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Corruption under Austerity

Abstract

We study how policies limiting the spending capacity of local governments may reduce corruption. We exploit the extension of one such policy, the Domestic Stability Pact (DSP), to small Italian municipalities. The DSP led to a decrease in both recorded corruption rates and corruption charges per euro spent. This effect emerges only in areas in which the DSP put a binding cap on municipal capital expenditures. The reduction in corruption is linked to accountability incentives as it emerges mostly in pre-electoral years and for re-eligible mayors. We then estimate the impact of the extension of the DSP on local public good provision in the following years, finding a null effect in the short run. Overall, our findings suggest that budget constraints might induce local governments to curb expenditures in a way that dampens their exposure to corruption without depressing local welfare.

JEL Codes: D720, D730, H620, H720, K340.

Keywords: corruption, austerity, fiscal rules, European funds, local public finance, public procurement.

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1 Introduction

Suppose a country is subject to a new budget constraint, like a set of austerity measures. How will politicians accordingly change public spending? Is this budget shock going to be beneficial to the society? These are relevant questions for economists and policymakers, which became even more salient after the Great Recession, as austerity measures have been widely adopted by national governments and supra-national institutions. In this paper, we analyse fiscal austerity studying Italian municipalities which are subject to a new form of budget constrains. We find that a simple budget rule, the Domestic Stability Pact (henceforth 'DSP'; *Patto di Stabilità Interno*, in Italian), led to a reduction in corruption driven by lower discretionary spending, without involving a measurable reduction in local welfare.

The DSP is a set of fiscal rules adopted by the Italian government that constraints public spending at the local level. The national government sets numerical limits on budgetary aggregates, and establishes sanctions for local governments that overspend their target. Similar policies are common in decentralized countries, in which local authorities receive transfers from the national government, and may not entirely internalize the cost of spending. In the last decades, similar budget constraints rules have become increasingly common, due to the substantial rise in public debts threatening fiscal sustainability. The International Monetary Fund (IMF) currently lists 96 countries that have adopted local, national or supra-national fiscal rules (Lledó et al.,2017).[1]

Ex-ante, budgets constraints might have opposite effects on corruption: on the one hand, budget constraints might pressure politicians to reduce inefficient expenditures, resulting in a decrease in rent-seeking. This could be due to an accountability motive. To ensure they comply with fiscal rules, local politicians may be more willing to reduce inefficient expenditures because alternative policy choices, such as increasing local taxes or reducing service provision, might be more likely to threaten their chances of re-election. Therefore, career-motivated politicians face a trade-off between cutting inefficient expenditures and reducing their own rent seeking. Moreover, the DSP might convince local politicians that the national government is more closely monitoring municipal spending, which may motivate them to reduce rent seeking in order to avoid being sanctioned.

On the other hand, if electoral accountability is weakened or distorted, politicians might instead reduce welfare-enhancing expenditures without affecting their rent-seeking, resulting in a higher share of corruption-affected public spending. For instance, this would be the case if some voters keep supporting a corrupt politician in exchange of targeted or clientelistic benefits (Nannicini et al., 2013; Boas et al., 2018). A similar outcome is plausible if short term rent-seeking provides a higher utility to politicians than being re-appointed in office (Pereria et al., 2009). Overall, it does remain an empirical question whether and how public budget constraints affect corruption.

The analysis we conduct relies on the extension of the DSP to Italian municipalities with population below 5,000 inhabitants, which occurred in 2013. The DSP already applied to towns with more than 5,000 inhabitants before the reform. Using data from the period 2004-2015, we employ a Difference-in-Differences estimation strategy comparing municipalities below/above 5,000 inhabitants, before and after 2013, to test whether being subject to the DSP affects corruption rates and budgetary outcomes. However, as towns of different sizes may be affected by other policies

[1] According to the IMF, EU countries are the most heavily regulated; they typically adopt three or more levels of fiscal rules.

in a differentiated way, we restrict the sample to municipalities whose population is sufficiently close to the 5,000 threshold. In other words, we employ a 'local' version of the usual Difference-in-Differences, testing the robustness of our estimates across many population bandwidths.

Importantly, we do not expect the DSP to have uniformly affected Italian municipalities. In our period of interest, European transfers disproportionately reached Italian regions that were considered less developed as their GDP per capita was below 75% of the EU average (hereafter called 'high-funds regions' or 'HFRs').[2] Since expenditure financed by these transfers did not count towards total expenditure as targeted by the DSP, capital expenditure in towns located within HFRs was *de facto* much less constrained by fiscal rules.

In line with the first set of hypotheses, we find that the DSP decreased corruption rates (from 6% to 30% of a standard deviation depending on the strictness of the budget constraint) but only in municipalities receiving standard flows of European transfers (hereafter called 'low-funds regions' or 'LFRs'), in which the DSP actively constrained capital spending. The drop in corruption rates is linked to budgetary changes, and plausibly triggered by them. We document that municipalities in LFRs are induced by the DSP into reducing public investments (i.e., capital and procurement expenditure), that represent discretionary types of spending and, as such, are more liable to be affected by mismanagement and rent seeking (Hessami, 2014; Mauro, 1995; Liu and Mikesell, 2014).

Conversely, we do not observe any improvement in corruption in towns located in HFRs, where the grip of fiscal rules on public investments was feebler. Likewise, in HFRs municipalities, the DSP does not affect public budget outcomes. This can be explained by the presence of extra EU transfers, which financed capital expenditure and were not targeted by the fiscal rules. In other words, EU transfers allow these municipalities to comply with the requirements of the DSP without the need of cutting expenditure or increase taxes. Indeed, as discussed later in the paper, almost all municipalities in HFRs abide to the requirements of the DSP.

Importantly, our results in LFRs are not just due to a mechanical decrease in public expenditure. Local politicians retain a margin of discretion in choosing courses of action that are compatible with budget constraints. For instance, they might decide to increase local taxes or reduce current expenditure. Alternatively, they may cut the most likely rent-seeking-affected areas, i.e. capital and procurement expenditures. In line with this interpretation, we observe a decrease in corruption charges per euro spent in capital expenditures, which highlights an improvement in the corruption-proofness of public spending. Moreover, the analysis on public procurement allows us to exclude that local politicians are strategically shifting expenditures towards more discretionary forms of procurement that could facilitate hiding corruptive practices. Finally, in line with the idea that politicians are more efficient under the DSP, we document an increase in their productivity, measured by novel data on the number of approved municipal deliberations.

As mentioned above, electoral accountability might explain these findings. This is plausible if the DSP made corruption more expensive as it raised the opportunity cost of public spending. We can exploit two exogenous variations in electoral incentives to determine whether accountability is at work. First, Italian municipalities can be split into five groups, each on a different 5-year long electoral schedule: this staggered timing of municipal elections is due to historical reasons and provides exogenous variation in the electoral cycle (Repetto, 2018; Giommoni, 2019). We find that

[2] See Section 3 for details on European regional development policy.

corruption decreases in treated municipalities especially during the electoral period, in line with the idea of politicians reducing rent-seeking for electoral purposes. Second, Italian mayors face a two-term limit, whereby we should expect electoral incentives being at work for re-eligible mayors. Indeed, we find that corruption decreases mostly for mayors in their first term who can stand for re-election (we provide additional evidence to validate a causal interpretation of this test). Overall, both tests suggest that, under the DSP, accountability incentives lead local politicians to reduce rent-seeking.

Overall, these findings might imply a trade-off between a beneficial effects of budget constraints on corruption and a drop in potentially welfare-enhancing public investments. To investigate the overall welfare effects of fiscal rules, we test whether the DSP affected GDP (measured by aggregate taxable income), inequality and a newly collected set of outcomes, including all main municipal services (i.e. waste management, kindergartens, police, school canteens and street lighting). We do not find any effect on these measures of local welfare. These results suggest an allocative efficiency gain for local public finance, although the recent extension of the DSP does not allow for an assessment of its long term effects on local public good provision.

The paper is organized as follows. Section 2 presents the contribution to the literature while Section 3 documents the institutional background and describes the data used in our analysis. Section 4 discusses the empirical strategy and Section 5 shows our main results on municipalities in LFRs. Section 6 presents the results on HFRs. In Section 7 we provide some final remarks on the generalizability of our findings.

2 Contribution to the literature

This paper advances four strands of the literature.

First, we relate to studies on the design of public budget constraints and on the effects of austerity policies. The main goal of this literature has been to understand whether and how fiscal adjustment can improve welfare systems sustainability (Alesina and Perotti, 1995; Alesina and Perotti, 1996). Recent studies have also focused on the political effects of austerity policies (Stiglitz, 2016; Fetzer, 2019). Our approach is similar to the studies which exploit within-country variation to test the effects of budget constraints: previous evaluations find mixed results in terms of deficit and public debt reduction (Grembi et al., 2016; Coviello et al., 2017; Gamalerio, 2017; Heinemann et al., 2018; Asatryan et al., 2015). The main contribution of this paper is to show that budget constraints may lead to an unintended reduction in corruption through a drop in discretionary spending. While fiscal policy obviously is (and ought to be) motivated by considerations other than its potential spill-over impact on corruption, these unintended effects might be of interest to international organizations and governments debating whether (and how) to introduce budgetary constraints. Our findings are also timely, as we shed light on the effects on corruption of two salient and highly debated policies – EU transfers and fiscal rules – that affect a multitude of local governments on the European continent.

Second, we relate to the literature on anti-corruption policies. While previous studies have compiled considerable evidence on the detrimental effects of corruption (*e.g.*, Olken and Pande, 2012), there is still a lack of agreement on how to best fight it (*e.g.*, Golden, 2018; Fisman and Golden, 2017; De Vries and Solaz, 2017). A common feature of anti-corruption policies is the creation of specialized authorities dedicated to devising and implementing anti-corruption strategies, ranging from regulations to promote fair competition and transparency, to audits of bureaucrats' and politicians' behavior. These tools entail relevant costs (i.e., investments in new technologies, auditors' wages and training, and the design of specific regulations), which are at least partially passed on to the monitored agents. For instance, previous studies examined the effects of anti-corruption audits promoted by the national government in Brazil (Avis et al., 2018; Ferraz and Finan, 2011; Zamboni, et al., 2018), Mexico (Larreguy et al., 2015), Puerto Rico (Bobonis et al., 2016), Argentina (Di Tella and Schargrodsky, 2003) and Indonesia (Olken, 2007). Conversely, we study a policy which reduces corruption without incurring in additional implementation costs and with little scope for manipulation.

Third, we relate to the studies on decentralization and elite capture (Boffa et al., 2016; Enikolopov and Zhuravskaya, 2007; Fisman, and Gatti, 2002; Rodden, 2004). A central question of this literature is whether fiscal decentralization leads to more efficient governance. On the one hand, local politicians have access to superior information on citizens' preferences; on the other hand, local governments might be captured by private interests without an effective monitoring by the central government. In this paper, we provide an interesting insight for this puzzle, as we show how a national government might adopt budget constraints to strengthen the incentives for local politicians to reduce rent-seeking.

Finally, this paper relates to studies of the impact of governmental transfers on rent seeking, which show that transfers may amplify corruption at the local level (Brollo et al., 2013; Barone and Narciso, 2015). Differently from this literature, in this paper we focus on a reduction in the size of the budget and we study whether the policies enacted to comply with it may affect rent-seeking. Nevertheless, we also illustrate how governmental transfers (i.e. European funds) may offset the beneficial effects of budget constraints on corruption. The finding that the provision of European funds lessens the effects of fiscal rules on corruption is in line with Becker et al. (2018), who show that EU funds prompt economic growth only in regions with reliable institutions.[3]

3 Institutional background and data

3.1 The Domestic Stability Pact

Following the European Union adoption of the Stability and Growth Pact in 1997, some European countries (including Italy) enforced fiscal rules to keep local governments accountable. Our analysis of the impact of fiscal rules on corruption is based on the so-called Domestic Stability Pact: the DSP consisted of a set of budgetary policies that applied to Italian local governments between 1999 and 2015 (after 2015, a new system of local public deficit control has entered into force). The DSP aimed at regulating expenditure by local governments in Italy (regions, provinces, and municipalities), so to constrain national public spending.[4]

In this paper, we focus on the effects of the DSP on municipalities, the smallest administrative units in Italy. Our identification of the effects of fiscal rules on corruption is based on the extension

[3]We also relate to studies analyzing the performance of bureaucrats and politicians (e.g. Bandiera et al., 2009; Limodio, 2019), especially in relation to rent seeking and public procurement procedures (Conley and Decarolis, 2016; Decarolis, 2018; Geys and Titl, 2019; Di Cataldo and Mastrorocco, 2020; Gallego et al., 2020).

[4]See law 448/1998, which first introduced the DSP in Italy.

of DSP to towns with population in the 1,000-5,000 range, that occurred in 2013.[5] Before that, since 2001, only municipalities with more than 5,000 inhabitants had been subject to the policy, according to the annual Italian budget laws -*Legge Finanziaria*- in the years 1999-2012 (Chiades and Mengotto, 2013, Bonfatti and Forni, 2017, Gamalerio 2017).[6]

The extension of the DSP to small municipalities can be interpreted in the light of the Italian precarious macroeconomic situation during the Great Recession, in which several austerity measures have been adopted to reduce the risk of defaulting. In the period 2006–2011, local governments had debts for a value of about 7% of the Italian GDP and many municipalities incurred in high deficits (Banca D'Italia, 2012). This situation was worsened by the spread of risky financial derivatives, which municipalities used to finance ordinary expenses.[7]

In the period we consider (2004–2015), the DSP imposed restrictions on accrual-based current expenditure and actual capital expenditure (for details, see Bonfatti and Forni, 2017 and Chiades and Mengotto, 2013). The DSP established that, for each municipality and year, the overall budget balance had to be proportional to a (moving) average of balances obtained in previous years in the same municipality.[8] The operative details of this rule (including exceptions for specific expenditure items and the way reference surpluses had to be computed) were subject to changes across years, but such changes were uniformly applied to all involved municipalities.

Lack of adherence to the financial limits imposed by the DSP resulted in a number of sanctions being imposed on municipalities. These included caps on programmed expenditure, decreased transfers from the central budget, limits to hiring and to the subscription of new debt contracts, and reductions of local politicians' salary. According to evidence recorded by the national government, non-compliance was limited to a few cases. Indeed, the overall public finance goals of the DSP were attained in every region.

As explained in the previous section, the municipal government might react to the DSP by reducing capital and/or current expenditures. These strategies are plausible in the Italian scenario. As explained by Grembi et al. (2016), local politicians can considerably shape the local budget, as about one third of current and capital expenditures are classified as not rigid (i.e. not included into payroll expenses or debt service).[9] In line with the idea that there's room for reducing rent seeking, Bandiera et al., (2009) show that Italian municipalities pay different prices for the same local municipal services, which they interpret as evidence of passive waste. Moreover, mayors might also react by increasing local taxes, which are often used by local politicians for electoral purposes (see for instance Giommoni, 2019).

[5]At the beginning of 2013, Italy was divided into 8,092 municipalities, with a median population of 2,438 inhabitants.

[6]The DSP uniformly applied to ordinary-statute regions (15 out ot 20), as well as to Sicily and Sardinia, which have limited autonomy in terms of public finance despite having a special statute. In fact, the three remaining special-statute regions also endorsed a form of the DSP, and both Friuli Venezia-Giulia and the autonomous province of Trento were subject to a similar extension of the lower population limit from 5,000 to 1,000 in 2013. Our main results are robust to variations of the sample that keep account of municipalities that were exempt from applying the DSP. Specifically, in the robustness checks section we report our main findings on corruption excluding special-statute regions from the sample.

[7]The peak was reached in 2007 with 671 municipalities adopting financial derivatives to finance their budget.

[8]For instance, in the period 2012-2014, the budget had to be proportional to the average of balances in the period 2006-2008.

[9]This applies also to current expenditures. For instance, in about 50% of municipalities in our sample, current expenditures (per capita) vary by more than 50 euros on yearly basis. This is a sizable magnitude, similar to estimated effects of the DSP on spending, which are presented later in the paper.

3.2 European funds

EU funds for investment programs were excluded from the DSP restrictions. Italy is an important recipient of European funds through the Regional Policy, the EU's main investment policy. Regional Policy is delivered through two main funds: the European Regional Development Fund (ERDF) and the Cohesion Fund (CF).[10] The policy is implemented by national and regional governments in partnership with the European Commission. Importantly, six out of twenty Italian regions are HFRs (high-funds regions): Apulia, Basilicata, Calabria, Campania, Sardinia and Sicily. These regions receive large transfers, which in fact made DSP restrictions on capital expenditures hardly binding for their municipalities. Importantly, the division between HFRs and LFRs (low-funds regions), which receive fewer EU funds, is exogenously determined at the European level according to regional GDP, by the above-mentioned 75% GDP rule.

We collect data on European funds from the OpenCoesione.gov portal.[11] The data consist of the EU budget for 2007–2013, which includes funds that could be spent up to the end of 2015, complemented by national and private co-financing.[12] The total Italian expenditure certified to the EU was €46.2 billions.[13]

Figure 1 plots the aggregate amount of European funds spent by LFRs and HFRs over time, in per capita (left) and aggregate terms (right). On average, LFRs spend €228 millions per year, compared to the €618 millions spent by HFRs. The gap between the two areas widens over time.

Are those trends similar across treated municipalities? As anticipated, we will consider the enforcement of the DSP among municipalities between 1,000 and 5,000 inhabitants, which determines our treatment group. On average, in the period 2013–2015, treated municipalities in HFRs received every year 226 Euros per capita of EU funds, which corresponds to the 57% of their capital expenditure budget, while those in LFRs received only 22 Euros per capita, corresponding to 11% of their capital expenditure budget. This comparison highlights how EU funds could finance an extensive share of public investments in HFRs utterly reducing the effectiveness of fiscal rules.

As we do not expect an effect of fiscal rules in HFRs, we report these tests in the last section of the paper, while in the main analysis, we focus only on municipalities in LFRs.

3.3 Data on corruption

Information on corruption is based on the Italian Investigation System (henceforth 'SDI'; *Sistema d'indagine*, in Italian), a data collection system managed by the Ministry of the Interior. The SDI records details on investigation procedures authorized by the judiciary and carried out by police forces. The data cover the years 2004-2014, and allow us to compute the number of initiated procedures by municipality, year, and type of alleged offence.[14] Three important remarks apply to such data: first, investigations occur at the beginning of the prosecution process (therefore there is generally a short time in between the actual crime and the start of the investigation), so they represent alleged offenses rather than verified crimes; second, each investigation may involve

[10] https://ec.europa.eu/regional_policy/en/policy/what/investment-policy/

[11] See www.opencoesione.gov.it/en/. OpenCoesione is an open government project managed by the Department for Cohesion Policy at the Presidency of the Council of Ministers. It publishes data on all projects covered by the EU Regional Policy, including those with a national co-financing requirement.

[12] http://eur-lex.europa.eu/legal-content/EN/ALL/?uri=CELEX:52007DC0798.

[13] https://opencoesione.gov.it/en/spesa-certificata/.

[14] SDI information are not publicly available and not at all available after 2014.

several alleged perpetrators, but we only observe the total number of investigations, rather than the number of people involved; third, if one investigation concerns alleged offences falling under the scope of more than one article of the Italian Penal Code, it will be counted as many times as are the articles involved.

In this paper we aggregate the number of investigations pertaining to corruptive phenomena (i.e., bribery, graft, and malfeasance/resource embezzlement) to construct a time-varying index of corruption at the municipal level. These crimes are referred to in the articles 317-323 of the Italian Penal Code. It is important to mention that these specific articles contemplate crimes that always involve public officials. The average number of corruption episodes reported in the SDI per municipality and year is 0.17. The average cumulative number of corruption episodes investigated between 2004 and 2014 per municipality is 1.81. As represented in Figure 2, more than three out of four municipalities display no corruption episodes in the period under consideration. Figure 2 highlights that corruption is spread across all Italian regions, although it appears more common in Southern towns. Overall, the average number of episodes recorded each year in the whole country is around 1,300. Furthermore, the phenomenon affects towns of our interest: municipalities between 1,000 and 5,000 inhabitants have on average 1.41 total corruption episodes between 2004 and 2014. Figure 3 plots the aggregate number of corruption investigations for large, medium and small Italian municipalities over time.

In what follows, we use three normalized versions of the corruption index (see Panel A of Table 1), dividing the number of corruption episodes observed in each municipality and year respectively by municipality population, expressed in thousands (this is our main dependent variable, which we label *Corruption (PC)* in what follows), by capital expenditure -expressed in logarithm- (*Corruption (capital exp.)*) and by total expenditure -expressed in logarithm- (*Corruption (total exp.)*). In subsequent regression analyses, these indicators are standardized by region group (LFR or HFR).

3.4 Data on local public finance and procurement

Data on local public finance come from municipal balance sheets collected by the Italian Ministry of the Interior, that include detailed information on revenues, expenditures, transfers, deficit and debt of municipal administrations for the period 2004–2015. Panel B of Table 1 contains the descriptive statistics for these variables.

Information on public procurement is drawn from data collected by Telemat, a private firm. Available data cover the large majority of public works contracts tendered by Italian municipalities between 2009 and 2015, but information is essentially limited to the award stage, starting when the tender is publicized and ending when the contract is assigned to the winning firm(s). Overall, around 115,000 tenders are recorded in the dataset. In this paper, we mainly use the value of public works tendered by Italian municipalities over the whole period 2009-2015. Around 18% of all contracts have base price smaller than €40,000, which is the threshold below which direct assignment of contracts (i.e., without any competitive tendering process) was allowed by law in the period under observation. Panel C of Table 1 contains the descriptive statistics for procurement variables.

Moreover, Panel E shows information on local GDP, measured by the aggregate taxable income declared in Italian municipalities: the average amount is €85.5 millions. The source of this data is the Italian Ministry of the Economy. We complement our analysis with political data on local elections, local politicians and a novel dataset on municipal services provided by the Italian Ministry of Interior. The descriptive statistics are shown in Panel D and F of Table 1.

4 Empirical analysis

4.1 A local Difference-in-Differences approach

The goal of this paper is to estimate the causal impact of introducing the DSP on corruption in Italian municipalities. We study this relationship by exploiting the introduction of the DSP described in Section 3. We cannot simply focus on the population threshold of 5,000 inhabitants, above which the policy applied until 2013, and compare towns that were subject to fiscal rules with those that were not. Such comparison might indeed provide confounded estimates as another policy, namely mayors' salary, also changes sharply at the same cutoff (Gagliarducci and Nannicini, 2013). Therefore, we rely on the change in the extension of the DSP that took place in 2013: this intervention reduced the population threshold from 5,000 to 1,000 inhabitants, extending these fiscal rules to 3,751 new municipalities. We exploit this policy intervention to test our hypothesis by a 'local' Difference-in-Differences methodology. We compare towns around the 5,000 inhabitants threshold before and after 2013, and we limit the sample to towns in a neighbourhood of this population cutoff, in order to raise comparability between the treatment and control groups (that consist, respectively, of towns with less than 5,000 inhabitants – which are subject to the policy from 2013 on – and towns with more than 5,000 inhabitants – that are subject to the policy throughout the whole period of observation).

This exercise allows us to identify the effects of the introduction of fiscal rules in the treatment group, and to overcome the issue of overlapping policies around the same cutoff, that a Regression Discontinuity Design technique would not be able to deal with. Moreover, the local approach of this methodology, which considers towns in a neighborhood of the population threshold, makes treatment and control groups more comparable respect to a standard Difference-in-Differences with a global approach. Finally, differently from the Difference-in-Discontinuity technique (Grembi et al., 2016, Gamalerio, 2017), this method allows to control for a rich set of fixed effects, including municipal ones.[15]

4.2 Identification assumptions

The key identifying assumption of this identification strategy requires that there are no other interventions, simultaneous to the DSP reform, differently affecting municipalities around the threshold. Likewise, trends in corruption between treatment and control groups should be comparable in the absence of the reform. We conduct a background institutional check to exclude the presence of overlapping policies in 2013, and we test for the presence of pre-trends in the main analysis, whose outcomes are reported in Section 5.

The only other policy change that concerned the 5,000 inhabitants threshold in 2013 was the introduction of double preference voting conditioned on gender (Law 215/2012) in municipalities

[15] However, our results remain unchanged when applying a Difference-in-Discontinuity technique (e.g. Campa, 2011; Grembi et al., 2016). We report those findings among our robustness tests.

above the threshold: voters can cast a vote for two candidates (instead of one), provided they are of different gender. The aim of this policy was to increase the share of female politicians in local councils. However, in 2014 a reinforced version of this gender quota policy was extended to towns above 3,000 inhabitants (Law 56/2014), stating that the fraction of elected politicians of each gender cannot represents less than 40% of municipal government seats.[16] In those municipalities, gender quotas could bias our results if female politicians have different attitudes towards corruption, as shown by Brollo and Troiano (2016) in the Brazilian context.

To reduce concerns regarding this potential bias, we control for the share of elected female politicians in all our specifications. As a stricter robustness test (in Appendix Table 10) we replicate our findings dropping from the sample the post-electoral periods of all municipalities that had elections both in 2013 and 2014. Our findings are unaffected.

Lastly, the evolution of the scope of fiscal rules would also, in principle, allow to study the 1,000 threshold, as towns below this threshold were never subject to the DSP. However, this is not feasible for two reasons: i) the diffusion of 'unions of municipalities' (*Unioni di Comuni*) among very small towns, allowing them to jointly manage some of their functions without being subject to the DSP; ii) the very low incidence of detectable corruption in towns around the 1,000 threshold, which limits our analysis. We provide further explanations and these additional analyses in Section 5.7.

4.3 Detected and actual corruption

The analysis relies on corruption investigations and not on conviction rates. We rely on investigations as those are timely, while convictions take place several years later. Moreover, in the Italian context, convictions data include only the conviction date (without mentioning when the crime was committed) and are available only at the regional level (and not at the municipal level).[17] Importantly, most investigations on corruption crimes seem to end up into trials and then convictions: in the period 2010–2014, there were 7,638 corruption related convictions, i.e. about 90% of the total number of corruption investigations in the same period (8,493). This ratio is quite similar when considering lags between investigations and convictions.

A second and more substantial limitation of our data is that investigations account only for a share of the total number of corruption cases, which is obviously unobserved. The number of corruption investigations can be considered a function of the total number of actual corruption cases and the detection efforts of the police. Our assumption is that detection is not affected by the introduction of the DSP.

A first concern is that the police may be more or less willing to start investigations on corruption-related allegations when they know that politicians are constrained by the DSP. This may happen, for instance, if police expects a change in corruption following the introduction of the DSP.

We consider such strategic reaction unrealistic in this setting as: i) no official document or media report from the time when fiscal rules were first introduced links the DSP to any corruption consequence; ii) at that time, there was not in Italy a central anti-corruption agency able to

[16]Italian municipal elections vary based on a five-year calendar, whereby every year a different group of municipalities hold local elections.

[17]In Appendix Figure 13, we show that conviction rate related to corruption crimes does not differentially change across LFRs and HFRs over time: data on trials refer to the end date of the trial.

coordinate and direct anti-corruption efforts.[18] Corruption investigations are mostly undertaken by the local branches of the *Guardia di Finanza*, a law enforcement agency with offices in each Italian province; iii) this shift in policing would be more plausible when fiscal rules were first debated and introduced in larger Italian municipalities in 1999.[19]

A related concern is that the DSP might lead politicians to decrease spending on local police, which, in turn, would become less likely to detect and report corruption. This concern is not of primary importance, as municipal police forces are not in charge of conducting anti-corruption investigations. Nonetheless, we show in a robustness test that mayors have not decreased expenditure on local police following the introduction of the DSP.[20]

Finally, note that all these possible sources of bias are inconsistent and not related to the main mechanism explaining the effects of the DSP on corruption, *i.e.* the changes in municipal spending.

4.4 Specification

Our dependent variable varies at the municipal/year level. The set of dependent variables includes the measure of corruption incidence and the set of public finance and procurement indicators. The estimated empirical model is as follows:

$$y_{it} = \beta_0 + \beta_1 S_{it} + \beta_2 S_{it} \cdot T_t + \beta_3 P_i^* + \zeta' X_{it} + \delta_t + \gamma_i + \epsilon_{it}\ , \tag{1}$$

where y_{it} is the dependent variable in municipality i, in year t. S_{it} indicates the treatment group: it is a dummy indicating municipalities below the 5,000-inhabitant threshold (population of 1,000–5,000)[21]; it relies on two available population censuses (conducted in 2001 and 2011)[22]. T_t denotes the post-reform period: it is a dummy equal to one after 2013. To further increase the comparability between the Treatment and the Control group, we control for the distance to the population threshold ($P_i^* = P_i - P_c$, where $P_c = 5,000$) for municipality i. The population of town i, P_i, is based on the value recorded in the two censuses of 2001 and 2011. The fact that the population census is pre-determined to the reform itself avoids the risk of endogenous sorting of cities around the threshold, which was not known when the census was completed[23]. The local DID estimator is obtained by the interaction term $S_{it} \cdot T_t$, which captures the effect of introducing the DSP, with the comparison of treated and control municipalities before and after 2013.

[18] A central anti-corruption agency (ANAC) was established at the end of 2014.

[19] To further investigate this reasoning, we also contacted two top-officials from the *Guardia di Finanza* to understand their *modus operandi*. They dismissed as highly unrealistic the idea that *Guardia di Finanza* officials might change investigations' strategies based on the approval of the DSP or any other public finance related policy. They both requested to remain anonymous.

[20] An additional concern is that companies losing business due to the decreased spending on public procurement (which is, as shown later on, a direct outcome of budget constraints) might react by pressing corruption-related charges against their competitors more often. We are not able to ascertain whether single corruption investigations are spurred by such complaints. Still, an increase in reported corruption through this channel would work against our main result, making it a lower bound estimation of corruption reduction after the introduction of the DSP. As a further check, in a robustness test we show that the level of enforcement of the DSP is not linked to a differential trend in firms' accusation charges in procurement.

[21] Even if S_{it} is estimated in the model, since it is a time-varying indicator, we do not show its coefficient in the output tables for the sake of brevity. Nevertheless, the corresponding coefficient is never statistically significant.

[22] In particular, the reference population is drawn from 2001 census for the years 2005-2010 and from 2011 census for the years 2011-2014.

[23] Nevertheless, we formally test this assumption conducting the standard McCrary test where we study the density around the threshold of 5,000 inhabitants, using the population figures of 2011 census. The results, shown in Appendix Figure 10, suggest that there is no evidence of sorting around the threshold of interest as the density does not show any discontinuity in correspondence of that population level.

X_{it} is a vector of controls including age, education, gender of the mayor and the municipal councillors.[24] We include municipality fixed effects, γ_i, and year fixed effects, δ_t, and we cluster robust standard errors at the municipal level. Finally, the sample of municipalities included in the analysis is restricted to those at a distance h from the 5,000 threshold, $P_i \in [P_c - h; P_c + h]$. We do not arbitrarily select h; we instead test the sensitivity of our results using multiple bandwidths of h, in line with regression discontinuity design methodology.[25]

Furthermore, we estimate the following alternative empirical model in order to study the dynamic effect of the treatment and to evaluate pre-trends:

$$y_{it} = \beta_0 + \beta_1 S_{it} + \beta_2 S_{it} \cdot \delta_t + \beta_3 P_i^* + \zeta' X_{it} + \delta_t + \gamma_i + \epsilon_{it} \,. \tag{2}$$

The local DID estimator is the interaction term $S_{it} \cdot \delta_t$, which compares treated and control municipalities every year, using 2012 (the last year before the reform) as the benchmark year. All other terms are as in Model 1.

5 Results

5.1 Effect on corruption complaints

In this section we study the impact of introducing budget constraints on corruption at the local level. As highlighted in Section 3, our main analysis focuses on municipalities in LFRs, in which fiscal rules are fully enforced. Later in the paper, we will go back to the results in HFRs, in which budget constraints are not binding. Corruption is measured by the number of investigations per 1,000 inhabitants and standardized to have a mean of zero and a standard deviation of one.

Figure 5 shows the main graphical outcomes from the estimation of Models 1 and 2. It illustrates that corruption significantly decreased in the group of municipalities to which the DSP was extended in 2013. In the left panel, we present the DID analysis, according to Model 2, and the effect is between 6,1% and 11.5% standard deviations. Moreover, there are no differences between the treatment and control groups prior to 2013, which suggests that local trends in corruption are parallel before 2013. Furthermore, the figure demonstrates that the results are very similar across three bandwidths: 2,000, 2,500 and 3,000 inhabitants.

In the right panel, we study how sensitive this result is to the choice of population bandwidth: we plot the DID coefficient, according to Model 1, varying the population bandwidth in the range of 1,000–4,000. The effect of the policy is always negative and sizeable, and the magnitude of the coefficient is rather stable as the sample widens and the effect is statistically significant in nearly all cases. This output suggests that this relationship does not depend on the sample of municipalities included and shows that it is robust to many different population bandwidths.

Columns 1 and 2 of Table 2 display the full results of one of these tests, using a 2,500-inhabitants bandwidth.[26]

[24]We generally present the findings with the complete set of controls. Due to space constraints, we provide the estimates with and without controls only for the main results of the analysis.

[25]Figure 4 shows the geographical distribution of Italian cities in the treatment and in the control groups, in this case we arbitrarily use a bandwidth of 2,500 inhabitants, which we use also for additional and robustness tests.

[26]In Appendix Table 11, instead of using the standard corruption measure as the dependent variable, we look at the four main types of corruption charges included in our dataset: strict corruption, graft, malfeasance (including official misconduct and abuse of office) and embezzlement (including misappropriation of public funds). Also these

In a further step, in order to clarify which channels explain this result on corruption, we focus on the DSP's impact on local public finance and procurement outcomes.

5.2 Effect on local public finance

In this section we study the DSP's impact on local public finance in order to determine why corruption is decreasing. We show that municipalities that experience a drop in corruption are simultaneously decreasing their capital expenditures. Figure 6 includes the findings of this analysis (Models 1 and 2). All variables are expressed in per capita terms (winsorized at the 1%), and the outcomes are presented with three bandwidths: 2,000, 2,500 and 3,000 inhabitants. The results for the 2,500 bandwidth are reported in Table 3.

The first sub-figures display the impact on capital expenditure: the introduction of a budget constraint leads to an immediate and consistent reduction in local spending. The DID approach in the right panel shows a reduction of about 70 euros per capita (per year): more precisely, the effect is between 3.7% and 5.4% standard deviations. The left panel shows that there are no clear pre-trends before 2013. The sub-figures in the second rows of Figure 6 illustrate the impact of fiscal rules on current expenditure. Also in the case, the coefficients are negative and significant. However, the effect is smaller than the one on capital spending, as it amounts to about 20 euros per capita and it is between 2.6% and 3.7% standard deviations.[27]

This result suggests that fiscal rules' effect on capital expenditures may explain why corruption drops: municipalities experience a drop in corruption driven by the reduction in capital expenditures. This interpretation is consistent with the empirical literature suggesting that capital spending is the budget component most vulnerable to corruption (e.g. Hessami, 2014).

In the third row, we focus on the effect on the property tax rate (on main residence), which is the main source of fiscal revenues among Italian cities: fiscal revenues represent an important fraction of total revenues in Italian municipalities (in 2007 they represent 43% of total revenues, Angeli, 2009). Moreover, local politicians have a complete control over this tax which is modified very frequently according to the local needs, as shown by Giommoni (2019). The figures show that the property tax rate weakly decreases after 2012. As the effect takes place already in 2012, it cannot be attributed to the DSP. This downward trend is likely due to a property tax law change in 2012, which might have differentially affected municipalities below 5,000 inhabitants. However, the size of the effect is very small, i.e. about 1.1% of the variable mean. Overall, this test shows that politicians do not increase local taxation to cope with the DSP.

The fourth row reports the findings on procurement spending. Procurement expenditures are an important component of local public spending, in which local politicians have considerable discretionary powers: this explains why this part of the budget may be an important source of corruption and rent seeking. The figures show that the DSP produced an immediate and persistent

variables are standardized by macro regions and expressed in per thousands inhabitants. Our findings are mostly driven by a reduction in malfeasance charges. This result is mainly due to the fact that malfeasance represents the great majority of the corruption-related events committed in our sample, precisely the 69.2%. Moreover, as explained in the next sections, in some heterogeneity tests, we find an effect also on investigations related to resource embezzlement.

[27] For the analysis on current expenditures some weak pre-trends emerge: these may be the result of the public finance reforms of 2011-2012 with potential asymmetric effects between small and middle-sized cities. Nevertheless, we do not think this represents a concern for our analysis as, i) there were no interventions relying on the population threshold of 5,000 and ii) we do not observe pre-trends in any other budget variable, suggesting that asymmetric effects, if any, were limited to current spending.

drop in procurement expenditures. This result is in line with our findings on capital expenditures, suggesting that municipalities react to the policy with a reduction in discretionary public investments. The effect is very sizeable: the average reduction induced by the policy corresponds to about 250 euros per capita, this corresponds to a reduction between 3.6% and 6.9% standard deviations. The magnitude of the coefficient is bigger than the one observed on capital expenditure as here we are considering the total value of single auctions, which are recorded in the budget over several years.

An important aspect for the interpretation of these results is whether the investigations on corruption pertain cases that imply a large cost for the society, as in case of infractions in procurement auctions, or, instead, they cause a limited economic damage, such as the case of small bribing. In order to provide some evidence on this, we conduct a specific test with the use of text analysis. In particular, we follow the approach of Giommoni (2017) and we screen newspaper articles released by the main Italian press agency, *ANSA*. The main goal is to select the articles discussing corruption cases that involve local politicians and to identify the object of corruptive behaviour. This may allow us to quantify the economic cost associated to that corruption episode. We focus on the same time span of the main analysis, 2004-2014, but clearly this does not guarantee that the corruption stories we identify in the newspapers are the same covered by the investigations. Furthermore, it is important to mention that selected articles only covers local politicians while the investigations include all possible public officials.

The procedure we employed consists in two steps. First, we select the articles dealing with corruption according to two criteria: 1) the presence of at least one keyword related to political corruption in articles' text, e.g. embezzlement, 2) the mention in the text of the surname of a local politician in office in the place where the article was geo-localized, in the period when the article was written. Appendix 2 discusses the details of the identification of corruption-related articles. Second, we identify the specific corruptive behaviour associated to each case. The results of this test suggest that the majority of these episodes are costly for the society. In particular, we identify 1,585 articles about infractions in procurement procedures, 431 on fraud, 315 on public hiring, 161 about refund usage, 81 on construction crimes and only 63 concerning theft and embezzlement. These findings provide some motivating evidence that corruption cases under analysis do represent an important cost for the community.

These outcomes suggest that the immediate impact of fiscal rules is a reduction in the level of spending rather than an increase in fiscal revenues, perhaps due to the high political cost for local governments of raising taxes.

5.3 Impact on the corruption-proofness of public spending

In the previous sections, we show that fiscal rules might lead to a substantial drop in corruption charges driven by a reshuffling of public spending: more precisely, politicians cut capital and procurement expenditures, i.e. discretionary spending, which are more affected by corruption. A further question is whether they specifically target the most inefficient types of capital expenditures. The drop in corruption we observe, indeed, may be a mechanical consequence of the reduction in investments or, differently, could be due to the cut in inefficient spending.

To test this hypothesis, we replicate our main analysis introducing as a dependent variable the ratio between the standardized number of corruption charges and the annual capital expenditures (expressed in logarithm). Figure 7 displays this test. We find a decrease in corruption per Euro spent in capital expenditures of a magnitude similar to the main results on corruption per capita. This implies that the DSP leads to an improvement in the corruption-proofness of public spending, which suggests that local politicians are not just reducing public investments, but are cutting the least efficient ones. Numerical results are shown in Table 2 (column 5). We also conduct this test using as dependent variable the ratio between the standardized number of corruption charges and the annual total expenditures (expressed in logarithm): the results are similar and are shown in Table 2 (columns 6).

An alternative explanation could be that politicians are strategically shifting rent seeking to spending areas that are less likely to be observed by law enforcement officials: a displacement effect might then explain the above findings.

We directly test for a plausible strategy to displace rent seeking, looking at the share of public procurement assigned below 40,000 euros. This is an important threshold, as politicians can award contracts below this amount without a competitive bidding process. Table 4 replicates our main estimation, introducing as dependent variables: i) the percentage of tenders for amounts under 40,000 euros, ii) the percentage of total amount in tenders whose value is lower than 40,000 euros, and iii) the overall tendered amount (per capita) in tenders with value lower than 40,000 euros. Overall, we find a decrease in the total tendered amount below this threshold, but we do not find a significant decrease in the number or percentage of tenders below 40,000 euros. These findings show that politicians are not resorting to less transparent tenders to potentially hide rent seeking.

Finally, columns 4-5 of Table 4 show that local politicians are more productive under fiscal rules. Specifically, we collect a novel dataset including the number of deliberations taken by the municipal council and by the municipal government, i.e. all official decisions taken at the municipal level prior to voting. On average they respectively increase by 7% and 4% in treated municipalities. We believe this finding is: i) in line with the idea that politicians are generally more performant; ii) in contrast with the idea that lower spending mechanically leads to less corruption as politicians are taking fewer policy decisions.

5.4 Intensity in the application of the DSP

As explained above, the DSP was not uniformly applied to all municipalities; the exact target was determined by a formula that took historical levels of public spending into account. Intuitively, we should expect the DSP to have a stronger effect in municipalities that were subject to a more stringent budget constraint. In particular, we take into account the level of surplus in the balance sheet set as the DSP target, and focus on the top 50% and 20% of this variable distribution.

We conduct these analysis in Table 2 (columns 3-4 and 7-8) and in Table 3 (columns 5-6). In line with our expectations, we find a stronger decrease in capital expenditures especially for towns subject to a more stringent budget constraint (Table 3, columns 5-6). Moreover, we replicate a similar analysis focusing on corruption charges. The effect is remarkably stronger (between 30% and 57% of a standard deviation) for municipalities experiencing more stringent fiscal rules (Table 2, columns 3-4 for corruption per-capita and columns 7-8 for corruption per Euro spent in capital expenditures).

Finally, in line with the idea that stricter fiscal rules push politicians to improve the municipal surplus, Appendix Figure 11 shows the correlation at the municipal level between the fiscal rules target (horizontal axis) and the realized surplus (vertical axis). Overall, this set of specifications highlights that i) a stricter budget constraint leads to stronger changes in public spending, and in turn, in corruption levels and ii) compliance has a crucial impact on budgetary and rent seeking outcomes.[28]

5.5 Mechanisms: Accountability

As explained in the introduction, accountability may explain why politicians reduce rent-seeking when facing this new budget constraint. If accountability is at play, we should observe a stronger corruption decrease in the presence of electoral incentives.

In the context of Italian municipalities, there are two potentially exogenous sources of variation in electoral incentives. First, the electoral schedule is pre-determined and staggered over time (Repetto, 2018). This implies that every year a different group of cities held elections, each on a different 5-year long calendar. We can therefore separate year fixed effects from the effect of time until the next election. If electoral incentives are at play, we should expect a stronger decrease in corruption in the electoral period. Specifically, we expect treated local governments to reduce corruption during pre-electoral and electoral years. We report this test in columns 1 and 2 of Table 5. In the table, we report only the $S_{it} \cdot T_t$ coefficient and the triple interaction between $S_{it} \cdot T_t$ and a dummy equal to 1 for the electoral year and the year before elections (for sake of brevity we do not show the other interacted terms). Table 5 shows that mayors are more likely to reduce corruption in the electoral period. This emerges for the different definitions of corruption in the analysis: corruption per-capita (column 1) and over capital expenditure (column 2).

Second, Italian mayors can be elected for a maximum of two consecutive electoral terms. We compare mayors in the first term to the ones in their second term (who face a term limit) to identify the effects of reelection incentives. Therefore, the focus is on the triple interaction between $S_{it} \cdot T_t$ and a dummy equal to 1 for term limited mayors. We find that mayors with reelection incentives receive significantly less corruption charges per capita than mayors without reelection incentives (column 3 of Table 5). This effect is barely not statistically significant when looking at corruption charges per Euro spent (column 4). A limitation of this approach is that politicians might self-select into a second term depending on whether they face or not fiscal rules. To reduce this concern, in columns 5 and 8, we exclude post-electoral periods of municipalities that held elections in 2013 and 2014. Therefore, we exclude mayors who might have decided not to re-run because of the DSP. Previous findings are strengthened. In a similar vein, voters might elect different types of politicians when facing the DSP. To tackle this point, we restrict the sample to cities in which the election outcome was uncertain: specifically, we focus on re-eligible mayors who won the election with a tiny margin (lower than 10% in columns 6 and 9, lower than 15% in columns and 7 and 10), and we control for the election victory margin. Also in this case, previous findings are confirmed. Overall, both tests suggest that, under the DSP, accountability incentives lead local politicians to reduce rent-seeking.[29]

[28] We run those heterogeneity tests for each type of corruption-related crime. While malfeasance drives our findings, we also find a reduction on embezzlement-related investigations, when testing the differential effect as a function of the intensity of the DSP application. These outcomes are not shown for the sake of brevity.

[29] An alternative explanation for our findings is related to an increase in perceived monitoring by local politicians.

Finally, we also test whether mayors in treated municipalities, which adopt fiscal rules, are more likely to be re-elected given the lower levels of corruption. We might expect two opposite effects: on the one hand, if voters are informed about the corruption decrease, they might reward the local government. On the other hand, voters might not like the decrease in capital expenditures, punishing the incumbent government. Note that this is not in contrast with the above mentioned political accountability mechanism: fiscal rules constrain the policy set of local politicians, who might have to choose among a set of unpopular policies (i.e. reducing expenditures and/or increasing local taxes) to respect the budgetary constrain. In turn, they might try to adopt the less unpopular policy, which in our context is a reduction in inefficient capital expenditures.

In this test, we limit the sample to electoral years and first-term mayors (who are eligible for re-election). Appendix Table 10 (column 7) presents the results, which suggest that the policy does not have a significant impact on the incumbent's chances of re-election. We also tested for alternative measures of electoral competitiveness, including the winning candidate's margin of victory, and found no significant heterogeneity on our dependent variable. Overall, these findings are inconclusive and might be explained by the fact that the two above mentioned effects are at work offsetting each other.

5.6 Impact on local welfare

Our analysis suggests that fiscal rules might tackle corruption by reducing inefficient capital expenditures. Yet, a substantial reduction in public investments might deter local economic growth and the quality of the local welfare. In this section, we find no negative effect of the DSP on the economy and on a comprehensive set of public services provided by local governments. Overall, those findings are in line with a reduction in inefficient expenditure, which does not significantly harm local welfare.[30]

5.6.1 Municipal GDP

We first test the effect of fiscal rules on per capita municipal-level GDP, which is proxied by individual's income, as declared to the Italian fiscal agency. Table 6 (column 1) reports these estimates. We report the effect of the DSP on local GDP up to 2015. Overall, we find that fiscal rules have no effect on local GDP. While a reduction in local public investments might deter economic growth, a drop in inefficient spending and rent seeking might have the reverse effect: the two effects seem to cancel each other out. However, several other explanations might be at work: i) our measure might be noisy as it includes only declared income; ii) multiplier effects might just

Although we cannot directly test this mechanism, the LFR/HFR heterogeneity can help us to distinguish between accountability and monitoring. The monitoring effect should be at work in HFRs as the DSP is *de jure* enforced. Conversely, the accountability channel is shut down, as those municipalities are not *de facto* financially constrained by the policy. Therefore, mayors in HFRs do not face a trade-off between cutting inefficient expenditures and reducing their own rent seeking. In other words, both channels, accountability and monitoring, are at work in LFR municipalities, while monitoring is the only relevant channel in HFRs. As shown in Section 6, we do not find any effect in the latter group, which implies that monitoring is not likely to be the most relevant channel. Moreover, we also test for another channel – politicians' ability. We differentiate between mayors with and without a university degree and we show that educated mayors are much more likely to reduce corruption. These analysis are not shown and are available upon request.

[30] It is important to mention that the same results also emerge if we limit the analysis to the set of cities that are subject to stricter budget constraints. These outcomes are not shown and are available upon request.

be too small; iii) fiscal sustainability might improve expectations resulting in a welfare increase; iv) or the DSP may take several years to have an effect on GDP.[31]

5.6.2 Inequality

As a complementary test, we investigate the DSP's effect on inequality. We have information on municipal income distribution, aggregated at the income bracket level: in particular, for every city and year we have data on the number of taxpayers and the total income declared for seven income groups (*i.e.* 0-10,000 euros, 10,000-15,000 euros, 15,000-26,000 euros, 26,000-55,000 euros, 55,000-75,000 euros, 75,000-120,000 euros, more than 120,000 euros).[32] To measure income inequality at the municipal level, we look at the difference between the average incomes declared in the top and bottom income brackets. The top bracket includes taxpayers with an income between 75,000–120,000 euros,[33] and we define the bottom bracket in two ways: 0–10,000 euros and 0–15,000 euros. We report these tests in columns 2-3 of Table 6. We do not find any significant effect of the DSP on this measure of inequality, which implies that, on average, income differences between the top and bottom earners have not changed.

5.6.3 Municipal services

Although we do not find any change in a set of economic outcomes, we cannot exclude that a substantial reduction in investments might worsen public services provided by the municipal government. To this aim, we collect – for the first time in the Italian context – a dataset which include all the main municipal services supplied by municipalities. This data have the advantage of measuring outcomes which directly depend upon local political activity and are financed by the municipal budget. We specifically collect data on school canteens, kindergartens, waste collection and street lightening. Overall, we do not find a substantial effect of the DSP on those outcomes (we only observe an increase in the number of children attending public kindergartens). We graphically report those results in Figure 8.

5.7 Robustness tests

In this section we list a set of additional tests, which are reported and discussed in the Online Appendix.

First, as noted above (Section 4.2), we show that the DSP does not affect corruption at the 1,000-inhabitants threshold (Appendix Table 9).

Second, as explained in Section 4.3, we document that the DSP does not reduce local police expenditures (Column 1 of Appendix Table 10).

[31] In a complementary test, not shown for the sake of space, we use provinces as the unit of analysis. We exploit the fact that after 2013 there was an increase in the share of municipalities subject to the DSP across Italian provinces. This increase was heterogeneous, as each province has a different share of municipalities with a population of 1,000–5,000. We create a dummy *Post-2013*, equal to 1 after 2013, and a continuous time-invariant variable (*Share*) scaled from 0 to 1, which measures the share of municipalities with 1,000–5,000 inhabitants. We conduct the DID analysis studying the interaction between the indicators *Post 2013* and *Share*. We consider as dependent variables some macro-level indicators expressed in per-capita terms: GDP, the log of the total number of employed individuals and the total number of firms. We find a null effect on these different outcomes.

[32] The dataset used for this analysis is the set of yearly "Dichiarazioni fiscali", provided by the Italian Ministry of the Economy.

[33] The very top bracket includes incomes over 120,000 euros, which we do not consider as very few municipalities in our sample report individuals declaring income above this threshold.

Third, as discussed in Section 4.3, we show the lack of differential trends on firms' accusations charges across LFRs and HFRs (Appendix Figure 12).

Fourth, we then report our main findings in three ways: i) estimated using a difference-in-discontinuity approach (column 2 of Appendix Table 10); ii) dropping regions with a special statute (column 3 of Appendix Table 10); and iii) dropping from the sample (in the post-electoral periods) municipalities that held elections in 2013 and 2014, to account for the incidence of gender quotas (column 4-5 of Table 10). Our findings are confirmed in all cases.

Fifth, column 6 of Appendix Table 10, tests for displacement effects in neighboring towns, checking whether corruption spillover in cities sharing a border with treated town (we find a null effect).

Sixth, columns 8 and 9 of Appendix Table 10, test for an alternative explanation of our findings, i.e. that corrupt firms are moving from areas with binding fiscal rules (in LFRs) to areas without binding ones (in HFRs): to this aim, we replicate our findings dropping municipalities in LFRs neighboring a HFR.

Finally, column 10 of Appendix Table 10, presents a placebo test in which we study the impact of the reform on non-corruption crimes, expressed in per-capita terms. A null effect emerges.

6 Effects in high funds regions (HFRs)

Our analysis has so far focused on LFRs in which fiscal rules are binding as they receive ordinary amounts of EU funds. In this section we focus on HFRs.

We start by reporting the effects of the DSP on municipalities in these regions in terms of corruption and municipal budget. Figure 9 reports our findings based on model 1. First, we do not observe any effect on corruption. Second, we do not find any change in spending both in terms of capital and current expenditure. Third, we find a weak decrease in the property tax rate similarly to the case of LFRs. Those findings are reported in Table 7 with a 2,500 bandwidth.

Overall, fiscal rules do not affect corruption in this area, as local politicians are not facing a strict budget constraint. This interpretation is confirmed by the fact that almost all municipalities in HFRs respect the requirements of the DSP. Indeed, according the Italian Ministry of Interior (with a decree issued on 28 September 2015), only 60 Italian municipalities (out of about 6,000 under fiscal rules) were to receive sanctions for non-compliance with the DSP in 2013 and 2014.[34] In other words, municipalities in HFRs did not need to cut expenditure or increase taxes to respect fiscal rules, as they were receiving additional transfers from the EU (as explained in Section 3.2).

To provide quantitative evidence of this interpretation, i.e. to the mediating role of the European funds, we consider the entire sample of both LFRs and HFRs, and we analyse the differential impact of fiscal rules depending on i) the amount of funds received by province in which each municipality is located and on ii) the macroarea, *i.e.* LFRs or HFRs, in which the municipality is located.[35]

[34] Specifically, 22 municipalities are located in LFRs and 38 in HFRs: `https://dait.interno.gov.it/documenti/decreto_fl_28-09-2015-01_0.pdf`.

[35] For the analysis in which we use the amount of funds, we prefer the provincial to the municipal allocation, as EU fund allocation at the municipal level would be endogenous: each region is in charge of allocating funds among local governments, and mayors in municipalities with the DSP might have differential incentives to apply for EU funds.

Table 8 shows the results of these triple-differences analyses with a 2,500-inhabitant bandwidth: *Post-reform (T)*Treatment group (S)* captures the DID estimator (i.e. being in a treated municipality after 2013), while the interaction term *Post-reform (T)*Treatment group (S)*interaction*, represents the differential impact of the policy depending on the transfers received by each province (column 1) and on being located in a LFR (column 2).[36]

First, the amount of European funds spent locally seems to modify the effect of the policy: DSP's beneficial impact on corruption weakens as provincial transfers increase. This result suggests that European transfers offset the positive impact of fiscal rules on corruption levels and may facilitate the emergence of corruption-related phenomena. Second, as expected, the negative impact of fiscal rules on corruption only emerges in LFRs as the interaction term is negative and significant and the DiD estimator is not.[37] These outputs are consistent with the findings of De Angelis et al., (2018), who show that EU transfers increase corruption in Southern Italy.

An alternative approach to validate this mechanism could be to focus only on cities in HFRs that receive limited amounts of EU funds. This is problematic for two reasons: i) as mentioned above, the amount of funds assigned to each municipality is determined by a negotiation with the regional government, therefore this is not exogenously set (differently from the regional amount); ii) only 78 municipalities in our sample of HFRs (i.e. 8%) receive less EU funds than the average town in LFRs: this implies that only very few municipalities in HFRs receive small amounts of EU funds. Nevertheless, if we run our main analysis restricting the sample to these 78 municipalities, we estimate a negative coefficient of -0.06 (p-value=0.34). This coefficient is very similar to the ones in our main analysis in Table 2. The lack of statistical significance can be explained by the fact that this test is underpowered due to the small number of observations (78 municipalities for a total of 677 observations).

7 Discussion and final remarks

In this paper, we study the impact of fiscal austerity on corruption. To do so, we exploit the extension of a specific set of fiscal rules – the Domestic Stability Pact – to Italian municipalities with a population below 5,000 that occurred in 2013. We employ a 'local' Difference-in-Differences estimation to study how the policy affected local corruption levels and budgetary outcomes. We find that the DSP produced a substantial decrease in both corruption levels per capita and in the intensity of corruption over total capital expenditures, interpreting the latter as a measure of 'corruption-proofness' of local investments. These effects are driven by a reduction in capital expenditures, and emerge only in areas in which the DSP was fully binding. Indeed, we do not observe a reduction in corruption among municipalities eligible to receive extra transfers from the EU, which are *de facto* less constrained by the DSP: in these municipalities no reductions in capital expenditures are observed. Finally, we find that local welfare is not significantly affected by the imposition of a budget constraint. Importantly, this result only holds for the short-medium run,

[36] European funds are measured as the total amount of provincial transfers per capita (in thousands of euros), spent in 2013–2015. Note that we are focusing on when the EU funds were *spent*. All EU funds in our analysis, i.e. those from the 2007–2013 budget , were *assigned* to each region by the end of 2013.

[37] This second result also helps us in excluding the relevance of other policies varying at the 5,000 population threshold in 2013 (*e.g.* gender quotas). In particular the fact that the triple interaction term is significant suggests that these (alternative) policies are not driving our main results as they are likely to affect cities in LFRs and HFRs similarly: this further validates our main identifying assumptions.

while we cannot exclude repercussions in local growth for the long-run.

Our findings are timely, as they shed light on the effects on corruption of two salient and highly debated European policies: European transfers and fiscal rules, which affect thousands of European local governments. More generally, our results may be of interest for governments and international organizations committed to enforcing fiscal rules and/or anti-corruption policies.

How general are these results? We believe at least three elements are important in our context. First, Italian mayors (as explained in Section 3) can modify both the revenue and spending side of the municipal budget: therefore, their reaction to the DSP can actually vary depending on electoral incentives. In the absence of such fiscal powers, local governments response is going to be more constrained and predictable. A second caveat is that in the Italian context, fiscal rules are highly binding, as the national government can apply sanctions for non-compliance. As explained above, only about 1% of municipalities do not respect the DSP. The effect of compliance is noticeable also when looking at municipalities with a stricter budget requirement, which drive the reduction in corruption. Finally, electoral accountability feedbacks seem to determine local politicians' response to the budget shock, as shown in Table 5. This dynamics is particularly strong in this setting as our sample consists of small-medium cities where political accountability is likely to work more accurately. Overall, other types of budget shocks and local institutional arrangements, or different accountability incentives, might lead to distinct effects on corruption and local welfare. A similar reasoning applies to the time frame of our analysis which is limited to the short-medium run.

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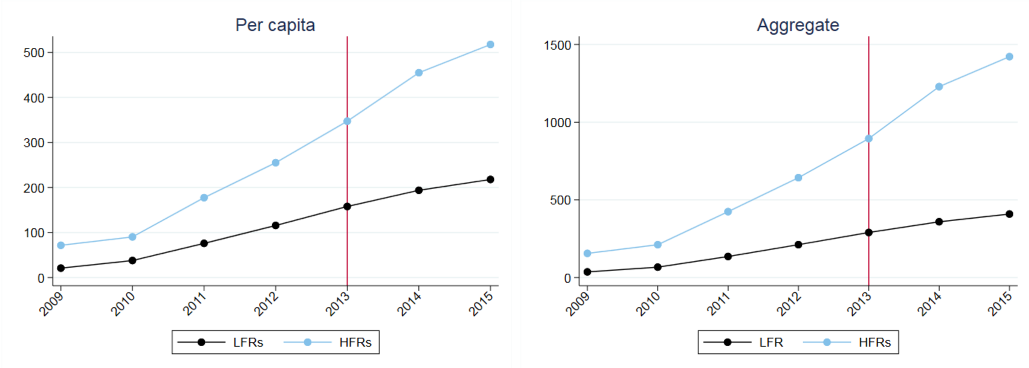
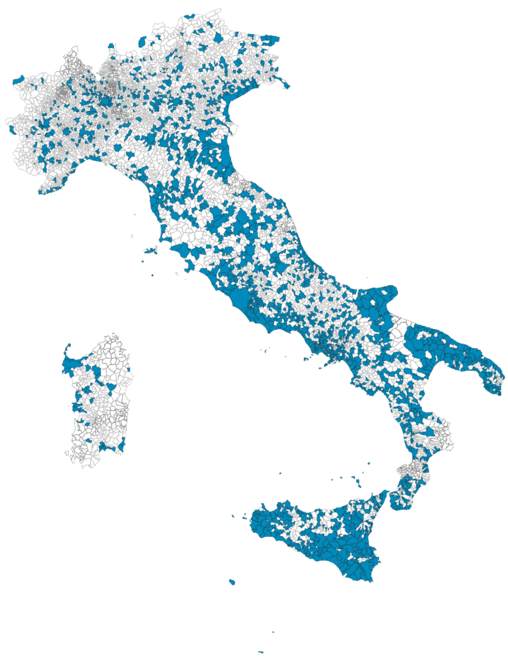
Figures

Figure 1: Evolution of European funds, 2009–2015



The plots show the amounts of European funds spent over time by LFRs and HFRs in per capita terms (left panel; measured in Euros) and aggregate terms (right panel; measured in millions of euros).

Figure 2: Corruption investigations in Italian municipalities



Municipalities with at least one corruption-related investigation in the time span under analysis (2004-2014) are highlighted in light blue. Municipalities in gray are those where no corruption episodes were recorded.

Figure 3: Aggregate corruption investigations by municipal size (population)

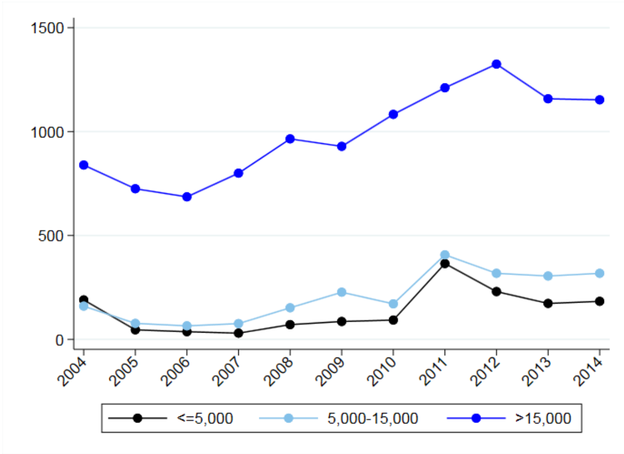
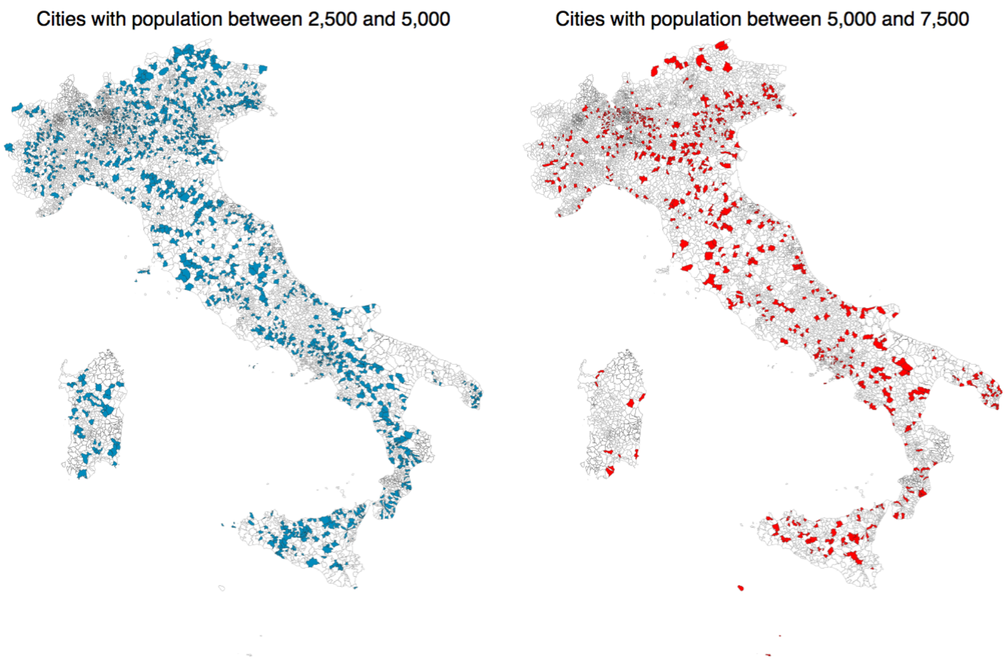
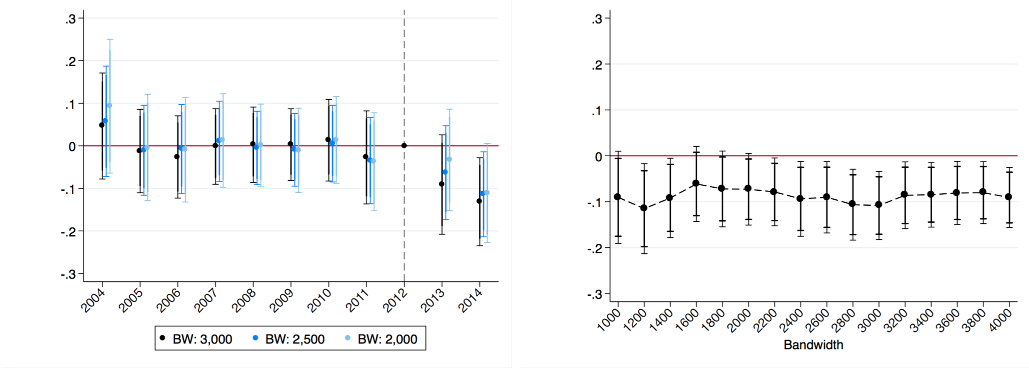


Figure 4: Geographical distribution of treatment and control



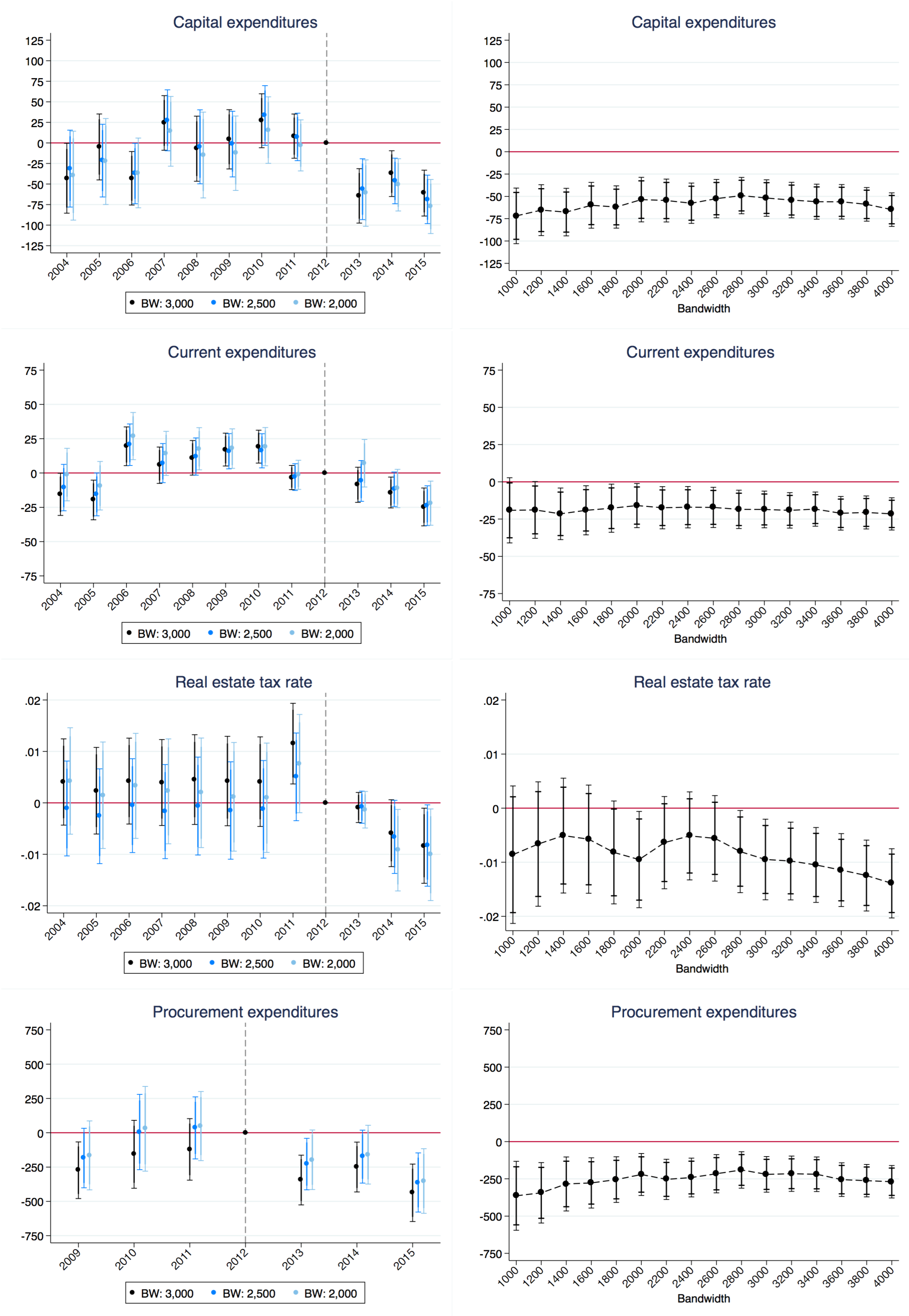
Geographical distribution of cities in the treatmet (left figure) and in the control group (right figure), using a bandwidth of 2,500 inhabitants, according to 2011 census.

Figure 5: Effect of the DSP on corruption



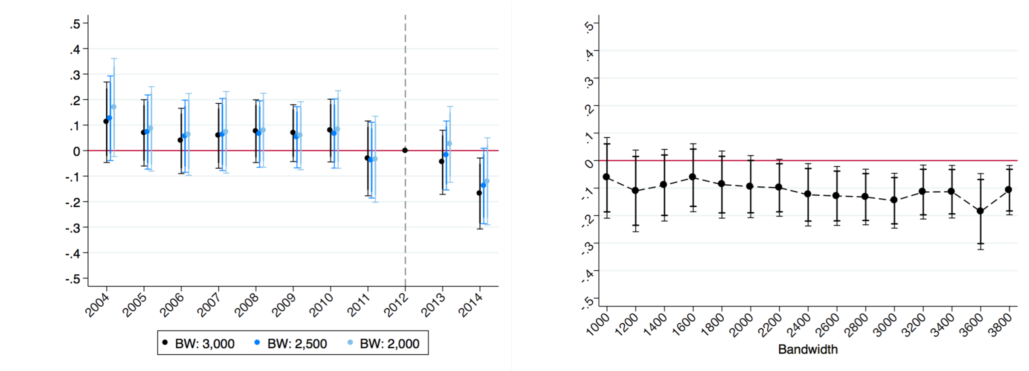
The left plot shows the outcomes of the local DID estimation for cities in LFRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID for cities in LFRs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variable is corruption investigations per 1,000 inhabitants, standardized by region group.

Figure 6: Effects of the DSP on local public finance and procurement



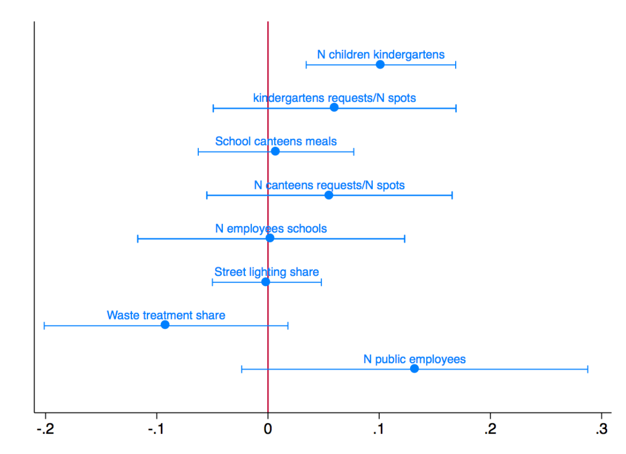
The left plot shows the outcomes of the local DID estimation for cities in LFRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID for cities in LFRs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables are the public finance and the procurement indicators, expressed in per capita terms.

Figure 7: Effect of the DSP on corruption over capital expenditures



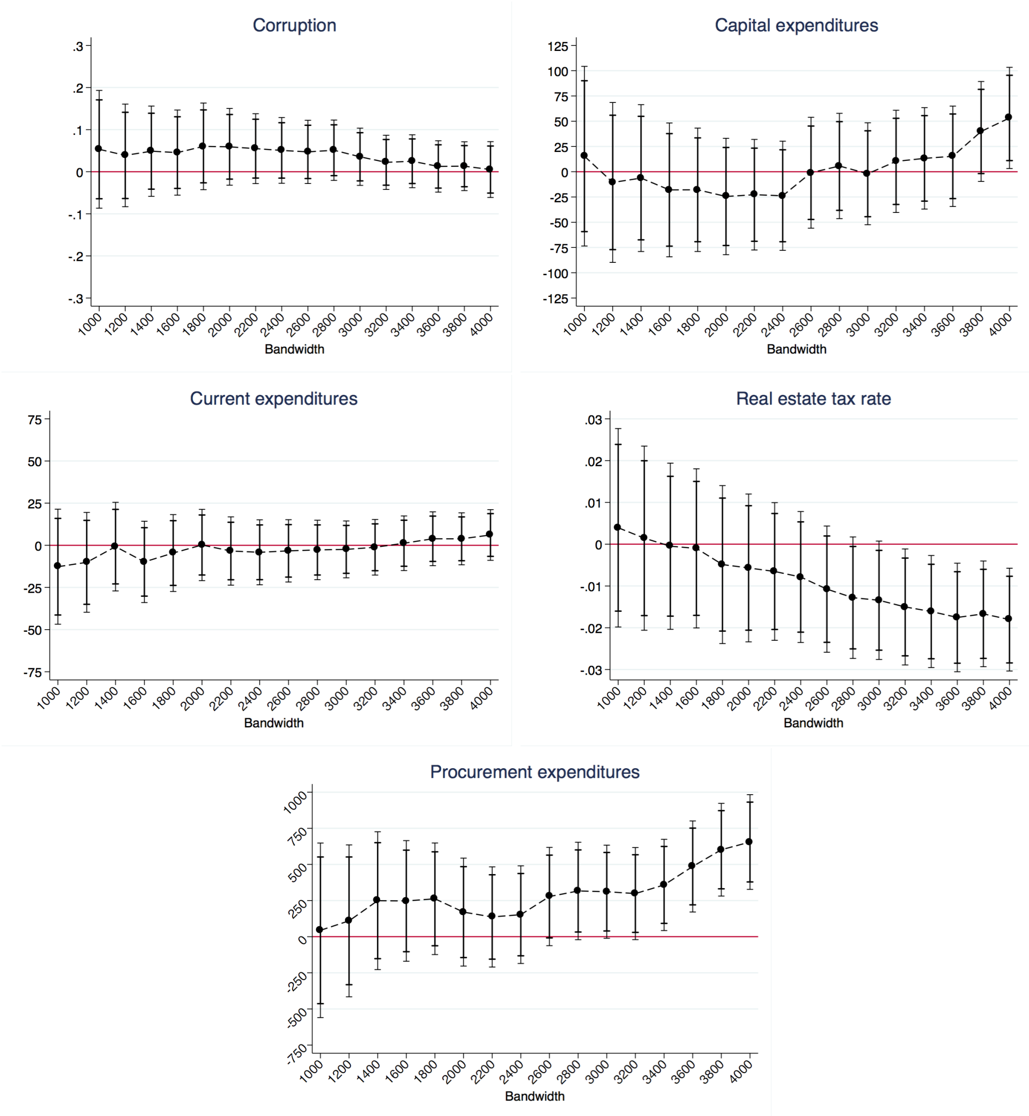
The dependent variable is corruption investigations per euros spent in investments, standardized by region group. The left plot shows the outcomes of the local DID estimation, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals.

Figure 8: Impact on municipal public services



The plot shows the impact of the introduction of the DSP on a set of municipal service (in a standardized version). Each dot is a distinct analysis and represents the DID estimator and the corresponding confidence intervals in a distinct regression according to Model 2. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors are clustered at the municipal level. *N children kindergartens* refers to the number of children attending public kindergartens; *kindergartens requests / N spots* is the share between the number of children requests to be admitted to public kindergardens and the number of available spots; *School canteens meals* is the number of meals provided by public schools; *N canteens requests / N spots* is the share between the number of students requests to be admitted to schools canteens and the number of available spots; *N employees schools* is the number of public employees in municipal schools; *Street lightening share* is the share of municipal roads (in km) covered by street lightening; *Waste management share* is the share of houses covered by waste management collection; *N public employees* is the log number of municipal public employees in the local administration.

Figure 9: Effects of the DSP in HFRs



The plots show the sensitivity analysis of the local DID for cities in HFRs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables include corruption investigations per 1,000 inhabitants, standardized by region group and the set of public finance and procurement indicators, expressed in per-capita terms.

Tables

Table 1: Descriptive statistics

	All cities	LFRs	HFRs
Panel A: corruption			
Corruption (PC)	0.015	0.0088	0.032
Corruption (capital exp.)	0.010	0.0066	0.021
Corruption (total exp.)	0.009	0.0059	0.0184
Panel B: public finance (€ per capita)			
Real estate tax rate	0.485	0.489	0.474
Current expenditures	887.6	900.2	850.3
Capital expenditures	585.1	568.5	634.4
Panel C: local procurement (€ per capita)			
Total amount	1,212.0	785.2	2,419.9
Panel D: local politics			
Electoral turnout	0.760	0.770	0.730
Term limit	0.332	0.347	0.291
Mayor university degree (1 = univ. degree or above)	0.845	0.822	0.912
Age of the mayor (years)	49.4	49.6	48.9
Gender of the mayor (1 = female)	0.109	0.128	0.058
Av. education councillors (1 = univ. degree or above)	0.537	0.487	0.677
Councillors' age (Av.)	44.39	44.84	43.11
Proportion female councillors (1 = female)	0.202	0.219	0.153
Incumbent re-election	0.419	0.434	0.372
Panel E: local growth			
Municipal GDP (per-capita)	10,721.13	12,255.7	6,399.1
Province employment (per-capita)	.407	437	335
Province N firms (per-capita)	0.076	0.083	0.059
Panel F: local Services			
N children kindergartens	28.6	35.2	10.0
kindergartens requests / N spots	73.7	1.2	479.8
School canteen meals	922.4	1,083.3	468.2
N canteens requests / N spots	0.994	0.996	0.989
N employees schools	2.9	3.4	1.9
Sport facilities	5.7	5.0	7.6
Roads (km)	25.8	22.0	36.8
Street lightening (share)	0.571	0.569	0.577
Waste management share	0.903	0.904	0.897
N public employees	9.5	8.4	12.5
Urban planning approval	0.755	0.821	0.569

Corruption (PC) is expressed in number of investigations per 1,000 inhabitants; *Corruption (exp.)* is expressed in number of investigations over capital expenditure (measured in euros). All amounts in Panel B and C are expressed in euros per capita by municipality and year. *Incumbent re-election* is a dummy variable equal to one in case the incumbent is re-elected in cities where the incumbent is not term limited. Amounts in Panel E are in millions of euros.

Table 2: Impact of DSP on corruption charges

	Corruption (PC)				Corruption (over spending)			
			(top 50)	(top 20)	Capital exp.	Total exp.	(top 50)	(top 20)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stability pact (S*T)	-0.0750	-0.0855	-0.0923	-0.305	-0.178	-0.171	-0.201	-0.572
	(0.0394)*	(0.0402)**	(0.0633)	(0.185)*	(0.0839)**	(0.0840)**	(0.112)*	(0.277)**
Dep. var. average value	0.0088	0.0088	0.0088	0.0088	0.0066	0.0059	0.0066	0.0066
Observations	17,992	17,481	8,759	3,513	17,712	17,719	8,894	3,589
Adjusted R^2	0.010	0.011	0.012	0.017	0.011	0.011	0.012	0.017
City, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1-4), the ratio between corruption and capital expenditure (expressed in logarithm) in column (5), (7), (8) and the ration between corruption and total expenditure (expressed in logarithm) in column (6). The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). In columns (3-4) and (7-8), municipalities are required to accumulate a level of surplus above the top 50% or 20% of the variable distribution. The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Impact of DSP on public finance and procurement

	Real estate tax rate	Current spending (PC)	Procurement spending (PC)	Capital spending (PC)		
					(top 50)	(top 20)
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact (S*T)	-0.00516	-17.32	-227.0	-54.95	-129.4	-229.2
	(0.00410)	(6.984)**	(65.76)***	(11.19)***	(20.22)***	(53.66)***
Dep. var. average value	0.489	900.2	785.2	568.5	568.5	568.5
Observations	19,319	18,890	10,954	18,890	9,405	3,749
Adjusted R^2	0.441	0.130	0.023	0.091	0.107	0.110
City, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500	2,500

The dependent variables are the public finance and procurement measures expressed in per-capita terms. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). In columns (5-6), municipalities are required to accumulate a level of surplus above the top 50% or 20% of the variable distribution. The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Impact on discretionary tenders and politicians productivity

	Perc. tenders <40K	Perc. amount <40K	Amount PC <40K	N council resolutions	N government resolutions
	(1)	(2)	(3)	(4)	(5)
Stability pact (S*T)	-0.0102	-0.00679	-2.832	4.424	7.656
	(0.00910)	(0.00820)	(1.084)***	(1.270)***	(3.664)**
Dep. var. average value	0.044	0.031	12.45	46.19	122.08
Observations	11,185	11,185	10,954	10,263	10,268
Adjusted R^2	0.003	0.003	0.005	0.035	0.037
City, year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500

Perc. tenders <40K captures the percentage of tenders for amounts under 40,000 euros, *Perc. amount <40K* measures the percentage of the total amount in tenders for less than 40,000 euros, and *Amount PC <40K* captures the overall tendered amount (per capita) that is lower than 40,000 euros. *N council resolutions* and *N government resolutions* capture the number of resolutions approved yearly, respectively, by the municipal council and by the municipal government. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Accountability vs. ability - Corruption

	Electoral period ($interaction_1$)		Mayor term limited ($interaction_2$)							
Dependent variable:	Corruption (PC)	Corruption (on capital exp.)	Corruption (PC)	Corruption (on capital exp.)	Corruption (PC)			Corruption (on capital exp.)		
					Pre-DSP elections	Vote margin 10%	Vote margin 15%	Pre-DSP elections	Vote margin 10%	Vote margin 15%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Stability pact (S*T)	0.0294	-0.0237	-0.125	-0.249	-0.143	-0.181	-0.160	-0.272	-0.330	-0.333
	(0.0657)	(0.125)	(0.0529)**	(0.109)**	(0.0510)***	(0.0841)**	(0.0860)*	(0.103)***	(0.174)*	(0.182)*
Stability pact (S*T)*$interaction_i$	-0.170	-0.229	0.115	0.211	0.145	0.217	0.243	0.243	0.490	0.480
	(0.0716)**	(0.132)*	(0.0636)*	(0.132)	(0.0647)**	(0.117)*	(0.105)**	(0.135)*	(0.251)*	(0.220)**
Dep. var. average value	0.0088	0.0066	0.0088	0.0066	0.0088	0.0088	0.0088	0.0066	0.0066	0.0066
Observations	17,481	17,712	17,481	17,712	15,750	5,119	7,359	16,026	5,231	7,512
Adjusted R^2	0.012	0.012	0.012	0.012	0.011	0.013	0.012	0.011	0.014	0.013
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500	2,500

The dependent variables are corruption investigations per 1,000 inhabitants (standardized) and corruption on capital expenditure (standardized). *Interaction* is a term which represents *Electoral period* (columns 1-2), which is a dummy equal to one in the electoral year and in the year before elections, and *Mayor term limited* (columns 3-11), which is a dummy equal to one if the mayor is not eligible for re-election. The specification includes municipality and year fixed effects, the distance from the population threshold, the characteristics of mayor and municipal councillors (age, education and gender). The specification also includes $S * interaction_i$, $T * interaction_i$ and $interaction_i$, which are not displayed in the table. In columns 6, 7, 9 and 10 the specification also includes election victory margin. The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Impact on local welfare

	Municipal welfare	Municipal income inequality	
	Municipal GDP (PC)	Measure 1	Measure 2
	(1)	(2)	(3)
Stability pact (S*T)	-54.48	0.00727	0.00730
	(84.84)	(0.00533)	(0.00545)
Dep. var. average value	12,255.7	11.61	11.58
Observations	19,399	19,314	19,314
Adjusted R^2	0.146	0.013	0.013
City (or province), year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500

The dependent variables are the following. Column (1): local GDP measured as the sum of individuals income, expressed in per capita terms. Columns (2) and (3): the difference between the average income declared in the last and first income brackets. The last bracket included taxpayers with an income of 75,000– 120,000 euros, and the first income bracket includes taxpayers with an income of 0–15,000 euros (Measure 1 in column 2) or 0–10,000 euros (Measure 2 in columns 3). The dependent variables are drawn from Eurostat. Province and year fixed effects are included. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of DSP in HFRs

	Corruption (PC)		Real estate tax rate	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact (S*T)	0.0356	0.0440	-0.00746	-5.893	-13.71	254.5
	(0.0366)	(0.0383)	(0.00773)	(9.487)	(27.40)	(178.2)
Dep. var. average value	0.032	0.032	0.474	850.3	634.4	2,419.9
Observations	7,104	6,650	7,682	7,156	7,156	4,243
Adjusted R^2	0.019	0.020	0.417	0.253	0.035	0.034
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500	2,500

The table shows the results for the cities in the HFRs. The dependent variables include corruption investigations per 1,000 inhabitants (standardized), and the set of public finance and procurement indicators. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: European funds

Dep. var.: Corruption (PC)	*Interaction*	
	EU funds	LFRs
	(1)	(2)
Post-reform (T)*Treatment group (S)	-0.0995	0.0676
	(0.0333)***	(0.0665)
Post-reform (T)**interaction*	-0.0735	0.00107
	(0.128)	(0.0536)
Treatment group (S)**interaction*	-0.182	0.0624
	(0.141)	(0.0835)
Post-reform (T)*Treatment group (S)**interaction*	0.303	-0.116
	(0.155)**	(0.0685)*
Dep. var. average value	0.0088	0.0088
Observations	23,881	24,131
Adjusted R^2	0.012	0.011
City, Year FE	Yes	Yes
Controls	Yes	Yes
Bandwidth	2,500	2,500

The dependent variable is corruption investigations per 1,000 inhabitants (standardized). The sample includes all Italian municipalities, both from LFRs and HFRs. "EU funds" measures the total amount of province European funds spent starting from 2013, measured in thousands Euros PC. "LFRs" is a dummy variable indicating the municipalities located in LFRs. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix (for online publication): Robustness checks and additional analysis

Effect of the DSP on corruption – 1,000-inhabitant threshold

The DSP was enforced after 2013 for municipalities with 1,000–5,000 inhabitants. Nevertheless, we only exploit the 5,000 threshold. Ideally, we could also compare municipalities right below/above the 1,000 threshold. Unfortunately, the 1,000 threshold cannot be included in our analysis for two reasons. First, about 38% of municipalities below 2,000 joined a "union of municipalities" (*Unioni di Comuni*), which are in charge of all public services and administrative functions that were previously the responsibility of individual municipalities (Law 148, September 2011). Such unions are exempt from the DSP. Second, there is little variation in our dependent variable when considering very small municipalities. For municipalities with a population of 3,000–7,000, we observe an average of 0.07 corruption charges per year. There are only 0.007 corruption charges per year in municipalities with a population below 1,000.

In Appendix Table 9, we replicate our analysis exploiting the 1,000 threshold. Specifically, we compare municipalities with a population below/above 1,000 before/after the introduction of the DSP in 2013. As expected, we do not find any effect of the DSP on public spending or corruption charges when considering the 1,000 threshold.

Effect of the DSP on local police expenditures

An alternative explanation for our findings is that the DSP pushes local politicians to cut spending on local police, which in turn reduces the probability that corrupt officials are detected by the authorities. This explanation is unlikely for two reasons. First, the municipal police is not in charge of pursuing corruption-related crimes, which are investigated by a specific branch of the national police. Second, the results in Appendix Table 10 (column 1) demonstrate that the DSP did not generate a decrease in spending on local police.

Analysis with a difference-in-discontinuity methodology

In this section we conduct the main analysis on corruption using the difference-in-discontinuity methodology in order to check whether the main results are also robust to the application of this method. This methodology has been used in many recent studies (e.g. Campa, 2011; Grembi et al., 2016) and it is based on comparing outcomes before and after the reform for municipalities around the population threshold.[38] Column 2 of Appendix Table 10 shows this test. The negative effect of DSP on corruption also emerges in this case, and the magnitude of the estimate is similar to the one estimated using the local DID methodology. As explained in the empirical strategy section, we use a local DID methodology for two reasons. First, it is more precise because it allows us to

[38] Following Grembi et al. (2016), the empirical model to be estimated is as follows:

$$y_{it} = \beta_0 + \beta_1 P_i^* + \beta_2 S_{it}(\gamma_0 + \gamma_1 P_i^*) + \beta_3 T_t[\delta_0 + \delta_1 P_i^* + S_{it}(\zeta_0 + \zeta_1 P_i^*)] + \eta_t + \xi_p + \epsilon_{it} \tag{3}$$

Where P_i^*, S_{it} and T_t are defined as in Model 1, and η_t represents year fixed effects and ξ_p province fixed effects. The difference-in-discontinuity indicator is the interaction term between S_{it} and T_t, captured by the coefficient ζ_0. This coefficient is estimated by local linear regression as it is estimated for the sub-sample of observations in the interval $P_i^* \in [-h; +h]$, where the optimal bandwidth h is calculated following Calonico et al., (2014).

include municipality fixed effects, controlling for time-invariant municipal characteristics. Second, it facilitates a more standard evaluation of the absence of pre-trend effects.

Alternative explanation: corruption on the move

An alternative explanation of our findings could be that corruption is decreasing in LFRs, not because of a change in politicians' behaviors, but due to corruption-prone firms moving their business to areas not affected by public spending cuts. In other words, such firms might be shifting their interests from LFRs to HFRs. This explanation is unlikely in the Italian framework, in which competition in public procurements markets is relatively low and typically local firms are the ones successfully applying to public procurements issued by small/medium size municipalities (Branzoli and Decarolis, 2015). Nevertheless, we provide some quantitative evidence to discard this explanation: we suggest that the cost of moving to another area is increasing in distance, whereby firms located in LFRs neighboring a HFR should have a lower cost of moving their business to municipalities not binded by fiscal rules. This implies that our results might be driven by municipalities in LFRs geographically close to HFRs. In columns 8 and 9 of Appendix Table 10, we replicate our findings dropping observations from provinces in LFRs which neighbor a HFR (column 8) or from LFRs which neighbor a HFR (column 9). Our findings are confirmed as the estimated coefficients are very similar to the ones in the main analysis.

Placebo test: impact on non-corruption crimes

In order to show that the results we obtain on corruption-related crimes is not due to an increase in the detection activity by the public authority, we test the effect on non-corruption related crimes. In particular, we use as dependent variable the number of committed infractions on non-corruption crimes recorded yearly in every municipality, expressed in per-capita terms (we draw this data from the Italian Ministry of the Interior). Importantly, this data covers the period 2004-2013, therefore we only have one year after the reform for this specific analysis. The main results are reported in column 10 of Appendix Table 10 and show that the reform did not impact the number of non-corruption crimes, suggesting that the detection activity does not change after the introduction of fiscal rules.

Other tests

In this section we briefly recall three additional robustness tests discussed in Sections 2 and 4.

First, the application of the DSP was completely enforced for ordinary Italian regions, but some of the special regions could decide to what extend adhere to this policy. To check the robustness of our findings, we re-run the main specification, excluding the special regions. The results, reported in Columns 3 of Appendix Table 10, show that the negative impact on corruption is still sizeable and significant.

Second, a potential threat to this identification strategy may come from the introduction of gender quotas. This policy was adopted in 2013 in municipalities with more than 5,000 inhabitants and it represents a double-preference voting, conditioned on gender. However, a reinforced version of this policy was extended in 2014 to towns with populations over 3,000, which states that elected politicians of either gender cannot have less than 40% of municipal seats. In order to check for the robustness of our findings, we estimate the main model excluding towns that held an election in 2013 and 2014 in the post-electoral periods. We show this specification in Appendix Table 10 (columns 4-5), which suggests that this policy overlap is not a problem for our results. We also run this robustness test for Capital expenditures and the results are similar to the main outcomes.

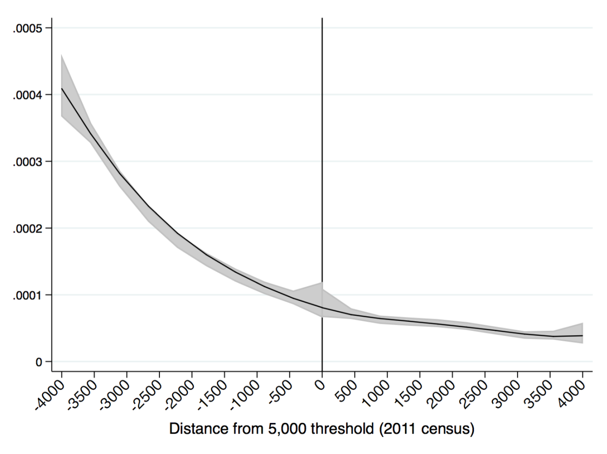
Third, one possible concern may be that the DSP could affect firms' propensity to charge their own competitors due to the reduction in procurement spending. This mechanism should be at work only in LFRs. Since data on firms' appeals related to public procurement are not available at the municipal level, we use regional-level data that differentiates between LFRs and HFRs to rule out this confounding story. Appendix Figure 12 plots the total number of appeals to courts over time for LFRs and HFRs: the trends in accusations are parallel. This suggests that the introduction of fiscal rules in LFRs did not modify firms' charging behaviour there, compared to HFRs.

Fourth, column 6 of Appendix Table 10, tests for displacement effects in neighbouring towns. The scope of this test is to investigate possible displacement effects, whereby the drop in corruption among treated cities might be replaced by a growth in neighbouring towns. In this case, the treatment group includes municipalities neighbouring those in the interval 1,000–5,000 inhabitants, while the control group includes all other municipalities (except for those between 1,000 and 5,000 inhabitants). We do not observe any effect of the DSP on neighbouring cities, which suggests a net decrease rather than a corruption displacement.[39]

Finally, a last concern relates our choice of focusing on corruption investigations rather than convictions. The choice is motivated by two reasons: i) as explained in the main text, the time span between an investigation and the actual crime is much shorter compared to a conviction, which could take place several years later; ii) conviction data are available only at the regional level. Our results might be biased if conviction rates differentially change across cities with or without fiscal rules: for instance, if judges strategically modify their efforts. Similarly to the case of firms' accusations, this effect should take place especially in LFRs. In Appendix Figure 13, we exploit the LFRs – HFRs heterogeneity to show that the conviction rate related to corruption crimes does not seem to change across the two groups before/after 2013.

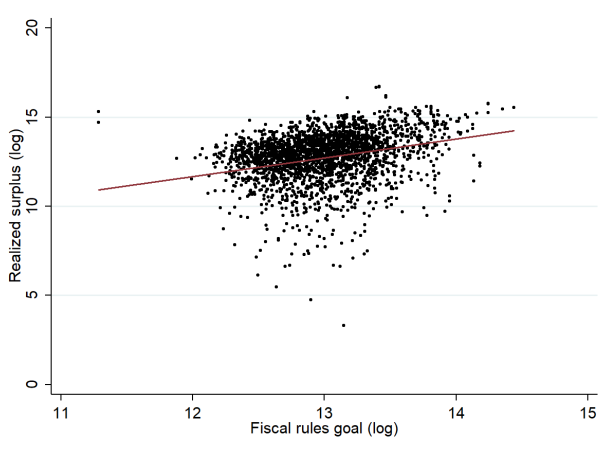
[39]To provide further evidence on this, we conduct two additional tests. In particular, we run the main specification using as control group i) only cities neighbouring treated municipalities (in the interval 5,000-7,500 inhabitants) and ii) only cities that are not neighbours of treated municipalities (in the interval 5,000-7,500 inhabitants): in case of displacement effect, we would expect only the coefficient in the former specification to be negative. Instead, the effect is similar in the two tests, suggesting the absence of corruption displacement. These tests are not shown and are available upon request.

Figure 10: McCrary test – density around the 5,000 population threshold



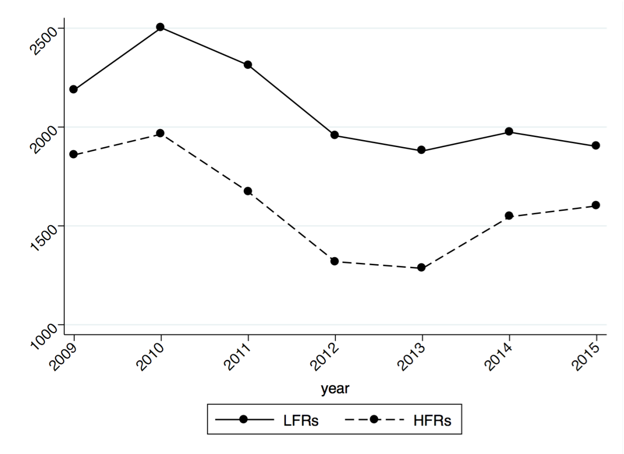
The plot shows the McCrary test conducted using the population data of the last census of 2011. The population threshold studied is the one of 5,000 inhabitants.

Figure 11: Stability pact – realized and targeted surplus (LFRs)



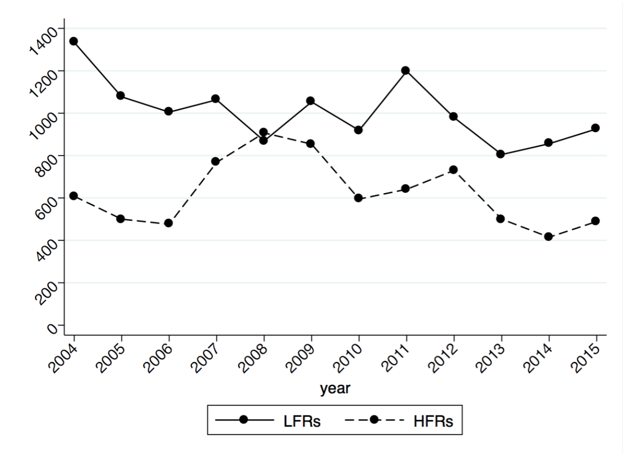
The plot shows the relationship between the amount of surplus that the DSP requires to accumulate in logarithm ("Fiscal rules goal") and the surplus actually accumulated by Italian municipalities in logarithm ("Realized surplus"). The sample includes towns in the LFRs with a population of 2,500–5,000 and covers the years 2013–2015.

Figure 12: Appeals to courts over time (filed)



This plot shows the total number of appeals to courts filed over time. *Source*: Italian Ministry of Justice.

Figure 13: Number of convictions on corruption crimes



This plot shows the total number of convictions on corruption crimes over time, dividing between LFRs and HFRs. *Source*: Italian Institute of Statistics (ISTAT).

Table 9: Effect of the DSP on corruption – 1,000 inhabitant threshold

	Corruption (PC)	Real estate tax rate	Current spending (PC)	Capital spending (PC)
	(1)	(2)	(3)	(4)
Stability pact (S*T)	0.00310	0.00684	-4.448	-29.77
	(0.0278)	(0.00411)*	(10.55)	(29.48)
Dep. var.. average value	0.0088	0.489	900.2	568.5
Observations	16,450	18,028	17,704	17,704
Adjusted R^2	0.005	0.554	0.140	0.025
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	1,000	1,000	1,000	1,000

The dependent variables include corruption investigations per 1,000 inhabitants (standardized) as well as the set of local public finance indicators (in per capita terms). The local DID analysis relies on the 1,000-inhabitant threshold. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Effect of DSP on single corruption charges

	Strict corruption	Graft	Malfeasance	Embezzlement
	(1)	(2)	(3)	(4)
Stability pact (S*T)	0.0118	-0.0111	-0.0947	-0.0624
	(0.0181)	(0.0355)	(0.0460)**	(0.0442)
Dep. var. average value	0.0012	0.0004	0.0056	0.0017
Observations	17,481	17,481	17,481	17,481
Adjusted R^2	0.004	0.001	0.008	0.004
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500

The dependent variables include specific corruption charges, according to SDI classification. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). The sample only includes municipalities located in LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Robustness checks

	Police spending	Diff-in-Disc	Excluding special regions	Gender quotas		Neighbour cities	Re-election probability	Excluding cities in the border with HFRs (Province)	(Region)	Other crimes
	Police spending (PC)	Corruption (PC)	Corruption (PC)	Corruption (PC)	Capital spending (PC)	Corruption (PC)	Incumbent re-elected	Corruption (PC)	Corruption (PC)	Non-corruption crimes (PC)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Stability pact (S^*T)	-1.124	-0.151	-0.0960	-0.0807	-64.70	-0.0275	0.0267	-0.0697	-0.0769	-0.0000241
	(0.769)	(0.0698)**	(0.0431)**	(0.0369)**	(13.48)***	(0.0479)	(0.0718)	(0.0387)*	(0.0380)**	(0.000553)
Dep. var. average value	35.05	0.0088	0.0088	0.0088	568.5	0.0088	.351	0.0088	0.0088	0.037
Observations	17,364	10,924	15,806	17,381	17,772	7,671	3,304	15,523	16,821	15,927
Adjusted R^2	0.027	0.049	0.011	0.011	0. 70	0.011	0.255	0.010	0.011	0.058
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	1,707	2,500	2,500	2,500	-	2,500	2,500	2,500	2,500

In column (1) the dependent variable is spending in local police per-capita. In columns (2) the analysis is structured as a difference-in-discontinuity estimation, following Grembi et al. (2016), see Section 6.3 for details. In columns (3) Italian special regions have been excluded from the sample. Municipalities that voted in 2013 and 2014 are excluded from the analysis of columns (4) and (5). Column (6) contains the analysis conducted on only cities with population outside the interval 1,000-5,000 inhabitants, with treatment being cities that share a border with cities in the treatment group of the standard analysis. In column (7) the dependent variable is a dummy equal to one if the incumbent mayor is re-elected. The sample is limited to electoral years and mayors who are eligible for re-election. In columns (10) the dependent variable is the number of committed infractions for non-corruption crimes, expressed in per-capita terms. The sample only includes municipalities located in LFRs, except for columns (8) and (9), that include municipalities located in HFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Data sources

Data	Source
Corruption data	SDI (*Sistema d'indagine*) - Italian Ministry of the Interior
Municipal balance sheets	*Certificati consuntivi* - Italian Ministry of the Interior `https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4`
European funds	Department for cohesion policy at the Presidency of the Council of Ministers - opencoesione.gov `https://opencoesione.gov.it/en/spesa-certificata/`
Public procurement data	Telemat
Local GDP and inequality	Italian Ministry of the Economy `https://www1.finanze.gov.it/finanze3/pagina_dichiarazioni/dichiarazioni.php`
Data on local elections	*Archivio storico delle elezioni* - Italian Ministry of the Interior `https://elezionistorico.interno.gov.it/`
Data on local governments	*Anagrafe degli amministratori locali e regionali* - Italian Ministry of the Interior `https://dait.interno.gov.it/elezioni/anagrafe-amministratori`
Data on provincial welfare	*Eurostat* `https://ec.europa.eu/eurostat/web/rural-development/data`
Municipal services	*Certificati consuntivi* - Italian Ministry of the Interior `https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4`

Appendix 2 (for online publication): Text analysis on newspaper articles

In this appendix we discuss in detail the text analysis methods we applied to select the articles dealing with corruption and to identify the specific corruptive behaviour. Following Giommoni (2017), we apply an automatic two-steps procedure:

1. The first step consists in the identification of the articles dealing with corruption cases that involve local politicians. We rely on the the main Italian press agency, *ANSA*. The selection of articles proceeds as follows:

    - *Articles' screening:* Through the portal Factiva, we screened the title and the first paragraph of the articles released by ANSA in the time span 1999-2014. We relied on a set of corruption-related keywords to select and download the articles containing these keywords.[40]
    - *Geo-localization:* We geo-localized selected articles based on places mentioned in the text. In particular, the text of the articles have a standard structure and the first word is usually the name of the place where the piece of news comes from. We used the province as unit of analysis and we traced back all the places mentioned to the corresponding province. We focus on all Italian provinces.
    - *Politicians' identification:* We further screened selected articles identifying the names of local politicians within the text. We consider all politicians in charge between 1999-2014, at any administrative level, *i.e.* regions, provinces and municipalities (this information comes from *Anagrafe degli Amministratori Locali e Regionali*-Italian *Ministry of Internal Affaires*). To identify the name of a local politician in the text of the articles she/he had to be in charge in the place where the article was geo-localized and in the period when the article was released.

2. The second step consists in the identification of the specific criminal behaviour discussed in the articles. We rely on a set of keywords to extract this piece of information and we screen articles' text. In particular, we classify the articles in six different areas: procurement, fraud, public hiring, refund usage, construction crimes and theft/embezzlement. The presence of the corresponding keywords signal that the article is dealing with a specific topic.[41]

[40] We use a python code to perform the extraction from the portal Factiva. Moreover, in the extraction, we select all the available sources for ANSA. We use the roots of the following keywords (in Italian) as well as related synonyms: accuse, arrest, bribe, convict, corruption, detention, embezzlement, graft, hearing, incarcerate, interrogate, investigate, judiciary, malfeasance, prosecutor, scandal, sentence, testify, trial.

[41] We use the roots of the following keywords (in Italian): appointment, authorization, buildable, construction, public contract, damage, fraud, funds, hiring, investment, license, loan, procurement, public works, recommendation, reimbursement, subcontract, supply contract, tender, urban planning.