Corruption under Austerity*

Gianmarco Daniele[1] and Tommaso Giommoni[2]

[1]University of Milan
[2]ETH Zurich

April 2022

Abstract

We study how policies limiting the spending capacity of local governments may reduce corruption. We exploit the extension of one such policy, the Domestic Stability Pact (DSP), to small Italian municipalities. The DSP led to a decrease in both recorded corruption rates and corruption charges per euro spent. This effect emerges only in areas in which the DSP put a binding cap on municipal capital expenditures. The reduction in corruption is linked to accountability incentives as it emerges mostly in pre-electoral years and for re-eligible mayors. We then estimate the impact of the extension of the DSP on local welfare in the following years, finding a null effect. Overall, our findings suggest that budget constraints might induce local governments to curb expenditures in a way that dampens their exposure to corruption.

JEL codes: D72, D73, H62, H72, K34.
Keywords: corruption, austerity, fiscal rules, European funds, local public finance, public procurement.

*We thank Zareh Asatryan, Elliott Ash, Audinga Baltrunaite, Sascha Becker, Francesco Di Comite, Eliana La Ferrara, Vincenzo Galasso, Sergio Galletta, Matteo Gamalerio, Ethan Kaplan, Marko Köthenbürger, Federico Masera, Giovanna Messina, Massimo Morelli, Tommaso Nannicini, Antonio Nicolò, Tommaso Orlando, Anna Peta, Paolo Pinotti, Guido Tabellini, Alessio Terzi, Marcin Zogala and seminar participants at the DG ECFIN (European Commision); Economics and Politics Workshop in Brussels (Universitè Libre de Bruxelles); Workshop on Political Economy in Bruneck (University of Bolzano); Centre for Law and Economics (ETH Zurich); 5th Workshop on the Economics of Organized Crime (Bocconi University – Baffi Carefin); IEB Workshop on Political Economy (Institut d'Economia Barcelona); Do Re Mee Workshop Dondena (Bocconi University); Workshop on Institution, Individual Behavior and Economic Outcomes (Alghero, Italy); Oz Virtual Econ Research Seminar; SIOE 2020 (online edition) A special thanks in memory of Alberto Alesina, who inspired us towards this line of research. Gianmarco Daniele gratefully acknowledges financial support of UniCredit Foundation Modigliani Research Grant 10th edition.
Gianmarco Daniele: gianmarco.daniele@unimi.it; Tommaso Giommoni: giommoni@kof.ethz.ch

1 Introduction

Suppose a country is subject to a new budget constraint, like a set of austerity measures. How will politicians accordingly change public spending? Is this budget shock going to be beneficial to the society? These are relevant questions for economists and policymakers, which became even more salient after the Great Recession, as austerity measures have been widely adopted by national governments and supra-national institutions. In this paper, we analyse fiscal austerity studying Italian municipalities which are subject to a new form of budget constraint. We find that a simple budget rule, the Domestic Stability Pact (henceforth 'DSP'; *Patto di Stabilità Interno*, in Italian), led to a reduction in corruption charges driven by lower discretionary spending.

The DSP is a set of fiscal rules 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. As already shown by Grembi et al. (2016), fiscal rules are effective in reducing deficit accumulation among Italian municipalities. Similar policies are common in decentralized countries, in which local authorities receive transfers from the national government, and may not entirely internalize the cost of spending. In the last decades, similar budget constraints rules have become increasingly common, due to the substantial rise in public debts threatening fiscal sustainability. The International Monetary Fund (IMF) currently lists 96 countries that have adopted local, national or supra-national fiscal rules (Lledó et al.,2017).[1]

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

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

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

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

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.

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

As we expect the DSP to affect corruption through its effects on public spending, we start by testing the effect of the DSP on municipal budget outcomes, which represents the first-stage of the analysis. We find that municipalities receiving standard flows of European transfers (hereafter called 'low-funds regions' or 'LFRs') are induced by the DSP into reducing public investments (i.e., capital and procurement expenditure). Conversely, in HFRs municipalities, the DSP does not affect public budget outcomes. This can be explained by the presence of large EU transfers, which financed capital expenditure and were not targeted by the fiscal rules. An alternative explanation could be that municipalities in HFRs are just not respecting the DSP requirements: this is unlikely as 99% of treated municipalities abide to the requirements of the DSP (in both LFRs and HFRs). In other words, EU transfers allow these municipalities to comply with the requirements of the DSP without the need of cutting expenditure or increase taxes.

In line with the first-stage findings, we find a change in corruption charges only in LFRs municipalities. Specifically, the DSP substantially decreased corruption per capita and per euro spent in municipalities receiving standard flows of European transfers, in which the DSP actively constrained capital spending. The drop in corruption rates can be explained by the reduction in discretionary spending that are more liable to be affected by mismanagement and rent seeking (Hessami, 2014; Mauro, 1995; Liu and Mikesell, 2014). Instead, we do not observe any improvement in corruption in towns located in HFRs, where the grip of fiscal rules on public investments was feebler. Overall, when considering the full sample of all Italian regions, we find a mild reduction on corruption investigations per euro spent and on corruption investigations per capita. To further validate the heterogeneous effect between LFRs and HFRs, we adopt a triple interaction approach, exploiting the local amount of EU funds received in treated and control municipalities after 2013.[2]

The results in LFRs are not just due to a mechanical decrease in public expenditure. Local politicians retain a margin of discretion in choosing courses of action that are compatible with budget constraints. For instance, they might decide to increase local taxes or reduce current expenditure. Alternatively, they may cut the most likely rent-seeking-affected areas, i.e. capital and procurement expenditures. In line with this interpretation, we observe a decrease in corruption charges per euro 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. Finally, in line with the idea that politicians are

[2]In this light, we show that i) corruption decreases also in HFRs, when considering areas receiving a low amount of EU funds and ii) we test the robustness of our findings restricting the sample to only municipalities in Southern Italy, as two of them are included in the LFRs group (Molise and Abruzzo).

more efficient under the DSP, we document an increase in their productivity, measured by novel data on the number of approved municipal deliberations.

As mentioned above, electoral accountability might explain these findings. This is plausible if the DSP made corruption more expensive as it raised the opportunity cost of public spending. We can exploit two exogenous variations in electoral incentives to determine whether accountability is at work. First, Italian municipalities can be split into five groups, each on a different 5-year long electoral schedule: this staggered timing of municipal elections is due to historical reasons and provides exogenous variation in the electoral cycle (Repetto, 2018; Giommoni, 2019). We find that corruption decreases in LFRs 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 LFRs mayors in their first term who can stand for re-election. Overall, both tests suggest that, under the DSP, accountability incentives lead local politicians to reduce rent-seeking.

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

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

2 Contribution to the literature

This paper advances four strands of the literature.

First, we relate to studies on the design of public budget constraints and on the effects of austerity policies. The main goal of this literature has been to understand whether and how fiscal adjustment can improve welfare systems sustainability (Alesina and Perotti, 1995; Alesina and Perotti, 1996). Recent studies have also focused on the political effects of austerity policies (Stiglitz, 2016; Fetzer, 2019). Our approach is similar to the studies which exploit within-country variation to test the effects of budget constraints: previous evaluations find mixed results in terms of deficit and public debt reduction (Grembi et al., 2016; Coviello et al., 2017; Gamalerio, 2017; Heinemann et al., 2018; Asatryan et al., 2015).[3] The main contribution of this paper is to show that budget constraints may lead to an unintended reduction in corruption through a drop in discretionary spending. While fiscal policy obviously is (and ought to be) motivated by considerations other

[3]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).

than its potential spill-over impact on corruption, these unintended effects might be of interest to international organizations and governments debating whether (and how) to introduce budgetary constraints. Our findings are also timely, as we shed light on the effects on corruption of two salient and highly debated policies – EU transfers and fiscal rules – that affect a multitude of local governments on the European continent.

Second, we relate to the literature on anti-corruption policies. While previous studies have compiled considerable evidence on the detrimental effects of corruption (*e.g.*, Olken and Pande, 2012), there is still a lack of agreement on how to best fight it (*e.g.*, Golden, 2018; Fisman and Golden, 2017; De Vries and Solaz, 2017). A common feature of anti-corruption policies is the creation of specialized authorities dedicated to devising and implementing anti-corruption strategies, ranging from regulations to promote fair competition and transparency, to audits of bureaucrats' and politicians' behavior. These tools entail relevant costs (i.e., investments in new technologies, auditors' wages and training, and the design of specific regulations), which are at least partially passed on to the monitored agents. For instance, previous studies examined the effects of anti-corruption audits promoted by the national government in Brazil (Avis et al., 2018; Ferraz and Finan, 2011; Zamboni, et al., 2018), Mexico (Larreguy et al., 2015), Puerto Rico (Bobonis et al., 2016), Argentina (Di Tella and Schargrodsky, 2003) and Indonesia (Olken, 2007).[4] Conversely, we study a policy which reduces corruption without incurring in additional implementation costs and with little scope for manipulation.

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

Finally, this paper relates to studies of the impact of governmental transfers on rent seeking, which show that transfers may amplify corruption at the local level (Brollo et al., 2013; Barone and Narciso, 2015). Differently from this literature, in this paper we focus on a reduction in the size of the budget and we study whether the policies enacted to comply with it may affect rent-seeking. Nevertheless, we also illustrate how governmental transfers (i.e. European funds) may offset the beneficial effects of budget constraints on corruption. The finding that the provision of European funds lessens the effects of fiscal rules on corruption is in line with Becker et al. (2018), who show that EU funds prompt economic growth only in regions with reliable institutions.[5]

[4] In the Italian context, Vannutelli, (2021) shows that moving from mayoral appointment of auditors to a system of random assignment improves the financial health of local governments.

[5] We also relate to studies analyzing the performance of bureaucrats and politicians (e.g. Bandiera et al., 2009; Limodio, 2019), especially in relation to rent seeking and public procurement procedures (Conley and Decarolis, 2016; Decarolis, 2018; Geys and Titl, 2019; Di Cataldo and Mastrorocco, 2020; Fenizia and Saggio, 2021; Gallego et al., 2020).

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 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 (Chiades and Mengotto, 2013, Bonfatti and Forni, 2017, Gamalerio 2017). We report a brief history of Italian fiscal rules in Appendix 2.[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 and Bonfatti and Forni, 2017). The DSP established that, for each municipality and year, the overall budget balance had to be proportional to a (moving) average of balances obtained in previous years in the same municipality.[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

[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.

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

were attained in every region.

As explained in the previous section, the municipal government might react to the DSP by reducing capital and/or current expenditures. These strategies are plausible in the Italian scenario. As explained by Grembi et al. (2016), local politicians can considerably shape the local budget, as about one third of current and capital expenditures are classified as not rigid (i.e. not included into payroll expenses or debt service).[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 HFRs (high-funds regions): Apulia, Basilicata, Calabria, Campania, Sardinia and Sicily. These regions receive large transfers, which in fact made DSP restrictions on capital expenditures hardly binding for their municipalities. Importantly, the division between HFRs and LFRs (low-funds regions), which receive fewer EU funds, is exogenously determined at the European level according to regional GDP, by the above-mentioned 75% GDP rule.

We collect data on European funds from the OpenCoesione.gov portal.[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.[15] The total Italian expenditure certified to the EU was €46.2 billions.[16]

Figure 1 plots the aggregate amount of European funds allocated and spent by LFRs and HFRs over time, in Euros per capita. HFRs received and spent systematically much more EU funds than LFRs. In absolute terms, LFRs spend €228 millions per year, compared to the €618 millions spent by HFRs.

Are those trends similar across treated municipalities? As anticipated, we will consider the enforcement of the DSP among municipalities between 1,000 and 5,000 inhabitants, which determines our treatment group. On average, in the period 2013–2015, treated municipalities in HFRs received every year 230 Euros per capita of EU funds, which corresponds to the 59% of their capital

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

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

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

[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/`.

expenditure budget, while those in LFRs 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 HFRs (i.e. 8%) receive less EU funds than the average town in LFRs. In LFRs, the 95th percentile receives 107 euros, still much less than the average town in HFRs.[17]

This comparison highlights how EU funds could finance an extensive share of public investments in HFRs utterly reducing the effectiveness of fiscal rules.[18] This leads to our hypothesis that fiscal rules are not going to be binding among treated municipalities in HFRs, and, in turn, they are not going to substantially affect budgetary outcomes and corruption. Conversely, we expect an effect among treated municipalities in LFRs, where fiscal rules actually represent a new budget constraint for the local government.

3.3 Data on corruption

Information on corruption is based on the Italian Investigation System (henceforth 'SDI'; *Sistema d'indagine*, in Italian), a data collection system managed by the Ministry of the Interior. The SDI records details on investigation procedures authorized by the judiciary and carried out by police forces. The data cover the years 2004-2014, and allow us to compute the number of initiated procedures by municipality, year, and type of alleged offence.[19] 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 2, more than three out of four municipalities display no corruption episodes in the period under consideration. Figure 2 highlights that corruption is spread across all Italian regions, although it appears more common in Southern towns. Overall, the average number of episodes recorded each year in the whole country is around 1,300. Furthermore, the phenomenon affects towns of our interest: municipalities between 1,000 and 5,000 inhabitants have on average 1.41 total corruption episodes between 2004 and 2014. Figure 3 plots the aggregate number of corruption investigations for large, medium and small

[17] Note that these differences are larger than the ones in Figure 1, as small municipalities in LFRs receive fewer EU funds than the average municipality in LFRs.

[18] 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 HFRs municipalities receiving extra EU funds (see for instance Gandolfo, 2014; Bocognani, 2013).

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

Italian municipalities over time.

In what follows, we use two normalized versions of the corruption index (see Panel A of Table 1), dividing the number of corruption episodes observed in each municipality and year respectively by municipality population, expressed in thousands (this is our main dependent variable, which we label *Corruption (PC)* in what follows) and by total expenditure -expressed in logarithm- (*Corruption (total exp.)*).[20] 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 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.

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 1 contains the descriptive statistics for these variables.

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

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

4 Empirical analysis

4.1 A local Difference-in-Differences approach

The goal of this paper is to estimate the causal impact of introducing the DSP on corruption in Italian municipalities. We study this relationship by exploiting the introduction of the DSP

[20] In subsequent regression analyses, these indicators are standardized by region group (LFR or HFR).

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

This exercise allows us to identify the effects of the introduction of fiscal rules in the treatment group, and to overcome the issue of overlapping policies around the same cutoff, that a Regression Discontinuity Design technique would not be able to deal with. Moreover, the local approach of this methodology, which considers towns in a neighborhood of the population threshold, makes treatment and control groups more comparable respect to a standard Difference-in-Differences with a global approach. Finally, differently from the Difference-in-Discontinuity technique (Grembi et al., 2016, Gamalerio, 2017), this method allows to control for a rich set of fixed effects, including municipal ones.[21]

4.2 Identification assumptions

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

The 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 according to Brollo and Troiano (2016)– simultaneously to the introduction of the DSP in treated units. Second, to exclude this channel, we conduct a

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

'horse-race' between the DSP and the gender quota reform, which we discuss in the robustness section.[22]

Lastly, the evolution of the scope of fiscal rules would also, in principle, allow to study the 1,000 threshold, as towns below this threshold were never subject to the DSP. However, this is not feasible for two reasons: i) the diffusion of 'unions of municipalities' (*Unioni di Comuni*) among very small towns, allowing them to jointly manage some of their functions without being subject to the DSP; ii) the very low incidence of detectable corruption in towns around the 1,000 threshold, which limits our analysis. We provide further explanations and these additional analyses in Section 6.5.

4.3 Detected and actual corruption

The analysis relies on corruption investigations and not on conviction rates. We rely on investigations as those are timely, while convictions take place several years later. Moreover, in the Italian context, convictions data include only the conviction date (without mentioning when the crime was committed) and are available only at the regional level (and not at the municipal level).[23] 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 in Italy a central anti-corruption agency able to coordinate and direct anti-corruption efforts.[24] 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.[25] [26]

[22] 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.

[23] In Appendix Figure A9, we show that conviction rate related to corruption crimes does not differentially change across LFRs and HFRs over time: data on trials refer to the end date of the trial.

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

[25] 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.

[26] 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.[27]

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

4.4 Specification

Our dependent variable varies at the municipal/year level. The set of dependent variables includes the 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).[28] 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).[29] 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 has been recorded.[30] The local DID estimator is obtained by the interaction term $S_{it} \cdot T_t$, which captures the effect of introducing the DSP, with the comparison of treated and control municipalities before and after 2013.

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.[31] We include municipality fixed effects, γ_i, and year fixed

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.

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

[28] 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.

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

[30] 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 Appendix Figure A6, 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.

[31] We generally present the findings with the complete set of controls. Due to space constraints, we provide the

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.[32]

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 Budget

We start by testing the effects of the DSP of municipal budgets, differentiating between LFRs and HFRs. Table 2 reports the estimation of Model 1, using a 2,500-inhabitants bandwidth: panel A focuses on LFRs, panel B on HFRs, and panel C includes the full sample.[33]

Column 1 of Panel A shows the effect on the most important local tax rate: house tax.[34] Here there are not sizeable effects.[35] Second, Panel A shows that municipalities in LFRs 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-

estimates with and without controls only for the main results of the analysis.

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

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

[34]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.

[35]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 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 other budget outcomes under analysis. We will provide evidence on this in the robustness checks section.

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 HFRs. 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 LFRs and HFRs: we still observe a statistically significant effect on both current and capital expenditures, although the size is smaller than in LFRs.

In Figure 5 (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 LFRs sample. Previous findings are confirmed, as the coefficients are very stable across different bandwidths. We report similar figures for HFRs and the entire sample in the Appendix Figures A1 and A2. 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 5 (left panels) presents this analysis on LFRs. 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 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. Appendix Figures A3 and A4 report a similar analysis on LFRs and on the full sample: overall, there are not substantial differential pre-trends.[36]

This analysis highlights that treated municipalities in LFRs reduce public spending in reaction to the DSP, while municipalities in HFRs do not modify their budget. In turn, HFRs municipalities 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.[37] In other words, municipalities in HFRs were simultaneously respecting fiscal rules without any significant change in their budget: they did not need to substantially cut expenditure or increase taxes to respect fiscal rules, as they were receiving additional transfers from the EU.[38]

5.2 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 LFRs, where they are fully enforced and they have an impact on municipal spending outcomes. Conversely, we do not expect a substantial effect in HFRs, where the DSP did

[36] Current spending in HFRs represents an exception with some pre-trends in earlier years. This might be due to the reliability of the data in earlier years when considering disaggregated spending data. In line with this interpretation, there are no pre-trends when considering aggregated measures, i.e. total spending (bottom figure).

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

[38] An additional alternative explanation could be that fiscal rules were less stringent in HFRs because of historically different spending patterns below/above the 5,000 threshold. Column (9) of Table A5 reports a cross-sectional analysis in which the dependent variable is the level of surplus set as the DSP target (expressed in log). The strictenss of the DSP does not differentially change for treated municipalities in HFRs. Therefore, the null effect in HFRs cannot be explained by a weaker stricteness of the DSP.

not affect municipal budgets. Table 3 reports the estimation of Model 1, using a 2,500-inhabitants bandwidth: panel A focuses on LFRs, panel B on HFRs, 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 3 in a next section.

Panel A shows that corruption significantly decreased in the group of municipalities in LFRs 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 HFRs, in which fiscal rules were extended but not fully enforced: the coefficients are positive but not statistically significant. Panel C shows that the effect is still negative and significant in the complete sample of both LFRs and HFRs.[39]

While in Table 3, we display only one bandwidth, in Figures 6 (right panels) we study how sensitive this result is to the choice of population bandwidth (the dependent variable is the number 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 LFRs, and 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 HFRs, the effect is weakly positive and not statistically significant. 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 6 report the event study approach (Model 2), across three bandwidths: 2,000, 2,500 and 3,000 inhabitants. In LFRs 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 HFRs 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.

There are two takeaways from this analysis. First, as expected, the DSP does not affect corruption in HFRs where fiscal rules are not binding. Second, the DSP reduces corruption charges in LFRs. The effects of fiscal rules on spending in LFRs 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 (i.e. capital and procurement spending) is the budget component most vulnerable to corruption (e.g. Hessami, 2014).

While in this and the previous section, we look at the extensive margin, i.e. a sample split

[39]Furthermore, in Table A2, 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 LFRs): 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.

between LFRs and HFRs, in the next, we propose an intensive margin approach, exploiting the amount of EU funds received by each area.

5.3 The role of EU funds

In Table 4, we consider the entire sample of both LFRs and HFRs 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. Table 4 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.[40]

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 LFRs. As expected, the negative impact of fiscal rules on corruption only emerges in LFRs as the interaction term is negative and significant and the DiD estimator is not.[41] 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 HFRs and differentiate by the amount of EU funds allocated in each province. Even within HFRs, 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 LFRs are taking place – to a less extent – also in HFRs. In column 6, we restrict the

[40] 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.

[41] 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 LFRs and HFRs similarly: this further validates our main identifying assumptions.

sample to the homogeneous group of Southern regions, including the six HFRs and Molise and Abruzzo, the only two Southern regions not included in the group of HFRs. 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*.[42]

6 LFRs additional findings

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

6.1 Impact on the corruption-proofness of public spending

In the previous sections, we show that, among LFRs, 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 7 displays this test for LFRs. 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 ones. Numerical results are shown in Table 3 (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 LFRs. This is an important threshold, as the administration can award contracts below this amount without a competitive bidding process. Table 5 (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

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

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 5 show that local politicians are more productive under fiscal rules. Specifically, we collect a novel dataset including the number of deliberations taken by the municipal council and by the municipal government, i.e. all official decisions taken at the municipal level prior to voting. On average they respectively increase by 4.2% and 5.6% 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.

6.1.1 News Text Analysis

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

The procedure we employed consists in two steps. First, we select the articles dealing with corruption according to two criteria: 1) the presence of at least one keyword related to political corruption in articles' text, e.g. embezzlement, 2) the mention in the text of the surname of a local politician in office in the place where the article was geo-localized, in the period when the article was written. Appendix 3 discusses the details of the identification of corruption-related articles. Second, we identify the specific corruptive behaviour associated to each case. The results of this test suggest that the majority of these episodes are costly for the society. In particular, we identify 1,585 articles about infractions in procurement procedures, 431 on fraud, 315 on public hiring, 161 about refund usage, 81 on construction crimes and only 63 concerning theft and embezzlement. These findings provide some descriptive evidence that corruption cases under analysis do represent an important cost for the community.

6.2 Intensity in the application of the DSP

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

We conduct these analysis in Table 6. 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 A7 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.[43]

6.3 Mechanisms: Accountability

As explained in the introduction, accountability may explain why politicians reduce rent-seeking when facing this new budget constraint. If accountability is at play, we should observe a stronger corruption decrease in the presence of electoral incentives. Also in this case we focus on LFRs, where fiscal rules were fully binding.

In the context of Italian municipalities, there are two potentially exogenous sources of variation in electoral incentives. First, the electoral schedule is pre-determined and staggered over time (Repetto, 2018). This implies that every year a different group of cities held elections, each on a different 5-year long calendar. We can therefore separate year fixed effects from the effect of time until the next election. If electoral incentives are at play, we should expect a stronger decrease in corruption in the electoral period. Specifically, we expect treated local governments to reduce corruption during pre-electoral and electoral years. We report this test in columns 1 and 2 of Table 7. 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 7 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).

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 7).[44]

[43] Moreover, we study the heterogeneous effect of the reform depending on the past (pre-reform) level of corruption: Table A5 (column 2) shows that the introduction of the DSP leads to a larger reduction in corruption in cities with higher pre-reform levels of corruption.

[44] 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 LFR/HFR heterogeneity can help us to distinguish between accountability and monitoring. In particular, the monitoring effect should be at work in HFRs as the DSP is *de jure* enforced. Conversely, the accountability channel is shut down, as those municipalities are not *de facto* financially constrained by the policy. Therefore, mayors in HFRs do not face a trade-off between cutting inefficient expenditures and reducing their own rent seeking. In other words, both channels, accountability and monitoring, are at work in LFR municipalities, while monitoring is the only relevant channel in HFRs. As shown above, we do not find any effect in the latter group, which implies that monitoring is not likely to be the most relevant channel.

In columns 7 and 8, we focus on another interesting dimension: politicians ability. We take into account a triple-interaction approach, focusing on treated municipalities ruled by mayors that hold a university degree. The reduction in corruption emerges only in this group of cities: more competent mayors might be better suited to reduce rent-seeking and inefficient expenditures under fiscal rules.

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

In this test, we limit the sample to electoral years and first-term mayors (who are eligible for re-election). Appendix Table A5 (column 7) presents the results, which suggest that the policy does not have a significant impact on the incumbent's chances of re-election. We also tested for alternative measures of electoral competitiveness (in unreported estimates), including the winning candidate's margin of victory, and found no significant heterogeneity on our dependent variable. Overall, these findings are inconclusive and might be explained by the fact that the two above mentioned effects are at work offsetting each other.

6.4 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.[45]

6.4.1 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.[46] Figure 8 report 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

[45] 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.

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

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.[47]

6.4.2 Inequality

As a complementary test, we investigate the DSP's effect on inequality. We have information on municipal income distribution, aggregated at the income bracket level: in particular, for every city and year we have data on the number of taxpayers and the total income declared for seven income groups (*i.e.* 0-10,000 euros, 10,000-15,000 euros, 15,000-26,000 euros, 26,000-55,000 euros, 55,000-75,000 euros, 75,000-120,000 euros, more than 120,000 euros). 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,[48] and we define the bottom bracket in two ways: 0–10,000 euros and 0–15,000 euros. We report these findings in Figure 8. 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.

6.4.3 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 – for the first time in the Italian context – 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 specifically collect data on school canteens, kindergartens, waste collection and street lightening. Overall, we do not find a substantial effect of the DSP on those outcomes (we only observe an increase in the number of children attending public kindergartens). We graphically report those results in Figure 8.

6.5 Robustness tests

In this section we briefly present a set of additional tests, which are reported and discussed extensively in the Appendix 1.

First, we conduct the main analysis on a series of additional measures of corruption. We start by focusing on the measure "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 Table A5 (columns 2, 3) and the

[47] 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.

[48] 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.

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 Table A5 (column 4).

Second, we conduct a series of tests to check the robustness of the adopted estimation. We start by showing that the main results are similar if we adopt the Difference-in-Discontinuity methodology (Grembi et al., 2017; Gamalerio, 2017). Table A4 contains these analyses on corruption and budget outcomes. We then 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. This test is shown in Figure A5.

Third, we focus on 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 A5, 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).[49] 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 A7, in Appendix 2, for the institutional details). The main results are unaffected, as shown in Table A3.

Fourth, we conduct the analysis on corruption excluding specific groups of municipalities that are subject to different administrative rules. We exclude from the sample those cities affected by the earthquake of 2012, located in the region Emilia-Romagna, as they have been exempted from the application of the DSP (*Decreto Legislativo* n. 174, 2012). Then, we do not include in the sample municipalities whose local government has been dissolved by the national government and temporarily assigned to an appointed commissioner. Finally, we exclude from the sample the (small) set of municipalities that violated the rules of DSP. In these cases the results are unaffected, as shown in Figure A5.

Fifth, we test for displacement effects. On the one hand, we show that firms located in LFRs (where the DSP is binding) are unlikely to move to areas in HFRs (where the constraint of DSP is weaker), where public investments are not declining. In particular, in Figure A5, we show that the main results are robust to the exclusions of municipalities located in LFRs that are geographically close to HFRs. 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 A5 (column 5) and a null effect emerges.

As an additional analysis, we focus on the other population threshold involved in the 2013 reform, i.e. 1,000 inhabitants. We conduct the main analysis on corruption charges and budget variables focusing on this threshold, in which we do not find any effect. This is likely due to the very low number of corruption investigations in such small municipalities. This test is shown in Table A1.

Finally, we conduct a series of additional analysis and robustness tests. First, we show that the reform of 2013 did not affect the spending in local police (per-capita) and this result further

[49]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.

disproves the idea that the introduction of DSP is related to changes in investigation effort at the local level (Table A5, column 6). Second, we show in Figure A8 that there is no difference in firms' accusation trend between LFRs and HFRs. This makes unlikely that the firms' accusation behaviour may confound our results on corruption as the behaviour of firms where the DSP is binding (LFRs) evolves similarly to those that operate in areas where the DSP poses a much weaker constraint (HFRs). Third, in Table A5 (column 8), 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.[50]

7 Discussion and final remarks

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

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

How general are these results? We believe 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 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

[50] 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 A9 that the conviction rate related to corruption crimes does not seem to change between LFRs (where DSP is binding) and HFRs (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.

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.[51]

[51] Therefore, we do not improve to the limitations of the current literature which struggles in finding effective long-term anti-corruption policies. As highlighted by Morse et al., (2018), when corruption is a systemic problem, it cannot be treated in the long term with individual-level solutions.

References

[1] Alesina, A. and Angeletos, G.M., 2005. Corruption, inequality, and fairness. *Journal of Monetary Economics*, 52(7), pp.1227-1244.

[2] Alesina, A. and Perotti, R., 1995. Fiscal expansions and adjustments in OECD countries. *Economic policy*, 10(21), pp.205-248.

[3] Alesina, A., and Perotti R., 1996. Fiscal Discipline and the Budget Process. *American Economic Review, Papers and Proceedings*, 86, 401–407.

[4] Alpino, M., Asatryan, Z., Blesse, S., and Wehrhöfer, N. (2020). Austerity and distributional policy. *ZEW-Centre for European Economic Research Discussion Paper*, (20-028).

[5] Angeli, F., 2009. La Finanza Locale in Italia: Rapporto 2009.

[6] Asatryan, Z., Feld, L.P. and Geys, B., 2015. Partial fiscal decentralization and sub-national government fiscal discipline: empirical evidence from OECD countries. *Public Choice*, 163(3-4), pp.307-320.

[7] Avis, E., Ferraz, C. and Finan, F., 2018. Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians. *Journal of Political Economy*, 126(5), pp.1912-1964.

[8] Banca d'Italia, 2012. Supplementi al bollettino statistico–Indicatori monetari e finanziari. Debito delle amministrazioni locali. Nuova Serie: Anno XXII - 31 Ottobre 2012.

[9] Bandiera, O., Barankay, I., and Rasul, I., 2009. Social connections and incentives in the workplace: Evidence from personnel data. *Econometrica*, 77(4), 1047-1094.

[10] Barone, G., Narciso G., 2015, Organized crime and business subsidies: Where does the money go?, *Journal of Urban Economics*, 2015, vol. 86, issue C, 98-110.

[11] Becker, S. O., Egger P. H., and von Ehrlich M., 2018. Effects of EU regional policy: 1989-2013. *Regional Science and Urban Economics* 69 (2018): 143-152.

[12] Boas, T. C., Hidalgo F. D., and Melo M. A., 2019. Norms versus action: Why voters fail to sanction malfeasance in Brazil. *American Journal of Political Science* 63, no. 2 (2019): 385-400.

[13] Bobonis, G.J., Cámara Fuertes, L.R. and Schwabe, R., 2016. Monitoring corruptible politicians. *American Economic Review*, 106(8), pp.2371-2405.

[14] Bocognani, R., 2013. L'incompatibilità dei vincoli di finanza pubblica con una politica di sviluppo del territorio. *Rivista giuridica del Mezzogiorno, Trimestrale della Svimez* 4/2013, pp. 785-792, doi: 10.1444/76337

[15] Boffa, F., Piolatto, A. and Ponzetto, G.A., 2016. Political centralization and government accountability. *The Quarterly Journal of Economics*, 131(1), pp.381-422.

[16] Bonfatti, A., and Forni, L., 2017. Fiscal rules to tame the political budget cycle: evidence from Italian municipalities, IMF Working Paper 17/6.

[17] Branzoli, N. and Decarolis, F., 2015. Entry and subcontracting in public procurement auctions. *Management Science*, 61(12), pp.2945-2962.

[18] Brollo, F. and Troiano, U., 2016. What happens when a woman wins an election? Evidence from close races in Brazil. *Journal of Development Economics*, 122, pp.28-45.

[19] Brollo, F., Nannicini T., Perotti R., Tabellini G., 2013, The Political Resource Curse, *American Economic Review* - 103 (2013), 1759-1796.

[20] Calonico S., Cattaneo M.D., and Titiunik R., 2014, "Robust nonparametric confidence intervals for regression discontinuity design", *Econometrica*, Vol. 82, No. 6 (No- vember, 2014), 2295-2326.

[21] Campa, P., 2011. Gender quotas, female politicians and public expenditures: quasi-experimental evidence. Mimeo.

[22] Chiades, P., and Mengotto, V., 2013, "Il calo degli investimenti nei Comuni tra Patto di stabilità interno e carenza di risorse", Bank of Italy Occasional Papers n. 210.

[23] Conley, T.G. and Decarolis, F., 2016. Detecting bidders groups in collusive auctions. *American Economic Journal: Microeconomics*, 8(2), pp.1-38.

[24] Coviello, D., and Gagliarducci S., 2017. Tenure in office and public procurement. *American Economic Journal: Economic Policy* 9, no. 3 (2017): 59-105.

[25] Coviello, D., Marino, I., Nannicini, T., & Persico, N. (2019). Effect of a Fiscal Demand Shock on Firm Investment. Working paper.

[26] De Angelis, I., De Blasio G., Rizzica L., 2018. On the unintended effects of public transfers: evidence from EU funding to Southern Italy, Bank of Italy Working Paper, Number 1180 - June 2018.

[27] De Vries, C. E., and H. Solaz, 2017. The electoral consequences of corruption. Annual Review of Political Science 20 (2017): 391-408.

[28] Decarolis, F., 2018. Comparing public procurement auctions. *International Economic Review*, 59(2), pp.391-419.

[29] Di Cataldo, M., Mastrorocco, N. (2020). Organised crime, captured politicians, and the allocation of public resources. *University Ca'Foscari of Venice, Dept. of Economics Research Paper Series No, 4.*

[30] Di Tella, R. and Schargrodsky, E., 2003. The role of wages and auditing during a crackdown on corruption in the city of Buenos Aires. *The Journal of Law and Economics*, 46(1), pp.269-292.

[31] Enikolopov, R. and Zhuravskaya, E., 2007. Decentralization and political institutions. *Journal of Public Economics*, 91(11-12), pp.2261-2290.

[32] Fenizia, A. and Saggio, R., 2021. Breaking the ties between the Mafia and the State: Evidence from Italian municipalities. Available at SSRN 3105798.

[33] Ferraz, C. and Finan, F., 2011. Electoral accountability and corruption: Evidence from the audits of local governments. *American Economic Review*, 101(4), pp.1274-1311.

[34] Fetzer, T., 2019. Did austerity cause Brexit? *American Economic Revie*w, 109(11), pp.3849-86.

[35] Fisman, R. and Gatti, R., 2002. Decentralization and corruption: evidence across countries. *Journal of Public Economics*, 83(3), pp.325-345.

[36] Fisman, R. and Golden, M., 2017. How to fight corruption. *Science*, 356(6340), pp.803-804.

[37] Gagliarducci, S. and Nannicini, T., 2013. Do better paid politicians perform better? Disentangling incentives from selection. *Journal of the European Economic Association*, 11(2), pp.369-398.

[38] Gallego, J., Prem, M., Vargas, J. F., 2020. Corruption in the times of pandemia, Documentos de Trabajo 018178, Universidad del Rosario.

[39] Gamalerio, M., 2017. Fiscal Rules and the selection of politicians: evidence from Italian municipalities. Mimeo.

[40] Gandolfo, D. 2014. I Fondi Strutturali europei 2007-2013 in Italia: un'analisi delle criticità. PhD Thesis, University of Bologna.

[41] Geys, B. and Titl, V., 2019. Political donations and the allocation of public procurement contracts. *European Economic Review*, 111, pp.443-458.

[42] Giommoni, T., 2017. Exposition to corruption and political participation: Evidence from Italian municipalities. CESifo working paper series no. 6645.

[43] Giommoni, T., 2019. Does progressivity always lead to progress? The impact of local redistribution on tax manipulation, CESifo Working Paper No. 7588.

[44] Golden, M. A., 2018. Corruption and the New Institutional Economics. A Research Agenda for New Institutional Economics. Edward Elgar Publishing, 2018.

[45] Grembi, V., Nannicini T. and Troiano U., 2016. Do fiscal rules matter? *American Economic Journal: Applied Economics* - 8 (2016), 1-30.

[46] Heinemann, F., Moessinger, M.D. and Yeter, M., 2018. Do fiscal rules constrain fiscal policy? A meta-regression-analysis. *European Journal of Political Economy*, 51, pp.69-92.

[47] Hessami, Z., 2014, Political corruption, public procurement, and budget composition: Theory and evidence from OECD countries, *European Journal of Political Economy*, Volume 34, June 2014, Pages 372-389.

[48] Larreguy, H.A., Marshall, J. and Snyder Jr, J.M., 2015. Publicizing malfeasance: When media facilitates electoral accountability in Mexico. Cambridge, Harvard University, Mimeo.

[49] Liu, C., Mikesell, J. L., 2019. Corruption and Tax Structure in American States. *The American Review of Public Administration*, 49(5), 585–600.

[50] Lledó, V., Yoon, S., Fang, X., Mbaye, S. and Kim, Y., 2017. Fiscal rules at a glance. IMF Background Note.

[51] Limodio, N., 2019. Bureaucrats allocation in the public sector: Evidence from the World Bank. IGIER Working Paper n. 655.

[52] Marattin, L., Nannicini, T., & Porcelli, F. 2019. Revenue vs Expenditure Based Fiscal Consolidation: The Pass-Through from Federal Cuts to Local Taxes. IGIER, Università Bocconi.

[53] Mauro P., 1995, Corruption and growth, *The Quarterly Journal of Economics*, Vol. 110, n. 3.

[54] Muraközy, B., Telegdy, Á. 2016. Political incentives and state subsidy allocation: Evidence from Hungarian municipalities. *European Economic Review*, 89, 324-344.

[55] Nannicini, T., Stella, A., Tabellini, G. and Troiano, U., 2013. Social capital and political accountability. *American Economic Journal: Economic Policy*, 5(2), pp.222-50.

[56] Olken, B.A., 2007. Monitoring corruption: evidence from a field experiment in Indonesia. *Journal of political Economy*, 115(2), pp.200-249.

[57] Olken, B.A. and Pande, R., 2012. Corruption in developing countries. *Annual Review of Economics* 2012 4:1, 479-509.

[58] Pavese, C., Rubolino, E. (2021). Do Fiscal Restraints Harm Test Scores? Evidence from Italy. *University Ca'Foscari of Venice, Dept. of Economics Research Paper Series* No, 2.

[59] Repetto, L., 2018. Political budget cycles with informed voters: evidence from Italy, *The Economic Journal* 128 (616), December 2018, pp. 3320-3353.

[60] Rodden, J., 2004. Comparative federalism and decentralization: On meaning and measurement. *Comparative Politics*, pp.481-500.

[61] Stiglitz, J.E., 2016. The euro: How a common currency threatens the future of Europe. WW Norton & Company.

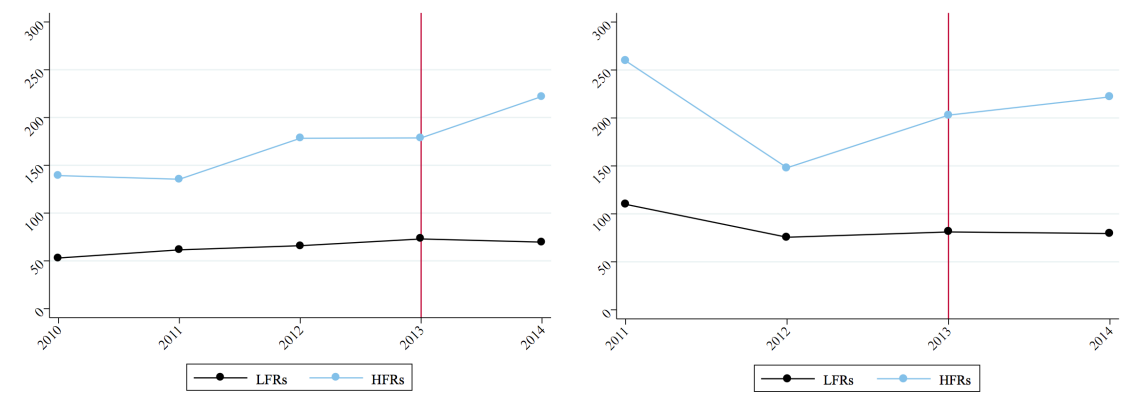
[62] Vannutelli, S. 2021. From lapdogs to watchdogs: Random auditor assignment and municipal fiscal performance in Italy. Job Market Paper: Yale University.

[63] Zamboni, Y. and Litschig, S., 2018. Audit risk and rent extraction: Evidence from a randomized evaluation in Brazil. *Journal of Development Economics*, 134, pp. 133-149.

Figures

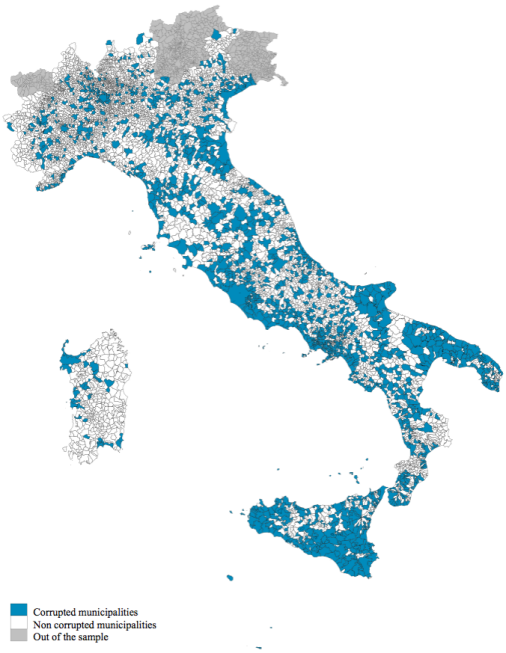
Figure 1: Evolution of European funds (per-capita)

(a) EU funds spent (b) EU funds received



The left plot shows the amounts of European funds spent over time by LFRs and HFRs in per capita terms (measured in Euros). The right plot shows the amounts of European funds received over time by LFRs and HFRs in per capita terms (measured in Euros).

Figure 2: Corruption investigations in Italian municipalities (ordinary regions)



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.

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

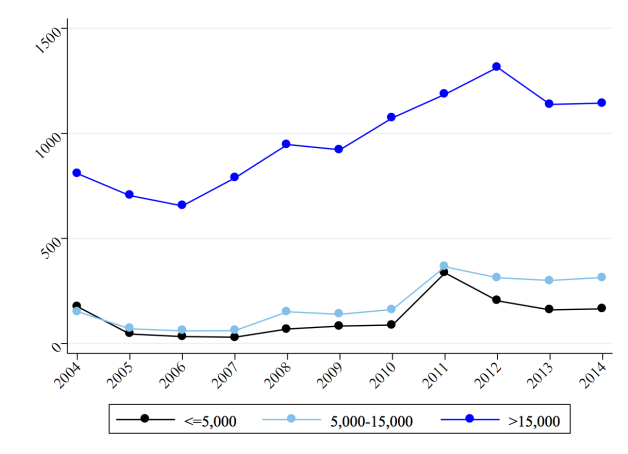
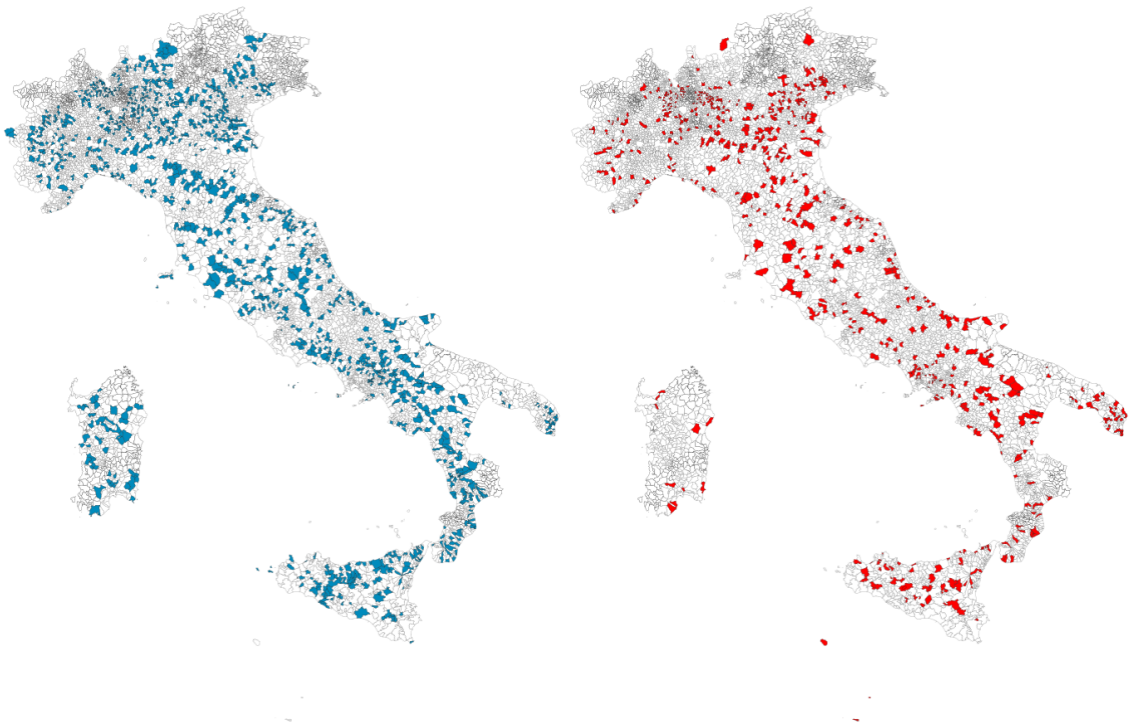


Figure 4: Geographical distribution of treatment and control

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

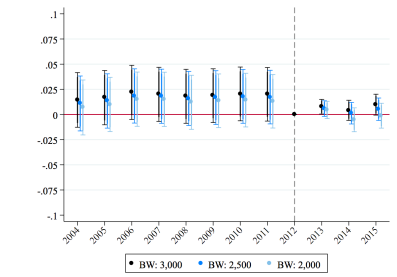
(b) Population between 5,000 and 7,500



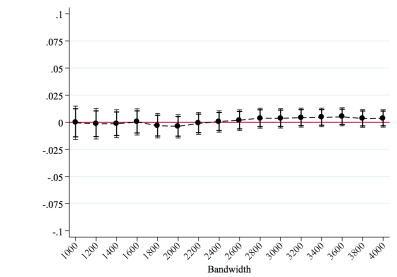
Geographical distribution of cities in the treatmet (left figure) and in the control group (right figure), using a bandwidth of 2,500 inhabitants, according to the population of reference for the DSP application.

Figure 5: Effects of the DSP on local public finance and procurement (LFRs)

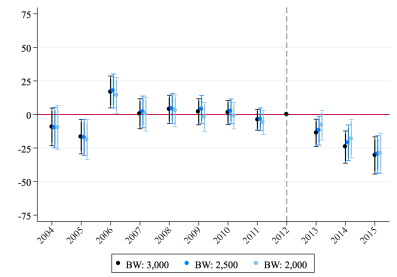
(a) Event-study: House tax



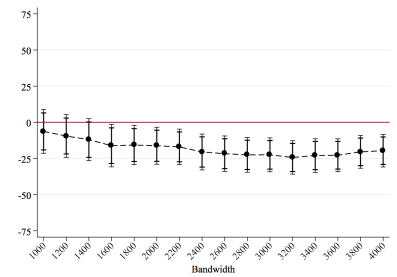
(b) DiD: House tax



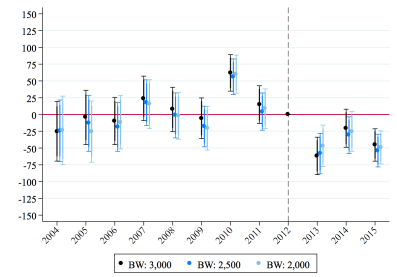
(c) Event-study: Current spend.



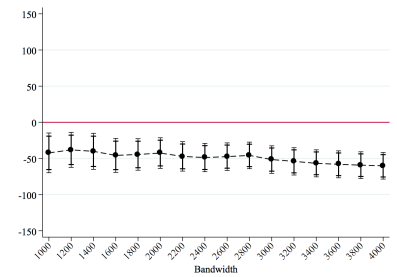
(d) DiD: Current spending



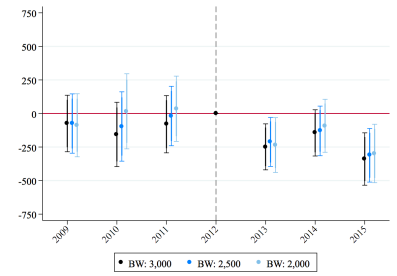
(e) Event-study: Capital spend.



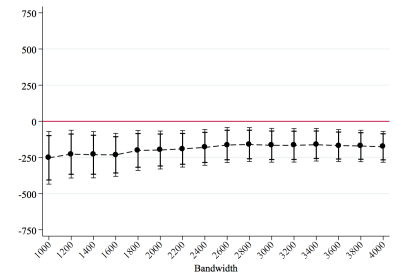
(f) DiD: Capital spending



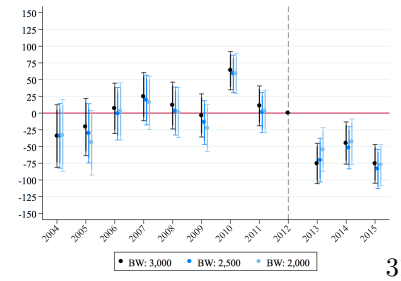
(g) Event-study: Procurement



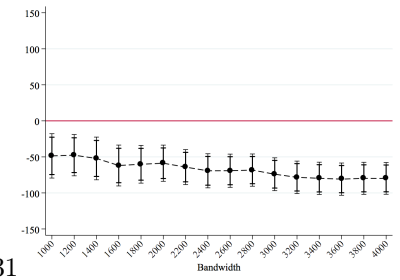
(h) DiD: Procurement



(i) Event-study: Total spending



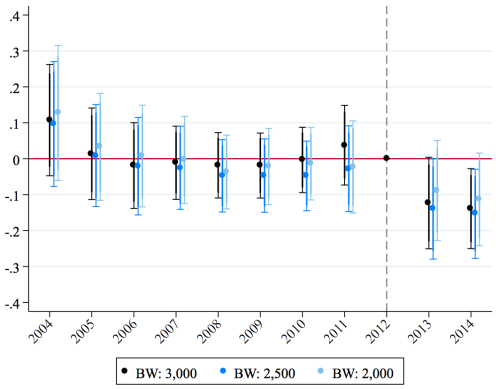
(l) DiD: Total spending



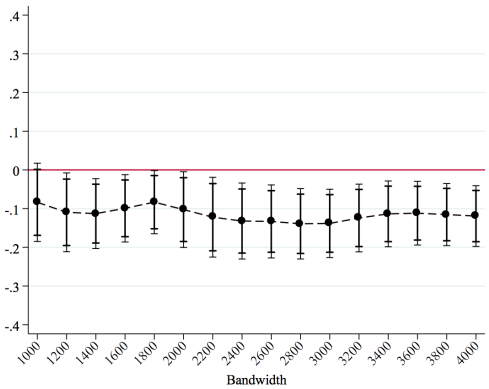
The left plot shows the outcomes of the local DID estimation for cities in LFRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID for cities in LFRs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The specification is the same as in Table 3 (column 3).

Figure 6: Effect of the DSP on corruption (per-capita)

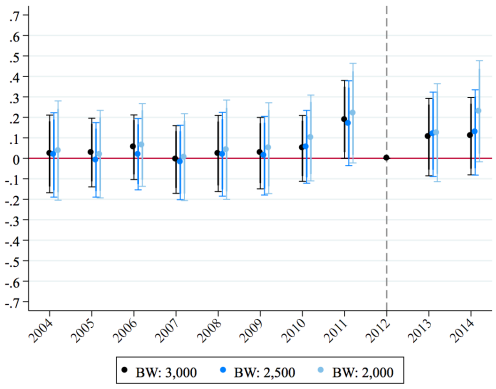
(a) Event-study: LFRs



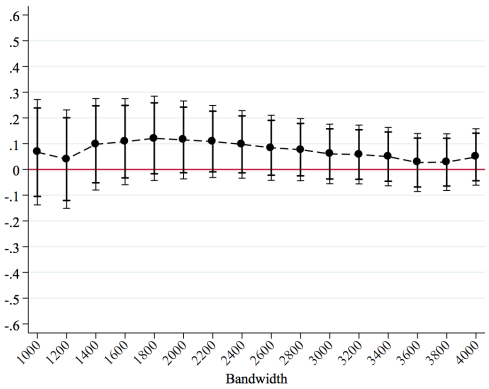
(b) DiD: LFRs



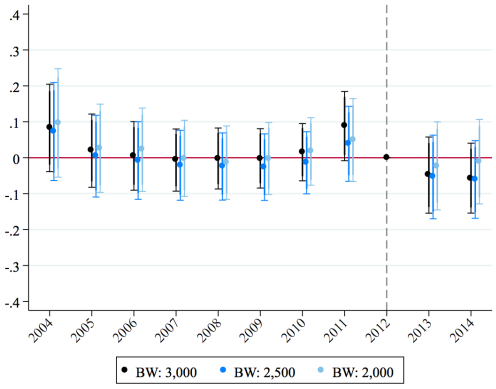
(c) Event-study: HFRs



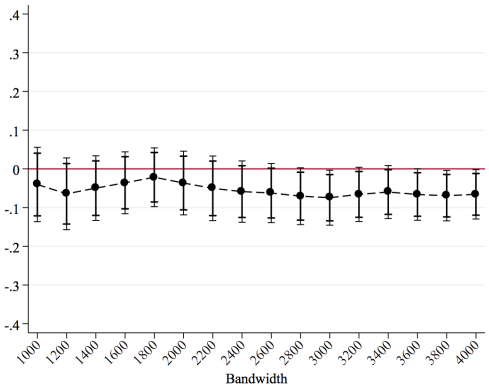
(d) DiD: HFRs



(e) Event-study: Italy



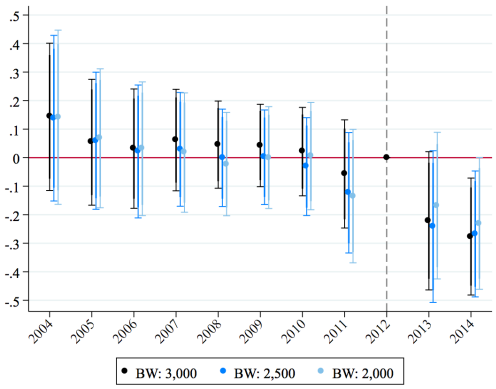
(f) DiD: Italy



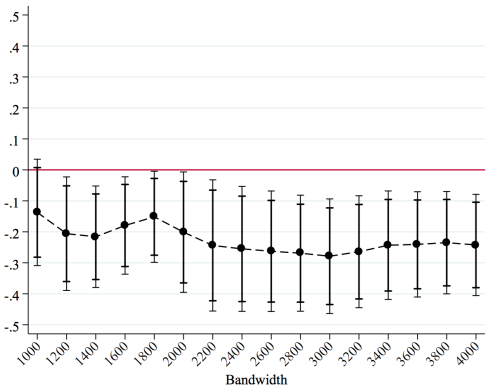
The left plot shows the outcomes of the local DID estimation, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local DID, according to 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 3 (column 3). The sample includes cities in the LFRs (first row), cities in the HFRs (second row) and all italian municipalities (third row).

Figure 7: Effect of the DSP on corruption (over spending)

(a) Event-study: LFRs

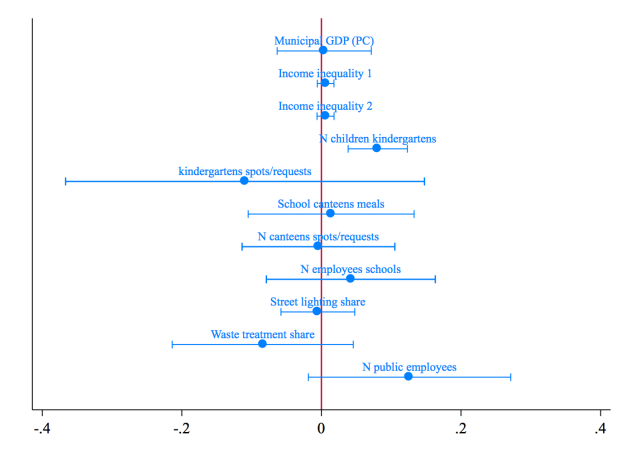


(b) DiD: LFRs



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 3 (column 3). The sample includes cities in the LFRs.

Figure 8: 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), 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 LFRs.

Tables

Table 1: Descriptive statistics

	All cities	LFRs	HFRs
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)	491.0	448.9	661.18
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
Incumbent re-election	.331	.338	.301
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
Roads (km)	24.9	22.4	35.1
Street lightening (share)	.568	.571	.555
Waste management share	.901	.902	.900
N public employees	9.1	8.4	11.8
Urban planning approval	.769	.809	.608

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. *Incumbent re-election* is a dummy variable equal to one in case the incumbent is re-elected in cities where the incumbent is not term limited.

Table 2: Impact of DSP on public finance and procurement

	House tax rate (1)	Current spending (PC) (2)	Capital spending (PC) (3)	Procurement spending (PC) (4)	Total spending (PC) (5)
Panel a: LFRs					
Stability pact (T^*S)	0.001	-21.153***	-49.824***	-170.960***	-70.976***
	(0.005)	(6.363)	(9.868)	(61.957)	(11.995)
N	17796	17720	17720	10420	17720
R^2	0.874	0.923	0.439	0.227	0.730
Panel b: HRFs					
Stability pact (T^*S)	0.005	-1.209	-24.892	88.182	-26.100
	(0.008)	(9.279)	(23.895)	(144.967)	(25.410)
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)	(61.982)	(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 LFRs in Panel a, municipalities located in HFRs in Panel b and all Italian municipalities in Panel c. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Impact of DSP on corruption charges

	Corruption per-capita			Corruption over spending		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel a: LFRs						
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: HRFs						
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 LFRs in Panel a, municipalities located in HFRs in Panel b and all Italian municipalities in Panel c. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: European funds

Interaction term:	EU funds (spent)						EU funds (allocated)	LFRs
	Whole Italy				HFRs	HFRs+ Abruzzo and Molise	Whole Italy	Whole Italy
	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**	-68.273***	-260.902***	-0.411***	-0.404***	-0.123**	0.0461
	(0.047)	(8.144)	(14.710)	(93.186)	(0.156)	(0.155)	(0.055)	(0.0612)
Post-reform (T)*interaction	-0.284	-9.112	-158.197	1413.403***	-0.933***	-0.871***	-	-
	(0.223)	(35.527)	(99.231)	(511.612)	(0.287)	(0.283)		
Treatment group (S)*interaction	-0.319**	-16.996	12.177	-949.341	-0.466	0.170	-0.289	0.108
	(0.150)	(51.816)	(108.707)	(965.662)	(0.298)	(0.337)	(0.190)	(0.0808)
Post-reform (T)*Treatment group (S)*interaction	0.350**	11.146	107.848*	438.799	0.998***	0.963***	0.328*	-0.150**
	(0.161)	(22.328)	(58.851)	(331.743)	(0.321)	(0.309)	(0.197)	(0.0698)
N	23170	25085	25085	14807	7092	7978	23429	23429
R^2	0.034	0.268	0.111	0.094	0.049	0.049	0.033	0.024
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. "LFRs" is a dummy variable indicating the municipalities located in LFRs. The sample includes all Italian municipalities, both from LFRs and HFRs in columns (1-4, 7, 8), only the municipalities located in HFRs in column (5) and the municipalities located in South of Italy (HFRs 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 5: Impact on discretionary tenders and politicians productivity

	Perc. tenders <40K	Perc. amount <40K	Amount PC <40K	N council resolutions	N government resolutions
	(1)	(2)	(3)	(4)	(5)
Stability pact ($S*T$)	-0.003	-0.002	-2.308*	4.239***	5.624*
	(0.010)	(0.009)	(1.253)	(1.299)	(3.374)
N	10336	10336	10336	9373	9378
R^2	0.239	0.217	0.237	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 LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Intensity margin in the application of the DSP

	Corruption (PC)		Corruption (over spending)		Capital spending (PC)	
	(top 50)	(top 20)	(top 50)	(top 20)	(top 50)	(top 20)
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact ($S*T$)	-0.112*	-0.507*	-0.230*	-0.965**	-94.586***	-150.543***
	(0.066)	(0.265)	(0.119)	(0.471)	(18.630)	(48.008)
N	8007	3029	7996	3025	8653	3266
R^2	0.255	0.308	0.255	0.305	0.428	0.483
City, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500	2,500

The 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). 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 LFRs in columns (1-6). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Political accountability and ability

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)
Stability pact (S*T)	-0.019	-0.112	-0.130**	-0.258**	0.110	0.217
	(0.076)	(0.133)	(0.054)	(0.111)	(0.097)	(0.198)
Stability pact (S*T)*interaction	-0.169**	-0.228*	0.106*	0.179	-0.230**	-0.471**
	(0.080)	(0.132)	(0.063)	(0.127)	(0.100)	(0.199)
N	16314	16282	16544	16511	16544	16511
R^2	0.231	0.235	0.228	0.232	0.228	0.232
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, 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 LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix 1 (for online publication): Robustness checks and additional analyses

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 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 (2) and (3) of Table A5 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. Second, 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 (4) of Table A5.

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; Gamalerio, 2017) and it is based on the comparison between the outcome before and after the reform for municipalities around the population threshold.[52] Appendix Table A4 shows this test for the main outcomes variables, corruption charges (columns 1, 2) and budget indicators (columns 4-6). In Panel a the optimal population bandwidth is adopted, while in Panel b an augmented population bandwidth is used, i.e. two times the optimal bandwidth. The negative effect of DSP on corruption and on spending emerges also with this methodology (although the effects on spending are weakly statistically significant), and the magnitude of the coefficient is similar to the one estimated using the local DID methodology. As explained in the empirical strategy section, we use a local DID methodology for two reasons. First, it is more precise because it allows us to include municipality fixed effects, controlling for time-invariant municipal characteristics. Second, it facilitates a more standard evaluation of the absence of pre-trend effects.

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

[52] Following Grembi et al., 2016, 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} captures the set of controls included in Model 1. 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).

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 A5 and the effect on corruption is unaffected.

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 A5 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 2 and shown in Table A7. One important reform of the DSP, which is studied by Coviello et al. (2019), 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 A3 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 LFRs, not because of a change in politicians' behaviors, but due to corruption-prone firms moving their business to areas not affected by public spending cuts. In other words, such firms might be shifting their interests from LFRs to HFRs. This explanation is unlikely in the Italian framework, in which competition in public procurements markets is relatively low and typically local firms are the ones successfully applying to public procurements issued by small/medium size municipalities (Branzoli and Decarolis, 2015). Nevertheless, we provide some quantitative evidence to discard this explanation: we assume that the cost of moving to another area is increasing in distance, whereby firms located in LFRs neighboring a HFR should have a lower cost of moving their business to municipalities not binded by fiscal rules. This implies that our results might be driven by munic-

ipalities in LFRs geographically close to HFRs. We replicate our findings dropping observations from provinces in LFRs which neighbor a HFR or from regions in the LFR area which neighbor a HFR. These analyses are shown in Figure A5 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 A1, 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 A5 (column 6) 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 8 of Appendix Table A5 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, specific groups of municipalities that are subject to particular administrative rules. On the one hand, we exclude those cities that have been affected by the earthquake of 2012, that hit the region of Emilia-Romagna, as they have been exempted from the application of the DSP (*Decreto Legislativo* n. 174, 2012). The main results on corruption are unaffected, as shown in Figure A5. On the other hand, 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 Internal Affairs. Figure A5 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 A5.

Second, an additional concern may be that the DSP could affect firms' propensity to charge their own competitors due to the reduction in procurement spending. This mechanism should be at work only in LFRs. Since data on firms' appeals related to public procurement are not available at the municipal level, we use regional-level data that differentiates between LFRs (where the DSP is binding) and HFRs (where the DSP poses a much weaker constraint) to rule out this confounding story. Appendix Figure A8 plots the total number of appeals to courts over time for LFRs and HFRs: the trends in accusations are parallel. This suggests that the introduction of fiscal rules in LFRs did not modify firms' charging behaviour there, compared to HFRs.

Third, column 5 of Appendix Table A5, 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]

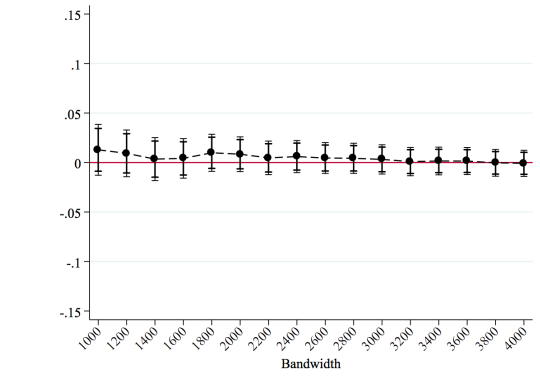
Fourth, a further concern relates our choice of focusing on corruption investigations rather than convictions. The choice is motivated by two reasons: i) as explained in the main text, the time span between an investigation and the actual crime is much shorter compared to a conviction, which could take 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 LFRs. In Appendix Figure A9, we exploit the LFRs – HFRs heterogeneity to show that the conviction rate related to corruption crimes does not seem to change across the two groups before/after 2013.

[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 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.

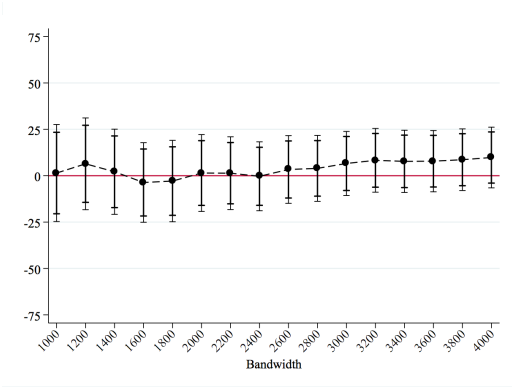
Finally, we study the heterogeneous effects of corruption depending on the level of past (pre-reform) malfeasance. In particular, we estimate the main model interacting the DID estimator with an indicator that captures whether the municipality experienced a positive level of corruption before the reform or not. Table A5, column 1, reports this tests, showing that the impact of DSP on corruption is larger in more corrupted municipalities.

Figure A1: Effects of the DSP on local public finance and procurement (HFRs) – DiD

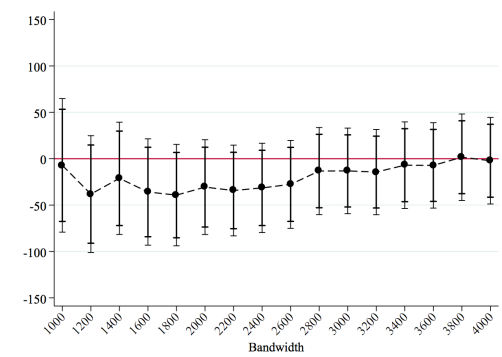
(a) House tax rate



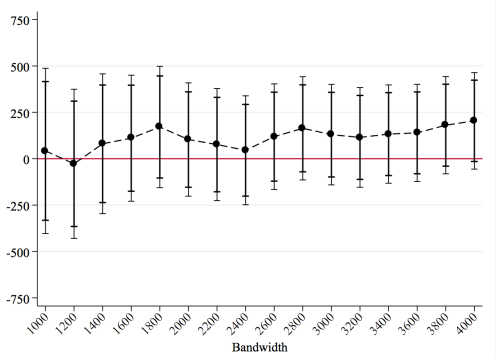
(b) Current spending (PC)



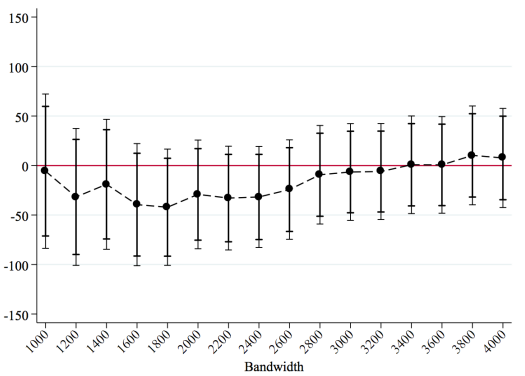
(c) Capital spending (PC)



(d) Procurement spending (PC)



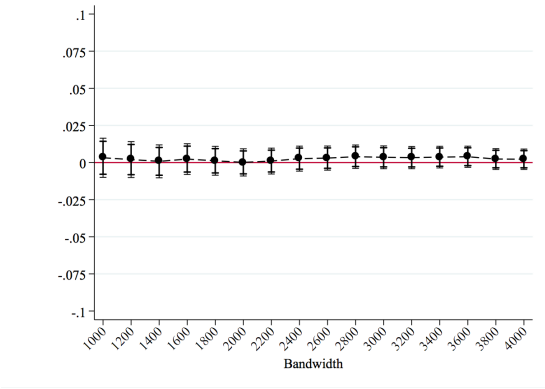
(e) Total spending



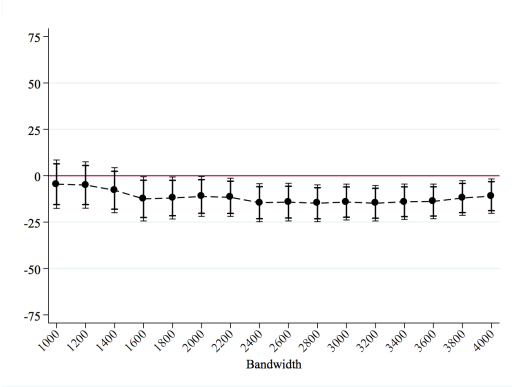
The plot shows the sensitivity analysis of the local DID for cities in HFRs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables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

Figure A2: Effects of the DSP on local public finance and procurement (Italy) – DiD

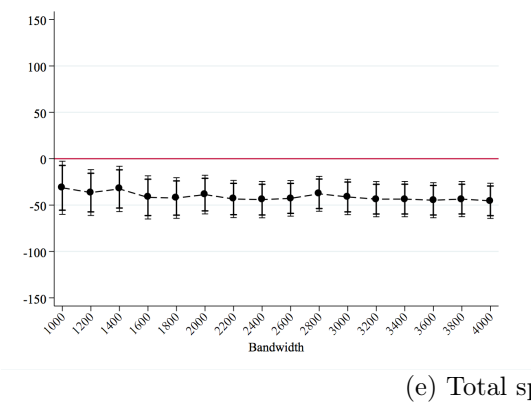
(a) House tax rate



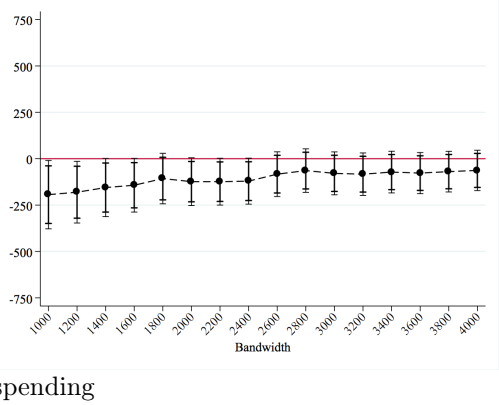
(b) Current spending (PC)



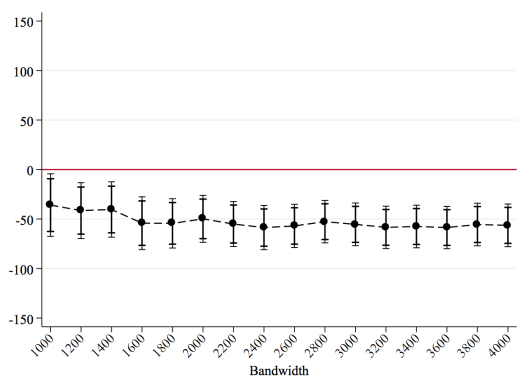
(c) Capital spending (PC)



(d) Procurement spending (PC)



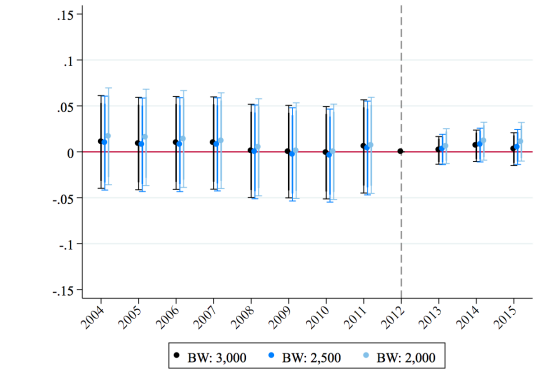
(e) Total spending



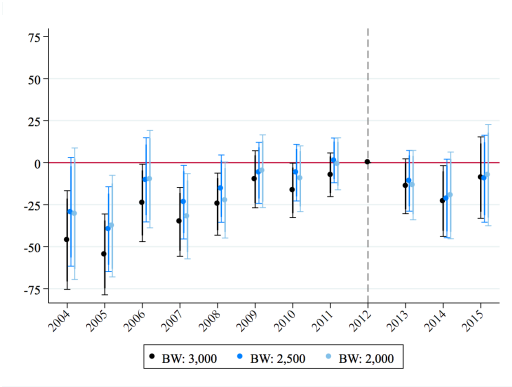
The plot shows the sensitivity analysis of the local DID for all Italian cities, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables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

Figure A3: Effects of the DSP on local public finance and procurement (HFRs) – Event-study

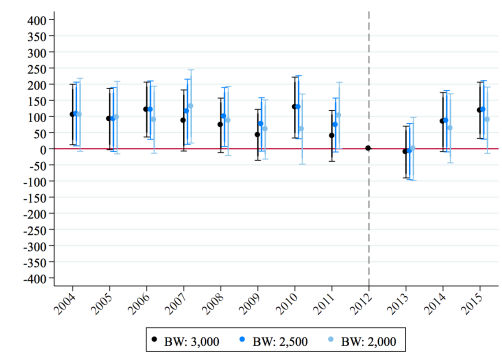
(a) House tax rate



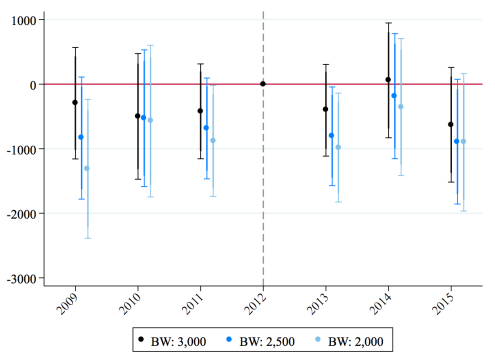
(b) Current spending (PC)



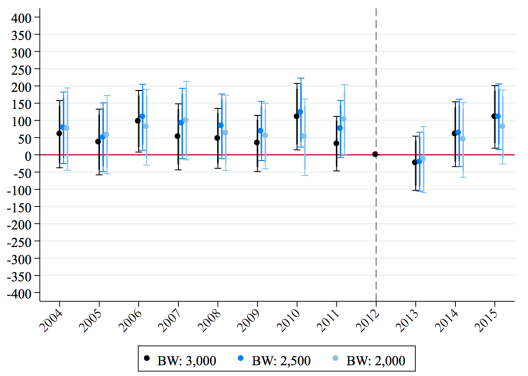
(c) Capital spending (PC)



(d) Procurement spending (PC)



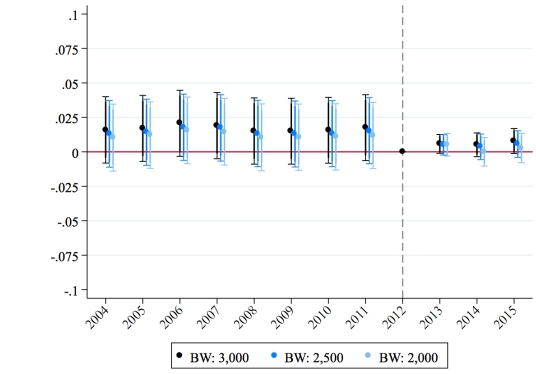
(e) Total spending



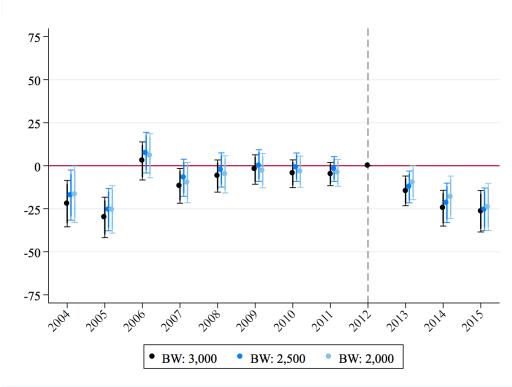
The plot shows the outcomes of the local DID estimation for cities in HFRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The dependent variables are the public finance and the procurement indicators, expressed in per capita terms. The standard specification is the same as in Table 3 (column 3).

Figure A4: Effects of the DSP on local public finance and procurement (Italy) – Event-study

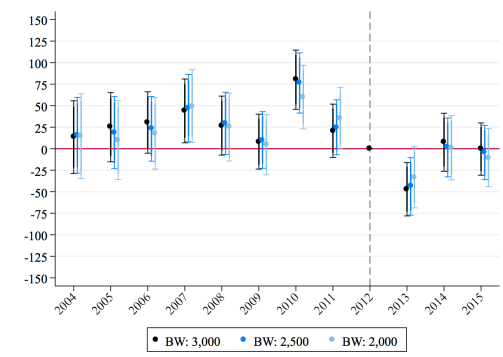
(a) House tax rate



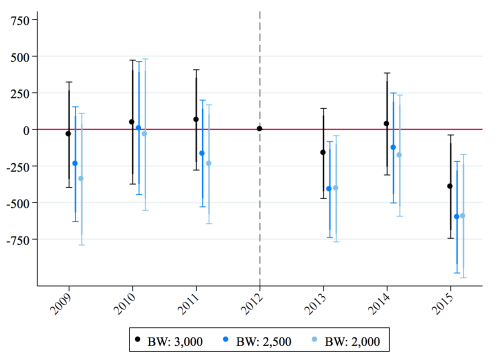
(b) Current spending (PC)



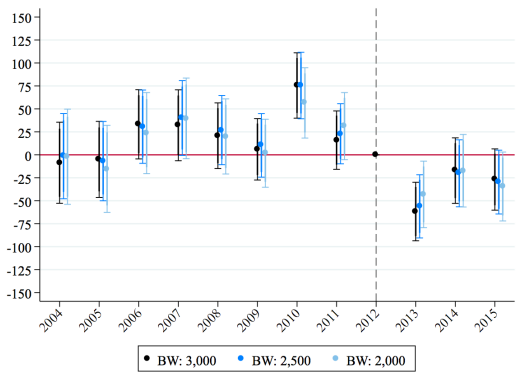
(c) Capital spending (PC)



(d) Procurement spending (PC)

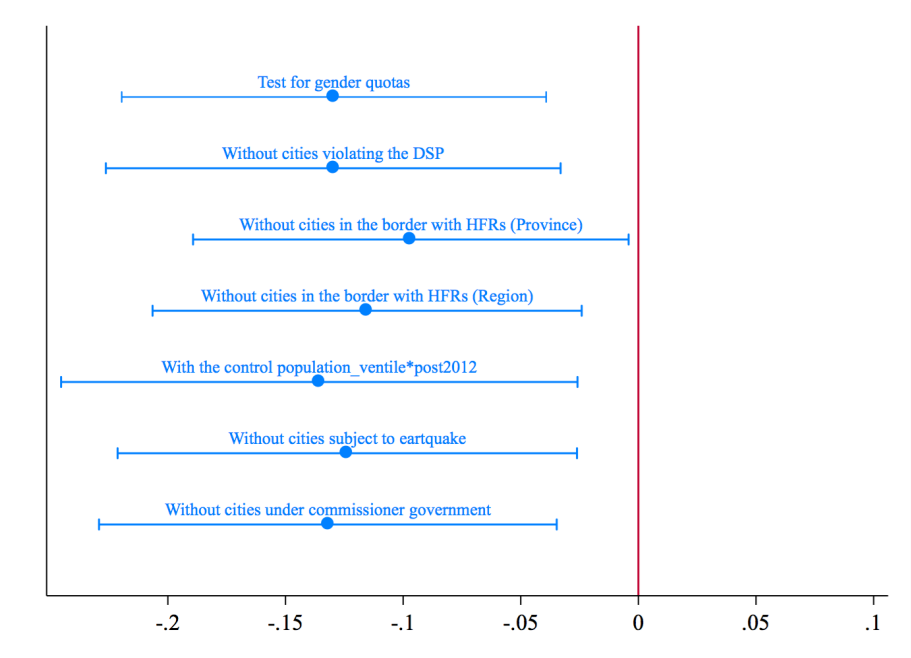


(e) Total spending



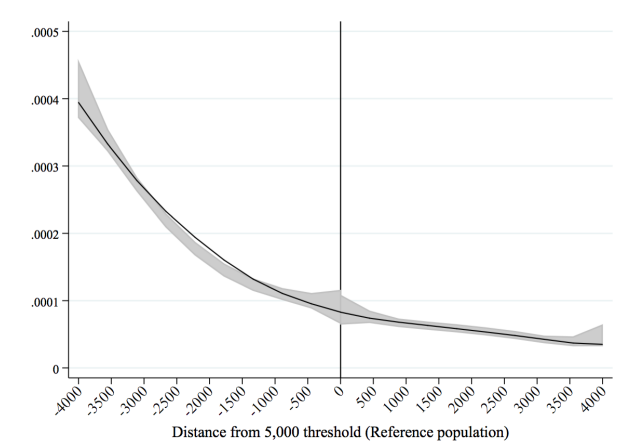
The plot shows the outcomes of the local DID estimation for all Italian cities, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The dependent variables are the public finance and the procurement indicators, expressed in per capita terms. The standard specification is the same as in Table 3 (column 3).

Figure A5: Robustness checks on corruption (PC)



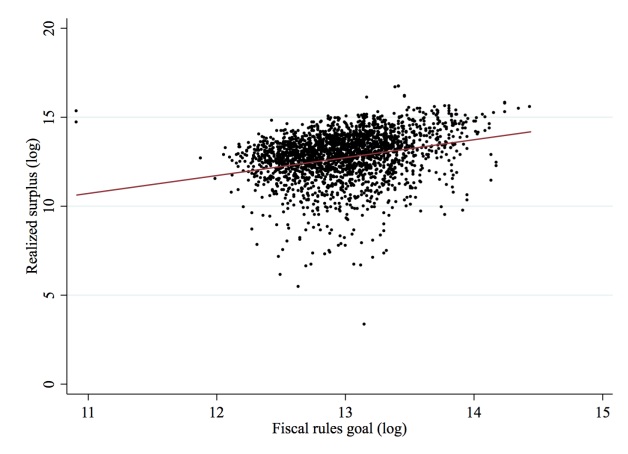
The plot shows the main robustness tests conducted on the dependent variable corruption investigations per 1,000 inhabitants (standardized). Each dot is a distinct analysis and represents the DID estimator and the corresponding confidence intervals (95%) in a distinct regression according to Model 2. 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 HFRs (Province)" and "Without cities in the border with HFRs (Region)" we exclude cities located in provinces/regions located close to the HTRs to control for potential displacement effects.

Figure A6: McCrary test – density around the 5,000 population threshold



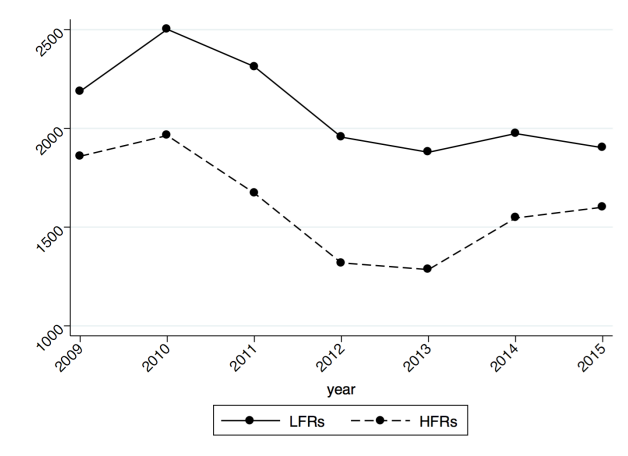
The plot shows the McCrary test conducted using the reference population for the application of the DSP for the year 2013. The population threshold studied is the one of 5,000 inhabitants.

Figure A7: Stability pact – realized and targeted surplus (LFRs)



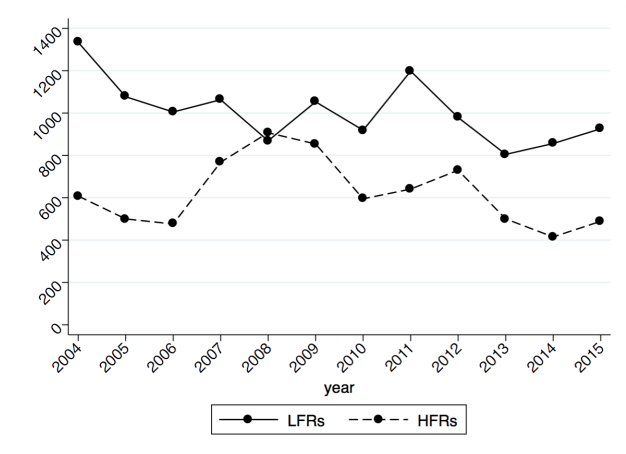
The plot shows the relationship between the amount of surplus that the DSP requires to accumulate in logarithm ("Fiscal rules goal") and the surplus actually accumulated by Italian municipalities in logarithm ("Realized surplus"). The sample includes towns in the LFRs with a population of 2,500–5,000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

Figure A8: Appeals to courts over time (filed)



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

Figure A9: Number of convictions on corruption crimes



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

Table A1: Effect of the DSP on corruption – 1,000 inhabitant threshold

	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	-15.069
	(0.029)	(9.045)	(11.698)	(33.376)
N	13254	14436	14489	14489
R^2	0.176	0.882	0.883	0.426
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 LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Effect of DSP on single corruption charges

	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 LFRs. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Impact of DSP - Sample 2008-2015

	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***	-61.339***	-170.960***
	(0.047)	(0.099)	(6.584)	(6.030)	(9.817)	(61.957)
N	10418	10398	11824	11836	11836	10420
R^2	0.333	0.322	0.911	0.932	0.441	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 LFRs. 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 A4: Analysis Difference-in-Discontinuities

	Corruption (PC) (1)	Corruption over spending (2)	Total revenues (PC) (3)	Current spending (PC) (4)	Capital spending (PC) (5)	Procurement spending (PC) (6)
Panel a: Optimal bandwidth						
Stability pact (T^*S)	-0.139*	-0.246	-4.185	-38.513	-47.901	-312.048**
	(0.071)	(0.151)	(33.844)	(40.026)	(40.886)	(155.316)
N	10355	5778	5877	6012	6360	5831
R^2	0.008	0.010	0.066	0.042	0.048	0.014
Panel b: Augmented opt. bandwidth						
Stability pact (T^*S)	-0.132**	-0.203	-3.289	-35.097	-48.734**	-261.642**
	(0.054)	(0.132)	(23.470)	(27.025)	(21.944)	(106.478)
N	25012	12212	12314	12647	13470	13477
R^2	0.005	0.009	0.057	0.039	0.050	0.014
Controls	Yes	Yes	Yes	Yes	Yes	Yes

The Table shows the results of the estimation of Model 3. 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 (5-8). The specification includes the characteristics of municipal councillors (age, education and gender), margin of victory and term limit (political variables refer to the previous electoral term). The sample includes municipalities located in LFRs. Panel a shows the results with the optimal population bandwidth, according to Calonico et al., (2014), Panel b shows the results with the augmented optimal bandwidth, i.e. two times the optimal bandwidth. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Additional robustness checks

	Past level of corruption	Corruption max	Corruption max	Corruption	Neighbour cities	Police spending	Re-election probability	Other crimes	Strictness DSP
	Corruption (PC)	Corruption max (PC)	Corruption max over spending	binary indicator	Corruption (PC)	Police spending (PC)	Incumbent re-elected	Non-corruption crimes (PC)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Stability pact (S*T)	-0.104**	-0.126**	-0.253**	-0.014*	-0.030	-0.052	-0.023	-0.000	
	(0.045)	(0.049)	(0.105)	(0.007)	(0.050)	(0.672)	(0.061)	(0.001)	
Stability pact (S*T)*_High corruption_	-0.326*								
	(0.178)								
Treatment(S)*HFR									0.023
									(0.025)
N	16314	16314	16282	17797	7535	17797	2664	14836	2117
R^2	0.236	0.224	0.231	0.208	0.255	0.764	0.612	0.874	0.650
City, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	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

The dependent variable is corruption investigations per 1,000 inhabitants (standardized) in columns (1) and (5), corruption max per 1,000 inhabitants (standardized) in column (2), corruption max over spending (standardized) in column (3), corruption binary in column (4), spending in local police per-capita in column (6), a dummy equal to one if the incumbent mayor is re-elected in column (7), the number of committed infractions for non-corruption crimes, expressed in per-capita terms in column (8), and the level of surplus set as the DSP target (expressed in log) in column (9). In column (5) 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, and the interaction term *High corruption* captures the municipalities with a positive level of pre-reform corruption. In column (7) the sample is limited to electoral years and mayors who are 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). The specification for column (9) also include the reference population for the application of the DSP. The sample includes municipalities located in LFRs. The sample only includes municipalities located in HFRs in column (9). The analysis of column (9) only focuses on year 2013. Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Data sources

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

Appendix 2 (for online publication): 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 A7 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) 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), the settings remained essentially unaltered. The only difference was the introduction of a "mixed" basis accounting, according to which current revenues and expenditures follow cash-basis accounting while capital ones follow the accrual-basis system. 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.

Table A7: 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 3 (for online publication): Text analysis on newspaper articles

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

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

   - *Articles' screening:* Through the portal Factiva, we screened the title and the first paragraph of the articles released by ANSA in the time span 1999-2014. We relied on a set of corruption-related keywords to select and download the articles containing these keywords.[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, public contract, damage, fraud, funds, hiring, investment, license, loan, procurement, public works, recommendation, reimbursement, subcontract, supply contract, tender, urban planning.