finally, the results of this paper are also interesting from the policy point of view. If one of the consequences of corruption is the inefficient allocation of funds to areas where corrupt politicians can extract more rents, our results show that the revelation of corrupt behavior reduces such inefficient expenditure and the revenues paid to fund it. The revelation of the corruption scandal, thus, frees up resources that can be used to fund activities with a higher social return.

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Annex

Table 1. Summary of Electoral results in Spanish Local Elections

	1995	1999	2003	2007	2011
Election Date	May 28	June 13	May 25	May 27	May 22
Participation Rate	69.87	63.99	67.67	63.97	66.16
PSOE Vote Share	30.84	34.26	34.83	34.92	27.79
PP Vote Share	35.27	34.44	34.29	35.62	37.54
Others Vote Share	33.89	31.3	30.88	29.46	34.67

Note: Calculated from electoral data from Ministerio de Interior

Table 2. Summary of Revenue and Expenditure categories

Revenues			Expenditures		
Categories	Euros per capita	%	Categories	Euros per capita	%
Direct Taxes	277.259	22.27	Wages	349.017	28.90
Indirect Taxes	58.948	4.73	Goods and Services	346.809	28.72
Licenses and Fees	208.793	16.77	Financial Expenses	12.626	1.05
Current Transfers	343.703	27.60	Current Transfers	65.523	5.43
Property income	29.209	2.35	Investment	374.929	31.05
Revenues from Selling of Real Estate	44.806	3.60	Capital Transfers	12.847	1.06
Capital Transfers	215.097	17.27	Assets	2.702	0.22
Assets	2.152	0.17	Liabilities	43.177	3.58
Liabilities	65.293	5.24			
Total Revenues per capita	1245.261	100	**Total Expenditures per capita**	1207.630	100

Notes: All variables are defined in real euros per capita. The entries are calculated using the mean of each variable over the whole period of analysis (2003-2010)

Table 3. Summary Statistics

	Whole sample	Municipalities with corruption scandals	Municipalities with no corruption scandals
Revenues per capita	1245.261	1272.087	1242.285
	[651.8416]	[627.9699]	[654.3836]
Expenditure per capita	1207.63	1228.489	1205.316
	561.1238	[526.2544]	[564.8244]
Corruption over the period	0.0998	1	0
	[.2998]	[0]	0
Corrupt	0.0506	0.5066	0
	[0.2191]	[0.5001]	0
After	-	0.3632	-
	-	[0.481]	-
Population	16470.8	58613.1	11795.9
	[82449.5]	[227730.1]	[39779.7]
Unemployment	8.5626	9.2013	8.4918
	[4.3751]	[4.747]	[4.3262]
Population Density	434.6945	824.8198	391.4169
	[1383.887]	[1930.112]	[1302.211]
% People Between 15 and 65	0.343	0.32	0.3456
	[0.0459]	[0.0402]	[0.0457]
Rightwing Vote Share	0.3738	0.3751	0.3736
	[0.1777]	[0.1691]	[0.1786]
Leftwing Vote Share	0.4353	0.4073	0.4385
	[0.2024]	[0.1883]	[0.2037]
PP	0.3049	.3530	0.2996
	[0.1976]	0.1763	[0.1991]
PSOE	0.3567	0.3398	0.3586
	[0.1752]	0.1575	[0.1769]
Log of Total Debt	55.8388	64.1945	54.9124
	93.1565	[82.3973]	[94.2303]
Observations	22,142	2,211	19,931

Notes: All monetary variables are defined in real euros per capita. The entries are calculated using the mean of each variable over the whole period of analysis. (2003-2010). Standard deviation in brakets.

Table 4. T-Test of differences in means between treatment and control groups. Non-Parametric Matching

	Matched Sample			
	Mean Treatment	Mean Control	t-test	p-value
Control Variables				
Rightwing Vote Share	0.377	0.387	0.943	0.346
Leftwing Vote Share	0.370	0.384	1.373	0.170
PP share	0.372	0.381	0.885	0.376
PSOE share	0.310	0.331	2.382	0.017
%Between 15 and 65	0.315	0.317	1.453	0.147
Density	690.307	565.965	-1.737	0.083
Population	47415.750	43993.660	-0.637	0.524
Unemployment	11.069	10.607	-1.608	0.108
Outcome variables				
Revenues	1235.494	1222.187	-0.503	0.615
Expenditures	1213.577	1202.392	-0.428	0.669

	Whole Sample Pre-Matching			
	Mean Treatment	Mean Control	t-test	p-value
Control Variables				
Rightwing Vote Share	0.377	0.373	-0.574	0.566
Leftwing Vote Share	0.370	0.413	4.758	0.000
PP share	0.372	0.347	-3.035	0.002
PSOE share	0.310	0.346	4.706	0.000
%Between 15 and 65	0.315	0.321	3.244	0.001
Density	690.307	706.694	0.211	0.833
Population	47415.750	37467.760	-2.361	0.018
Unemployment	11.069	8.205	-12.753	0.000
Outcome variables				
Revenues	1235.494	1300.026	2.004	0.045
Expenditures	1213.577	1247.105	1.211	0.226

Notes: The means of the matched model are calculated using one neighbour. The treatment group are municipalities in which a corruption scandal has already been revealed. The control group are municipalities in which a corruption scandal occurs over the period of study but for which such corruption has not been revealed on the year for which they serve as a match.

Table 5. Average Treatment Effect. Non-Paratric Matching.

	M=1	M=2	M=3	M=4
Revenue	-108.0825***	-118.1162***	-113.8589***	-98.14937***
	[30.6391]	[31.5571]	[30.9690]	[30.7800]
Expenditures	-77.39879***	-85.51254***	-78.95157***	-62.75888**
	[29.0127]	[29.8865]	[29.1803]	[28.3316]
Deficit	-1.158182	-1.171143	-1.305735	-1.414985
	[1.0933]	[1.0916]	[1.1055]	[1.1316]

M is the number of neighbours. Fiscal Variables are in real euros per capita. Robust Standard errors using Abadie and Imbens (2011) method in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Logit Model. Dependent variable: Corruption over the period

	Coefficient	Standard Error
Population	0.0118***	0.0022
Population squared	0.000***	0.0000
Debt	0.0015	0.0016
Debt squared	0.0000	0.0000
Unemployment	-0.2379***	0.085
Unemployment squared	0.0167***	0.006
Density	-0.0001	0.0001
Density squared	0.0000	0.0000
% Between 15 and 65	-11.9455	16.6111
% Between 15 and 66 squared	-2.4847	24.4388
Leftwing vote share	1.8041	2.4416
Leftwing vote share squared	-1.5131	2.2110
Leftwing vote share lagged	-0.1914	1.1478
Rightwing vote share	-6.7798**	3.1738
Rightwing vote share squared	4.9335	3.2023
Rightwing vote share lagged	-0.6265	1.4138
PSOE share	-3.6679	2.6724
Psoe share squared	2.7585	2.8862
PSOE share lagged	0.5907	1.2544
PP share	10.1840***	3.2449
PP share squared	-6.9512**	3.4373
PP share lagged	-0.8152	1.5780
Constant	2.6879	2.8061
Pseudo R2	0.1427	
Observations	2726	

*** p<0.01, ** p<0.05, * p<0.1

Table 7. T-Test of differences in means between treatment and control groups. Parametric matching.

	Matched Sample			
	Mean Treatment	Mean Control	t-test	p-value
Control Variables				
Rightwing Vote Share	0.373	0.361	-0.825	0.410
Leftwing Vote Share	0.402	0.400	-0.136	0.892
PP share	0.3495	0.338	-0.7594	0.448
PSOE share	0.335	0.338	0.220	0.826
%Between 15 and 65	0.324	0.319	-1.264	0.207
Density	775.479	696.048	-0.541	0.589
Population	56432.720	39756.810	-1.114	0.266
Unemployment	5.025	4.910	-0.557	0.578
Outcome variables				
Revenues	1235.416	1202.290	-0.591	0.555
Expenditures	1252.014	1197.598	-1.032	0.302

	Whole Sample Pre-Matching			
	Mean Treatment	Mean Control	t-test	p-value
Control Variables				
Rightwing Vote Share	0.373	0.379	0.516	0.606
Leftwing Vote Share	0.402	0.434	2.418	0.016
PP share	0.349	0.297	-4.151	0.000
PSOE share	0.335	0.354	1.623	0.105
%Between 15 and 65	0.324	0.351	8.767	0.000
Density	775.479	372.877	-4.738	0.000
Population	56432.720	11332.160	-8.850	0.000
Unemployment	5.025	4.939	-0.559	0.576
Outcome variables				
Revenues	1235.416	1124.355	-2.927	0.004
Expenditures	1252.014	1125.942	-3.477	0.001

Notes: The means are calculated for the year of the matching, which is 2003. The treatment group are municipalities that suffer at least one corruption scandal between 2003 and 2010. The control group are municipalities with no corruption scandals during the period.

Table 8. Fixed Effects Differences-in-Differences Regressions.

	1	2	3	4	5	6
	Matched sample			Whole Sample		
VARIABLES	Revenues	Expenditures	Surplus	Revenues	Expenditures	Surplus
Corrupt	2.3373	2.447	-0.602	-22.6181	-14.0494	-0.3028
	[31.181]	[22.813]	[0.942]	[29.549]	[19.677]	[0.626]
After	8.0858	9.7526	-0.4885			
	[24.096]	[25.172]	[0.800]			
Corrupt*After	-98.7321***	-75.5702**	-0.2823	-106.2143***	-84.9539***	-1.3726
	[35.897]	[32.771]	[1.371]	[29.399]	[23.750]	[1.009]
Population (in thousands)	-1.427	-1.2262	-0.0164	-6.0004	-5.1201	0.0030***
	[1.846]	[1.493]	[0.028]	[3.785]	[3.170]	[0.001]
Unemployment	-17.8013**	-13.0590**	-0.2816	-11.7996***	-7.9423***	-0.0927***
	[7.003]	[5.055]	[0.189]	[2.541]	[2.009]	[0.032]
Density	-0.3404**	-0.2253	-0.0075*	-0.5345***	-0.4128***	0
	[0.161]	[0.142]	[0.004]	[0.127]	[0.100]	[0.000]
%Between 15 and 65	-2,099.8656*	-1,908.6021*	27.9213	-792.7210*	-886.0554**	-1.1293
	[1,183.597]	[971.646]	[34.391]	[427.044]	[394.467]	[2.343]
Rightwing vote share	94.9586	6.4702	6.0835	77.0067	73.5058	0.0647
	[274.086]	[199.935]	[7.075]	[95.736]	[87.492]	[1.092]
Leftwing vote share	772.7720*	613.8494	12.4874	75.6715	98.0355	1.333
	[425.100]	[384.904]	[8.806]	[111.678]	[94.402]	[0.937]
PP	-277.207	92.2058	-16.6599*	-107.795	-85.3855	-1.4946
	[334.638]	[248.252]	[9.091]	[128.726]	[111.229]	[1.278]
PSOE	-486.9071	-438.254	-10.2245	37.4006	-2.1221	-0.8369
	[465.980]	[423.535]	[9.504]	[128.316]	[102.917]	[1.008]
Debt	11.9982	-3.36	0.0453	-2.623	-7.5474**	0.0389
	[19.117]	[11.256]	[0.446]	[4.821]	[3.815]	[0.080]
Constant	2,165.6452***	2,003.4179***	-1.1791	1,742.0522***	1,712.6405***	-0.2154
	[367.400]	[318.216]	[11.465]	[155.754]	[142.617]	[1.061]
Regional effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Electoral term effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,270	4,270	4,270	22,142	22,142	22,142
R-squared	0.051	0.046	0.067	0.05	0.079	0.05

Fiscal variables are in real euros per capita. Robust standard errors clustered by municipality in brackets
*** p<0.01, ** p<0.05, * p<0.1

Table 9. Falsification Test. Non-Paratric Matching.

VARIABLES	M=1	M=2	M=3	M=4
Revenue	-30.8346	5.5039	17.2944	23.2909
	[42.6633]	[40.0372]	[38.2052]	[37.4616]
Expenditures	-31.0094	7.9623	14.8437	17.2077
	[39.4631]	[37.1747]	[34.8598]	[33.9817]
Deficit	0.7714	.7260	0.3701	0.5795
	[1.2086]	[1.1665]	[1.1235]	[1.1164]

Fiscal variables are in real euros per capita. Robust standard errors using Abadie and Imbens (2011) method in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table 10. Falsification test. Fixed Effects Differences-in-Differences Regressions.

	1	2	3	4	5	6
	Matched sample			Whole Sample		
VARIABLES	Revenues	Expenditures	Surplus	Revenues	Expenditures	Surplus
Corrupt	-26.8041	20.6825	-0.5368	28.1524	49.6066**	0.0192
	[50.572]	[34.824]	[1.306]	[26.671]	[19.926]	[0.493]
After	16.4976	16.6979	0.6907			
	[31.229]	[25.567]	[0.903]			
Corrupt*After	21.9721	-13.9071	0.0112	-3.6011	-19.2759	0.3205
	[51.543]	[41.267]	[1.554]	[34.784]	[27.700]	[0.950]
Population (in thousands)	-3.0301	-2.8885	-0.0454	-13.4794**	-11.6614***	0.0047***
	[3.343]	[2.717]	[0.078]	[5.239]	[4.336]	[0.001]
Unemployment	-8.2943	-6.4544	-0.2719	-8.9781***	-5.3798**	-0.0862***
	[9.042]	[6.458]	[0.236]	[2.650]	[2.094]	[0.033]
Density	-0.8549**	-0.5563*	-0.0114**	-0.5109***	-0.3757***	0
	[0.403]	[0.296]	[0.005]	[0.141]	[0.108]	[0.000]
%Between 15 and 65	844.3474	-170.1408	56.6047	-514.9705	-662.4660*	-1.419
	[1,652.136]	[1,174.689]	[40.398]	[437.956]	[396.232]	[2.549]
Rightwing vote share	-419.904	-298.9398	-0.1774	91.6729	84.4467	0.3208
	[399.296]	[401.565]	[8.062]	[95.843]	[88.094]	[1.086]
Leftwing vote share	123.9956	188.9425	-11.6961	66.2239	106.5501	1.0099
	[467.590]	[461.500]	[9.271]	[116.160]	[97.462]	[0.951]
PP share	486.7757	595.8938	-16.0235	-77.5588	-89.823	-1.6291
	[515.391]	[451.463]	[11.015]	[132.105]	[113.362]	[1.273]
PSOE share	-17.7888	-110.2654	11.0534	30.719	-11.7913	-0.5457
	[492.289]	[472.414]	[9.959]	[132.803]	[105.602]	[1.017]
Debt	11.6473	-6.5166	1.2517**	-4.0152	-8.8215**	0.0414
	[16.610]	[11.019]	[0.524]	[4.748]	[3.943]	[0.080]
Constant	1,666.5668***	1,735.2833***	-7.1494	1,685.6703***	1,666.6336***	-0.0731
	[408.302]	[317.786]	[12.428]	[160.597]	[144.413]	[1.125]
Regional effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
Electoral term effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,243	3,243	3,243	21,134	21,134	21,134
R-squared	0.047	0.052	0.068	0.051	0.083	0.044

Fiscal variables are in real euros per capita. Robust standard errors clustered by municipality in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 11. Revenue categories. Average Treatment Effect. Non-Parametric Matching.

	M=1	M=2	M=3	M=4
Direct Taxes	7.6866	7.0805	11.7141	16.8575*
	[11.2930]	[10.6666]	[10.1962]	9.9688
Indirect Taxes	-6.7829	-8.7161	-8.0197	-8.2113
	[5.8521]	[5.6276]	[5.7143]	[5.8343]
Licenses and Fees	-30.3444**	-33.3862**	-33.0820**	-24.9616*
	[13.9092]	[14.3976]	[14.0386]	[13.7770]
Current Transfers	-24.2901***	-30.1304***	-32.3697***	-29.5502***
	[6.4465]	[6.0694]	[5.9351]	[5.9012]
Property income	-3.0313	-3.1275	-2.3663	-0.8451
	[5.0509]	[5.0148]	[4.8834]	[4.9186]
Revenues from Selling of Real Estate	-27.2001***	-27.9002***	-27.4749***	-28.8450***
	[10.0082]	[10.4088]	[10.8174]	[10.9909]
Capital Transfers	-25.5441***	-24.6367***	-25.8577***	-27.1202***
	[9.7310]	[9.3572]	[9.5214]	[9.2711]

M is the number of neighbours used in the matching. Fiscal Variables are in real euros per capita. Robust Standard errors using Abadie and Imbens (2011) method in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table 12. Expenditure categories. Average Treatment Effect. Non-Parametric Matching.

	M=1	M=2	M=3	M=4
Wages	15.1653	14.0830	15.8914*	20.6189**
	[9.3045]	[9.0095]	[8.4629]	[8.1573]
Goods and Services	-9.8454	-11.4988	-7.9511	-4.4239
	[8.7253]	[8.8772]	[8.7723]	[8.7484]
Financial Expenses	2.5467*	2.6373*	2.7726**	3.0508**
	[1.3035]	[1.4024]	[1.3244]	[1.2944]
Current Transfers	-10.7935**	-12.0449**	-12.6065***	-8.4436*
	[5.1047]	[4.8274]	[4.7422]	[4.6416]
Investment	-61.1416***	-66.8864***	-65.3574***	-61.7622***
	[16.1152]	[16.5540]	[16.5712]	[16.3716]
Capital Transfers	-1.0147	-0.2921	-0.3816	0.2942
	[2.9635]	[2.9165]	2.9468	[3.0737]

M is the number of neighbours used in the matching. Fiscal Variables are in real euros per capita. Robust Standard errors using Abadie and Imbens (2011) method in brackets.
*** p<0.01, ** p<0.05, * p<0.1