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The effects of revealing the prosecution of political corruption on local finances

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Abstract

This paper analyzes the financial implications on local public budgets of disseminating information about the prosecution of political corruption at the local level. We build a database from a wave of corruption scandals in Spain to use a quasi-experimental design and find that after corruption is revealed, both local public revenues and expenditures decrease significantly (approximately by 7 and 5%, respectively) in corruption-ridden municipalities. The effect lasts for a period of time equivalent to a full electoral term and comes mostly from other economic agents' unwillingness to fund or start new projects in municipalities where the prosecution of corruption has been revealed. These results imply that if one of the consequences of corruption is the inefficient allocation of funds to areas where corrupt politicians can extract more rents, the revelation of the corruption scandal frees up resources that can be used to fund activities with a higher social return.

Keywords Revealed corruption · Public expenditures · Public revenues

JEL Classification D72 · D78

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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 democracies 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; Hopland 2014).

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 cases. 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 rely 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. 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 toward corrupt acts. For example, corrupt politicians may exhibit different ability levels, or biases toward deficits or surpluses, or biases toward 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 tackle 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.

Our identification strategy combines a propensity matching estimator with a staggered difference in differences analysis (Sant'Anna and Zhao 2020; Callway and Sant'Anna 2020; Goodman-Bacon 2021). 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, in 2003. Then, to eliminate further biases and to control for unobserved differences, time effects, and the possibility of treatment effects being heterogeneous, we estimate the doubly robust differences-in-differences estimator developed by Sant'Anna and Zhao (2020) on the matched sub-sample. We also perform a variety of tests to confirm the robustness of our results to several potential threats.

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, revenues and expenditures decrease by approximately 7 and 5 percent, respectively. 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 from license fees. 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.

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). Some papers have also found that corruption has negative effects on the number of business establishments (Bologna and Ross 2015).

Most of this literature has used cross-country data and perceived corruption as a measure of corruption. However, research such as Liu and Mikesell (2014) using state level data from within the USA 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%.[1] Hopland (2014) reaches a similar conclusion for the case of Norway.

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 2018). 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 in 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 (2012) shows that voters punish corrupt politicians more when the revelation of corrupt behavior results in lower resources to

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

the municipality due to the punishment of the central government authorities in terms of intergovernmental transfers.

In this paper, we contribute to the literature by focusing on the non-electoral effects of the dissemination of information about corruption scandals. Similar to voters taking their vote away from corrupt incumbents, other economic agents such as firms or other levels of government will be more reluctant to deal with corruption-ridden municipalities once the case is out on the news. The main hypothesis tested in the paper is that this will likely have a negative impact on local finances. Only Brollo (2012), Timmons and Garfias (2015) for the case of Brazil, and Beekman et al. (2014) for the case of Liberia have estimated related—albeit different—hypotheses.

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 suggests (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 politicians in 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 (2012) 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 bring higher awareness to the public and make 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 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, which is consistent with the findings of Beekman et al. (2014) for the case of Liberia that corruption reduces incentives for voluntary contributions to local public goods and may reduce private investments of individuals.

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 a 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 prosecution of corruption on overall and on each category of revenues and expenditures separately, we are able to shed light on: (1) how information about corruption affects economic and political agents, (2) how it affects the allocation of public goods at the local level, (3) how the revelation of corruption may cause short run welfare losses in municipalities affected by scandals, and (4) how these effects may mediate the electoral response observed at the polls.

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.

3.1 Electoral system

Spanish local elections take place (usually) 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 local levels. Over our period of study, two main parties dominated each side of the left–right scale. The Popular Party (*PP*) has traditionally been the main rightwing party while the socialist party (PSOE) is the main leftwing party. The Popular Party has not faced much competition on the right until recently (but not over our period of study), while the socialist party faces competition from the left (from the United Left over our period of study and more recently from Podemos). In addition, in several regions and particularly in Catalonia and Basque Country there are several nationalist parties that receive large support in those areas.

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 election immediately after of 2011. According to this table,

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

Calculated from electoral data from the Spanish Ministerio de Interior

the combined support to both PSOE and PP remained fairly constant at around 70% during our period of study.[2]

3.2 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.[3] 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.[4] They can also obtain additional funds through transfers from other levels of 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

[2] This has changed over the last few elections (since 2015) with the emergence of newly created parties such as *Podemos* or *Ciudadanos*.

[3] 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.*

[4] 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*).

Table 2 Summary of revenue and expenditure categories

Revenues			Expenditures		
Categories	Euros per capita	%	Categories	Euros per capita	%
Direct taxes	275.29	22.14	Wages	347.77	28.85
Indirect taxes	59.13	4.76	Goods and services	345.91	28.70
Licenses and fees	208.65	16.78	Financial expenses	12.55	1.04
Current transfers	343.49	27.63	Current transfers	64.85	5.38
Property income	29.11	2.34	Investment	375.95	31.19
Revenues from Selling of real estate	44.32	3.57	Capital transfers	12.66	1.05
Capital transfers	215.95	17.37	Assets	2.68	0.22
Assets	2.15	0.17	Liabilities	43.00	3.57
Liabilities	65.06	5.23			
Total revenues per capita	1243.15	100	Total Expenditures per capita	1205.37	100

All variables are defined in real euros per capita. The entries are calculated using the mean of each variable in the estimating sample over the whole period of analysis (2003–2010). $N = 21{,}556$ observations

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. On the revenue side, the larger source of revenues is current transfers from other levels of government (27.63%), tax revenues (indirect and direct taxes add up to 26.9%), and revenues from capital transfers (17.37%). On the expenditure side, current spending (Wages + Goods and Services + Financial Expenses + Current Transfers) represents 65.24%, while investment and other capital spending amounts 34.76%.

3.3 Local corruption in Spain

Political corruption exploded in Spain between 2000 and 2010. Several research papers and databases have reported a significant increase in the number of corruption cases at the local level in Spain in this period (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,* find 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. At the period studied, in the 2012 CIS Barometer corruption appeared as the fourth most relevant problem (and it continues in 2020). 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 have 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.[5] 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 scandal involving the city of Alicante, the mayor of the city was charged with 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.[6]

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. We now explain how we do so.

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

[6] A brief description of several other corruption cases at this period is found in Appendix tables reported in Fundación Alternativas (2007).

4.1 Parametric staggered matched difference in differences

A reasonable starting point would be to estimate a fixed effects difference in differences regression of the type:

$$Y_{it} = \alpha_0 + \alpha_1 \text{Corrupt}_{\text{iterm}} + \alpha_2 \text{Corrupt}_{\text{iterm}} * \text{After}_{\text{it}} + \beta X + \gamma_t + \lambda_i + \varepsilon_{it} \quad (1)$$

The dependent variable in this model, Y_{it}, is the fiscal outcome of interest (revenues, expenditures or deficit) of municipality i in year t. The coefficient of interest is α_2, which captures the causal effect of revealing corruption in an average corruption-ridden municipality on the outcome variable. The coefficient α_2 identifies a causal effect because we control for unobserved differences between treatment and controls and for common shocks through the variables *Corrupt* and the vector of year and municipality fixed effects (γ_t and λ_i). The variable *Corrupt* takes value 1 for municipalities that are corruption-ridden in a given electoral term and 0 otherwise.

The vector of controls, X, includes a combination of political and socio-demographic variables such us unemployment, size, population density, debt, percentage of population between 15 and 65, and several controls for the overall ideology distribution of the municipality. 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 each electoral term and by the vote share of the two main parties (PSOE and PP), which captures the peculiarities of party competition in the municipality.[7] Finally, the additional socioeconomic controls are the population size of the municipality,[8] 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.

The model of Eq. (1) relies on the assumption that treatment and control observations follow parallel trends before the break out of the corruption scandal. A standard way to provide evidence consistent with the fulfillment of this assumption is to show in a graph the evolution of the outcome variables before, during, and after the scandal for both municipalities affected by corruption scandal and those not affected by corruption scandals. In our case, such analysis is complicated because the before and after periods are different for each corrupt municipality and there are no after periods for non-corrupt municipalities. However, one can center the time of corruption variable for each treatment observation (so that it takes value 0 on the year in which corruption is revealed, − 1 the year before, + 1 the year after, and so on) and compare the pre-trends outcomes with those of the sample of non-corrupt municipalities for which the "after" periods have been randomly assigned to match the distribution of

[7] In some regions, and particularly in the Basque Country and Catalonia, the two main parties at the national level are not the main parties. Sørensen (2014) shows that lack of party competition reduces public efficiency.

[8] Bergh et al. (2017) found that Swedish municipalities with more local council seats have more reported corruption problems. The number of councils by municipalities in Spain is related to local population. So this variable controls this potential effect.

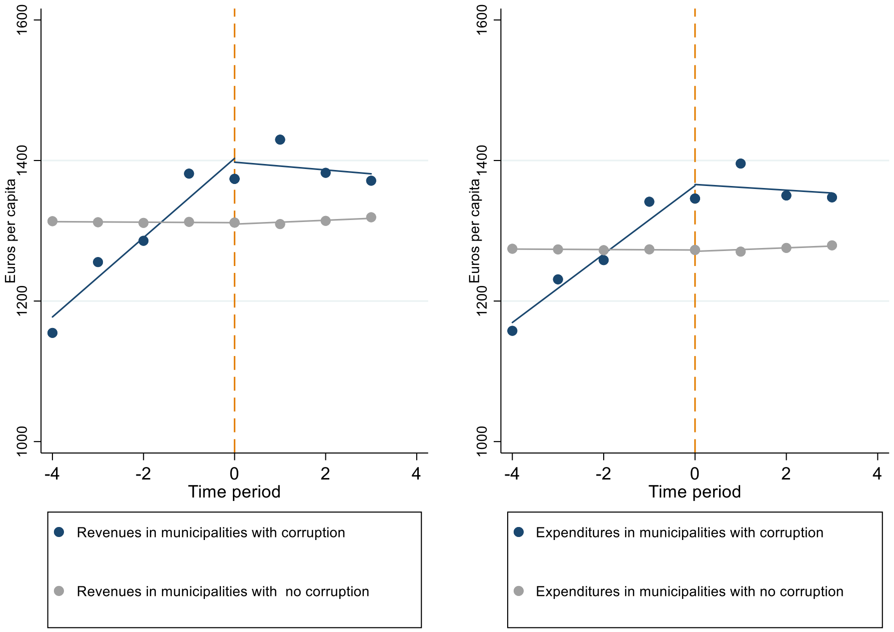

Fig. 1 Evolution of public finances in corrupt municipalities vs non-corrupt municipalities Note: The horizontal axis represents years around the break out of the corruption scandal which is labeled as year 0. The vertical axis plots revenues and expenditures per capita. We show in dots actual values obtained from the residuals of a regression of the public finance variable on time dummies and in a line the linear fit to the scatterplot

after years in the treatment sample.[9] That is, we can compare the average treatment observation on each pre- and post-period with the average control observation.

Such comparison is shown inFig. 1.[10] One can see a clear upward trend in both revenues and expenditures for treatment observations during the pre-scandal period and a decrease right afterwards. For the control group, Fig. 1 shows no drastic change in the randomly assigned pre-scandal and post-scandal periods. Figure 1, therefore, provides preliminary evidence consistent with a causal effect of the revelation of corruption. However, Fig. 1 does not support the parallel trends assumption as both treatment and control observations seem to follow a different pattern pre-scandal, at least if observable differences between them are not controlled for.

Additionally, even if observables are accounted for, the fulfillment of the standard parallel trend assumption it is not enough to identify a causal effect through two-way fixed effects difference in differences model when the treatment is heterogeneous and is staggered across different periods for different treatment units. As recent literature

[9] We matched the distribution of treatments and controls by looking at the percentage of treatment observations in each pre and post scandal year and then assigning the same percentage of control observations to each of those periods.

[10] Figures 1 and 2 plot the residuals of a regression of revenues and expenditures on time dummies. We show results controlling for time shocks to account for the fact that the scandal year is different for each municipality.

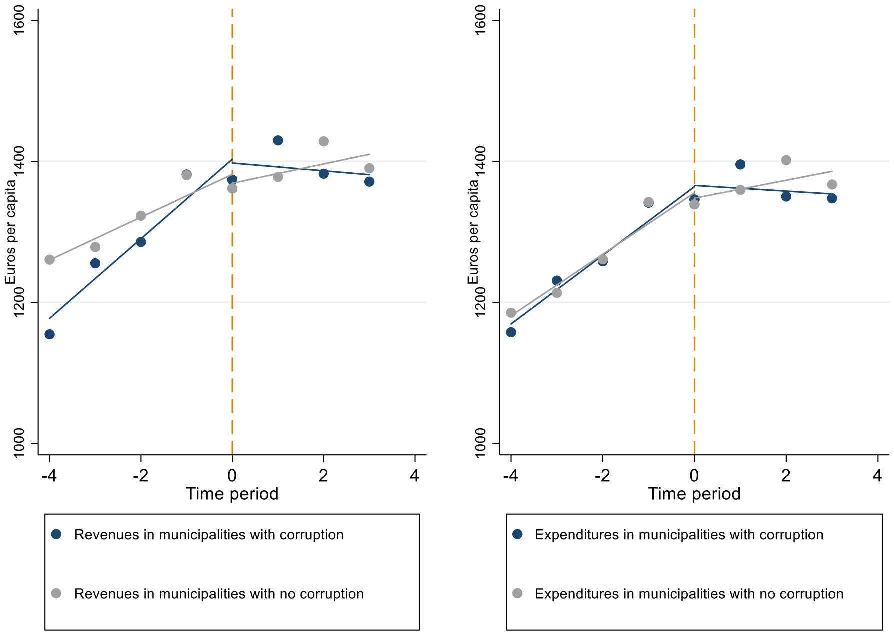

Fig. 2 Evolution of public finances in corrupt municipalities vs matched sample Note: The horizontal axis represents years around the break out of the corruption scandal. which is labeled as year 0. The vertical axis plots revenues and expenditures per capita. We show in dots actual values obtained from the residuals of a regression of the public finance variable on time dummies and in a line the linear fit to the scatterplot

has shown (see Roth et al. 2022, for a review), this would be because in a model such as the one in Eq. (1) units that are treated later in time serve as part of the control group for units that are treated earlier. If the treatment is heterogeneous across time (e.g., if later treated units are punished more for being corrupt than earlier treated units), post-treatment trends would be different for both groups, making the comparison inappropriate. This problem is solved in recently developed DiD estimators such as Callaway and Sant'Anna (2020), Sant'Anna and Zhao (2020), Goodman-Bacon (2021), and Athey and Imbens (2022). The basic intuition of these methods is to use as the control group units that are not subject to the potential heterogeneity of the treatment problem, such as never-treated units. Those papers also provide appropriate tests for the parallel trends assumptions for staggered panel data models like ours. In this paper, we use the Sant'Anna and Zhao (2020) doubly roubust DiD estimator now easily implemented in Stata through the command *crdid* developed by Rios-Avila et al. (2021).

In our benchmark estimation, we implement the Sant'Anna and Zhao (2020) estimator using all available municipalities in our database. Given heterogeneity across municipalities, as an additional robustness test, we will pre-process our data using a matching model to select from the group of potential controls those that at the beginning of the period were more similar to corruption-ridden municipalities in terms of observable characteristics.

In the matching models, we match units at the beginning of the period, in 2003 using the cross-section of municipalities for that year. 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. For the matching, we use 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 variables that are related to both the treatment and the outcome. The previous literature for the case of Spain (Solé-Ollé and Sorribas-Navarro 2018) finds that corruption is correlated with both socio-economic and political variables. Therefore, our covariates include measures of economic activity at the local level such as unemployment and previous debt, political controls such as ideology, and other socio-demographic covariates. We also add lagged public finance outcomes to our matching to facilitate the fulfillment of the parallel trends assumption.

Unemployment is defined as the percentage of people registered as unemployed in the municipality's unemployment office. Monetary variables such as debt, revenues, or expenditures 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.[11]

Finally, the additional socioeconomic controls are the population size of the municipality,[12] 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. We also include a one year lag of the variables that capture ideology so that we incorporate the political structure of the previous electoral period.

After the estimation of the logit model and the propensity scores, we find the counterfactual of each treatment observation using nearest-neighbor matching. Figure 2 shows the pre- and post-scandal outcomes for the treatment and matched control sample. We can see now that both groups follow a much more similar pattern in the pre-scandal period compared to Fig. 1. In the pre-scandal period, both groups follow an upward trend. We can also see that in the post-scandal period there seems to be a clear drop in both revenues and expenditures for the treatment group. We take this graph as initial evidence consistent with the fulfillment of the identification assumptions. We will revisit the parallel trend assumption more formally in a later section.

[11] In some regions, and particularly in the Basque Country and Catalonia, the two main parties at the national level are not the main parties. Sørensen (2014) shows that lack of party competition reduces public efficiency.

[12] Bergh et al. (2017) found that Swedish municipalities with more local council seats have more reported corruption problems. The number of councils by municipalities in Spain is related to local population. So this variable controls this potential effect.

5 Data and measurement issues

5.1 Data

In order to develop our empirical strategy, we use a database of Spanish municipalities from 2003 to 2010. This period includes two complete electoral terms (2003–2006 and 2007–2010). We consider 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 Públicas*. 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 1000 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.[13] 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

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

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 the judiciary, and for which we could reliably identify the date in which the formal accusation took place.[14]

5.2 Measurement issues

After merging and cleaning the data of missing and implausible values, our final sample is a panel containing 21,556 observations corresponding to 3053 municipalities observed during the period 2003–2010.[15] 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.

The variable that measures corruption in Table 3 is *Corruption over the period*. It is defined as 1 for observations that correspond to a municipality that faced a corruption scandal at some point during the period of analysis (2003–2010). It is the variable used to calculate the propensity score in the parametric matching. According to this variable, 9.6% of the observations correspond to municipalities that suffered a corruption scandal during the period.[16]

The variable *After* has a value of 0.3362, which implies that approximately 33% of the observations corresponding to corrupt municipalities are from years after the

[14] 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-Vázquez et al. 2015; 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 (2018). 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 are 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 (2000–2008), 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.

[15] 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. We observe 2642 municipalities for which we have information since 2003.

[16] In some of the alternative robustness tests in which we estimate the standard DiD model of Eq. (1) instead of the staggered DiD, we used as an alternative definition of corruption the variable *Corrupt in the electoral term,* which accounts for whether corruption occurred in the first or in the second electoral term. This variable is 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. 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." In our benchmark models, we 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.

Table 3 Summary Statistics

	Whole sample	Municipalities with corruption scandals	Municipalities with no corruption scandals
Revenues per capita	1243.15	1272.248	1240.046
	[651.5624]	[620.1172]	[654.7669]
Expenditure per capita	1205.372	1224.932	1203.285
	[560.8871]	[516.9258]	[565.3478]
Corruption over the period	0.04903	0.508	0.000
	[0.2159]	[0.50]	[0.000]
Corrupt	0.0964	1	0.000
	[0.2951]	[0]	[0.000]
After	0.3362	0.3421	0.3289
	[0.4725]	[0.4745]	[0.4699]
Population	14.607	57.9135	9.986
	[77.928]	[232.259]	[27.327]
Unemployment	8.5538	9.1098	8.494
	[4.3689]	[4.7517]	[4.322]
Population density	424.2056	835.5115	380.3257
	[1377.314]	[1981.624]	[1288.665]
% People between 15 and	0.344	0.3210	0.3465
	[0.0454]	[0.0406]	[0.0452]
Rightwing vote share	0.3731	0.3747	0.3729
	[0.17843]	[0.1697]	[0.1792]
Leftwing vote share	0.4374	0.4098	0.4403
	[0.2029]	[0.1889]	[0.2041]
PP	0.3029	0.3518	0.2977
	[0.1983]	[0.1772]	[0.1998]
PSOE	0.3582	0.3432	0.3597
	[0.1759]	[0.1587]	[0.1776]
Total debt	55.507	65.0063	54.489
	[92.887]	[83.3693]	[93.795]
Observations	21,556	2078	19478

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 brackets

corruption is revealed. It is arguable whether we should define the after years including also the year in which corruption occurs or only the years strictly after that. We prefer to include the year in which the scandal occurs as part of the "after" period as many effects may already occur during that year, particularly if the scandal is revealed at the beginning of the year.[17]

[17] The results are similar when we define the after years as years strictly after the year in which the scandal took place.

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

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

6.1 Matching and benchmark models

We show first the results of the matching and then the estimates of the treatment effects from the straggered differences-in-differences model both for the whole database and for the matched sub-sample.

6.2 Matching model

Table 4 shows the results of the logit model used to calculate the propensity scores. This model estimates the probability of being corrupt at a later period using information available 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, lagged revenues and expenditures, 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).

The model of Table 4 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 5. 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 analysis.

This table shows that when we look at the whole sample (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 generally balanced across treatment and control groups, with only *Population* being significantly different across treatment and control groups. *Population* is different across treatment and controls due to the fact that both Madrid and Barcelona which are significantly

Table 4 Logit Model.

	Coefficient	Standard error
Revenues lagged	0.0011**	[0.001]
Expenditures lagged	− 0.0004	[0.001]
Revenues lagged squared	− 0.0000*	[0.000]
Expenditures lagged squared	0.0000	[0.000]
Population	0.0000***	[0.000]
Population squared	− 0.0000***	[0.000]
Debt	0.0004	[0.002]
Debt squared	0.0000	[0.000]
Unemployment	− 0.1948**	[0.087]
Unemployment squared	0.0148**	[0.006]
Density	0.0000	[0.000]
Density squared	0.0000	[0.000]
% Between 15 and 65	− 5.6198	[16.700]
% Between 15 and 66 squared	− 8.6792	[24.399]
Leftwing vote share	1.1173	[2.498]
Leftwing vote share squared	− 1.0466	[2.262]
Leftwing vote share lagged	− 0.0485	[1.170]
Rightwing vote share	− 6.1885*	[3.169]
Rightwing vote share squared	4.2211	[3.226]
Rightwing vote share lagged	− 0.7872	[1.421]
PSOE share	− 2.8007	[2.742]
Psoe share squared	2.0197	[2.944]
PSOE share lagged	0.437	[1.282]
PP share	9.8606***	[3.240]
PP share squared	− 6.4930*	[3.454]
PP share lagged	− 0.6203	[1.589]
Constant	0.4406	[2.922]
Pseudo R2		0.1442
Observations		2726

Dependent variable: Corruption over the period
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

more populous than any other city in Spain are part of the treatment group. When we exclude them from the sample, *Population* is also balanced across treatment and controls. As the results are the same overall regardless of whether these two cities are included or not, we decided to keep them.

Overall this implies that our matching model identifies a sample of controls that were alike in terms of socio-economic situation and were ideologically similar at the beginning of the period of analysis, in 2003.

Table 5 T-Test of differences in means between treatment and control groups at the beguining of the period. Parametric matching

Matched sample

	Mean treatment	Mean control	*t*-test	*p*-value
Control variables				
Rightwing Vote Share	0.37	0.35	– 1.0786	0.2813
Leftwing Vote Share	0.408	0.415	0.4139	0.6791
PP share	0.35	0.32	– 1.5184	0.1296
PSOE share	0.34	0.33	– 0.1080	0.9141
%Between 15 and 65	0.324	0.325	0.3624	0.7172
Density	793.47	713.98	– 0.479	0.6319
Population	56,651.57	29,845	– 1.6504	0.0995
Unemployment	4.94	5.15	0.9234	0.3563
Debt	78.03	77.44	– 0.0577	0.9540
Outcome variables				
Revenues	1201.11	1212.373	0.0405	0.9677
Expenditures	1230.049	1198.735	– 0.5809	0.5616
Number of observations	255	216		

Whole sample pre-matching

	Mean treatment	Mean control	*t*-test	*p*-value
Control variables				
Rightwing Vote Share	0.37	0.3787	0.6559	0.5120
Leftwing Vote Share	0.408	0.4364	2.0907	0.0366
PP share	0.35	0.2955	– 4.1611	0.000
PSOE share	0.34	0.355	1.2417	0.2144
%Between 15 and 65	0.324	0.3524	8.9352	0.000
Density	793.47	360.95	– 4.9481	0.000
Population	56,651.57	9554.1	– 9.444	0.000
Unemployment	4.94	4.94	– 0.0602	0.9520
Debt	78.03	57.41	– 4.1218	0.000
Outcome variables				
Revenues	1201.11	1120.29	– 2.3498	0.0189
Expenditures	1230.049	1121.088	– 2.9762	0.0029
Number of observations	255	2.387		

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 6 Doubly Robust Differences in Differences estimates of Average Treatment Effects

ATT	Revenues	Expenditures	Surplus
Whole sample	− 84.90**	− 62.74***	− 0.65
	[35.59]	[23.98]	[1.64]
Matched sample	− 82.86**	− 69.40***	0.03
	[39.44]	[27.02]	[0.987]

This table shows the results of the implementation of the Sant'Anna and Zhao (2020) doubly robust Differences in Differences estimator using Stata command *crdid*

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

6.3 Staggered differences-in-differences model

Table 6 presents the results of the Sant'Anna and Zhao (2020) difference in differences doubly robust model. 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]. Each row is estimated using a different sample. In the first row, we use the whole sample, while in the second one we used the matched sub-sample.

The table shows the estimation of the average treatment effect on the treated (ATT), which measures the causal effect of the revelation of the scandal in the municipality. For the whole sample, the coefficient for revenues has a value of − 84.90 and is significant at the 5%. This number implies that after a corruption scandal is revealed, revenues decrease in the municipality by 84.90 euros per capita, which is approximately a 6.8% decrease compared to what would have occurred had corruption not been revealed.[18] The coefficient is very similar and also statistically significant when the matched sub-sample is used. Column 2 shows that expenditures also decrease significantly as a consequence of the revelation of corruption. The decrease in expenditures is smaller (62.74 euros per capita for the whole sample and 69.4 euros for the matched sub-sample) and amounts between 5.2% in the whole sample and 5.7% in the matched subsample.[19] The combination of the decrease in both revenues and expenditures produces a negative effect on surplus, although it is not statistically significant.

Overall, Table 6 shows 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 may have a negative effect on surpluses, although this effect is not statistically robust, so we cannot discard the possibility that the combined effects on revenues and expenditures are neutral to surpluses.

[18] We calculate the decrease compared to the sample mean from table 3 (84.90/1243.15*100 = 6.8%).

[19] This percentage is obtained comparing the decrease to the simple mean from Table 3: 62.74/1205.7 *100 = 5.2%

6.4 Dynamics and parallel trends

Figure 2 shows initial evidence of the fulfillment of the parallel trend assumption and of a different behavior post-scandal, which is confirmed in Table 6. In Table 7, we show the estimates of an event study estimation in which we estimate the ATT in each period pre- and post-treatment separately following Sant'Anna and Zhao (2020). This

Table 7 Dynamic effects around corruption event dates

ATT	Revenues (whole sample)	Expenditures (whole sample)	Revenues (matched sample)	Expenditures (matched sample)
Pre-treatment year – 7	20.25	7.32	67.87	60.97
	[45.68]	[33.34]	[53.32]	[56.99]
Pre-treatment year – 6	97.36	– 14.46	173.92	16.08
	[86.96]	[29.31]	[119.10]	[39.33]
Pre-treatment year – 5	33.40	83.21*	17.91	109.05*
	[70.69]	[43.85]	[87.68]	[56.02]
Pre-treatment year – 4	– 45.39	– 0.45	– 6.46	11.06
	[54.18]	[36.70]	[63.27]	[44.55]
Pre-treatment year – 3	25.38	9.44	– 59.30	– 44.45
	[67.16]	[55.62]	[54.59]	[31.45]
Pre-treatment year – 2	– 14.82	– 73.64	10.09	– 49.45
	[49.30]	[50.56]	[38.98]	[31.78]
Pre-treatment year – 1	– 21.39	0.68	– 17.72	– 3.12
	[31.58]	[20.92]	[37.27]	[23.25]
Pre-treatment year 0	– 71.91**	– 36.51	– 66.61**	– 41.63*
	[31.00]	[23.53]	[32.72]	[25.91]
Pre-treatment year - 1	– 61.39	– 44.21	– 72.38	– 64.78**
	[51.62]	[28.17]	[55.62]	[32.37]
Pre-treatment year - + 2	– 166.89	– 140.46***	– 160.04***	– 147.65***
	[52.72]	[50.72]	[58.32]	[53.44]
Pre-treatment year + 3	– 119.93	– 80.55	– 122.51	– 86.11
	[88.4]	[53.99]	[94.29]	[57.03]
Pre-treatment year + 4	– 99.75	– 149.77**	– 83.22	– 115.68*
	[85.15]	[63.48]	[90.81]	[70.01]
Pre-treatment year + 5	163.68	13.33	225.79*	51.27
	[106.28]	[52.72]	[117.72]	[62.42]
Pre-treatment year + 6	29.93	71.47	26.45	80.73
	[64.25]	[55.16]	[117.84]	[90.14]

This table shows the results of the implementation of the Sant'Anna and Zhao (2020) doubly robust Differences in Differences estimator using Stata command *crdid*

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

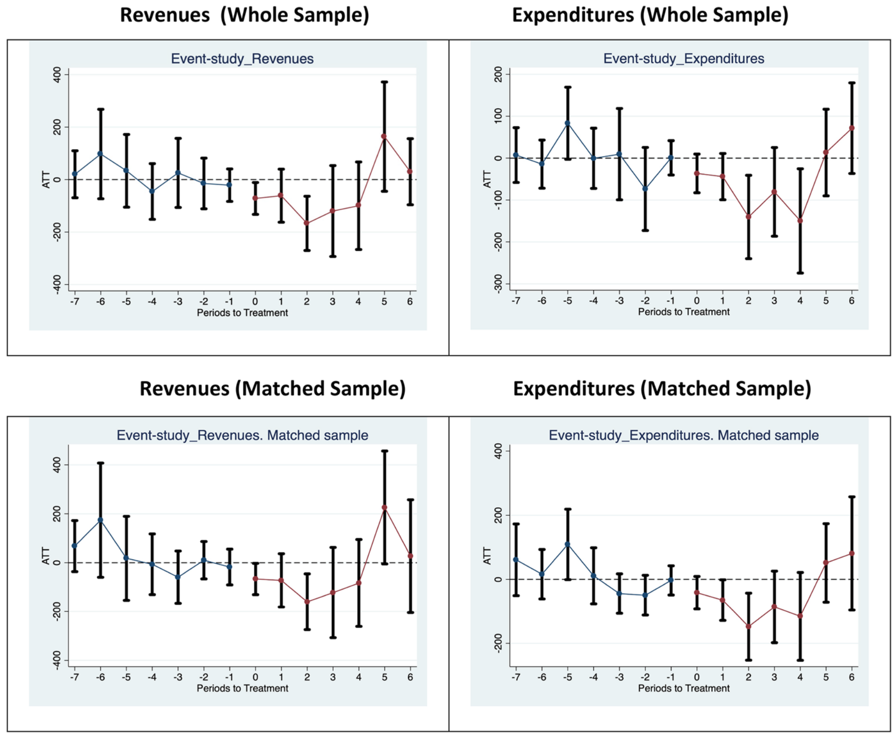

Fig. 3 Parallel trends and dynamic effects. Note: This Figure plots the ATT coefficients of pre- and post-corruption years obtained from the regression of Table 7. We build the graph using the Stata command *crdid* event option. Confidence intervals are calculated at the 95%

is a more formal test of whether trends are parallel in the pre-treatment period, and also allows us to study the dynamics of the effects of the revelation of the corruption scandal on fiscal outcomes.

For ease of interpretation, we have plotted the results of Table 7 in the graphs shown in Fig. 3. In the case of revenues (left panel), we can see that all the coefficients of the the pre-treatment years are non-significant, implying that in the pre-treatment period there are no statistically significant differences between treatment and controls. On the year in which the scandal is revealed, we see a negative effect. The effect persists during four more years and is particularly high on year 1 after the revelation. The analysis on the matched sub-sample leads to the same conclusions.

In the case of spending, we see a similar pattern. Note that while some coefficients in the years immediately after the treatment are only marginally significant or marginally insignificant, there is a clear pattern of expenditure decrease compared to the pre-treatment period. Additionally, note also that although not statistically significant, there are some pre-treatment periods in which the magnitude of the coefficient is sufficiently large to imply that we cannot rule out that there might be some anticipation effects in our sample (these may occur if the public or the political actors know about the judicial investigations before the scandal breaks out).

Table 8 Falsification test

ATT	Revenues	Expenditures	Surplus
Whole sample	11.90	− 32.89	3.70**
	[41.92]	[31.03]	[1.45]
Matched sample	19.66	− 19.62	3.47**
	[42.86]	[31.94]	[1.46]

This table shows the results of the implementation of the Sant'Anna and Zhao (2020) doubly robust Differences in Differences estimator using Stata command *crdid*

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

Overall these results show that: (1) the trends before the scandal is revealed are similar in treatment and control groups, (2) the effects are higher during the first two or three years after the scandal, and (3) the revelation of the scandal has effects that seem to last for a period equivalent to a whole electoral term.

6.5 Falsification tests

We now discuss a falsification exercise that provides support to the causal interpretation of our results. The results of the exercise are presented in Table 8. In this table, we estimate the same models as in Table 6 but using fake dates for the revelation of corruption variable. To construct the fake revelation of corruption dates, we subtract 3 years from the real revelation date.[20] 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 addition, we exclude from the estimation the after years for corrupt municipalities, so that we know that corruption has not been revealed yet in any of the observations in this falsified database. Therefore, there should be no differences between the treatment and control group if the model is correctly specified. This test allows us to rule out that unobserved heterogeneity between treatment and controls is driving the results (e.g., corruption might be more likely in urban denser areas, which may also differ on other unobserved factors from non-corrupt municipalities).

Table 8 shows that our matched difference-in-differences model finds no effect of the fake treatments on neither revenues or expenditures. As one would expect, the estimates of the "placebo" treatments are smaller in magnitude for both revenues and expenditures and are non-significant across the falsification tests. Note, however, that the placebo estimates, while insignificant and smaller in magnitude, are not close to zero, ranging between − 25 and + 50% of the main estimates and being of different sign for revenues and expenditures. This makes the pre-treatment surplus significant, which could be an indication of some pre-treatment divergence or of anticipation effects.

[20] We also estimated the model using 4 years and a random number between 2 and 5 years, and results are similar.

Table 9 Revenue and Expenditure categories. Average Treatment Effect on Treated

Revenue categories used as dependent variable	ATT	Expenditure categories used as dependent variable	ATT
Direct taxes	− 7.31	Wages	− 7.98
	[7.17]		[5.66]
Indirect taxes	− 2.35	Goods and services	− 7.96
	[8.15]		[6.10]
Licenses and fees	− 33.73	Financial expenses	1.57
	[23.33]		[0.98]
Current transfers	− 5.44	Current transfers	− 9.61*
	[5.42]		[5.22]
Property income	− 11.06**	Investment	− 37.35**
	[5.59]		[17.68]
Revenues from selling of real estate	− 6.78	Capital transfers	− 3.34
	[11.13]		[3.92]
Capital transfers	− 10.29		
	[10.07]		

This table shows the results of the implementation of the Sant'Anna and Zhao (2020) doubly robust Differences in Differences estimator using Stata command *crdid*

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

6.6 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 9 presents the results on different types of revenues and expenditures.

Table 9 shows that the only category of revenues in which revealed corruption has a statistically significant effect is property income. In terms of magnitude, the reduction in licenses and fees is also relevant, although in this case the coefficient is slightly below standard significance levels (p-value is 0.149). 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 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 in 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) and on capital transfers. This result is different than the one found for Brazil for capital transfers in Brollo (2012). It is also different from 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, the last column of Table 9 completes the picture by looking at the expenditure side. In this case, the effects occur mostly through a reduction in investment expenditures (e.g., infrastructure building). The coefficient of investment expenditures has a magnitude of − 37.35 euros per capita in column 1 of Table 9, which represents approximately 60% of the total estimated reduction in expenditures (which was 62.74 in column 2 of Table 6). The implied reduction in investment is therefore quantitatively large and it amounts to approximately a 10% 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 effect on public finances at the municipality level. Revenues and expenditures decrease by approximately 7 and 5%, respectively. The reduction is concentrated in the areas of revenue and expenditure most related to construction activity. This is due to both a reduction in publicly funded projects (e.g., less capital grants to fund infrastructure projects) and privately funded projects (e.g., reduction in revenues from construction licenses and fees). 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 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 affects 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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Declarations

Conflict of interest Authors declare that we have no conflict of interest.

Ethical approval This article does not contain any studies with human participants performed by any of the authors.

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