Fiscal Rules, Corruption and Electoral Accountability

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Abstract

As corruption mostly takes place through the misuse of public spending, it is crucial to understand how policies limiting the spending capacity of local governments may affect corruption. We study the extension of fiscal rules to small Italian municipalities. First, we find a decrease in both corruption charges and corruption charges per euro spent. This effect emerges only in areas in which fiscal rules put a binding cap on municipal capital expenditures. Second, the reduction in corruption is linked to accountability incentives as it emerges mostly in pre-electoral years and for re-eligible mayors. Third, we do not find any meaningful impact on local public goods or living standards. Overall, our findings suggest that fiscal rules together with electoral incentives might reduce rent-seeking through lower public spending without depressing local welfare.

Keywords: corruption, fiscal rules, elections, accountability.

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1 Introduction

Worldwide, trillions of dollars of public money are lost in corrupt every year (Transparency International, 2018). Corruption is a symptom of weak institutions and is perceived as prevalent also in high-income countries, like European ones. In 2022, 68% of Europeans believe that corruption is widespread in their country and 34% of companies in the EU see corruption as a serious problem.[1] Corruption is a main obstacle to development (Rose-Ackerman, 1999) and an important cause of distrust towards democratic institutions (Solé-Ollé and Sorribas-Navarro, 2018; Daniele et al., 2023). As corruption generally takes place through the misuse of public spending (Bandiera et al., 2009), it is crucial to understand how policies limiting the fiscal space might affect rent-seeking. Will politicians reduce their rent-seeking or will they shrink important public goods provision? And how does electoral accountability play a role?

Following the Great Recession, policies setting a budget constraint, i.e. fiscal rules, have become a common policy tool, often promoted by international organizations, due to the substantial rise in public debts. Fiscal rules have been for instance adopted by the European Union to constrain public spending at the local level. Such 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 (Rodden, 2002). As at the end of 2021, the International Monetary Fund (IMF) lists 105 countries that have adopted local, national or supra-national fiscal rules.[2]

While economists have focused on the macroeconomic consequences of fiscal rules, we lack evidence on how politicians change their rent-seeking in reaction to this new budget constrain. This is a relevant question as corruption can be considered a form of "wasteful spending", which comes at the expense of public goods' spending. When politicians face a hard budget constraint, they have to decide whether to reduce their rents or instead cut public goods and services provision. In this trade-off, corruption might decrease when politicians face greater personal consequences, i.e. when they can be re-elected; or when there is greater scrutiny on their performance, i.e. before elections. In other words, electoral accountability might give the politician a stronger incentive to substitute away from corruption towards performance.

In this paper we explore this setting studying Italian municipalities which are subject to fiscal rules. We find that the introduction of fiscal rules, the Domestic Stability Pact (henceforth 'DSP'), led to a reduction in corruption investigations driven by electoral accountability.

[1] European Commission: link.

[2] IMF: link.

The DSP is a set of fiscal rules promoted by the European Commission and adopted by the Italian government that constrains 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. 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,[3] we employ a Difference-in-Differences estimation strategy comparing municipalities below and above 5,000 inhabitants, before and after 2013, to test whether being subject to the DSP affects corruption charges and budgetary outcomes. However, as towns of different sizes may be affected by other policies 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. Our outcome variables include (not publicly available) data on corruption investigations in each municipality-year. This data represents the best available measure of corruption in this context and the way they are collected is unlikely to be influenced by the introduction of fiscal rules (as discussed later in the paper).

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 (hereafter called 'Fiscally Unconstrained Regions'). Since expenditure financed by these transfers did not count as expenditure targeted by the DSP, capital expenditure in towns located within 'Fiscally Unconstrained Regions' was much less affected by fiscal rules. Indeed, in these areas, the DSP did not affect public spending, in turn, leaving unaffected corruption levels.

Conversely, when looking at the remaining group of regions, fully affected by fiscal rules (hereafter called 'Fiscally Constrained Regions'), we find a decrease in corruption investigations per capita and per euro spent. The drop in corruption rates is linked to budgetary changes, and plausibly triggered by them. We document that municipalities in 'Fiscally Constrained Regions' 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 (Brierley, 2020; Mauro, 1995; Gallego et al., 2021). The results in 'Fiscally Constrained Regions' are not just due to a mechanical decrease in public expenditure, as

[3]The analysis stops in 2015 as: i) corruption data are not available afterwards; ii) after 2015, a new system of local public deficit control has entered into force.

we also observe a decrease in corruption charges per euro of public spending, 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.

As mentioned above, electoral accountability might explain these findings. This is plausible if fiscal rules 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 five-year long electoral schedule: this staggered timing of municipal elections is due to historical reasons and provides exogenous variation in the electoral cycle (e.g. Daniele, Dipoppa 2017). We find that 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. Both tests suggest that, under fiscal rules, accountability incentives lead local politicians to reduce rent-seeking.

Overall, these findings might imply a trade-off between a beneficial effect of budget constraints on corruption and a drop in potentially welfare-enhancing public investments. As local politics retains a crucial role in the provision of local public goods (e.g. Meriläinen and Tukiainen, 2019), fiscal rules might affect them (Carreri and Martinez, 2022), and in turn, reduce living standards. Although we cannot observe the universe of local public goods provided by local governments, we test whether the DSP affected GDP (measured by aggregate taxable income), inequality and a newly collected set of municipal services (i.e. waste management, kindergartens, police, school canteens and street lighting). We do not find any effect on these measures.

The paper is organized as follows. Section 2 states our hypothesis and the contribution to the literature. In section 3, we describe the institutional background and the data used in our analysis. Section 4 discusses the empirical strategy and Section 5 shows our main results. In Section 6 we provide some final remarks.

2 Fiscal Rules and Corruption

Fiscal rules are guidelines or regulations that governments set for themselves to promote fiscal discipline and ensure the sustainability of public finances. These rules often involve specific targets

for budgetary outcomes, such as limits on budget deficits, public debt levels, or spending growth rates.

Fiscal rules emerged as a central topic in the public discourse, particularly in the 1990s following the establishment of the European Monetary Union (EMU). The creation of the EMU brought about a heightened focus on fiscal discipline, extending beyond Europe to encompass global conversations on fiscal governance and macroeconomic stability. A large literature has therefore studied the effectiveness of fiscal rules in achieving their intended outcomes (Alesina and Perotti, 1995; Alesina and Perotti, 1996) and on how this can vary depending on several factors, including the design, enforcement mechanisms, and political commitment to adhering to the rules (Hallerberg and Von Hagen, 1997; Hallerberg et al., 2007). Fiscal rules have not only been implemented at the national level but also increasingly at the local level, leading to a growing body of research examining their effectiveness in local contexts (Grembi et al., 2016; Heinemann et al., 2018; Asatryan et al., 2015).[4]

We take a different approach as our goal is to study how these budget constraints may affect corruption through a change in public 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. While previous studies have primarily examined institutional heterogeneity to explain the effectiveness of fiscal rules (e.g. Martin and Vanberg, 2013), we propose that the impact of fiscal rules on rent-seeking behavior is predominantly contingent on electoral accountability. We argue that when fiscal rules are accompanied by strong electoral accountability mechanisms, the deterrent effect on rent-seeking activities is likely to be more pronounced.

Budget constraints might pressure politicians to reduce rent-seeking due to an accountability motive: As corruption comes at the expense of a better public goods' provision, fiscal rules increase the relative cost of corruption. 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 public goods 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.

[4]Moreover, two recent papers study the 2013 DSP reform to evaluate the effects on distributional policies (Alpino et al., 2020) and on local education spending (Pavese and Rubolino, 2021), while Gamalerio and Trombetta, (2022) study fiscal rules introduction in 2001 and they find a negative effect on politicians' education levels.

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 even 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 (e.g. 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 (e.g. Pereria et al., 2009). Overall, it does remain an empirical question whether and how public budget constraints affect corruption.

This line of research is particularly relevant for scholars studying corruption: while previous studies have found substantial 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; Ash et al., 2020). 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.[5] Conversely, in this study, we investigate a policy which might affect corruption without incurring in additional implementation costs and with little scope for manipulation.

By studying the implementation of fiscal rules at the local level, we also relate to the debate on the effects of decentralization on public goods and services delivery, and its relation to elite caputre (Brollo et al., 2013 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: The empirical evidence is mixed (e.g. Salinas and Solé-Ollé, 2018; Grossman et al., 2017; Rodden and Wibbels, 2019), in line with the relevance of two distinct predictions. 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.

[5]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).

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.[6]

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 of the DSP to towns with population in the 1,000-5,000 range, that occurred in 2013.[7] 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 (Bonfatti and Forni, 2017). We report a brief history of Italian fiscal rules in Online Appendix 5.[8]

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.[9]

In the period we consider (2004–2015), the DSP imposed restrictions on accrual-based current expenditure and actual capital expenditure (for details, see Chiades and Mengotto, 2013). The DSP established that, for each municipality and year, the overall budget balance had to be proportional

[6] See *Legge* n. 448, 1998 which first introduced the DSP in Italy.

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

[8] The DSP uniformly applied to ordinary-statute regions (15 out of 20), as well as to Sicily and Sardinia, which have limited autonomy in terms of public finance despite having a special statute, as stated in *Legge* n. 228, 2012 (article 1). This is also reported in Parliamentary commissions documents (link1, link2) and reports of the Italian Court of Audits (link1, link2, link3). Therefore, we always exclude from the sample the three remaining special-statute regions: Valle d'Aosta, Trentino-Alto Adige and Friuli-Venezia Giulia.

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

to a (moving) average of balances obtained in previous years in the same municipality.[10] 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. 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).[11] 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).

3.2 European funds

EU funds for investment programs were excluded from the DSP restrictions. Specifically, the exclusion concerned the share directly financed by European funds, while fiscal rules still applied to local co-financing.[12] 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).[13] The policy is implemented by national and regional governments in partnership with the European Commission. Importantly, six out of twenty Italian regions are recipient of these funds (Apulia, Basilicata,

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

[11] 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 the estimated effects of the DSP on spending, which are presented later in the paper.

[12] https://leg16.camera.it/561?appro=809

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

Calabria, Campania, Sardinia and Sicily) based on the 75% GDP rule, i.e. only regions with a GDP per capita under 75% of the EU average are eligible for this funding. We call this group FURs ('Fiscally Unconstrained Regions'). These regions receive large transfers, which in fact made DSP restrictions on capital expenditures not binding for most municipalities in these regions. The 14 remaining Italian regions are eligible for other EU funds (e.g. Regional Competitiveness and Employment funds), whose budget is substantially smaller, therefore keeping fiscal rules binding in this area. We call this group FCRs ("Fiscally Constrained Regions').

We collect data on European funds from the OpenCoesione.gov portal.[14] 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. In this period, FURs received and spent systematically much more EU funds than FCRs. In absolute terms, FCRs spend €228 millions per year, compared to the €618 millions spent by FURs.[15] The total Italian expenditure certified to the EU was €46.2 billions.[16] Figure 1 graphically shows the allocation of EU funds between FCRs and FURs presented i) in total amount (panel a), ii) in per-capita terms (panel b), iii) over regional GDP (panel c) and, iv) over the number of local politicians (panel d). The difference between the two areas is striking.

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 FURs received every year 230 Euros per capita of EU funds, which corresponds to the 59% of their capital expenditure budget, while those in FCRs received only 22 Euros per capita, corresponding to 11% of their capital expenditure budget. The two distributions are actually weakly overlapping: only 78 municipalities in the sample of FURs (i.e. 8%) receive less EU funds than the average town in FCRs. In FCRs, the 95^{th} percentile receives 107 euros, still much less than the average town in FCRs.

This comparison highlights how EU funds could finance an extensive share of public investments in FURs utterly reducing the effectiveness of fiscal rules.[17] This leads to our hypothesis that fiscal

[14] 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.

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

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

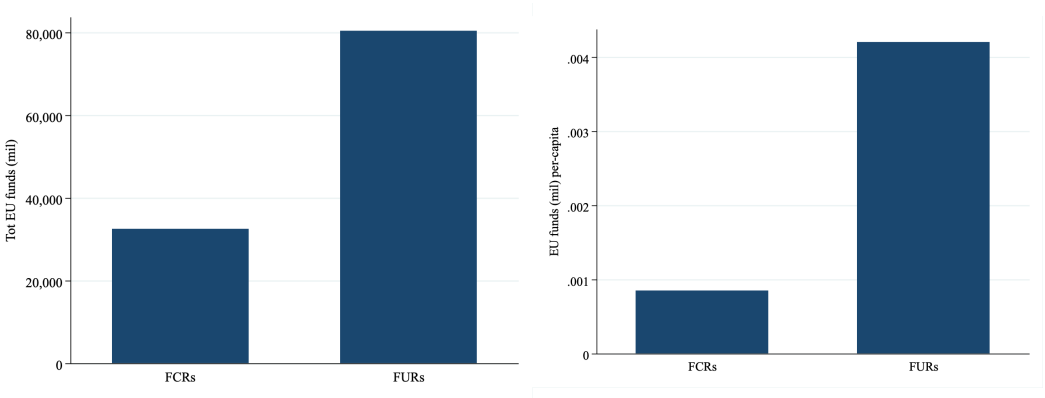
[17] Italian scholars have focused on the issues related to the simultaneous implementation of a contractionary fiscal policy (the DSP) and an expansionary one (EU transfers), and on how the DSP was less constraining in FURs

rules are not going to be binding among treated municipalities in FURs, and, in turn, they are not going to substantially affect budgetary outcomes and corruption. Conversely, we expect an effect among treated municipalities in FCRs, where fiscal rules actually represent a new budget constraint for the local government.

Figure 1: EU funds allocation between FCRs and FURs

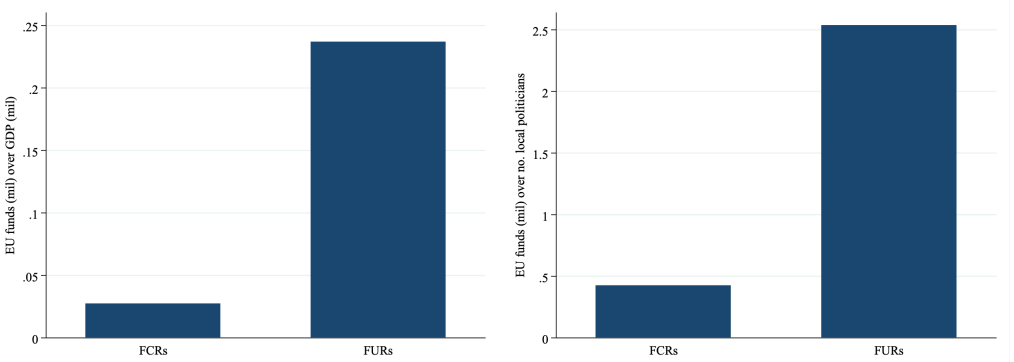
(a) Total amount

(b) Amount per-capita



(c) Amount over regional GDP

(d) Amount over no. of local politicians



The plot shows the amount of EU funds allocated between FCRs and FURs between 2007 and 2013. Panel a) shows the total amount, panel b) the amount per-capita, panel c) the amount over the regional GDP and panel d) the amount over the total number of local politicians.

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

municipalities receiving extra EU funds (see for instance Gandolfo, 2014 and Bocognani, 2013).

procedures by municipality, year, and type of alleged offence.[18] 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 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 A1, more than three out of four municipalities display no corruption episodes in the period under consideration. Figure A1 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 2 plots the aggregate number of corruption investigations for large, medium and small Italian municipalities over time.

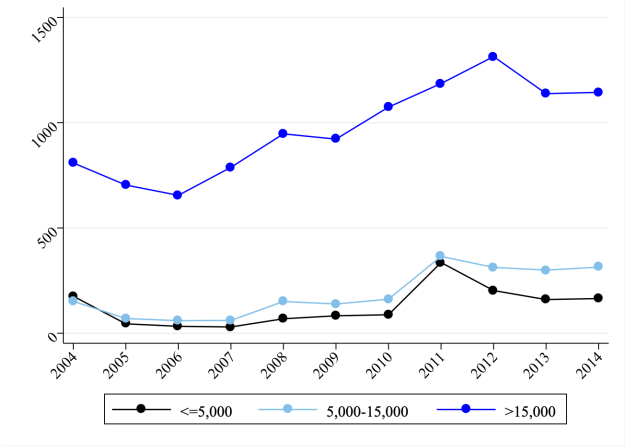
In what follows, we use two normalized versions of the corruption index (see Panel A of Table A1), dividing the number of corruption episodes observed in each municipality and year respectively by municipality population, expressed in thousands (this is our main dependent variable, which we label *Corruption (PC)* in what follows) and by total expenditure -expressed in logarithm- (*Corruption (total exp.)*).[19] Moreover, we also create additional corruption indicators: first, we calculate the index *Corruption Max*, which is similar to *Corruption (PC)* but it includes out of the four corruption crimes, only the one with the highest number in a municipality-year. As we only observe the total number of investigations for each crime, we might double-count cases in which the same person is charged with several corruption crimes: this indicator represents the lower bound

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

[19] In subsequent regression analyses, these indicators are standardized by region group (FCRs or FURs).

of the number of distinct corruption cases. Second, we compute the indicator *Corruption binary*, which is dummy variable capturing whether in a certain municipality-year a corruption charge has been registered. This measure is the simplest possible corruption indicator, and it only focuses on the extensive margin of this phenomenon.

Figure 2: Aggregate corruption investigations by municipal size (population)



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 (*Certificati consuntivi*), that include detailed information on revenues, expenditures, transfers, deficit and debt of municipal administrations for the period 2004–2015. Panel B of Table A1 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 A1 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 Panels D and F of Table A1.

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 consists 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, respectively).

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, an alternative - and less established approach - would be the Differences-in-Discontinuity (Campa, 2011, Grembi et al., 2016), but we deem a Difference-in-Differences approach more suitable to analyze this setting as it allows to control more efficiently for the presence of pre-trends as well as to include very fine grained fixed effects, i.e. municipal

fixed effects, being able to clean for municipal time invariant characteristics.[20] Importantly, our results remain unchanged when following a Differences-in-Discontinuity approach. We report those findings among our robustness tests in Appendix 9.

4.2 Identification assumptions

The key identifying assumption of this empirical 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 most important policy change that concerned the 5,000 inhabitants threshold in 2013 was the introduction of double preference voting conditioned on gender (*Legge* n. 215, 2012) in municipalities above the threshold, coupled with gender quotas on candidate lists: voters can cast a vote for two candidates (instead of one), provided they are of different gender, and electoral lists for the municipal council must include at least one third of candidates of each gender. The aim of this policy was to increase the share of female politicians in local councils. 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. First, this implies that we might be estimating a lower bound of the real effect, as control units increase the share of female politicians – likely to be less corrupted– simultaneously to the introduction of the DSP in treated units. Second, to exclude this channel, we conduct a 'horse-race' between the DSP and the gender quota reform, which we discuss in the robustness section.[21]

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

[20] Nonetheless, the two methods rely on significantly different specifications as, for instance, the Difference-in-Discontinuity requires to control for the effect of the policy differently with respect to the forcing variable (with the inclusion of the triple interaction between the forcing variable, the treatment dummy and the post-reform dummy, as well as all the double interaction terms).

[21] In 2014 a related reform took place (*Legge* n. 56, 2014) that stated that in towns above 3,000 inhabitants the fraction of elected politicians of each gender cannot represents less than 40% of municipal government seats.

the DSP; ii) the very low incidence of detectable corruption in towns around the 1,000 threshold, which limits our analysis.[22]

Finally, our data consists of corruption investigations, that is only a share of the total number of corruption cases. In the Online Appendix 1, we discuss the limitations and the details of this measure of detected corruption.

4.3 Specification

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

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

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).[23] The population of reference for the application of the DSP is the one recorded at the end of the year preceding the last one (*Decreto Legislativo* n. 77, 1995; *Decreto Legislativo* n. 267, 2000), according to the Italian Statistical Institute (ISTAT).[24] T_t denotes the post-reform period: it is a dummy equal to one from 2013 on. 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 at the end of the year preceding the last one, according to ISTAT. The fact that the population measure is pre-determined to the reform itself avoids the risk of endogenous sorting of cities around the threshold, which was not known when the population was recorded.[25] 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

[22] In line with the irrelevance of this threshold, in the Online Appendix, we conduct the main analysis on corruption charges and budget variables focusing on this threshold, and we do not find any effect, as shown in Table A7.

[23] Even if β_1 is estimated in the model, since S_{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.

[24] For instance, for the year 2012, the reference population is the one recorded on December 31st, 2010.

[25] 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 reference population for the year 2013. The results, shown in Online Appendix Figure A2, 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.

2013.

X_{it} is a vector of lagged controls including age, education, gender of municipal councillors, margin of victory and term limit. All these political variables refer to the previous electoral term and are differential across years.[26] We include municipality fixed effects, γ_i, and year fixed effects, differential across regions, δ_{rt}, 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.[27]

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

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

The local DID estimator is the interaction term $S_{it} \cdot \eta_t$ (with η_t representing year fixed effects), 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 Corruption

In this section we study the impact of introducing budget constraints on corruption charges at the local level. As highlighted in the previous sections, our hypothesis is that fiscal rules will affect corruption mostly in FCRs, where they are fully enforced and they have an impact on municipal spending outcomes. Conversely, we do not expect a substantial effect in FURs. Table 1 reports the estimation of Model 1, using a 2,500-inhabitants bandwidth: panel A focuses on FCRs, panel B on FURs, and panel C includes the full sample. In Columns 1, 2 and 3, the dependent variable is the number of investigations per 1,000 inhabitants and standardized to have a mean of zero and a standard deviation of one: we show it without controls, with a reduced control set and with the complete control set. We will discuss columns 4 to 6 of Table 1 in a next section.

[26] 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.

[27] Figure A3 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.

Panel A shows that corruption significantly decreased in the group of municipalities in FCRs to which the DSP was extended in 2013. The reduction is about 9% of a SD (column 2). Conversely, Panel B does not show any effect in FURs, in which fiscal rules were extended but not fully enforced: the coefficients are positive, small in magnitude and not statistically significant. Panel C shows that the effect is still negative and significant in the complete sample of both FCRs and FURs.[28]

Table 1: Impact of DSP on corruption charges

Dependent variable:	Corruption per-capita			Corruption over spending		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel a: FCRs						
Stability pact (T^*S)	-0.088**	-0.089**	-0.130***	-0.191**	-0.192**	-0.255**
	(0.042)	(0.042)	(0.049)	(0.086)	(0.086)	(0.101)
N	16314	16314	16314	16282	16282	16282
R^2	0.215	0.215	0.231	0.220	0.220	0.235
Panel b: FURs						
Stability pact (T^*S)	0.028	0.028	0.088	-0.034	-0.034	0.044
	(0.056)	(0.056)	(0.065)	(0.066)	(0.066)	(0.077)
N	7077	7077	7077	7042	7042	7042
R^2	0.250	0.250	0.263	0.257	0.257	0.271
Panel c: Italy						
Stability pact (T^*S)	-0.060*	-0.060*	-0.058	-0.148**	-0.148**	-0.157**
	(0.033)	(0.033)	(0.040)	(0.061)	(0.061)	(0.074)
N	23391	23391	23391	23324	23324	23324
R^2	0.225	0.225	0.239	0.226	0.226	0.240
City, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls (not interacted)	No	Yes	Yes	No	Yes	Yes
Controls (interacted)	No	No	Yes	No	No	Yes
Bandwidth	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 (1-3) and corruption investigations over total spending (standardized) in columns (4-6). The specification includes municipality and year fixed effects and the distance from the population threshold in columns (1) and (4). Columns (2) and (5) also include characteristics of municipal councillors (age, education and gender), margin of victory and term limit, that refer to the previous electoral term. Columns (3) and (6) also include characteristics of local politicians interacted with year fixed effects as well as region-year fixed effects. The sample includes municipalities located in FCRs in Panel a, municipalities located in FURs in Panel b and all Italian municipalities in Panel c. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

While in Table 1, we display only one bandwidth, in Figures 3 (right panels) we study how sensitive this result is to the choice of population bandwidth (the dependent variable is the number

[28] Furthermore, in Table A8, 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 (for the sample of FCRs): strict corruption, graft, malfeasance (including official misconduct and abuse of office) and embezzlement (including misappropriation of public funds). Also these variables are standardized by macro regions and expressed in per thousands inhabitants. Our findings are mostly driven by a reduction in malfeasance and embezzlement charges. This effect on malfeasance is not surprising as this charge represents the great majority of the corruption-related events committed in our sample.

of investigations per 1,000 inhabitants, standardized): we plot the DID coefficient, according to Model 1, varying the population bandwidth in the range of 1,000–4,000. The top panel shows that the effect of the policy is always negative and sizeable in FCRs, the magnitude of the coefficient is rather stable as the sample widens and the effect is always statistically significant. 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. Conversely, when looking at FURs, the effect is never statistically significant, weakly positive for bandwidths between 1500 and 2500 and close to zero for the other bandwidths. The bottom panel reports the effect on the overall sample: in line with the table, the effect is negative and weakly statistically significant.

The left panels of Figures 3 report the event study approach (Model 2), across three bandwidths: 2,000, 2,500 and 3,000 inhabitants. In FCRs the reduction is statistically significant immediately after the extension of fiscal rules. Importantly, 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. Panel C shows the results in FURs in which we do not observe nor a change after 2013 nor pre-trends in the previous years (the year 2011 somehow represents an outlier, although it remains statistically indistinguishable from the other years). Finally Panel E focuses on the entire sample and results are similar to Panel A but weaker.

5.2 Budget

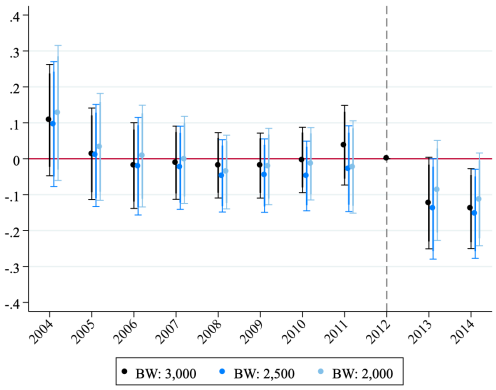
Overall, this first set of findings show that corruption is reduced only in FCRs. A plausible explanation is due to the reception of European transfers in FURs, which weakens the effects of fiscal rules. To validate this interpretation, we now focus on budget outcomes.

Table 2 reports the estimation of Model 1, using a 2,500-inhabitants bandwidth: panel A focuses on FCRs, panel B on FURs, and panel C includes the full sample.[29]

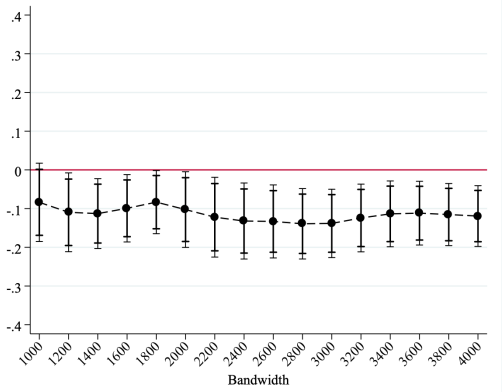
[29]For the sake of brevity, we report these findings with the full set of controls. The results are unaffected when removing them.

Figure 3: Effect of the DSP on corruption (per-capita)

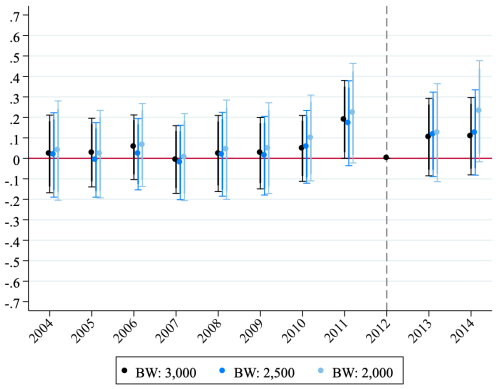
(a) Event-study: FCRs



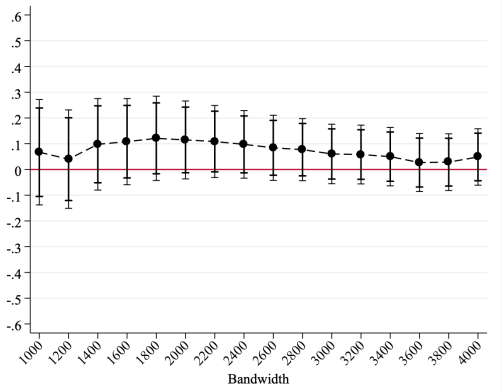
(b) DiD: FCRs



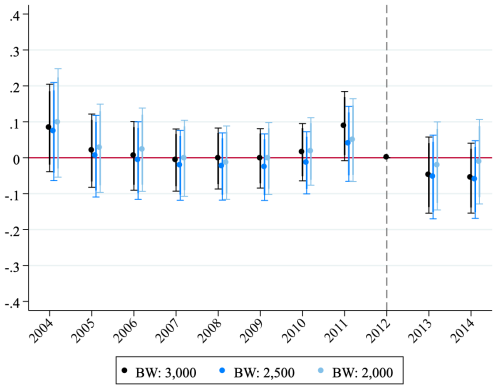
(c) Event-study: FURs



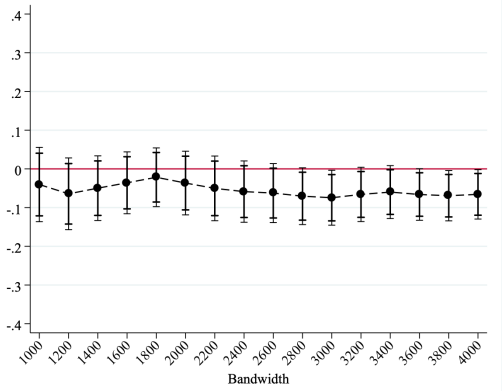
(d) DiD: FURs



(e) Event-study: Italy



(f) DiD: Italy



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. The dependent variable is corruption investigations per 1,000 inhabitants, standardized by region group. The specification is the same as in Table 1 (column 3). The sample includes cities in the FCRs (first row), cities in the FURs (second row) and all italian municipalities (third row).

Table 2: Impact of DSP on public finance and procurement

Dependent variable:	House tax rate	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)	Total spending (PC)
	(1)	(2)	(3)	(4)	(5)
Panel a: FCRs					
Stability pact (T^*S)	0.001	-21.153***	-49.824***	-170.960***	-70.976***
	(0.005)	(6.363)	(9.868)	(61.960)	(11.996)
N	17796	17720	17720	10420	17720
R^2	0.874	0.923	0.439	0.227	0.730
Panel b: FURs					
Stability pact (T^*S)	0.005	-1.209	-24.892	88.182	-26.100
	(0.008)	(9.280)	(23.897)	(144.983)	(25.412)
N	7719	7610	7610	4501	7610
R^2	0.842	0.895	0.377	0.362	0.530
Panel c: Italy					
Stability pact (T^*S)	0.002	-15.243***	-43.108***	-96.418	-58.351***
	(0.004)	(5.226)	(9.836)	(61.984)	(11.238)
N	25515	25330	25330	14921	25330
R^2	0.864	0.915	0.426	0.360	0.660
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

The dependent variables are the public finance and procurement measures expressed in per-capita terms. The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). In column (1) the specification also includes a control for the transfers reform of 2012 (differential effect of 2012 at the 5,000 inhabitants threshold). The sample includes municipalities located in FCRs in Panel a, municipalities located in FURs in Panel b and all Italian municipalities in Panel c. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Column 1 of Panel A shows the effect on the most important local tax, the house tax.[30] No sizeable effects emerge from the table.[31]

[30] In this analysis, we are considering the ordinary rate of the house tax. Results are similar if we consider the tax rate applied to the first residency, that has been abolished in 2013. These results are available upon request.

[31] In 2012, several local public finance reforms have been enforced by the Italian government, which affected the revenue side of the municipal budget. First, as documented by Marattin et al., (2019) transfers from the national government to local governments have been cut, with the exception of municipalities with population lower than 5,000 inhabitants, exempted from this reduction. Second, the house tax has been reformed. The new tax was more onerous for Italian taxpayers as i) it applies also to the first property, ii) it is computed on a larger tax base and iii) it includes a new set of tax rates and deduction mechanisms. To take into account this differential effect, we control

Second, Panel A shows that municipalities in FCRs are decreasing their capital expenditures (per-capita) of about 50 euros per year (Column 3). We also find a smaller decrease in current expenditure of about 20 euros per-capita (Column 2). The fourth column 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 table shows that the DSP produced an immediate and persistent 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 large: The average reduction induced by the policy corresponds to about 170 euros per-capita. 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. In other words, while the execution of the project can take more than one year, the bidding takes place only once at the beginning of the process. Finally, the fifth column of the table shows a significant reduction in total municipal expenditure.

Panel B shows a very different picture on FURs. Here fiscal rules do not seem to have any sizeable impact on budgetary outcomes. This is the case for all spending outcomes, as well for the house tax rate. Finally, panel C reports the overall effect on both FCRs and FURs: we still observe a statistically significant effect on both current and capital expenditures, although the size is smaller than in FCRs.

In Figure A4 (right panels), we study how sensitive this result is to the choice of population bandwidth, as we show the analysis varying the population bandwidth in the range of 1,000–4,000 for the FCRs sample. Previous findings are confirmed, as the coefficients are very stable across different bandwidths. We report similar figures for FURs and the entire sample in Figures A8 and A9. We complement this analysis by providing an event study approach to evaluate the absence of pre-trends (Model 2) across three bandwidths: 2,000, 2,500 and 3,000 inhabitants. Figure A4 (left panels) presents this analysis on FCRs. The reduction in spending is sizeable already in 2013, and then visible also in 2014 and 2015. Overall, there are not worrisome pre-trends in the budget

for differential trends before/after 2012 for municipalities with different population. This control is included only when looking at the revenue side of the budget outcomes. However, the inclusion of this control does not affect the impact of the DSP on the corruption, as reported in Figure A10, where we show the main analysis on corruption controlling for a differential trends before/after 2012 for municipalities with different population in order to control for the public finance reforms of 2012.

outcomes. Across the three spending outcomes only three years are statistically different from the baseline 2012, i.e. 2005-2006 for current spending and 2010 for capital and total spending.[32]

There are two takeaways from this first set of analyses. First, as expected, the DSP does not affect corruption in FURs where fiscal rules are not binding. Second, the DSP reduces corruption charges in FCRs. The effects of fiscal rules on spending in FCRs may explain why corruption investigations drop: Municipalities experience a drop in corruption driven by the reduction in capital and procurement expenditures. This interpretation is consistent with the empirical literature suggesting that discretionary spending is the budget component most vulnerable to corruption (e.g. Hessami, 2014; Titl and Geys, 2019).

As municipalities in FURs do not modify their budget, they should naturally be in deficit, i.e. they would not be complying with the DSP. However, this is not the case, as according to the Italian Ministry of Interior, 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.[33] This simple accountability fact explains why municipalities in FURs were simultaneously respecting fiscal rules without any significant change in their budget: They did not need to cut expenditure or increase taxes to respect fiscal rules, as they were receiving additional transfers from the EU.[34]

In the Online Appendix 2, we provide further evidence on the importance of EU funds to justify the irrelevance of fiscal rules in FURs. We consider the entire sample of both FCRs and FURs and we analyse the differential impact of fiscal rules depending on the amount of EU funds received at the local level. Specifically, we consider different outcomes, both corruption charges and spending. Table A2 shows the results of these triple-differences analyses. Overall, we find that EU funds consistently affect the policy's impact on corruption, weakening its positive effect

[32]This upturn in capital spending in 2010 (and in turn in total spending) is likely due to a 2009 law that promoted the creation of 'unions of municipalities' (Unioni di Comuni) among very small towns, allowing them to jointly manage some of their functions. The law jointly introduced for the next year a tax discount and an additional exceptional transfer of 120 millions to municipalities below 5,000 inhabitants. Since this aligns with our treatment group in the same year in which we notice this upward trend, we are inclined to consider it as the most plausible explanation.

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

[34]An additional alternative explanation could be that fiscal rules were less stringent in FURs because of historically different spending patterns below/above the 5,000 threshold. We conduct a cross-sectional analysis in which the dependent variable is the level of surplus set as the DSP target (expressed in log). The strictness of the DSP does not differentially change for treated municipalities in FURs. Therefore, the null effect in FURs cannot be explained by a weaker stricteness of the DSP. This analysis is omitted for the sake of space and it is available upon request.

as transfers increase. This suggests that European transfers counteract the impact of fiscal rules on corruption, potentially facilitating corruption-related phenomena. Similar patterns emerge for the budget outcomes, with lower spending (as effect of the DSP introduction) in areas receiving lower EU funds. These effects decrease in areas with higher EU funds.

An additional concern is that all FURs regions are clustered in the South, therefore fiscal rules might be ineffective due to unobserved time-varying differences between Northern and Southern regions. To this aim, in Table A2 we restrict the sample to FURs and differentiate by the amount of EU funds allocated to each province. Even within FURs, we find a decrease in corruption investigations among treated municipalities in provinces with low EU funds. Alternatively, we restrict the sample to the homogeneous group of Southern regions, including the six FURs and Molise and Abruzzo (included in FCRs). Corruption decreases only among treated municipalities in the two Southern regions not receiving extra EU funds. In Appendix 2, we report a more detailed description of these analyses.

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. To provide some evidence on this, we conduct a text analysis following Giommoni (2021) and we screen newspaper articles released by the main Italian press agency, ANSA. We first select the articles dealing with corruption that mention in the text the surname of a local politician in office in the place where the article was geo-localized. Appendix 6 discusses the details of the identification of corruption-related articles. Then, we identify the specific type of corrupt behavior. The results indicate that the majority of these episodes impose significant costs on society. Specifically, we find 1,585 articles related to infractions in procurement procedures, 431 articles on fraud, 315 articles on public hiring, 161 articles about refund usage, 81 articles on construction crimes, and only 63 articles concerning theft and embezzlement.

5.3 Mechanisms

In this section we provide additional findings to further investigate how and why the DSP is reducing rent-seeking. We focus only on FCRs because, as shown in the previous sections, the effects of the DSP was negligible in FURs.

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. 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 3. 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 3 shows that the effect of the reform is larger during the electoral years, as local politicians are more electorally constrained. This emerges for the different definitions of corruption in the analysis: corruption per-capita (column 1) and corruption over spending (column 2).[35]

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 are associated with significantly less corruption charges per capita than mayors without reelection incentives (columns 3 and 4 of Table 3).[36] [37]

In line with the relevance of accountability and a reduction of rent-seeking in FCRs, we provide three additional tests: i) we show that among treated municipalities in FCRs the drop in corruption and public spending is higher when fiscal rules are more stringent (intensive margin, in Online

[35] Note that no heterogeneous effects emerge if we focus on electoral competition with a focus on measures such as the number of running candidates, the number of lists and the margin of victory. These null results are probably due to the fact that in this setting local elections are relatively competitive, as the median municipality has three mayoral candidates and a margin of victory of only 15%.

[36] An alternative explanation for our findings is related to an increase in perceived monitoring by local politicians. Although we cannot directly test this mechanism, the FCRs/FURs heterogeneity can help us to distinguish between accountability and monitoring. In particular, the monitoring effect should be at work in FURs 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 FURs 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 FCRs municipalities, while monitoring is the only relevant channel in FURs. As shown above, we do not find any effect in the latter group, which implies that monitoring is not likely to be the most relevant channel.

[37] Following the idea that fiscal rules are muted in FURs, We should not expect any heterogeneous effects of fiscal rules depending on accountability in this area: this is indeed what we find in Table A5.

Appendix 3 Table A3); ii) in the same table (columns 7 and 8), we differentiate public spending components in those traditionally prone to rent-seeking and corruption from other categories and then we examine the distribution of these two groups within the municipal budget. While the reform has a negligible effect on the components of spending traditionally less affected by corruption (column 8), it emerges a 1.8 p.p. decrease in the expenditure share related to the components more linked to corruptive phenomena (i.e. trash collection and transportation spending; Di Cataldo and Mastrorocco, 2022; Carlucci et al., 2017): This further supports the idea that local politicians are adjusting the budget in a way to reduce corruption-related spending items; iii) we further dig into whether politicians are specifically targeting the most inefficient types of capital expenditures or whether the drop in corruption may be a mechanical consequence of the reduction in investments. Here, we replicate the main analysis introducing as a dependent variable the ratio between the standardized number of corruption charges and the annual total expenditures (expressed in logarithm). This analysis is reported in Table 1 (columns 4, 5 and 6): among FCRs, we find a decrease in corruption per Euro spent. 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 spending, but are cutting the least efficient one. We provide more details on this test in Online Appendix 3; iv) an alternative channel might be political selection, as the first modification of fiscal rules in 2001 led to lower educated politicians in the Italian setting, as shown by Gamalerio and Trombetta (2022).In Appendix Table A6, we replicate Table 3 by excluding municipalities with a potentially different political selection due to the DSP, i.e. observations with elections in the post-reform period. The results are unaffected suggesting that political selection is not the main channel of the findings.[38]

5.4 Additional tests

In this section we briefly present a set of additional tests, which are reported and discussed extensively in the Online Appendix 4.

5.4.1 Local Public Goods Provision

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

[38] Finally, we also analyzed directly whether the introduction of fiscal rules affected political selection, estimating the main model on a set of mayors' characteristics (i.e. education, age and gender). We did not find any evidence of a change in political selection in our setting.

Table 3: Political accountability

Interaction term:	Electoral period		Mayor term limited	
Dependent variable:	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending
	(1)	(2)	(3)	(4)
Stability pact (S*T)	-0.019	-0.112	-0.130**	-0.258**
	(0.076)	(0.133)	(0.054)	(0.111)
Stability pact (S*T)*interaction	-0.169**	-0.228*	0.106*	0.179
	(0.080)	(0.132)	(0.063)	(0.127)
N	16314	16282	16544	16511
R^2	0.231	0.235	0.228	0.232
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1 and 3) and corruption over spending (standardized) in columns (2 and 4). *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 3-4), which is a dummy equal to one if the mayor is not eligible for re-election. The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). In column (3) political controls only include age and gender of the councillors (that refer to the previous electoral term and are interacted with year fixed effects). The specification also includes $S * interaction_i$, $T * interaction_i$ and $interaction_i$, which are not displayed in the table. The sample only includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and the quality of the local public goods provision. On the other hand, if politicians are reducing rent seeking expenditure, we should not observe a negative effect on local public goods. In line with this idea, we test whether the reduction in public investments deters local economic growth and the quality of the local public goods provision. This analysis is discussed in Online Appendix 4, Figure A6. Overall, we do not find significant effects on any of the outcomes, in line with a reduction in rents, which does not effect local public goods provision. It is important to note that this test has inherent limitations, as local public goods encompass a diverse range of outcomes that are not easily quantifiable.

5.4.2 Alternative Specifications and Corruption Measures

We conduct the main analysis on a series of additional measures of corruption. All these tests are reported in Table A10. We start by focusing on the main corruption measures non standardized (columns 1 and 2), on the measure of corruption over spending expressing total spending in absolute values (not in logarithm) (column 3), and on the measure of corruption over the total number of councillors in a certain municipality (column 4). The main effect is always large and significant.

Then, we use as dependent variable "Corruption (max)", that includes out of the four corruption crimes, only the one with the highest number in a municipality-year, in order to avoid double-counting in case the same person is charged with several corruption crimes. We present this variable in per-capita terms and over spending. These tests are shown in columns (5) and (6) and the negative and significant effect is confirmed. Moreover, we generated a measure of binary corruption that is a dummy variable indicating whether a corruption charge has been registered in a specific year-municipality. Also in this case a negative and significant coefficient emerges, as reported in column (7).

Furthermore, we estimate the main model on corruption per-capita and corruption over spending with alternative specifications. These tests are reported in Table A11. First, we omit from the main specification the indicator controlling for the distance to the threshold (columns 1 and 2). Second, we include in the model province-year fixed effects (columns 3 and 4). Finally, we estimate the model with alternative standard errors clustering: province (columns 5 and 6), region (columns 7 and 8) and municipality, province-year (columns 9 and 10). The main effect is always confirmed.

5.4.3 Possible Confounders

A possible threat is related to other policies that took place simultaneously to the DSP, and that may confound our results. First, we focus on the reform of gender quotas of 2013 (discussed in Section 4.2). In particular, in Figure A10, we show that the main results on corruption emerge in a horse race in which we include in the same specification the effect of the DSP (differential treatment since 2013) and the effect of the gender quota reform (differential treatment since the first election from 2013).[39] Second, we conduct the analysis on corruption and budget outcomes in the reduced time window 2008-2015, as the formulation of the DSP substantially changed in 2008, including sanctions for not compliance (see Table A13, in Online Appendix 8, for the institutional details). The main results are unaffected, as shown in Table A9. Finally, we conduct the analysis on corruption excluding the municipalities whose local government has been dissolved by the national government and temporarily assigned to an appointed commissioner; and we exclude from the sample the (very small) set of municipalities that violated the rules of DSP. In these cases the results are unaffected, as shown in Figure A10.

[39] Note that, if female politicians lead to lower corruption, as found in Brazil by Brollo and Troiano (2016), this confounder would bias our results towards zero by reducing corruption in the control group.

5.4.4 Displacement and Placebo

A standard test when studying illegal phenomena is whether crime moved elsewhere, i.e. displacement effects. On the one hand, we show that firms located in FCRs (where the DSP is binding) are unlikely to move to areas in FURs (where the constraint of DSP is weaker), where public investments are not declining. In particular, in Figure A10, we show that the main results are robust to the exclusions of municipalities located in FCRs that are geographically close to FURs. On the other hand, we analyze whether there are geographical spillover effects of corruption in cities sharing a border with treated town. This test is shown in Table A10 (column 8) and a null effect emerges.

Finally, we show that the reform of 2013 did not affect the spending in local police (per-capita) and this result further disproves the idea that the introduction of DSP is related to changes in investigation effort at the local level (Table A10, column 9). In this light, in Table A10 (column 11), we conduct a placebo test showing that the introduction of the DSP does not affect non-corruption related crimes. This test suggests that the results of the main analysis are not due to a general increase in police detection among treated municipalities.[40]

6 Discussion and final remarks

In this paper, we study the impact of fiscal rules on corruption among Italian municipalities with a population below 5,000 that occurred in 2013. We find that the DSP produced a substantial decrease in both corruption charges per capita and in the intensity of corruption over total expenditures. These effects are driven by a reduction in capital expenditures, and enhanced by electoral accountability.

How general are these results? We believe several 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

[40] An additional concern is related to our choice of focusing on corruption investigations rather than convictions. This choice is motivated by two reasons: i) 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. Moreover, we show in Figure A12 that the conviction rate related to corruption crimes does not seem to change between FCRs (where DSP is binding) and FURs (where DSP is less binding). This make unlikely that there is a bias in conviction rate due to the application of DSP, in the areas more affected by fiscal rules.

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. A related point is that the effect depends not only upon whether fiscal rules are binding or not, but also on how binding they are: more binding fiscal rules are associated with larger reduction in corruption investigations. Third, electoral accountability feedbacks seem to determine local politicians' response to the budget shock. This dynamic is particularly strong in this setting as our sample consists of small-medium cities where political accountability is likely to work more accurately. Fourth, we measure the marginal effects of the DSP on corruption in a context with a quite impartial and established anti-corruption detection system, which includes different branches of the police, the justice system and an independent anti-corruption authority.

Overall, other types of budget shocks and local institutional arrangements, or different accountability incentives, might lead to distinct effects on corruption and local public goods provision. A last caveat applies to the time frame of our analysis which is limited to the short-medium run, due to unavailability of corruption data in the next years.

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Online Appendices

- Appendix 1 (for online publication): Detected and actual corruption
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Appendix 1 (for online publication): Detected and actual corruption

The main measure of corruption we use in the paper 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).[41] 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. An additional 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 a central anti-corruption agency able to coordinate and direct anti-corruption efforts.[42] 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.[43] [44]

[41] In Appendix Figure A12, we show that conviction rate related to corruption crimes does not differentially change across FCRs and FURs over time: data on trials refer to the end date of the trial.

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

[43] 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.

[44] It is important to notice that we conduct, in the robustness checks section, a placebo test showing that the introduction of the DSP did not affect the number of local non-corruption related crimes. This test further confirms

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 municipal governments have not decreased expenditure on local police following the introduction of the DSP.

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.

Appendix 2 (for online publication): The role of EU funds

In this Appendix, we study the relevance of EU funds in affecting the impact of the DSP on corruption. In Table A2, we consider the entire sample of both FCRs and FURs and we analyse the differential impact of fiscal rules depending on the amount of EU funds received by the province in which each municipality is located (columns 1 to 4). Here, we consider different outcomes on both corruption charges and spending. The table 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. In this analysis, we prefer the provincial to the municipal allocation, as EU fund allocation at the municipal level is 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.[45]

that the introduction of fiscal rules is unlikely to be related with a modification in the investigation effort by police forces at the local level.

[45] European funds are measured as the total amount of provincial transfers from the European Union 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. Importantly, when computing this (provincial) measure for municipality i we exclude the amount of EU funds received by the municipality itself in order to reduce the risk of endogeneity of the measure. For instance, Muraközy and Telegdy (2016) provide evidence on how political networks matter for the allocation of EU funds among Hungarian municipalities. An additional concern could be that there is no substantial heterogeneity in EU funds allocation across provinces. This is not the case as there is substantial variation in the allocation of EU funds across provinces of a same region. To verify this, we consider for each region the average yearly amount of EU funds (post-2013), we then compute the standard deviation of this value across provinces within each region and this amounts to 0.6 billions Euros, confirming the previous statement.

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 (column 1). 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. In column 8, we conduct the analysis using as interaction term a dummy for being located in FCRs. As expected, the negative impact of fiscal rules on corruption only emerges in FCRs as the interaction term is negative and significant and the DiD estimator is not.[46] These outputs are consistent with the findings of De Angelis et al., (2018), who show that EU transfers increase corruption in Southern Italy.

We find a similar pattern when looking at budget outcomes (columns 2 to 4), although the estimates are less precise and the triple-interaction is statistically significant only for capital spending. Overall, we find a decrease in spending in areas receiving lower amounts of EU funds, while the effects fade away in areas receiving more EU funds (i.e. the triple-interactions take always positive values).

In column 5, we restrict the sample to FURs and differentiate by the amount of EU funds allocated in each province. Even within FURs, we find a decrease in corruption investigations among treated cities in provinces with low EU funds. This test validates that the mechanisms at work in FCRs are taking place – to a less extent – also in FURs. In column 6, we restrict the sample to the homogeneous group of Southern regions, including the six FURs and Molise and Abruzzo, the only two Southern regions not included in the group of FURs. Previous findings are confirmed: corruption decreases only among treated municipalities in the two Southern regions not receiving extra EU funds. Finally, in column 7, we consider the amount of EU funds that are *allocated* at the regional level. This is different from the previous analyses in which we consider when the EU funds were *spent*.[47]

[46] This 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 FCRs and FURs similarly: this further validates our main identifying assumptions.

[47] At the end of the cycle, in 2015, the entire allocated budget of EU funds was spent: `https://opencoesione.gov.it/it/spesa-certificata/`

Appendix 3 (for online publication): Fiscally Constrained Regions – additional findings

In this Appendix we provide additional findings to further investigate how and why the DSP is reducing rent-seeking. We focus only on FCRs because, as shown in the paper, the effects of the DSP was negligible in FURs.

Impact on the corruption-proofness of public spending

The main analyses of the paper show that, among FCRs, 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 would ideally need the share of misappropriated resources of total spending. However, as we do not have such data, we resort to a proxy. Specifically, we replicate our main analysis introducing as a dependent variable the ratio between the standardized number of corruption charges and the annual total expenditures (expressed in logarithm).

Figure A5 displays this test for FCRs. We find a decrease in corruption per Euro spent 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 spending, but are cutting the least efficient one. Numerical results are shown in Table 1 (columns 4, 5 and 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 in FCRs. This is an important threshold, as the administration can award contracts below this amount without a competitive bidding process. Appendix Table A4 (columns 1-3) 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 A4 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 increase by 4.2% and 5.6%, respectively, 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.

Intensity in the application of the DSP

As discussed in the paper, 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 among FCRs municipalities, and focus on the top 50% and 20% of this variable distribution.

We conduct these analysis in Table A3. In line with our expectations, we find a stronger decrease in capital expenditures especially for towns subject to a more stringent budget constraint, columns 5-6. We replicate a similar analysis focusing on corruption charges. The effects are remarkably stronger when considering the top 20% (columns 1-4).

Lastly, in line with the idea that stricter fiscal rules push politicians to improve the municipal surplus, Appendix Figure A11 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 charges and ii) compliance has a crucial impact on budgetary and rent seeking outcomes.

Appendix 4 (for online publication): Robustness checks and additional analyses

Impact on economic activity and local public goods provision

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 public goods provision. 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 the local economy.[48]

Municipal GDP

We first test the effect of fiscal rules on per-capita municipal-level GDP, which is proxied by individuals' income, as declared to the Italian fiscal agency.[49] Figure A6 reports the Diff-in-Diff coefficient of this analysis. 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 be too small; iii) fiscal sustainability might improve expectations resulting in an increase in economic activity; iv) or the DSP may take several years to have an effect on GDP.[50]

[48] 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.

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

[50] 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.

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). 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,[51] and we define the bottom bracket in two ways: 0–10,000 euros and 0–15,000 euros. We report these findings in Figure A6. 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.

Municipal services

Although we do not find any change in a set of economic outcomes, we cannot exclude that a substantial reduction in public investments might worsen public services provided by the municipal government. To this aim, we collect a dataset which includes all the main municipal services supplied by municipalities. This data have the advantage of measuring outcomes which directly depend upon local administrative activity and are financed by the municipal budget. We believe this represents the most comprehensive set of local public goods and performance indicators at the local level currently available in Italy. The data have been collected from the Italian Ministry of Interior. Specifically, we collect data on school canteens, kindergartens, waste collection and street lightening. We graphically report those results in Figure A6. *N children kindergartens* refers to the number of children attending public kindergartens; *kindergartens spots/requests* is the share between the number of available spots in public kindergardens and the number of children requests; *School canteens meals* is the number of meals provided by public schools; *N canteens spots/requests* is the share between the number of available spots in schools canteens and the number of students requests; *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*

[51] 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.

employees is the log number of municipal public employees in the local administration. 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).[52]

Additional corruption indicators

In this section we estimate the main model using as dependent variable a series of alternative measures of corruption. First, we focus on the main corruption measures non standardized (columns 1 and 2), on the measure of corruption over spending expressing total spending in absolute values (not in logarithm) (column 3), and on the measure of corruption over the total number of councillors in a certain municipality (column 4). Second, we focus on the measure "Corruption max" which is similar to the main measure of corruption but it includes out of the four corruption crimes, only the one with the highest number in a municipality-year. This indicator represents the lower bound of the number of distinct corruption cases and allows us avoiding double-count cases in which the same person is charged with several corruption crimes, as we only observe the total number of investigations for each crime. Columns (5) and (6) of Table A10 show these tests in which we use the versions per-capita and over spending, respectively. In both cases the effect is negative and statistically significant. Third, we estimate the main model using as dependent variable the indicator "Corruption binary" which is dummy variable capturing whether in a certain municipality-year a corruption charge has been registered. Also in this case the impact is negative and statistically significant, as reported in column (7) of Table A10.

Additional robustness on the estimation strategy

We conduct an additional robustness checks on the estimation strategy, in order to control for the public finance reforms of 2012. In detail, we estimate the main model on corruption controlling for a differential trends before/after 2012 for municipalities with different population. This control captures post-2012 effects between cities with different size, and may take into account the impact of the public finance reforms enacted in 2012. This analysis is reported in Figure A10 and the effect on corruption is unaffected.

[52] Importantly, these null effects also emerge in the sample of FURs, as displaied by Figure A7.

Controlling for the impact of gender quotas

A potential threat to the identification of our analysis may come from the introduction of gender quotas in Italian municipalities. The reform took place in 2013 and introduced a double preference voting conditioned on gender (*Legge* n. 215, 2012) in municipalities above the threshold of 5,000 inhabitants, coupled with gender quotas on candidate lists: voters can cast a vote for two candidates (instead of one), provided they are of different gender, and electoral lists for the municipal council must include at least one third of candidates of each gender. The aim of this policy was to increase the share of female politicians in local councils. Given the partial overlap between gender quota and the DSP reform of 2013, this policy 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 the concern related to this policy, we conduct a horse-race. In particular, we include in the same specification the main treatment, that captures the effect of the DSP, and the effect of the gender quota reform (differential treatment since the first election from 2013). The outcome of this test on the variable corruption charges is reported in Figure A10 and confirms the main finding.

Analysis on the time window 2008-2015

The legislation of the DSP has undergone several reforms and many aspects have been modified since its introduction in 1999, as discussed in the Appendix 3 and shown in Table A13. One important reform of the DSP took place in 2008 with three important modifications: i) the introduction of mixed basis accounting, ii) the revision of monitoring and iii) the revision of sanctions for non-compliance. In order to show that this reform did not contribute to our findings, we conduct the analysis on the main outcomes, corruption charges and budget variables, focusing on the reduced time windows 2008-2015. The results of this test are reported in Table A9 and show that the main findings are unaffected.

Alternative explanation: corruption on the move

An alternative explanation of our findings could be that corruption is decreasing in FCRs, 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 FCRs to FURs. 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 assume that the cost of moving to another area is increasing in distance, whereby firms located in FCRs neighboring a FUR 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 FCRs geographically close to FURs. We replicate our findings dropping observations from provinces in FCRs which neighbor a FUR. These analyses are shown in Figure A10 and our findings are confirmed as the estimated coefficients are very similar to the ones in the main analysis.

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

The DSP was enforced is 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 (*Legge* n. 148, 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, while there are only 0.007 corruption charges per year in municipalities with a population below 1,000.

In Appendix Table A7, 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, *Guardia di Finanza*. Second, the results in Appendix Table A10 (column 9) show

that the DSP did not lead to a decrease in spending on local police (per-capita).

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 Internal Affairs). 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 11 of Appendix Table A10 and show that the reform did not impact on 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 some additional analyses that we conduct to test the robustness of our main findings.

First, we exclude from the sample the municipalities that experienced in the time span in analysis a commissioner government (*commissariamento*). This type of administration consists in a technical government which is instituted after the early termination of the municipal council for specific reasons, upon decision of the Ministry of Interior. Figure A10 show that the main results on corruption emerge also in this case. Finally, we exclude from the sample the small groups of municipalities that violated the rules of DSP. Also in this case the results are unaffected, as shown in Figure A10.

Second, column 8 of Appendix Table A10, 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 an increase 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.[53]

[53] 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

Third, a further 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 FCRs. In Appendix Figure A12, we exploit the FCRs – FURs heterogeneity to show that the conviction rate related to corruption crimes does not seem to change across the two groups before/after 2013.

Appendix 5 (for online publication): A brief history of Italian fiscal rules

The Domestic Stability Pact or DSP (in Italian, *Patto di Stabilità Interno*) is a set of fiscal rules that the Italian government adopted in the period 1999-2016 to constrain public spending at the local level: Table A13 provides a description of the evolution of its institutional features. The purpose of these rules was to limit the rise in public debt in order to meet the requirements of the Stability and Growth Pact, that Italy signed alongside the other member states of the European Union.

The DSP was first introduced by the 1999 budget law (*Legge* n. 448, 1998) and underwent several changes over time, in a process of gradual refinement, before being completely replaced by the balanced budget amendment (*Legge Costituzionale* n. 1, 2012) in 2016. More specifically, the DSP sets precise quantitative objectives on budgetary aggregates at all local government levels (i.e., regions, provinces, and municipalities), and defined monitoring procedures and sanctions for governments which would not comply with the rules. Considering the focus of the paper, in what follows we only present the evolution of the DSP rules at the municipal level.

The first version of the DSP (*Legge* n. 448, 1998) targeted the fiscal gap. In particular, the growth of the fiscal gap, computed on a cash-basis, with respect to its value two years earlier was constrained to be zero. The rule applied to all municipalities. In 2001 (*Legge* n. 388, 2000)

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.

the growth of the fiscal gap was allowed to reach a maximum of 3 percent. From this year, municipalities below 5,000 inhabitants were exempted by the DSP. In 2002 (*Legge* n. 448, 2001) the maximum growth of the fiscal gap was set at 2.5 percent, and a constraint to the growth of current expenditure, both on a cash and accrual basis, was introduced. In 2003 (*Legge* n. 289, 2002) the focus was restricted again on the fiscal gap, with the only difference that it had to be verified both using cash-basis accounting and accrual basis accounting (but excluding capital expenses). The discipline underwent a major revision in 2005 (*Legge* n. 311, 2004), with the constraint on the fiscal gap replaced by a cap on the growth of total expenditure (including capital expenditure). The reasoning behind this shift was to keep under control local taxation, which local governments could leverage on for improving the balance without reducing expenditure. The same rules applied in 2006. With the new Parliament (XV legislature) taking office in April 2006, the old approach, which focused on financial balances instead of expenditure, was restored. Starting from 2007 (*Legge* n. 296, 2006), the gap between total revenues and total expenditure (including capital expenditure) became again the main target of the DSP. In 2008 (*Legge* n. 244, 2007), there were some main changes already discussed in the paper. This discipline of 2008 was confirmed for the period 2009-2012. Some changes were introduced in 2013 (*Legge* n. 228/2012). In particular, new entities became subjected to the rules of the DSP: the municipalities between 1,000 and 5,000 residents. From 2014, also unions of municipalities with a population above 1,000 were subjected to the DSP rules.

Appendix 6 (for online publication): Text analysis on newspaper articles

An important aspect for the interpretation of the results of the paper 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 (2021) 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.

To construct this newspaper-based corruption measure, 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.[54]
    - *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.[55]

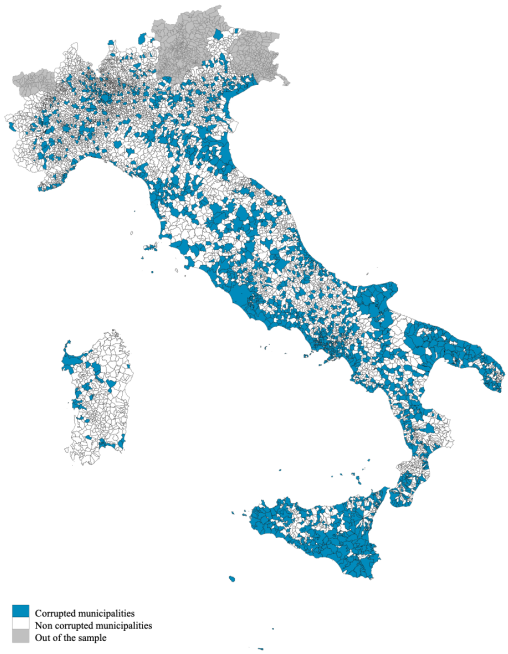
[54] We use a python code to perform the extraction from the portal Factiva. Moreover, in the extraction, we select all the available sources for ANSA. We use the roots of the following keywords (in Italian) as well as related synonyms: accuse, arrest, bribe, convict, corruption, detention, embezzlement,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

[55] We use the roots of the following keywords (in Italian): appointment, authorization, buildable, construction,

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 descriptive evidence that corruption cases under analysis do represent an important cost for the community.

Appendix 7 (for online publication): Additional figures

Figure A1: 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 white are those where no corruption episodes were recorded. Municipalities in gray are excluded from the sample.

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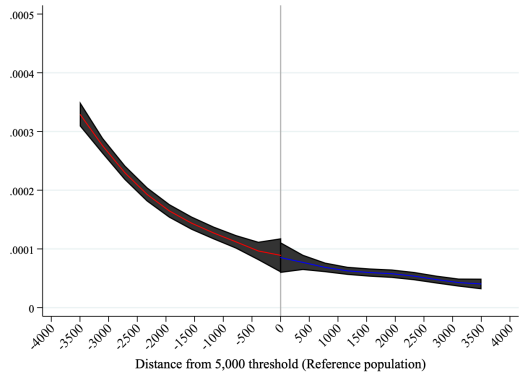
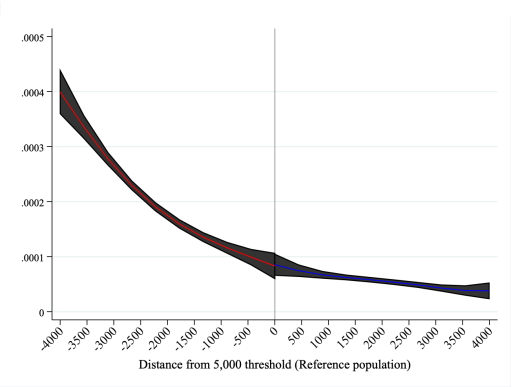
public contract, damage, fraud, funds, hiring, investment, license, loan, procurement, public works, recommendation, reimbursement, subcontract, supply contract, tender, urban planning.

Figure A2: McCrary test - density around the 5,000 population threshold

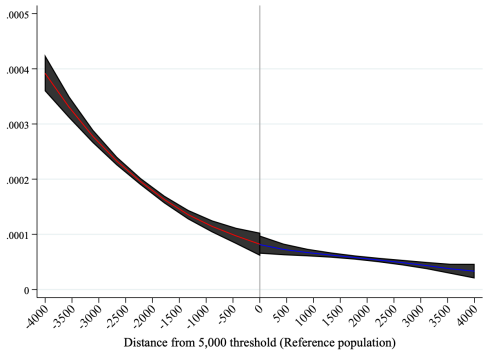
(a) Optimal bandwidth



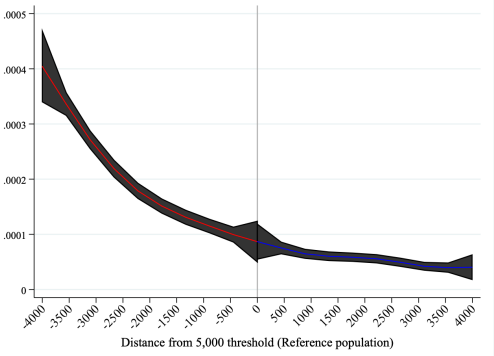
(b) 2X optimal bandwidth



(c) 3X optimal bandwidth



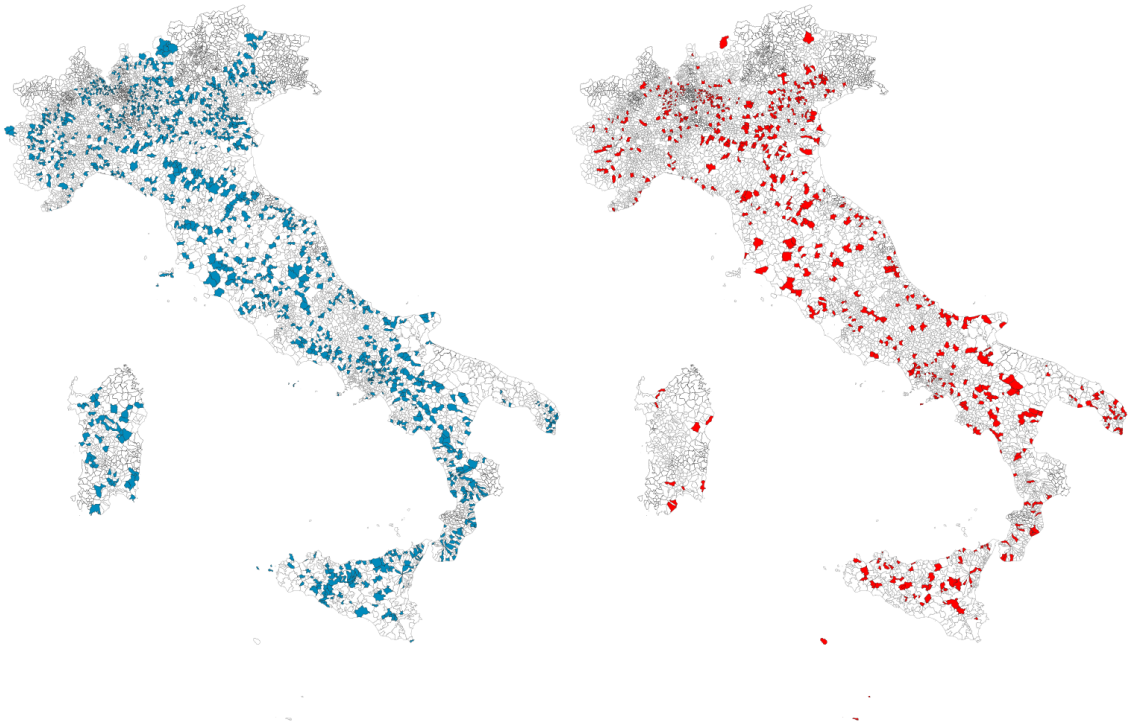
(d) Cubic polynomial



The plot shows the McCrary test, based on Cattaneo, Jansson and Ma (2020), conducted using the reference population for the application of the DSP for the year 2012. The population threshold studied is the one of 5,000 inhabitants. The test is conducted using the optimal bandwidth in panel a, using two times the optimal bandwidth in panel b and using three times the optimal bandwidth in panel c. Panel d shows the test executed using a cubic polynomial to construct the density estimators as well as the bias-corrected density estimators.

Figure A3: Geographical distribution of treatment and control

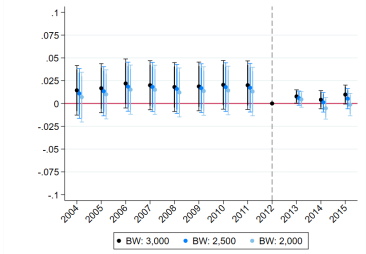
(a) Population between 2,500 and 5,000 (b) Population between 5,000 and 7,500



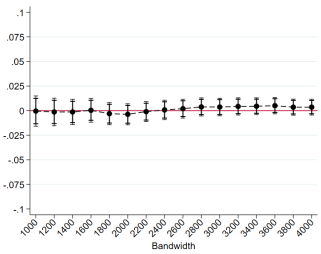
Geographical distribution of cities in the treatment (left figure) and in the control group (right figure), using a bandwidth of 2,500 inhabitants, according to the population of reference for the DSP application.

Figure A4: Effects of the DSP on local public finance and procurement (FCRs)

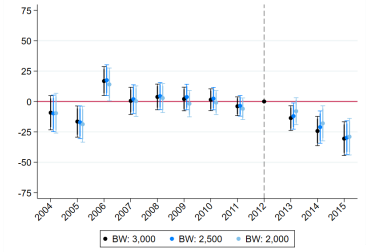
(a) Event-study: House tax



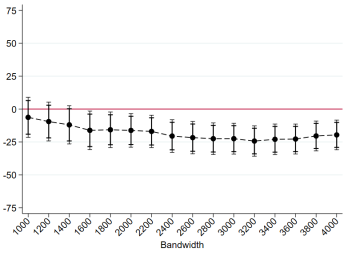
(b) DiD: House tax



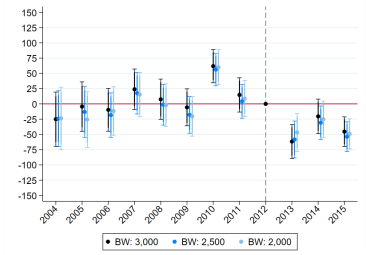
(c) Event-study: Current spend.



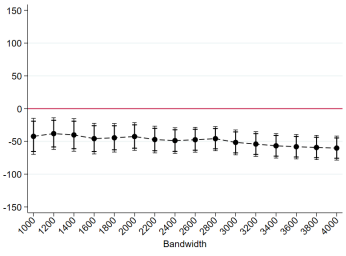
(d) DiD: Current spending



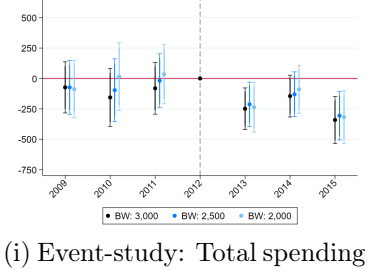
(e) Event-study: Capital spend.



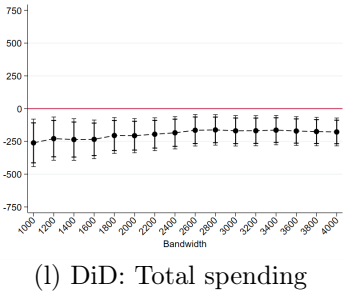
(f) DiD: Capital spending



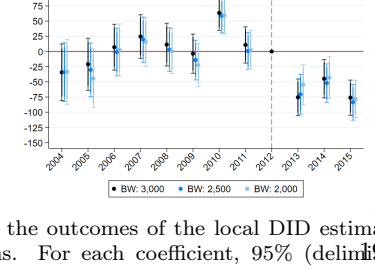
(g) Event-study: Procurement



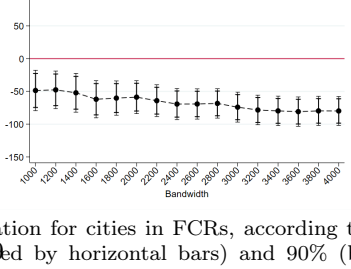
(h) DiD: Procurement



(i) Event-study: Total spending



(l) DiD: Total spending

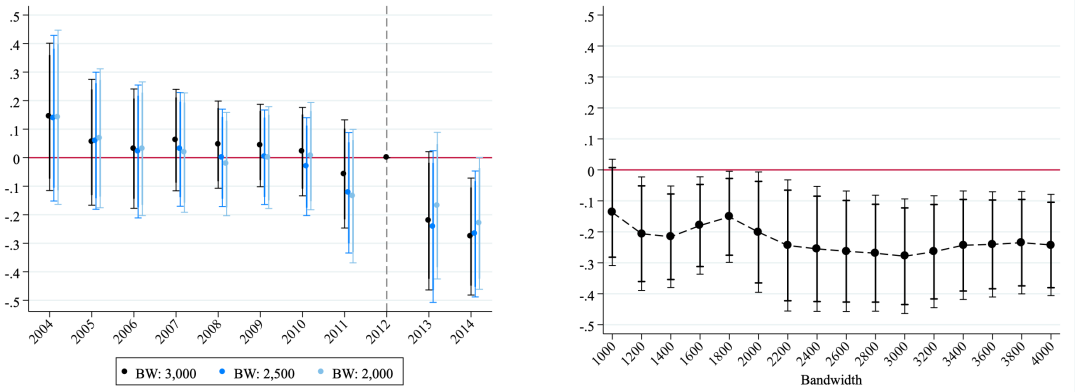


The left plot shows the outcomes of the local DID estimation for cities in FCRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID for cities in FCRs, 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 specification is the same as in Table 1 (column 3).

Figure A5: Effect of the DSP on corruption (over spending)

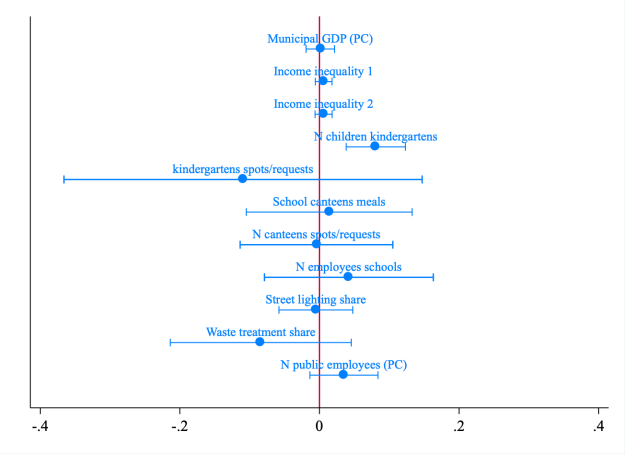
(a) Event-study: FCRs

(b) DiD: FCRs



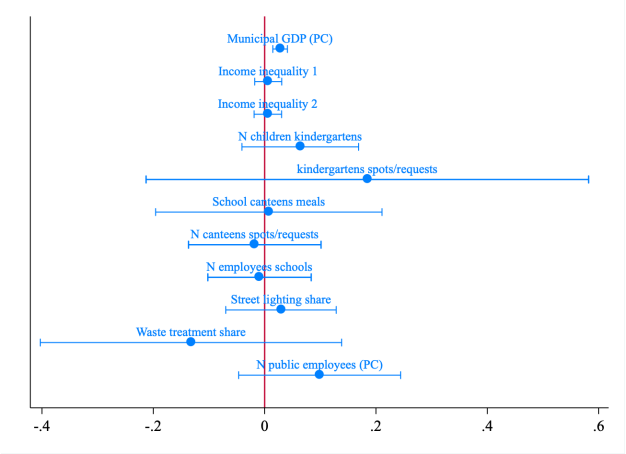
The left plot shows the outcomes of the local DID estimation, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID, according to 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. The dependent variable is corruption investigations per total spending (expressed in logarithm and standardized). The specification is the same as in Table 1 (column 3). The sample includes cities in the FCRs.

Figure A6: Impact on GDP, income inequality and municipal public services



The plot shows the impact of the introduction of the DSP on local GDP, inequality and 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 (95%) in a distinct regression according to Model 2. *Municipal GDP (PC)* measures the sum of individuals income (in thousands Euros, standardized), expressed in per capita terms, *Income inequality 1* and *Income inequality 2* captures the difference between the average income declared in the last and first income brackets: the last bracket includes 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) or 0–10,000 euros (Measure 2). *N children kindergartens* refers to the number of children attending public kindergartens; *kindergartens spots/requests* is the share between the number of available spots in public kindergardens and the number of children requests; *School canteens meals* is the number of meals provided by public schools; *N canteens spots/requests* is the share between the number of available spots in schools canteens and the number of students requests; *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. The sample includes municipalities located in FCRs.

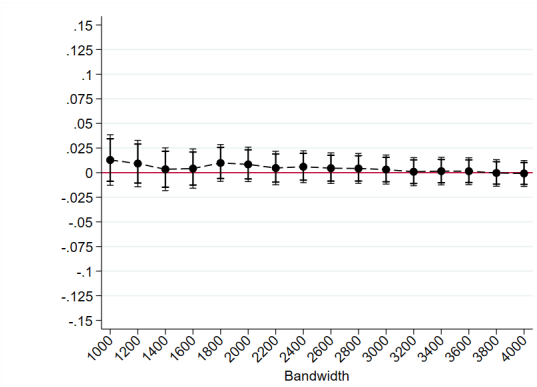
Figure A7: Impact on GDP, income inequality and municipal public services - FURs



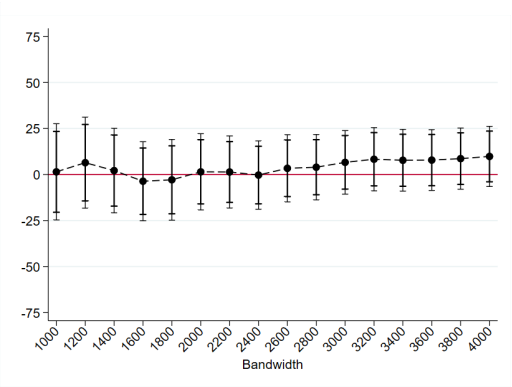
The plot shows the impact of the introduction of the DSP on local GDP, inequality and a set of municipal service (in a standardized version). Each dot is a distinct analysis and represents the DID estimator and the corresponding confidence intervals (95%) in a distinct regression according to Model 1. *Municipal GDP (PC)* measures the sum of individuals income (in thousands Euros, standardized), expressed in per capita terms, *Income inequality 1* and *Income inequality 2* captures the difference between the average income declared in the last and first income brackets: the last bracket includes 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) or 0–10,000 euros (Measure 2). *N children kindergartens* refers to the number of children attending public kindergartens; *kindergartens spots/requests* is the share between the number of available spots in public kindergardens and the number of children requests; *School canteens meals* is the number of meals provided by public schools; *N canteens spots/requests* is the share between the number of available spots in schools canteens and the number of students requests; *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. The sample includes municipalities located in FURs.

Figure A8: Effects of the DSP on local public finance and procurement (FURs) – DiD

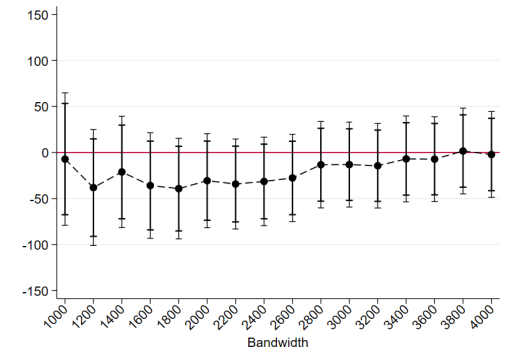
(a) House tax rate



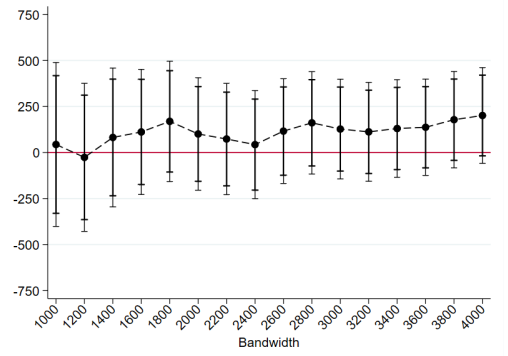
(b) Current spending (PC)



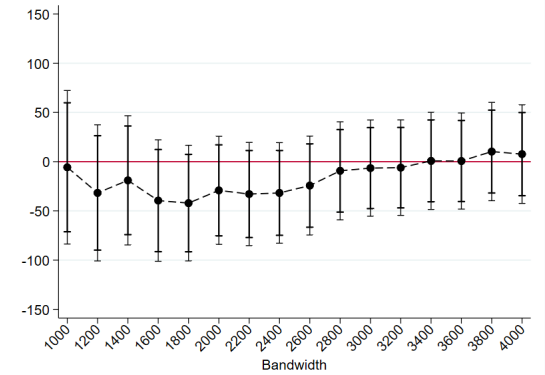
(c) Capital spending (PC)



(d) Procurement spending (PC)



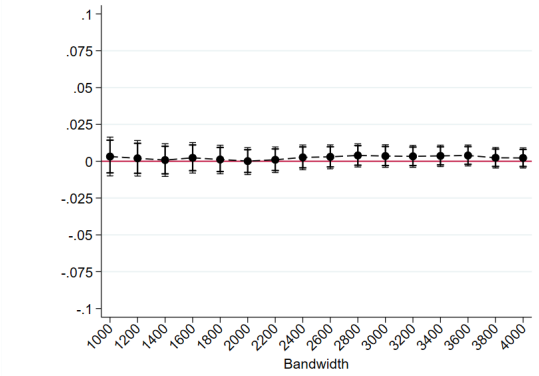
(e) Total spending



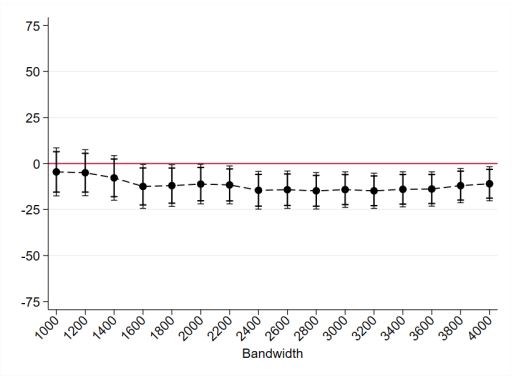
The plot shows the sensitivity analysis of the local DID for cities in FURs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables are the public finance and the procurement indicators, expressed in per capita terms. The specification is the same as in Table 1 (column 3).

Figure A9: Effects of the DSP on local public finance and procurement (Italy) – DiD

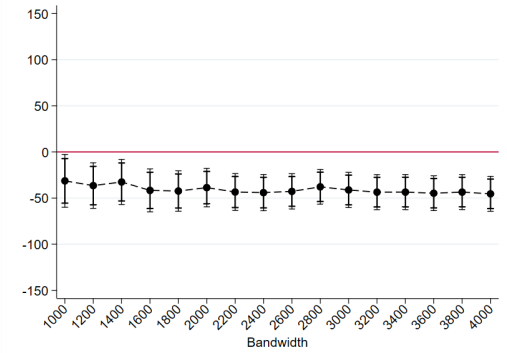
(a) House tax rate



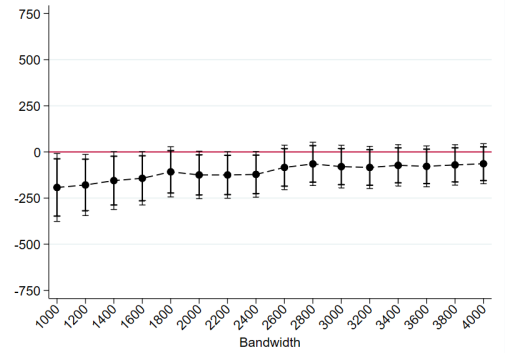
(b) Current spending (PC)



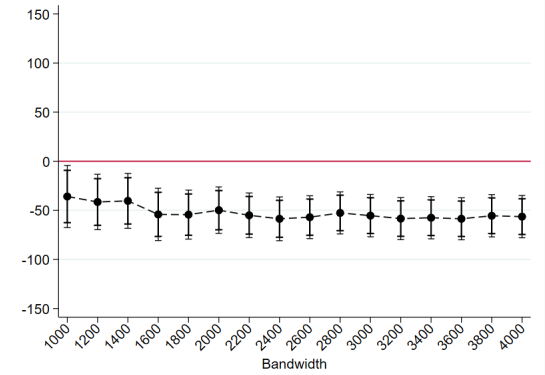
(c) Capital spending (PC)



(d) Procurement spending (PC)

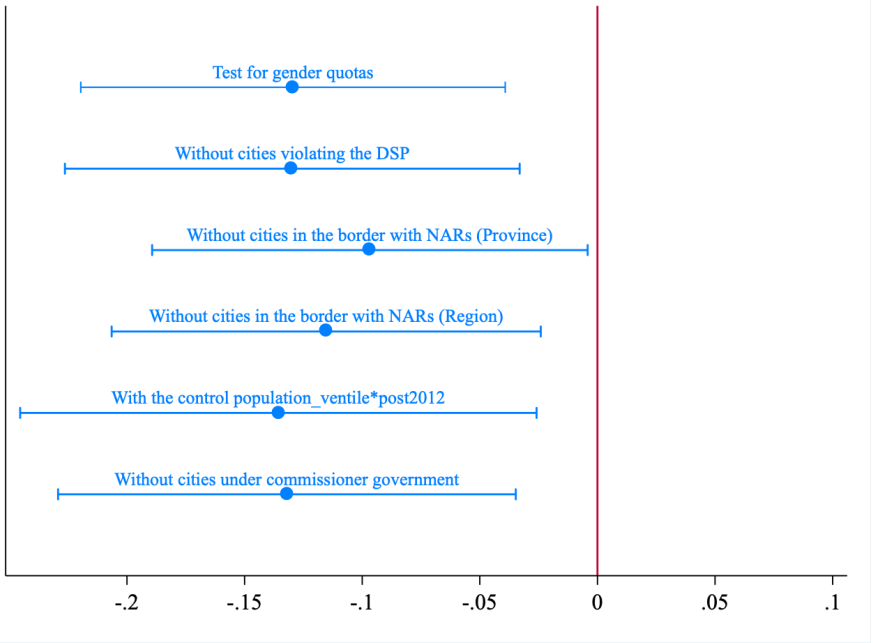


(e) Total spending



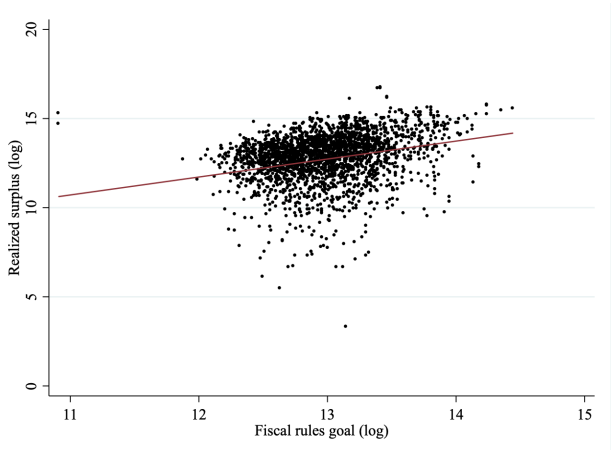
The plot shows the sensitivity analysis of the local DID for all Italian cities, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables are the public finance and the procurement indicators, expressed in per capita terms. The specification is the same as in Table 1 (column 3).

Figure A10: Robustness checks on corruption (PC)



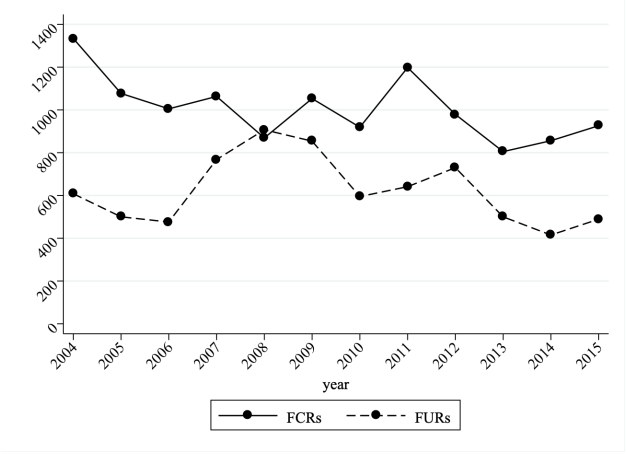
The plot shows the main robustness tests conducted on the dependent variable corruption investigations per 1,000 inhabitants (standardized). Each dot is a distinct analysis and represents the DID estimator and the corresponding confidence intervals (95%) in a distinct regression according to Model 2. The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). Robust standard errors are clustered at the municipal level. In the "Test for gender quota" we enrich the main specification with a control that captures the impact of the reform of gender quota of 2013. The test "Without cities violating the DSP" we exclude from the sample the cities that violate the DSP. The tests "Without cities in the border with FURs (Province)" and "Without cities in the border with FURs (Region)" we exclude cities located in provinces/regions located close to the FURs to control for potential displacement effects.

Figure A11: Stability pact – realized and targeted surplus (FCRs)



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 FCRs with a population of 2,500–5,000 and covers the years 2013–2015.

Figure A12: Number of convictions on corruption crimes



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

Appendix 8 (for online publication): Additional tables

Table A1: Descriptive statistics

	All cities	FCRs	FURs
Panel A: corruption			
Corruption (PC)	.013	.008	.033
Corruption (over spending)	.009	.006	.020
Corruption max (PC)	.012	.007	.030
Corruption max (over spending)	.006	.004	.015
Corruption binary	.054	.037	.120
Panel B: public finance			
House tax rate (%)	.708	.711	.695
Current expenditures (€ per capita)	808.5	829.2	726.1
Capital expenditures (€ per capita)	448.4	411.0	596.6
Panel C: local procurement			
Total amount (€ per capita)	778.2	483.9	1940.6
Panel D: local politics			
Term limit	.326	.332	.304
Margin of victory	872.6	823.9	1063.2
Mayor university degree (1 = univ. degree or above)	.850	.833	.918
Av. education councillors (1 = univ. degree or above)	.550	.503	.739
Councillors' age (Av.)	44.6	44.9	43.5
Proportion female councillors (1 = female)	.201	.219	.130
Panel E: local growth			
Municipal GDP (per-capita)	10379.4	11392.4	6370.8
Inequality measure 1	110154.7	112829.7	98231.9
Inequality measure 2	107629.2	110159.2	96352.7
Panel F: local Services			
N children kindergartens	32.0	38.1	8.0
N spots/kindergartens requests	1.3	1.3	.874
School canteen meals	1026.8	1158.9	512.1
N spots/N canteens requests	.987	.990	.977
N employees schools	3.02	3.5	1.1
Sport facilities	5.89	5.2	8.4
Street lightening (share)	.568	.571	.555
Waste management share	.901	.902	.900
N public employees	9.1	8.4	11.8

Corruption (PC) and *Corruption max (PC)* are expressed in number of investigations per 1,000 inhabitants; *Corruption (over spending)* and *Corruption max (over spending)* are expressed in number of investigations over total expenditure (expressed in logarithm and measured in euros). All amounts in Panel B and C are expressed in euros per capita by municipality and year.

Table A2: European funds

Interaction term:	EU funds (spent)						EU funds (allocated)	FCRs
	Whole Italy				FURs	FURs+ Abruzzo and Molise	Whole Italy	Whole Italy
Dependent variable:	Corruption (PC)	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)	Corruption (PC)	Corruption (PC)	Corruption (PC)	Corruption (PC)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform (T)*Treatment group (S)	-0.146***	-18.340**	-64.688***	-261.233***	-0.412***	-0.405***	-0.123**	0.046
	(0.047)	(8.146)	(13.821)	(93.222)	(0.156)	(0.155)	(0.055)	(0.061)
Post-reform (T)*interaction	-0.286	-9.122	-93.632	1411.325***	-0.937***	-0.874***	-	-
	(0.223)	(35.561)	(89.267)	(511.998)	(0.288)	(0.283)		
Treatment group (S)*interaction	-0.320**	-17.023	17.353	-949.263	-0.468	0.169	-0.289	0.108
	(0.150)	(51.830)	(108.160)	(965.683)	(0.298)	(0.337)	(0.190)	(0.081)
Post-reform (T)*Treatment group (S)*interaction	0.351**	11.160	88.210*	440.430	1.001***	0.967***	0.328*	-0.150**
	(0.162)	(22.359)	(53.197)	(332.159)	(0.321)	(0.309)	(0.197)	(0.070)
[0.5em] N	23165	25079	25079	14801	7087	7973	23429	23429
R^2	0.034	0.268	0.134	0.094	0.049	0.049	0.033	0.033
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	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

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1, 5, 6, 7, 8) and the public finance and procurement measures expressed in per-capita terms in columns (2-4). "EU funds (spent)" measures the total amount of province European funds spent starting from 2013, measured in thousands Euros PC. "EU funds (allocated)" measures the total amount of European funds received at the regional level starting from 2013, measured in thousands Euros PC. " FCRs" is a dummy variable indicating the municipalities located in FCRs. The sample includes all Italian municipalities, both from FCRs and FURs in columns (1-4, 7, 8), only the municipalities located in FURs in column (5) and the municipalities located in South of Italy (FURs as well as Abruzzo and Molise). The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Intensity margin in the application of the DSP

Dependent variable:	Corruption (PC)		Corruption (over spending)		Capital spending (PC)		Public spending (share)	
	(top 50)	(top 20)	(top 50)	(top 20)	(top 50)	(top 20)	Corruption related	Not corruption related
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stability pact (S^*T)	-0.112*	-0.507*	-0.230*	-0.965**	-88.160***	-152.288***	-0.018***	0.004
	(0.066)	(0.265)	(0.119)	(0.471)	(15.493)	(36.585)	(0.003)	(0.006)
N	8007	3029	7996	3025	8653	3266	17704	17704
R^2	0.255	0.308	0.255	0.305	0.490	0.555	0.796	0.434
City, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	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

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1-2), corruption investigations over total spending (standardized) in columns (3-4) and capital spending (per-capita) in columns (5-6) and public spending expressed in share in columns (7-8), this is divided in corruption-related spending (including trash collection, environment and transportation) in column (7) and in not corruption-related spending (all other spending categories) in column (8). Municipalities are required to accumulate a level of surplus above the top 50% or 20% of the variable distribution, respectively, in columns (1, 3, 5) and (2, 4, 6). The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). The sample only includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Impact on discretionary tenders and politicians productivity

Dependent variable:	Perc. tenders <40K	Perc. amount <40K	Amount PC <40K	N council resolutions	N government resolutions
	(1)	(2)	(3)	(4)	(5)
Stability pact ($S*T$)	-0.004	-0.003	-2.374*	4.239***	5.624*
	(0.010)	(0.009)	(1.237)	(1.299)	(3.374)
[0.5em] N	10420	10420	10420	9373	9378
R^2	0.238	0.216	0.236	0.542	0.713
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 region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and, in columns 1-3 are interacted with year fixed effects). The sample includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Political accountability and ability - FURs

Interaction term:	Electoral period		Mayor term limited	
Dependent variable:	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending
	(1)	(2)	(3)	(4)
Stability pact (S*T)	0.0283	-0.0228	0.0817	0.0150
	(0.0758)	(0.0890)	(0.0736)	(0.0881)
Stability pact (S*T)*interaction	0.152	0.170	0.0350	0.0791
	(0.118)	(0.141)	(0.122)	(0.150)
N	7077	7042	7382	7347
R^2	0.263	0.271	0.257	0.264
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1, 3 and 5) and corruption over spending (standardized) in columns (2, 4 and 6). *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, *Mayor term limited* (columns 3-4), which is a dummy equal to one if the mayor is not eligible for re-election and *Mayor high education* (columns 5-6), which is a dummy capturing those cities whose mayor has a university degree. The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). In columns (3-6) political controls only include age and gender of the councillors (that refer to the previous electoral term and are interacted with year fixed effects). The specification also includes $S * interaction_i$, $T * interaction_i$ and $interaction_i$, which are not displayed in the table. The sample only includes municipalities located in FURs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Political accountability and ability - controlling for selection

Interaction term:	Electoral period		Mayor term limited	
Dependent variable:	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending
	(1)	(2)	(3)	(4)
Stability pact (S*T)	0.008	-0.076	-0.124**	-0.246**
	(0.076)	(0.122)	(0.049)	(0.098)
Stability pact (S*T)*interaction	-0.193**	-0.255*	0.094	0.153
	(0.091)	(0.151)	(0.063)	(0.125)
N	16213	16181	16443	16410
R^2	0.231	0.235	0.228	0.232
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1, 3 and 5) and corruption over spending (standardized) in columns (2, 4 and 6). *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, *Mayor term limited* (columns 3-4), which is a dummy equal to one if the mayor is not eligible for re-election and *Mayor high education* (columns 5-6), which is a dummy capturing those cities whose mayor has a university degree. The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). In columns (3-6) political controls only include age and gender of the councillors (that refer to the previous electoral term and are interacted with year fixed effects). The specification also includes $S * interaction_i$, $T * interaction_i$ and $interaction_i$, which are not displayed in the table. The sample only includes municipalities located in FCRs. Municipalities with elections after the introduction of fiscal rules in 2013 are excluded from the sample. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effect of the DSP on corruption – 1,000 inhabitant threshold

Dependent variable:	Corruption (PC)	Total revenues (PC)	Current spending (PC)	Capital spending (PC)
	(1)	(2)	(3)	(4)
Stability pact ($S*T$)	0.044	-1.557	-17.849	-1.142
	(0.029)	(9.045)	(11.698)	(22.42)
N	13254	14436	14489	14489
R^2	0.176	0.882	0.883	0.451
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	500	500	500	500

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 region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). The sample includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Effect of DSP on single corruption charges

Dependent variable:	Strict corruption	Graft	Malfeasance	Embezzlement
	(1)	(2)	(3)	(4)
Stability pact ($S*T$)	-0.008	-0.051	-0.143**	-0.086*
	(0.021)	(0.049)	(0.065)	(0.046)
N	16314	16314	16314	16314
R^2	0.166	0.168	0.214	0.125
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 region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). The sample includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Impact of DSP - Sample 2008-2015

Dependent variable:	Corruption (PC)	Corruption over spending	Total revenues (PC)	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact (T^*S)	-0.097**	-0.194**	2.659	-21.594***	-55.620***	-170.960***
	(0.047)	(0.099)	(6.584)	(6.030)	(8.775)	(61.957)
N	10418	10398	11824	11836	11836	10420
R^2	0.333	0.322	0.911	0.932	0.466	0.227
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 variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1), corruption investigations over total spending (standardized) in columns (2) and public finance and procurement measures expressed in per-capita terms in columns (3-6). The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). The sample includes municipalities located in FCRs. The time span of the analysis is limited to the period 2008-2015. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Additional robustness checks

	Variables non-standardized		Spending in absolute value		Corruption max	Corruption max	Corruption	Neighbour cities	Police spending	Other crimes
Dependent variable:	Corruption per-capita	Corruption over spending	Corruption over spending	Corruption over no. councillors	Corruption max (PC)	Corruption max over spending	binary indicator	Corruption (PC)	Police spending (PC)	Non-corruption crimes (PC)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Stability pact (S^*T)	-0.010***	-0.003**	-0.1564**	-0.3033**	-0.126**	-0.253**	-0.014*	-0.030	-0.052	-0.000
	(0.004)	(0.001)	(0.0648)	(0.1301)	(0.049)	(0.105)	(0.007)	(0.050)	(0.672)	(0.001)
N	16314	16282	16282	16099	16314	16282	17797	7535	17797	14836
R^2	0.231	0.235	0.195	0.232	0.224	0.231	0.208	0.255	0.764	0.874
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 variable is corruption investigations per 1,000 inhabitants non standardized in column (1), corruption over spending non standardized in column (2), corruption over spending in absolute value (standardized) in column (3), corruption over the total number of councillors in the municipality in column (4), corruption max per 1,000 inhabitants (standardized) in column (5), corruption max over spending standardized in column (6), corruption binary in column (7), corruption investigations per 1,000 inhabitants standardized in column (8), spending in local police per-capita in column (9) and the number of committed infractions for non-corruption crimes, expressed in per-capita terms in column (10). In column (8) the analysis is conducted only on 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. The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). The sample includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Robustness checks: alternative specifications

	Omitting distance to the threshold		Including province-year FE		Clustering: province		Clustering: region		Clustering: municipality, province-year	
Dependent variable:	Corruption per-capita	Corruption over spending	Corruption per-capita	Corruption over spending	Corruption per-capita	Corruption over spending	Corruption per-capita	Corruption over spending	Corruption per-capita	Corruption over spending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Stability pact (T^*S)	-0.116**	-0.238**	-0.113**	-0.234**	-0.130**	-0.255**	-0.130*	-0.255*	-0.130**	-0.255**
	(0.045)	(0.094)	(0.048)	(0.103)	(0.040)	(0.081)	(0.061)	(0.138)	(0.051)	(0.105)
N	16314	16282	16314	16282	16314	16282	16314	16282	16314	16282
R^2	0.231	0.235	0.293	0.285	0.231	0.235	0.231	0.235	0.231	0.235
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 variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1), (3), (5), (7) and (9) and corruption investigations over total spending (standardized) in columns (2), (4), (6), (8) and (10). The specification includes municipality and region-year fixed effects, the distance from the population threshold, the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term and are interacted with year fixed effects). The sample includes municipalities located in FCRs. In columns (1) and (2) the distance from the population threshold is omitted, in columns (3) and (4) province-year fixed effects are included. The analysis of columns (5-10) have been conducted with a different standard errors clustering: at the province level (columns 5, 6), at the region level (column 7, 8) and at the municipality, province-year level (columns 9 and 10). In the other columns robust standard errors are clustered at the municipal level: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: 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

Table A13: Domestic Stability Pact - evolution of institutional features

Year	Target of the DSP rules	Rule	Basis of accounting	Covered entities	Size of covered municipalities	Changes	Laws
1999	Fiscal gap	Zero growth	Cash basis	Regions, provinces, municipalities	All		L.448/1998, art. 28 (1999 budget law)
2000	Fiscal gap	Zero growth	Cash basis	Regions, provinces, municipalities	All		L.488/1999, art. 30 (2000 budget law)
2001	Fiscal gap	Max 3% growth wrt 2 yrs before	Cash basis	Regions, provinces, municipalities	Above 5,000	Population threshold for covered municipalities	L.388/2000, art. 53 (2001 budget law)
2002a	Fiscal gap	Max 2.5% growth wrt 2 yrs before	Cash basis	Regions, provinces, municipalities	Above 5,000	Double constraint	L.448/2001, art. 24 (2002 budget law)
2002b	Current expenditure	Max 6% growth wrt 2 yrs before	Cash basis and accrual basis	Regions, provinces, municipalities	Above 5,000	Double constraint	L.448/2001, art. 24 (2002 budget law)
2003	Fiscal gap (excluding capital expenses)	Zero growth wrt 2 yrs before	Cash basis and accrual basis	Regions, provinces, municipalities	Above 5,000	Constraint must be verified both on cash and accrual basis	L.289/2002, art. 29 (2003 budget law)
2004	Fiscal gap (excluding capital expenses)	Max 1.7% growth wrt 1 yr before	Cash basis and accrual basis	Regions, provinces, municipalities	Above 5,000		L.350/2003, art. 3 (2004 budget law)
2005	Total expenditure (including capital expenses)	Different thresholds depending on virtuosity of the entity	Cash basis and accrual basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000	Extension of the entities subjected to the DSP	L.311/2004, art. 1, co. 33 e seg.; co. 98 (2005 budget law)
2006	Total expenditure (including capital expenses)	Different thresholds depending on virtuosity of the entity	Cash basis and accrual basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000		L.266/2005, art. 1, co. 189 e seg. (2006 budget law)
2007	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Cash basis and accrual basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000	Revision of monitoring (introduction of mandatory transmission of financial statements) Revision of sanctions (introduction of cd. meccanismo di automatismo fiscale)	L.296/2006, art. 1, co. 676 e seg. (2007 budget law)
2008	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000	Introduction of mixed basis accounting Revision of monitoring (introduction of SIOPE as monitoring platform + DSP infringment for failure of financial statements transmission) Revision of sanctions	L.196/2007 L.244/2007, art. 1, co. 368 e seg. (2008 budget law)
2009	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000		D.L.112/2008, art. 77 L.133/2008, art. 61, co. 10 (penalties for non-compliers) L.203/2008, art. 2 (2009 budget law)
2010	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000		L.191/2009
2011	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000		D.L.78/2010, art. 14 L.220/2010, art. 1, co. 141 e seg. (2011 budget law)
2012	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities	Above 5,000	Revision of sanctions	D.L. 98/2011, art. 20, c. 3 L.183/2011, art. 31 (2012 budget law)
2013	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities (above 5,000 inhabitants)+ municipal companies, administrations dismissed because of Mafia infiltration	Above 1,000	Population threshold for covered municipalities Extension of the entities subjected to the DSP	D.L. 95/2012 L.228/2012, art. 1, co. 428-447 (2013 budget law)
2014	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities (above 1,000 inhabitants)+ municipal companies, administrations dismissed because of Mafia infiltration	Above 1,000	Extension of the entities subjected to the DSP	L.147/2013 (2014 budget law)
2015	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities (above 1,000 inhabitants)+ municipal companies, administrations dismissed because of Mafia infiltration	Above 1,000		L.190/2014, art. 1, co. 461 e seg. (2015 budget law)
2016	Fiscal gap (including capital expenses)	Different thresholds depending on virtuosity of the entity	Mixed basis	Regions, provinces, municipalities + mountain and island communities, union of municipalities (above 1,000 inhabitants)+ municipal companies, administrations dismissed because of Mafia infiltration	Above 1,000		L.208/2015 (2016 budget law)

The table shows the legislative evolution of DSP for the Italian local jurisdictions in the time span 2004-2016. The sources of the institutional information are the following: 2001-2005 (Web link), 2006 (Web link), 2007-2010 general (Web link), 2007-2010 local jurisdictions (Web link), 2011-2013 (Web link), 20132015 (Web link).

Appendix 9 (for online publication): Analysis with a difference-in-discontinuity methodology

We conduct the main analysis on corruption using the Difference-in-Discontinuity methodology, in order to check whether the main results are robust to the application of this alternative empirical strategy. This methodology has been used in many recent studies (e.g. Campa, 2011, Grembi et al., 2016) and it is based on the comparison between the outcome before and after the reform for municipalities around the population threshold.[56] Appendix Table A14 shows this test for the results on corruption charges, relying on the optimal bandwidth according to Calonico et al., (2014). We also report this analysis with augmented bandwidths by 25% and 50%. Using a similar approach, we report the results on local budget in Appendix Tables A15, A16 and A17. The negative effect of DSP on corruption and on spending emerges also with this methodology and the magnitude of the coefficients is similar to the one estimated using the local DID methodology. In Table A18, we report the findings on accountability. also in this case, the results are similar although the significance disappears in some specifications.

Finally, only for the main findings on corruption, we report a placebo where we estimate the effects at artificial thresholds (instead of 5,000 inhabitants) increasing of decreasing the optimal thresholds with multiples of 200. In line with our expectations, the results are weak and almost never statistically significant adopting these placebo thresholds (Table A19).

[56] The empirical model to be estimated is as follows:

$$y_{it} = \beta_0 + \beta_1 P_i^* + S_{it}(\gamma_0 + \gamma_1 P_i^*) + T_t[\delta_0 + \delta_1 P_i^* + S_{it}(\zeta_0 + \zeta_1 P_i^*)] + \xi' X_{it} + \epsilon_{it} \quad (3)$$

Where P_i^*, S_{it} and T_t are defined as in Model 1, and X_{it} includes year and province fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of the mayor and elected councillors - all these variables refer to the previous electoral term). 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).

Table A14: Diff-in-Disc analysis on corruption

	Italy		FCRs		FURs	
Dependent variable:	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Optimal bandwidth						
Stability pact (T^*S)	-0.107	-0.185*	-0.148**	-0.225*	-0.011	-0.017
	(0.073)	(0.106)	(0.072)	(0.125)	(0.134)	(0.153)
N	14965	16662	10168	8948	4838	5305
R^2	0.052	0.049	0.066	0.057	0.053	0.055
Panel B: Optimal bandwidth + 25%						
Stability pact (T^*S)	-0.110*	-0.188**	-0.132**	-0.192	0.036	0.002
	(0.056)	(0.084)	(0.065)	(0.148)	(0.117)	(0.139)
N	19498	21901	13169	11472	6460	7007
R^2	0.045	0.043	0.051	0.053	0.049	0.053
Panel C: Optimal bandwidth + 50%						
Stability pact (T^*S)	-0.083	-0.132	-0.111*	-0.253**	0.036	0.020
	(0.052)	(0.082)	(0.058)	(0.111)	(0.111)	(0.127)
N	24586	27749	16531	14142	8228	8903
R^2	0.043	0.044	0.045	0.047	0.046	0.054
Province, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1), (3) and (5) and corruption investigations over total spending (standardized) in columns (2), (4) and (6). The specification is the one of model 3 and it includes province and year fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of mayor and elected councillors - all these variables represent the previous electoral term. The sample includes municipalities located in FCRs in columns (3-4), municipalities located in FURs in columns (5-6) and all Italian municipalities in columns (1-2). Panel A shows the estimates computed using the optimal bandwidth, panel B shows the estimates with the optimal bandwidth augmented by 25%, panel C shows the estimates augmented by 50%. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Diff-in-Disc analysis on public finance - Optimal bandwidth

Dependent variable:	House tax rate	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)	Total spending (PC)
	(1)	(2)	(3)	(4)	(5)
Panel a: Italy					
Stability pact (T^*S)	-0.002	-34.436	-63.385*	-346.124**	-102.747**
	(0.012)	(25.957)	(32.461)	(146.470)	(46.357)
N	9632	8676	9781	9172	8891
R^2	0.741	0.427	0.195	0.156	0.269
Panel b: FCRs					
Stability pact (T^*S)	-0.009	-38.895	-76.070**	-263.855*	-120.531**
	(0.013)	(34.114)	(31.729)	(152.608)	(51.957)
N	8257	5977	6526	5967	5681
R^2	0.770	0.400	0.174	0.051	0.277
Panel c: FURs					
Stability pact (T^*S)	0.012	-13.493	23.231	-392.894	29.543
	(0.023)	(36.215)	(82.691)	(351.255)	(104.797)
N	2847	2274	3634	2466	2873
R^2	0.685	0.466	0.160	0.140	0.198
Province, year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

The dependent variables are the public finance and procurement measures expressed in per-capita terms. The specification is the one of model 3 and it includes province and year fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of mayor and elected councillors - all these variables represent the previous electoral term. The sample includes municipalities located in FCRs in panel a, municipalities located in FURs in panel b and all Italian municipalities in panel c. All the estimates computed using the optimal bandwidth. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Diff-in-Disc analysis on public finance - Optimal bandwidth + 25%

Dependent variable:	House tax rate	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)	Total spending (PC)
	(1)	(2)	(3)	(4)	(5)
Panel a: Italy					
Stability pact (T^*S)	-0.005	-27.590	-52.017*	-222.572*	-82.970**
	(0.011)	(23.444)	(28.408)	(132.881)	(40.560)
N	12283	11084	12438	11918	11324
R^2	0.740	0.429	0.191	0.155	0.283
Panel b: FCRs					
Stability pact (T^*S)	-0.006	-38.092	-68.456***	-253.120*	-121.765**
	(0.012)	(32.655)	(25.884)	(133.407)	(48.747)
N	10283	7654	8361	7663	7254
R^2	0.765	0.411	0.181	0.046	0.291
Panel c: FURs					
Stability pact (T^*S)	0.020	-12.417	-10.225	-70.277	31.465
	(0.021)	(35.360)	(70.388)	(321.919)	(92.551)
N	3653	2904	4769	3221	3683
R^2	0.691	0.456	0.155	0.132	0.207
Province, year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

The dependent variables are the public finance and procurement measures expressed in per-capita terms. The specification is the one of model 3 and it includes province and year fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of mayor and elected councillors - all these variables represent the previous electoral term. The sample includes municipalities located in FCRs in panel a, municipalities located in FURs in panel b and all Italian municipalities in panel c. All the estimates computed using the optimal bandwidth by 25%. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Diff-in-Disc analysis on public finance - Optimal bandwidth + 50%

Dependent variable:	House tax rate	Current spending (PC)	Capital spending (PC)	Procurement spending (PC)	Total spending (PC)
	(1)	(2)	(3)	(4)	(5)
Panel a: Italy					
Stability pact (T^*S)	-0.004	-17.815	-33.681	-301.573**	-55.358
	(0.010)	(21.681)	(25.040)	(122.160)	(37.662)
N	14878	13402	15071	15044	13713
R^2	0.738	0.416	0.189	0.162	0.270
Panel b: FCRs					
Stability pact (T^*S)	-0.005	-23.094	-42.478*	-313.314**	-87.182*
	(0.011)	(29.892)	(23.440)	(122.710)	(45.543)
N	12669	9232	10004	9578	8846
R^2	0.751	0.383	0.170	0.042	0.277
Panel c: FURs					
Stability pact (T^*S)	0.009	14.415	-15.354	-118.952	-12.463
	(0.020)	(34.546)	(59.951)	(301.247)	(82.104)
N	4535	3582	6054	4126	4620
R^2	0.690	0.477	0.154	0.127	0.204
Province, year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

The dependent variables are the public finance and procurement measures expressed in per-capita terms. The specification is the one of model 3 and it includes province and year fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of mayor and elected councillors - all these variables represent the previous electoral term. The sample includes municipalities located in FCRs in panel a, municipalities located in FURs in panel b and all Italian municipalities in panel c. All the estimates computed using the optimal bandwidth augmented by 50%. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Diff-in-Disc analysis on political accountability

Interaction term:	Electoral period		Mayor term limited		Mayor high education	
Dependent variable:	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending	Corruption (PC)	Corruption over spending
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Optimal bandwidth						
Stability pact (S^*T)	-0.120*	-0.172	-0.177**	-0.228*	0.062	0.040
	(0.073)	(0.137)	(0.077)	(0.131)	(0.110)	(0.150)
Stability pact (S^*T)*interaction	-0.083	-0.181	0.106	0.026	-0.223*	-0.298**
	(0.078)	(0.149)	(0.070)	(0.117)	(0.118)	(0.138)
N	10168	8948	10168	8948	10314	9074
R^2	0.066	0.057	0.066	0.057	0.067	0.058
Panel B: Optimal bandwidth + 25%						
Stability pact (S^*T)	-0.085	-0.129	-0.160**	-0.246*	0.089	0.170
	(0.069)	(0.161)	(0.068)	(0.138)	(0.110)	(0.236)
Stability pact (S^*T)*interaction	-0.148**	-0.200	0.094	0.194	-0.235**	-0.391*
	(0.070)	(0.142)	(0.074)	(0.150)	(0.107)	(0.203)
N	13169	11472	13169	11472	13376	11645
R^2	0.051	0.053	0.051	0.053	0.051	0.054
Panel C: Optimal bandwidth + 50%						
Stability pact (S^*T)	-0.059	-0.184	-0.137**	-0.297**	0.063	0.162
	(0.059)	(0.118)	(0.065)	(0.121)	(0.093)	(0.200)
Stability pact (S^*T)*interaction	-0.165**	-0.219*	0.089	0.144	-0.173**	-0.403**
	(0.069)	(0.129)	(0.068)	(0.138)	(0.087)	(0.191)
N	16531	14142	16531	14142	16764	14357
R^2	0.046	0.047	0.045	0.047	0.046	0.048
Province, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1), (3) and (5) and corruption investigations over total spending (standardized) in columns (2), (4) and (6). The specification is the one of model 3 and it includes province and year fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of mayor and elected councillors - all these variables represent the previous electoral term. *Interaction* is a term which represents *Electoral period* (columns 1-2), which is a dummy equal to one in the electoral year, *Mayor term limited* (columns 3-4), which is a dummy equal to one if the mayor is not eligible for re-election and *Mayor high education* (columns 5-6), which is a dummy capturing those cities whose mayor has a university degree. The sample includes municipalities located in FCRs. Panel A shows the estimates computed using the optimal bandwidth, panel B shows the estimates with the optimal bandwidth augmented by 25%, panel C shows the estimates augmented by 50%. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19: Diff-in-Disc analysis on corruption - Placebo thresholds

	band.+200	band.-200	band.+400	band.-400	band.+600	band.-600	band.+800	band.-800
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Corruption per-capita								
Stability pact (T^*S)	-0.025	-0.106	0.051	-0.122*	0.056	-0.066	-0.030	0.060
	(0.080)	(0.076)	(0.084)	(0.070)	(0.088)	(0.069)	(0.089)	(0.079)
N	7782	8649	6212	9418	5070	9214	7251	9430
R^2	0.069	0.065	0.075	0.057	0.073	0.058	0.042	0.054
Panel B: Corruption over spending								
Stability pact (T^*S)	-0.044	-0.155	0.092	-0.160	0.117	-0.121	-0.087	0.083
	(0.153)	(0.121)	(0.165)	(0.115)	(0.178)	(0.099)	(0.188)	(0.109)
N	7397	10276	5678	9988	4936	10645	6833	10131
R^2	0.080	0.057	0.054	0.055	0.082	0.050	0.047	0.050
Province, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in panel A and corruption investigations over total spending (standardized) panel B. The table shows the estimates of Table A14 computed permuting the optimal bandwidth by multiples of 200. The specification is the one of model 3 and it includes province and year fixed effects as well as a set of political controls (margin of victory, term limit indicator, education, age and gender of mayor and elected councillors - all these variables represent the previous electoral term. The sample includes municipalities located in FCRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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