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Corruption under Austerity *

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Abstract

In this paper, we study how policies limiting the spending capacity of local governments may lead to a reduction in corruption. We exploit the extension of one such policy, the Domestic Stability Pact (DSP), to Italian municipalities with less than 5,000 inhabitants that occurred in 2013. Using a ‘local Difference-in-Differences’ approach, we show that the extension of the DSP led to a substantial decrease in recorded corruption rates. This effect emerges only in areas in which the DSP put a binding cap on municipal capital expenditures, in line with the hypothesis that investments and procurement are naturally prone to corruptive phenomena. We also show that i) the reduction in corruption is linked to accountability incentives; ii) and it is not just a mechanical consequence of the decrease in investments, by pointing out evidence of an improvement in the corruption-proofness of public spending. We then estimate the impact of the extension of the DSP on local welfare, 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 without depressing local welfare.

JEL codes: D72, D73, H62, H72, K34.

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

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1 Introduction

Suppose a country is subject to a new budget constrain, like a set of austerity measures. How will politicians accordingly change public spending? And is this budget shock going to be beneficial to the society? Those 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 assess the causal effect of fiscal austerity studying Italian municipalities which are subject to a new form of budget constraints. We find that a simple budget rule, the Domestic Stability Pact (henceforth ‘DSP’; *Patto di stabilità interno*, in Italian), led to a reduction in corruption driven by lower discretionary spending, without involving a measurable reduction in local welfare.

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

Upon a theoretical point of view, budgets constraints might differentially affect 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 the pact, 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 sanctioning.

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

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

share of corruption-affected public spending. For instance, this would be the case if some voters keep supporting a corrupt politician in exchange of targeted or clientelistic benefits (Nannicini et al., 2013; Boas et al., 2018). A similar outcome is plausible if short term rent-seeking provides a higher utility to politicians than being re-appointed in office (Pereria et al., 2009). Overall, it does remain an empirical question whether and how public budget constraints affect corruption.

The analysis we conduct relies on the extension of the DSP to Italian municipalities with population below 5,000 inhabitants, which occurred in 2013. The DSP already applied to towns with more than 5,000 inhabitants before the reform. Using data from the period 2004-2015, we employ a Difference-in-Differences estimation strategy comparing municipalities below/above 5,000 inhabitants, before and after 2013, to test whether being subject to the DSP affects corruption rates and budgetary outcomes. However, as towns of different sizes may be affected by other policies in a differentiated way, we restrict the sample to municipalities whose population is sufficiently close to the 5,000 threshold. In other words, we employ a ‘local’ version of the usual Difference-in-Differences, testing the robustness of our estimates across many population bandwidths.

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

In line with the first set of hypotheses, we find that the DSP decreased corruption rates (from 6% to 30% of a standard deviation depending on the strictness of the budget constraint) but only in municipalities receiving standard flows of European transfers (hereafter called ‘low-funds regions’ or ‘LFRs’), in which the DSP actively constrained capital spending. Conversely, we do not observe any improvement in corruption in towns located in HFRs, where the grip of fiscal rules on public investments was feebler.

The drop in corruption rates is linked to budgetary changes. We document that municipalities in LFRs are induced by the DSP into reducing public investments (i.e., capital and procurement expenditure), that represent discretionary types of spending and, as such, are more liable to be affected by mismanagement and rent seeking (Hessami, 2014; Mauro, 1995; Liu and Mikesell,

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

2014). We validate this channel showing that i) the DSP does not affect public budget outcomes in HFRs; ii) nevertheless, municipalities in HFRs are able to respect the DSP requirements. This suggests that municipalities in HFRs offset the budget constraint with the inflow of EU transfers.

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

As mentioned above, electoral accountability might explain these findings. This is plausible if the DSP made corruption more expensive as it raised the opportunity cost of public spending. In line with this interpretation, corruption decreases more in electoral years and among re-eligible mayors. We also find that municipalities with more educated mayors are more likely to reduce corruption.

Overall, these findings might imply a trade-off between a beneficial effects of budget constraints on corruption and a drop in potentially welfare-enhancing public investments. To investigate the overall welfare effects of fiscal rules, we test whether the DSP affected GDP (measured by aggregate taxable income), inequality and a newly collected set of outcomes, including all main municipal services (i.e. waste management, kindergartens, police, sport facilities, school canteens, roads, street lighting and urban planning). Overall, we do not find any effect on these measures of local welfare. In a similar vein, we do not find any spillover effect when looking at provincial level economic outcomes. These results suggest an allocative efficiency gain for local public finance. Two possible explanations are consistent with this aggregate null result: (1) mayors cut only inefficient expenditures and/or (2) even if some efficient spending is cut, the reduction in corruption is beneficial for growth and thus compensates for these cuts.

The paper is organized as follows. Section 2 presents the contribution to the literature while

Section 3 documents the institutional background and describes the data used in our analysis. Section 4 discusses the empirical strategy and Section 5 shows our main results on municipalities in LFRs. Section 6 presents the results on HFRs and Section 7 concludes.

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](#); [Debrun et al., 2008](#)). Recent studies have also focused on the political effects of austerity policies ([Stiglitz, 2016](#); [Fetzer, 2019](#)). Our approach is similar to 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., 2016](#); [Gamalerio, 2017](#); [Heinemann et al., 2018](#); [Dovis and Kirpalani, forthcoming](#)). 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.³ While fiscal policy obviously is (and ought to be) motivated by considerations other than its potential spill-over impact on corruption, these unintended effects might be of interest to international organizations and governments debating whether (and how) to introduce budgetary constraints. Our findings are also timely, as we shed light on the effects on corruption of two salient and highly debated policies – EU transfers and fiscal rules – that affect a multitude of local governments on the European continent.

Second, we relate to the literature on anti-corruption policies. While previous studies have compiled considerable evidence on the detrimental effects of corruption (e.g., [Olken and Pande, 2012](#)), there is still a lack of agreement on how to best fight it. 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

³This finding relates to the literature on governments size and rent seeking ([Alesina and Angeletos, 2005](#)).

effects of anti-corruption audits promoted by the national government in Brazil ([Avis et al., 2016](#); [Ferraz and Finan, 2011](#); [Zamboni, et al., 2018](#); [Colonnelli and Prem, 2017](#)), Mexico ([Larreguy et al., 2015](#)), Puerto Rico ([Bobonis et al., 2016](#)), Argentina ([Di Tella and Schargrodsky, 2003](#)) and Indonesia ([Olken, 2007](#)). Conversely, we study a policy which reduces corruption without incurring in additional implementation costs and with little scope for manipulation.

Third, we contribute to the literature estimating the causal effects of government spending on local economic development ([Corbi et al., 2018](#); for a review, see [Chodorow-Reich, 2018](#)). To the best of our knowledge, our study is the first to causally estimate the effects of fiscal rules on local welfare: our null findings support the idea that local economic growth may involve a trade-off between higher public spending and a reduction in corruption. The finding that the provision of European funds lessens the effects of fiscal rules on corruption is in line with [Becker et al. \(2013\)](#), who show that EU funds prompt economic growth only in regions with reliable institutions.

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 ([De Angelis et al., 2018](#); [Brollo et al., 2013](#); [Barone and Narciso 2015](#)). In this paper, we illustrate how governmental transfers (i.e., European funds) may offset the beneficial effects of budget constraints on corruption.⁴

3 Institutional background and data

3.1 The Domestic Stability Pact

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 within the bounds set by Italy’s adherence to the European Stability and Growth Pact.⁵

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

⁴We also relate to studies on the links between public procurement procedures and corruption ([Bandiera et al. \(2009\)](#); [Conley and Decarolis \(2016\)](#); [Decarolis \(2018\)](#); [Coviello and Gagliarducci \(2017\)](#); [Geys and Titl \(2019\)](#)).

⁵See law 448/1998, which first introduced the DSP in Italy.

of DSP to towns with population in the 1,000-5,000 range, that occurred in 2013.⁶ Before that, since 2001, only municipalities with more than 5,000 inhabitants had been subject to the policy (Grembi et al., 2016).⁷

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

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

3.2 European funds

EU funds for investment programs were excluded from the DSP restrictions. Italy is an important recipient of European funds, especially through the European Territorial Cooperation programs. Moreover, 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 GDP, by the above-mentioned 75% GDP rule.

⁶At the beginning of 2013, Italy was divided into 8,092 municipalities, with a median population of 2,438 inhabitants.

⁷The 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. In fact, the three remaining special-statute regions also endorsed a form of the DSP, and both Friuli Venezia-Giulia and the autonomous province of Trento were subject to a similar extension of the lower population limit from 5,000 to 1,000 in 2013. Our main results are robust to variations of the sample that keep account of municipalities that were exempt from applying the DSP. Specifically, in the robustness checks section we report our main findings on corruption excluding special-statute regions from the sample.

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

We collect data on European funds from the OpenCoesione.gov portal.⁹ 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.¹⁰ The total Italian expenditure certified to the EU was €46.2 billions.¹¹

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

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

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

3.3 Data on corruption

Information on corruption is based on the Italian Investigation System (henceforth ‘SDI’; *Sistema d’indagine*, in Italian), a data collection system managed by the Ministry of the Interior. The SDI records details on investigation procedures authorized by the judiciary and carried out by police forces. The data cover the years 2004–2014, and allow us to compute the number of initiated procedures by municipality, year, and type of alleged offence.¹² 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

⁹See 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 Cohesion Policy, financed by European Structural Funds with a national co-financing requirement. Cohesion Funds are a crucial component of the EU’s regional policy, which aims to reduce regional disparities in economic development.

¹⁰<http://eur-lex.europa.eu/legal-content/EN/ALL/?uri=CELEX:52007DC0798>.

¹¹<https://opencoesione.gov.it/en/spesa-certificata/>.

¹²SDI information are not publicly available and not at all available after 2014.

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. The average number of corruption episodes reported in the SDI per municipality and year is 0.17. The average cumulative number of corruption episodes investigated between 2004 and 2014 per municipality is 1.81. As represented in Figure 2, more than three out of four municipalities display no corruption episodes in the period under consideration. Figure 2 highlights that corruption is spread across all Italian regions, although it appears more common in Southern towns. Overall, the average number of episodes recorded each year in the whole country is around 1,300. Furthermore, the phenomenon affects towns of our interest: municipalities between 1,000 and 5,000 inhabitants have on average 1.41 total corruption episodes between 2004 and 2014. Figure 3 plots the aggregate number of corruption investigations for large, medium and small Italian municipalities over time.

In what follows, we use 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 for municipality population, expressed in thousands (this is our main dependent variable, which we label *Corruption (PC)* in what follows); and for the total expenditure (*Corruption (exp.)*). In subsequent regression analyses, both indicators are standardized by region group (LFR or HFR).

3.4 Data on local public finance and procurement

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

Information on public procurement is drawn from data collected by Telemat, a private firm. Available data cover the large majority of public works contracts tendered by Italian municipalities between 2009 and 2015, but information is essentially limited to the award stage, starting when the tender is publicized and ending when the contract is assigned to the winning firm(s). Overall, around 115,000 tenders are recorded in the dataset. In this paper, we mainly use the value of

public works tendered by Italian municipalities over the whole period 2009-2015. Around 18% of all contracts has 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 with the aggregate taxable income declared in Italian municipalities: the average amount is €85.5 millions. The source of this data is the Italian Ministry of the Economy. We complement our analysis with political data on local elections, local politicians and a novel dataset on municipal services provided by the Italian Ministry of Interior. The descriptive statistics are shown in Panel D and F of Table 1.

4 Empirical analysis

4.1 A local Difference-in-Differences approach

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

This exercise allows us to identify the effects of the introduction of fiscal rules in the treatment group, and to overcome the issue of overlapping policies around the same cutoff, that a Regression

Discontinuity Design technique would not be able to deal with. Moreover, the local approach of this methodology, which considers towns in a neighborhood of the population threshold, makes treatment and control groups more comparable respect to a standard Difference-in-Differences with a global approach. Finally, differently from the Difference-in-Discontinuity technique ([Grembi et al. 2016](#), [Gamalerio 2017](#)), this method allows to control for a rich set of fixed effects, including municipal ones.¹³

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. Moreover, 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 outputs are reported in Section 5.

The only other policy change that concerned the 5,000 inhabitants threshold in 2013 was the introduction of double preference voting conditioned on gender (Law 215/2012) in municipalities above the threshold: voters can cast a vote for two candidates (instead of one), provided they are of different gender. The aim of this policy was to increase the share of female politicians in local councils. However, in 2014 a reinforced version of this gender quota policy was extended to towns above 3,000 inhabitants (Law 56/2014), stating that elected politicians of each gender cannot have less than 40% of municipal government seats¹⁴. In those municipalities, gender quotas could bias our results if female politicians have different attitudes towards corruption, as shown by [Brollo and Troiano \(2016\)](#) in the Brazilian context.

To reduce concerns regarding this potential bias, we control for the share of elected female politicians in all our specifications. As a stricter robustness test (in Table 8) we replicate our findings dropping from the sample the post-electoral periods of all municipalities that had elections both in 2013 and 2014. Our findings are unaffected.

Lastly, the evolution of the scope of fiscal rules would also, in principle, allow to study the

¹³However, our results remain unchanged when applying a Difference-in-Discontinuity technique (e.g. [Campa, 2011](#); [Casas-Arce and Saiz, 2015](#); [Grembi et al., 2016](#)). We report those findings among our robustness tests.

¹⁴Italian municipal elections vary based on a five-year calendar, whereby every year a different group of municipalities hold local elections.

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 the power of our analysis. We provide further explanations and these additional analyses in Section 5.7.

4.3 Alternative channels

A further identification assumption is that the introduction of the DSP does not affect corruption through other channels beyond the one suggested by our theory. We here briefly consider the behaviors of three agents involved in this process: the judicial system, the police, and the private sector.

A first concern is that judges who perceive the DSP as carrying anti-corruption content (i.e., anticipating the main result of this paper) may be 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 judges had the chance to observe a reduction in corruption following the original introduction of the DSP in larger municipalities. To the best of our knowledge, though, no official document, juridical opinion or media report from the time when fiscal rules were first introduced links the DSP to any anti-corruption consequence. Moreover, this behavioral bias would be more plausible when fiscal rules were first debated and introduced in bigger Italian municipalities in 1999. A related concern is that the conviction rate might differently change in respect to our treatment: we discuss this point in Section 5.7.

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

Third, some companies losing business due to the decreased spending on public procurement (which is, as shown later on, a direct outcome of budget constraints) might react by pressing corruption-related charges against their competitors more often. We are not able to ascertain whether single corruption investigations are spurred by such complaints. Still, an increase in

reported corruption through this channel would work against our main result, making it a lower bound estimation of corruption reduction after the introduction of the DSP. As a further check, in a robustness test we show that the level of enforcement of the DSP is not linked to a differential trend in firms' accusation charges in procurement.

Finally, note that all those possible source of bias are inconsistent with the changes in municipal spending, which represents, according to us, the mechanism explaining the effects of the DSP on corruption.

4.4 Specification

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

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

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); it relies on two available population censuses (conducted in 2001 and 2011).¹⁵ T_t denotes the post-reform period: it is a dummy equal to one after 2013. To further increase the comparability between the Treatment and the Control group, we control for the distance to the population threshold ($P_i^* = P_i - P_c$, where $P_c = 5,000$) for municipality i . The population of town i , P_i , is based on the value recorded in the two censuses of 2001 and 2011. The fact that the population census is pre-determined to the reform itself avoids the risk of endogenous sorting around the new threshold, which was not known when the census was completed. 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 controls including age, education, gender of the mayor and the municipal councillors.¹⁶ We include municipality fixed effects, γ_i , and year fixed effects, δ_t , and we cluster robust standard errors at the municipal level. Finally, the sample of municipalities included in the

¹⁵Even if S_{it} is estimated in the model, since it is a time-varying indicator, we do not show it in the output tables for the sake of brevity. Nevertheless, the corresponding coefficient is never statistically significant.

¹⁶We generally present the outputs with the complete set of controls. Due to space constraints, we provide the estimates with and without controls only for the main results of the analysis.

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 methodology¹⁷.

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

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

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

5 Results

5.1 Effect on corruption complaints

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

Figure 4 shows the main graphical outcomes from the estimation of Model 1 and 2. It illustrates that corruption significantly decreased in the group of municipalities to which the DSP was extended in 2013. In the left panel, we present the DID analysis, according to Model 2, and the effect is between 6,1% and 11,5% standard deviations. Moreover, there are no differences between the treatment and control groups prior to 2013, which suggests that local trends in corruption are parallel before 2013. Furthermore, the figure demonstrates that the results are very similar across three bandwidths: 2,000, 2,500 and 3,000 inhabitants.

In the right panel, we study how sensitive this result is to the choice of population bandwidth: we plot the DID coefficient, according to Model 1, varying the population bandwidth in the range of 1,000–4,000. The effect of the policy is always negative and sizeable, and the magnitude of the

¹⁷The figure 12 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.

coefficient is rather stable as the sample widens and the effect is statistically significant in nearly all cases. This output suggests that this relationship does not depend on the sample of municipalities included and shows that it is robust to many different population bandwidths.

Columns 1 and 2 of Table 2 display the full results of one of these tests, using a 2,500-inhabitants bandwidth.¹⁸

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

5.2 Effect on local public finance

In this section we study the DSP's impact on local public finance in order to determine why corruption is decreasing. We show that municipalities that experience a drop in corruption are simultaneously decreasing their capital expenditures. Figure 5 includes the outputs of this analysis (Models 1 and 2). All variables are expressed in per capita terms (winsorized at the 1%), and the outputs are presented with three bandwidths: 2,000, 2,500 and 3,000 inhabitants. The results for the 2,500 bandwidth are reported in Table 3.

The first sub-figures display the impact on capital expenditure: the introduction of a budget constraint leads to an immediate and consistent reduction in local spending. The DID approach in the right panel shows a reduction of about 70 euros per capita (per year): more precisely, the effect is between 3.7% and 5.4% standard deviations. The left panel shows that there are no clear pre-trends before 2013. The sub-figures in the second rows of Figure 5 illustrate the impact of fiscal rules on current expenditure. Also in the case, the coefficients are negative and significant. However, the effect is smaller than the one on capital spending, as it amounts to about 20 euros per capita and it is between 2.6% and 3.7% standard deviations.

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

¹⁸In Table 9, instead of using the standard corruption measure as the dependent variable, we look at the four main types of corruption charges included in our dataset: strict corruption, graft, malfeasance and embezzlement (also these variables are standardized by macro regions and expressed in per thousands inhabitants). Our findings are mostly driven by a reduction in malfeasance charges. This result is mainly due to the fact that malfeasance represents the great majority of the corruption-related events committed in our sample, precisely the 69.2%. Moreover, as explained in the next sections, in some heterogeneity tests, we find an effect also on investigations related to resource embezzlement.

In the third row, we focus on the effect on the property tax rate (on main residence), which is the main source of fiscal revenues among Italian municipalities (Bracco et al., 2019). The figures show that the property tax rate weakly decreases after 2012. As the effect takes place already in 2012, it cannot be attributed to the DSP. This downward trend is likely due to a property tax law change in 2012, which might have differentially affected municipalities below 5,000 inhabitants. The size of the effect is very small, i.e. about 1.1% of the variable mean. Overall, this test shows that politicians do not increase local taxation to cope with the DSP.

The fourth row reports the findings on procurement spending. Procurement expenditures are an important component of local public spending, in which local politicians have considerable discretionary powers: this explains why this part of the budget may be an important source of corruption and rent seeking. The figures show that the DSP produced an immediate and persistent drop in procurement expenditures. This result is in line with our findings on capital expenditures, suggesting that municipalities react to the policy with a reduction in discretionary public investments. The effect is very sizeable: the average reduction induced by the policy corresponds to about 250 euros per capita, this corresponds to a reduction between 3.6% and 6.9% standard deviations. The magnitude of the coefficient is bigger than the one observed on capital expenditure as here we are considering the total value of single auctions, which are recorded in the budget over several years.

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

5.3 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 stringer budget constraint. In Table 2 (columns 3-6), we take into account the level of surplus set as the DSP target, and focus on the top 50% and 20% of this variable distribution. In line with our expectations, we find a stronger decrease in capital expenditures especially for towns subject to a more stringent budget constraint. In columns (5-6), we replicate a similar analysis focusing on corruption charges. The effect is remarkably stronger (30% of a standard deviation)

for municipalities experiencing more stringent fiscal rules (Column 6).

Finally, in line with the idea that stricter fiscal rules push politicians to improve the municipal surplus, Figure 6 shows the correlation at the municipal level between the fiscal rules target (horizontal axis) and the realized surplus (vertical axis). Overall, this set of specifications highlights that i) a stricter budget constraint leads to stronger changes in public spending, and in turn, in corruption levels and ii) compliance has a crucial impact on budgetary and rent seeking outcomes.¹⁹

5.4 Impact on the corruption-proofness of public spending

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

To test this hypothesis, we replicate our main analysis introducing as a dependent variable the ratio between the standardized number of corruption charges and the annual capital expenditures. Figure 7 displays this test. We find a decrease in corruption per euro spent in capital expenditures of a magnitude similar to the main results on corruption per capita. This implies that the DSP leads to an improvement in the corruption-proofness of public spending, which suggests that local politicians are not just reducing public investments, but are cutting the least efficient ones.

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

We directly test for a plausible strategy to displace rent seeking, looking at the share of public procurement assigned below 40,000 euros. This is an important threshold, as politicians can award contracts below this amount without a competitive bidding process. Table 4 replicates our main estimation, introducing as dependent variables: i) the percentage of tenders for amounts under 40,000 euros, ii) the percentage of total amount in tenders whose value is lower than 40,000 euros, and iii) the overall tendered amount (per capita) in tenders with value lower than 40,000 euros.

¹⁹We run those heterogeneity tests for each type of corruption-related crime. While malfeasance drives our findings, we also find a reduction on embezzlement-related investigations, when testing the differential effect as a function of the intensity of the DSP application. These outcomes are not shown for the sake of brevity.

Overall, we find a decrease in the total tendered amount below this threshold, but we do not find a significant decrease in the number or percentage of tenders below 40,000 euros. These findings show that politicians are not resorting to less transparent tenders to potentially hide rent seeking.

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

5.5 Impact on local welfare

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

5.5.1 GDP

We first test the effect of fiscal rules on per capita municipal-level GDP, which is proxied by individual's income, as declared to the Italian fiscal agency. Table 5 (column 1) reports these estimates. We report the effect of the DSP on local GDP up to 2015. Overall, we find that fiscal rules have no effect on local GDP. While a reduction in local public investments might deter economic growth, a drop in inefficient spending and rent seeking might have the reverse effect: the two effects seem to cancel each other out. However, several other explanations might be at work: i) our measure might be noisy as it includes only declared income; ii) multipliers effects might just be too small; iii) or fiscal sustainability might improve expectations resulting in a welfare increase.

5.5.2 Inequality

As a complementary test, we investigate the DSP's effect on inequality. Our data include individual income grouped into seven brackets. 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,²⁰ and we define the bottom bracket in two ways: 0–10,000 euros and 0–15,000 euros. We report these tests in column 2-3 of Table 5. We do not find any significant effect of the DSP on this measure of inequality, which implies that, on average, income differences between the top and bottom earners have not changed.

5.5.3 Province welfare

In a complementary test we use provinces, instead of municipalities, 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 all 110 Italian provinces. This increase was heterogeneous across provinces, as each province has a different share of municipalities with a population of 1,000–5,000. Figure 13 plots the share of treated municipalities, illustrating the large between-province differences. Therefore, to estimate the effects of fiscal rules on provincial welfare, we exploit the fact that after 2013 there is a differential increase in the number of municipalities constrained by the DSP across provinces.

First, 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 standard (local) DID analysis studying the interaction between the indicators *Post 2013* and *Share*²¹. We consider some macro-level indicators, which are available at the provincial level from Eurostat (Nuts 3 level): GDP, the log of the total number of employed individuals and the total number of firms. We are interested in the interaction term *Share*Post-2013*, which captures the impact of adopting fiscal rules on these provincial-level outcomes.

This specification allows us to test for geographical spill-over effects of the DSP, as the effects on welfare might spread to local economies beyond the municipal borders. This is likely to be the

²⁰The 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.

²¹More precisely, we estimate the following model:

$$y_{jt} = \beta_0 + \beta_1 \text{Share}_{jt} + \beta_2 \text{Share}_{jt} \cdot T_t + \zeta' X_{jt} + \delta_t + \gamma_j + \eta_{jt}, \quad (3)$$

where y_{jt} is the dependent variable in province j , in year t . Share_{jt} indicates the share of municipalities in province j with 1,000–5,000 inhabitants. γ_j indicates province fixed effects and the other terms are defined as in Model 1.

case, since firms applying for public contracts in a municipality are often based in other towns in the province. Columns 5-6 of Table 5 report the results of this test. We find that the budget constraint does not have an effect on these different outcomes.

5.5.4 Municipal services

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

5.6 Mechanisms: Accountability

As already anticipated in the introduction, accountability may explain the observed reduction in politicians’ rent seeking. Mayors may comply with the DSP by increasing local taxes or reducing expenditures, some of which are prone to rent seeking. Intuitively, the electoral costs of reducing inefficient expenditures (i.e. rent seeking) would be lower than cutting essential local expenditures or increasing taxes. In other words, re-election incentives might push mayors towards more efficient spending.

To investigate this channel, we assess whether local politicians are more likely to reduce corruption during electoral years. Moreover, we exploit the fact that Italian mayors have a two-term limit to determine whether mayors who are eligible for re-election react differently than those who are not. Table 6 shows that mayors are more likely to reduce corruption – but not capital expenditures – in election years (column 1). Furthermore, we find that corruption is more likely to decrease when the budget constrain is managed by a first-term (re-eligible) mayor compared to second-term mayors (column 3).²²

²²The lack of a reduction in investment spending in election years may be due to the presence of large political budget cycles in Italian municipalities, in terms of spending and revenue, as documented by [Repetto \(2018\)](#) and [Giommoni \(2019\)](#).

In Columns 5-6 we test an additional channel – politicians’ ability. We differentiate between mayors with and without a university degree. Column (6) shows that educated mayors are much more likely to reduce corruption, but they do not reduce capital expenditures more than mayors without a degree. This finding suggests that they might be more efficient, as they are able to reduce corruption more than less-educated mayors without cutting public investments.²³

Finally, we also test whether mayors in treated municipalities, which adopt fiscal rules, are more likely to be re-elected given the lower levels of corruption. We might expect two opposite effects: on the one hand, if voters are informed about the corruption decrease, they might reward the local government. On the other hand, voters might not like the decrease in capital expenditures, punishing the incumbent government. Note that this is not in contrast with the above mentioned political accountability mechanism: fiscal rules constrain the policy set of local politicians, who might have to choose among a set of unpopular policies (i.e. reducing expenditures and/or increasing local taxes) to respect the budgetary constrain. In turn, they might try to adopt the least 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). Table 8 (column 7) presents the results, which suggest that the policy does not have a significant impact on the incumbent’s chances of re-election. We also tested for alternative measures of electoral competitiveness, including the winning candidate’s margin of victory, and found no significant heterogeneity on our dependent variable. Overall, these findings are inconclusive and might be explained by the fact that the two above mentioned effects are at work offsetting each other.

5.7 Robustness tests

In this section we list a set of additional tests, which are reported and discussed in Appendix 1.

First, as noted above (Section 4.2), we show that the DSP does not affect corruption at the 1,000-inhabitants threshold (Table 7).

²³A second possible 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 distinguish between accountability and monitoring. The monitoring effect should be at work in HFRs as the DSP is *de jure* enforced. Conversely, the accountability channel is shut down, as those municipalities are not *de facto* financially constrained by the policy. Therefore, mayors in HFRs do not face a trade-off between cutting inefficient expenditures and reducing their own rent seeking. In other words, both channels, accountability and monitoring, are at work in LFR municipalities, while monitoring is the only relevant channel in HFRs. As shown in Section 6, we do not find any effect in the latter group, which implies that monitoring is not likely to be the most relevant channel.

Second, as explained in Section 4.3, we document that the DSP does not reduce local police expenditures (Column 1 of Table 8).

Third, as discussed in Section 4.3, we show the lack of differential trends on firms' accusations charges across LFRs and HFRs (Figure 9).

Fourth, we then report our main findings in three ways: i) estimated using a difference-in-discontinuity approach (column 2 of Table 8); ii) dropping regions with a special statute (column 3 of Table 8); and iii) dropping from the sample (in the post-electoral periods) municipalities that held elections in 2013 and 2014, to account for the incidence of gender quotas (column 4-5 of Table 8). Our findings are confirmed in all cases.

Fifth, column 6 of Table 8, tests for displacement effects in neighboring towns (we find a null effect).

Finally, in Figure 10, we show that the conviction rate related to corruption crimes do not differentially change across LFRs and HFRs before/after 2013.

6 Effects in high funds regions (HFRs)

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

We start by reporting the effects of the DSP on municipalities in these regions in terms of corruption and municipal budget. Figure 11 reports our findings based on model 1. First, we do not observe any effect on corruption. Second, we do not find any change in spending both in terms of capital and current expenditure. Third, we find a weak decrease in the property tax rate similarly to the case of LFRs. Those findings are reported in Table 10 with a 2,500 bandwidth.

Overall, fiscal rules do not affect corruption in this area, as local politicians are not facing a strict budget constraint. This interpretation is confirmed by the fact that almost all municipalities in HFRs respect the requirements of the DSP. Indeed, according the Italian Ministry of Interior (with a decree issued on 28 September 2015), only 60 Italian municipalities (out of about 6,000 under fiscal rules)²⁴ were to receive sanctions for non-compliance with the DSP in 2013 and 2014. In other words, municipalities in HFRs did not need to cut expenditure or increase taxes to respect fiscal rules, as they were receiving additional transfers from the EU (as explained in Section 3.2).

²⁴Specifically, 22 municipalities are located in LFRs and 38 in HFRs: https://dait.interno.gov.it/documenti/decreto_f1_28-09-2015-01_0.pdf.

To provide quantitative evidence of this interpretation, i.e. to the mediating role of the European funds, we consider the entire sample of both LFRs and HFRs, and we analyse the differential impact of fiscal rules depending on the amount of funds received by province in which each municipality is located.²⁵

Table 11 shows the results of this triple-differences analysis (we also run this test using a 2,500-inhabitant bandwidth): *Stability pact* captures the DID estimator (i.e. being in a treated municipality after 2013), while the interaction term *Stability pact*EU funds*, the triple-differences estimator, represents the differential impact of the policy depending on the transfers received by each province. European funds are measured as the total amount of provincial transfers per capita (in thousands of euros), spent in 2013–2015.²⁶

On the one hand, the DID indicator is negative and significant, confirming that the introduction of the DSP reduced corruption. On the other hand, the amount of funds spent locally seems to modify the effect of the policy: DSP’s beneficial impact on corruption weakens as provincial transfers increase. This result suggests that European transfers offset the positive impact of fiscal rules on corruption levels and may facilitate the emergence of corruption-related phenomena. This output is consistent with the findings of [De Angelis et al. \(2018\)](#), who show that EU transfers increase corruption in Southern Italy.

7 Concluding remarks

In this paper, we study the impact of fiscal austerity on corruption. To do so, we exploit the extension of a specific set of fiscal rules – the Domestic Stability Pact – to Italian municipalities with a population below 5,000 that occurred in 2013. We employ a ‘local’ Difference-in-Differences estimation to study how the policy affected local corruption levels and budgetary outcomes. We find that the DSP produced a substantial decrease in both corruption levels per capita and in the intensity of corruption over total capital expenditures, interpreting the latter as a measure of ‘corruption-proofness’ of local investments. These effects are driven by a reduction in capital expenditures, and emerge only in areas in which the DSP was fully binding. Indeed, we do not

²⁵We prefer the provincial to the municipal allocation, as EU fund allocation at the municipal level would be endogenous: each region is in charge of allocating funds among local governments, and mayors in municipalities with the DSP might have differential incentives to apply for EU funds.

²⁶Note 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.

observe a reduction in corruption among municipalities eligible to receive extra transfers from the EU, which are *de facto* less constrained by the DSP: in these municipalities no reductions in capital expenditures are observed. Finally, we find that local welfare is not significantly affected by the imposition of a budget constraint.

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? A first important caveat is that in our 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. Second, electoral accountability feedbacks seem to determine local politicians' response to the budget shock, as shown in Table 6. This implies that other types of budget shocks and/or different accountability incentives might lead to distinct effects on corruption and local welfare. A similar reasoning applies to the time frame of our analysis which is limited to the short-medium run.

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Appendix 1: Robustness checks

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

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

In Table 7, we replicate our analysis exploiting the 1,000 threshold. Specifically, we compare municipalities with a population below/above 1,000 before/after the introduction of the DSP in 2013. As expected, we do not find any effect of the DSP on public spending or corruption charges when considering the 1,000 threshold.

Effect of the DSP on local police expenditures

An alternative explanation for our findings is that the DSP pushes local politicians to cut spending on local police, which in turn reduces the probability that corrupt officials are detected by the authorities. This explanation is unlikely for two reasons. First, the municipal police is not in charge of pursuing corruption-related crimes, which are investigated by a specific branch of the national police. Second, the results in Table 8 (column 1) demonstrate that the DSP did not generate a decrease in spending on local police.

Analysis with a difference-in-discontinuity methodology

In this section we conduct the main analysis on corruption using the difference-in-discontinuity methodology in order to check whether the main results are also robust to the application of this method. This methodology has been used in many recent studies (e.g. [Campa, 2011](#); [Casas-Arce and Saiz, 2015](#); [Grembi et al., 2016](#)) and it is based on comparing outcomes before and after

the reform for municipalities around the population threshold.²⁷ Column 2 of Table 8 shows this test. The DSP negative effect on corruption also emerges in this case, and the magnitude of the estimate is similar to the one estimated using the local DID methodology. As explained in the empirical strategy section, we use a local DID methodology for two reasons. First, it is more precise because it allows us to include municipality fixed effects, controlling for time-invariant municipal characteristics. Second, it facilitates a more standard evaluation of the absence of pre-trend effects.

Other tests

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

First, the application of the DSP was completely enforced for ordinary Italian regions, but some of the special regions could decide to what extent adhere to this policy. To check the robustness of our findings, we re-run the main specification, excluding the special regions. The results, reported in Columns 3 of Table 8, show that the negative impact on corruption is still sizeable and significant.

Second, a potential threat to this identification strategy may come from the introduction of gender quotas. This policy was adopted in 2013 in municipalities with more than 5,000 inhabitants and it represents a double-preference voting, conditioned on gender. However, a reinforced version of this policy was extended in 2014 to towns with populations over 3,000, which states that elected politicians of either gender cannot have less than 40% of municipal seats. In order to check for the robustness of our findings, we estimate the main model excluding towns that held an election in 2013 and 2014 in the post-electoral periods. We show this specification in Table 8 (columns 4-5), which suggests that this policy overlap is not a problem for our results. We also run this robustness test for Capital expenditures and the results are similar to the main outcomes.

Third, one possible concern may be that the DSP could affect firms' propensity to charge their own competitors due to the reduction in procurement spending. This mechanism should be at work only in LFRs. Since data on firms' appeals related to public procurement are not available at the municipal level, we use regional-level data that differentiates between LFRs and HFRs to rule out this confounding story. Figure 9 plots the total number of appeals to courts over time

²⁷Following Grembi et al. (2016), the empirical model to be estimated is as follows:

$$y_{it} = \beta_0 + \beta_1 P_i^* + \beta_2 S_{it}(\gamma_0 + \gamma_1 P_i^*) + \beta_3 T_t[\delta_0 + \delta_1 P_i^* + S_{it}(\zeta_0 + \zeta_1 P_i^*)] + \eta_t + \xi_p + \epsilon_{it} \quad (4)$$

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

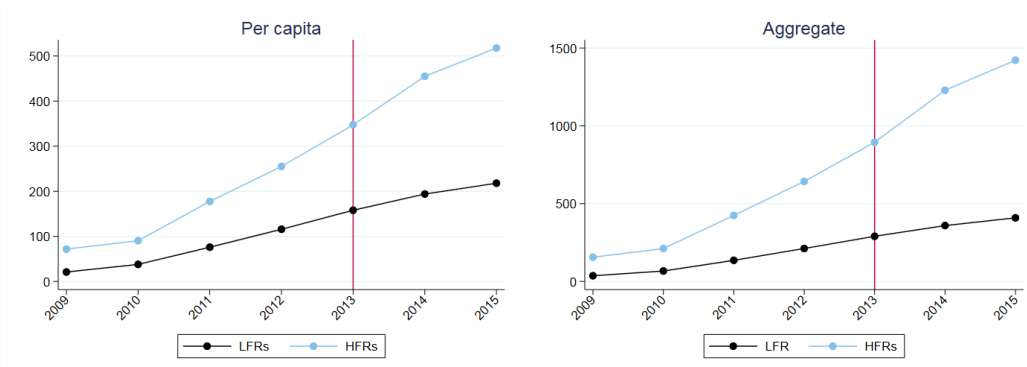
for LFRs and HFRs: the trends in accusations are parallel. This suggests that the introduction of fiscal rules in LFRs did not modify firms' charging behaviour there, compared to HFRs.

Fourth, column 6 of Table 8, tests for displacement effects in neighboring towns. The scope of this test is to investigate possible displacement effects, whereby the drop in corruption among treated cities might be replaced by a growth in neighboring towns. In this case, the treatment group includes municipalities neighboring 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 neighboring cities, which suggests a net decrease rather than a corruption displacement.

Finally, a last concern relates our choice of focusing on corruption investigations rather than convictions. The choice is motivated by two reasons: i) as explained in the main text, the time span between an investigation and the actual crime is much shorter compared to a conviction, which could take place several years later; ii) conviction data are available only at the regional level. Our results might be biased if conviction rates differentially change across cities with or without fiscal rules: for instance, if judges strategically modify their efforts. Similarly to the case of firms' accusations, this effect should take place especially in LFRs. In Figure 10, 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.

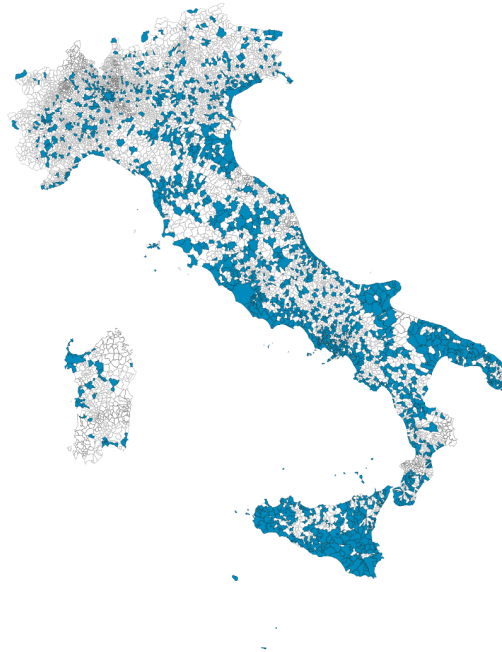
Figures

Figure 1: Evolution of European funds, 2009–2015



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

Figure 2: Corruption investigations in Italian municipalities



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

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

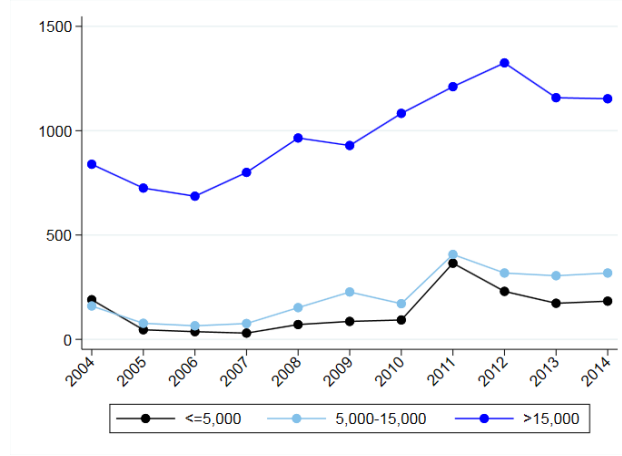
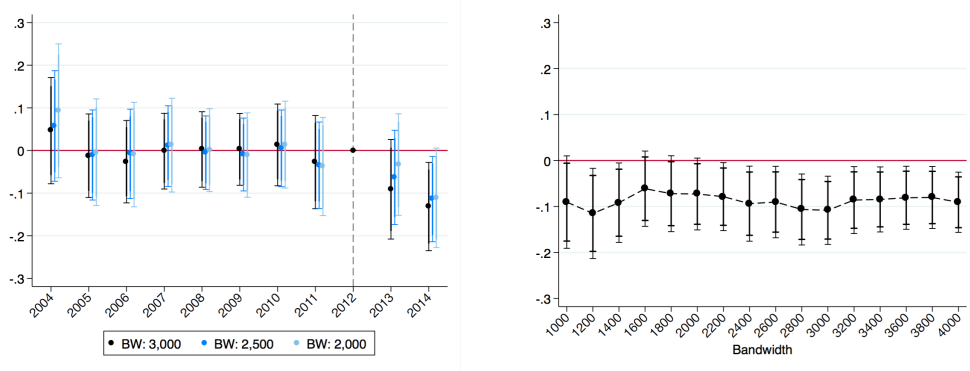
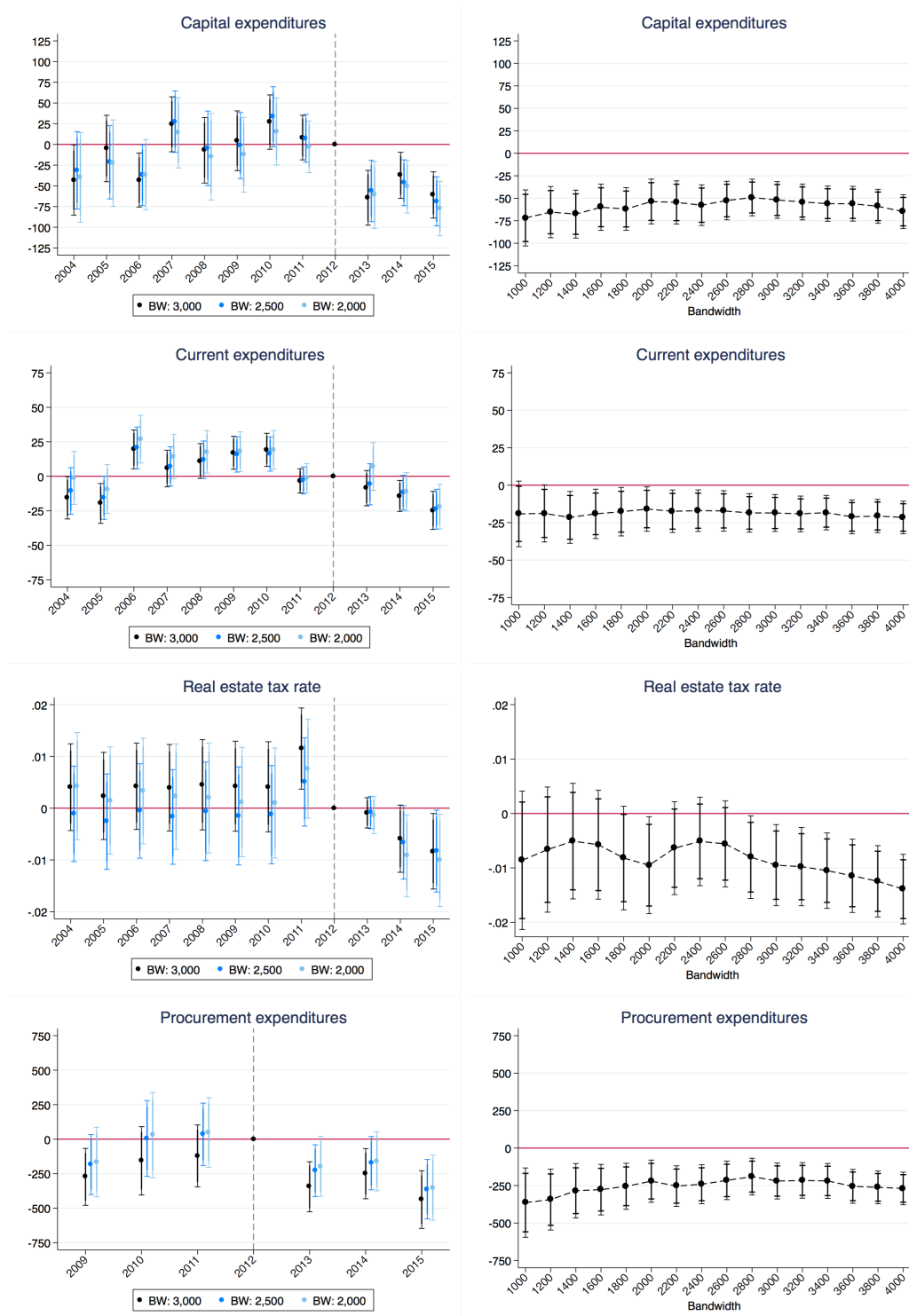


Figure 4: Effect of the DSP on corruption



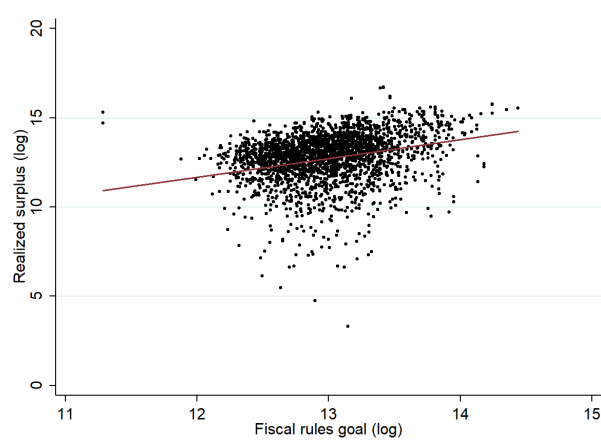
The left plot shows the outputs of the local DID estimation for cities in LFRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local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

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



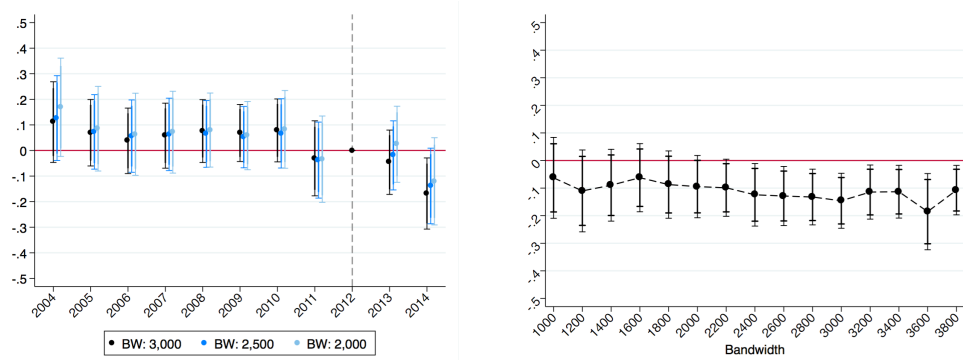
The left plot shows the outputs of the local DID estimation for cities in LFRs, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line) confidence intervals are shown. The right plot shows the sensitivity analysis of the local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

Figure 6: Stability pact – realized and targeted surplus (LFRs)



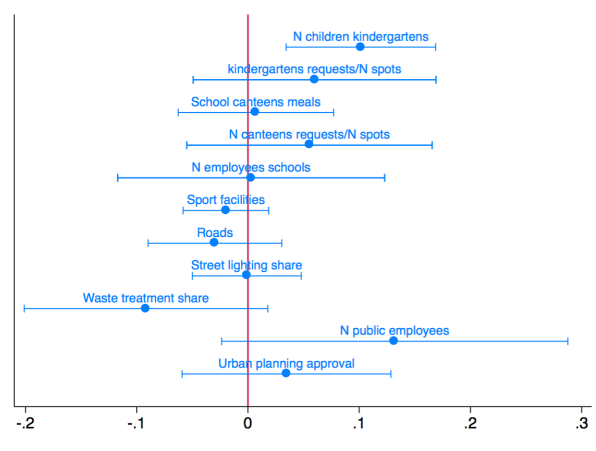
The plot shows the relationship between the amount of surplus that the DSP requires to accumulate in logarithm ("Fiscal rules goal") and the surplus actually accumulated by Italian municipalities in logarithm ("Realized surplus"). The sample includes towns in the LFRs with a population of 2,500–5,000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

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



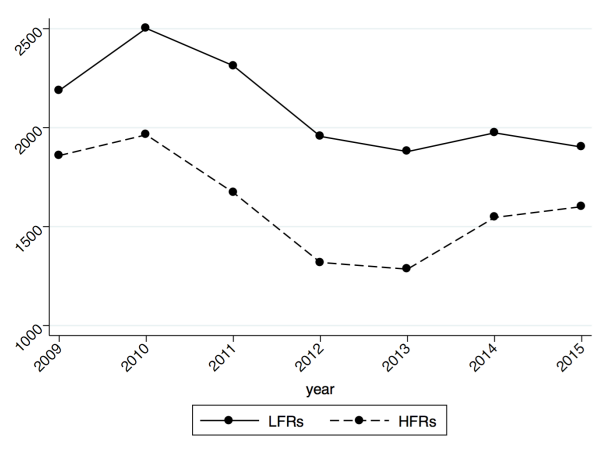
The dependent variable is corruption investigations per euros spent in investments, standardized by region group. The left plot shows the outputs of the local DID estimation, according to Model 2, for three different bandwidths. For each coefficient, 95% (delimited by horizontal bars) and 90% (bold line)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

Figure 8: Impact on municipal public services



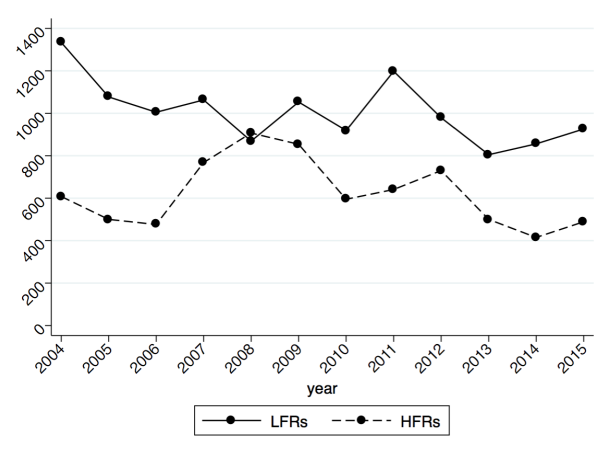
The plot shows the impact of the introduction of the DSP on a set of municipal service (in a standardized version). Each dot is a distinct analysis and represents the DID estimator and the corresponding confidence intervals in a distinct regression according to Model 2. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors are clustered at the municipal level. *N children kindergartens* refers to the number of children attending public kindergartens; *kindergartens requests / N spots* is the share between the number of children requests to be admitted to public kindergartens and the number of available spots; *School canteens meals* is the number of meals provided by public schools; *N canteens requests / N spots* is the share between the number of students requests to be admitted to schools canteens and the number of available spots; *N employees schools* is the number of public employees in municipal schools; *Sport facilities* is the number of public sport facilities; *Roads* measures the length of municipal roads; *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; *Urban planning approval* is a dummy equals to 1 if the city council approved an urban planning strategy (in Italian ‘piano regolatore’).

Figure 9: Appeals to courts over time (filed)



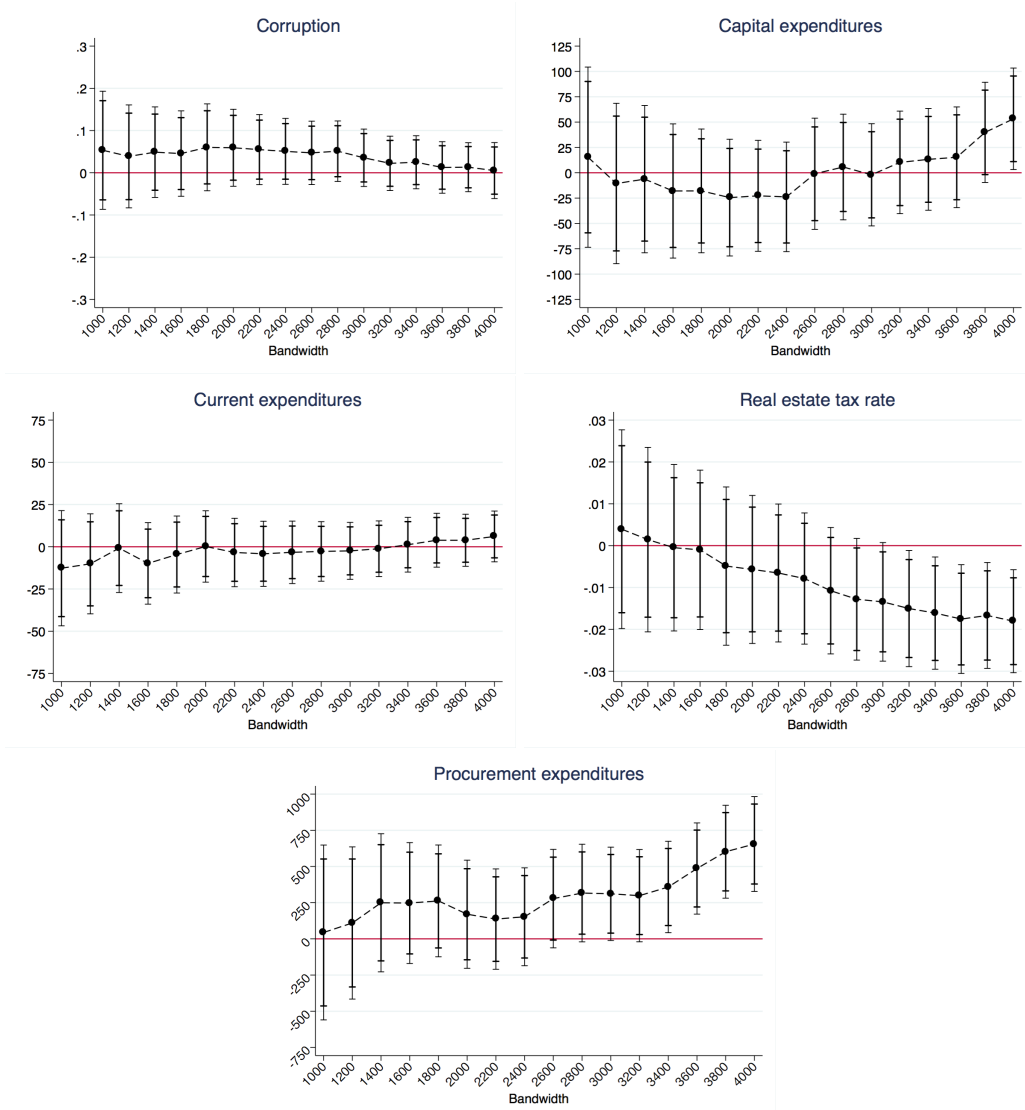
This plot shows the total number of appeals to courts filed over time. *Source:* Italian Ministry of Justice.

Figure 10: Number of convictions on corruption crimes



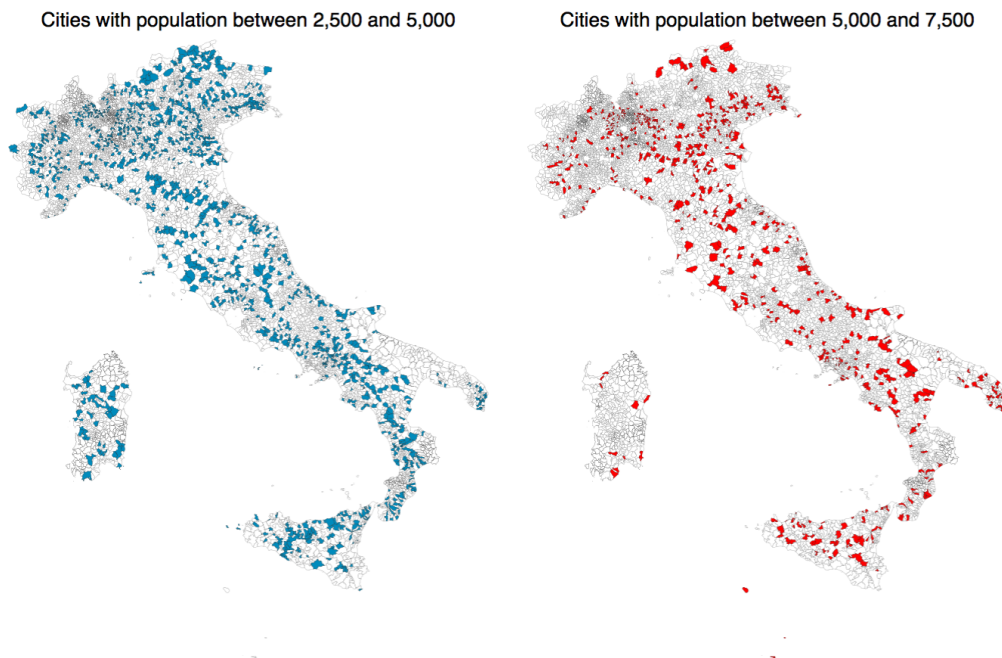
This plot shows the total number of convictions on corruption crimes over time, dividing between LFRs and HFRs.
Source: Italian Institute of Statistics (ISTAT).

Figure 11: Effects of the DSP in HFRs



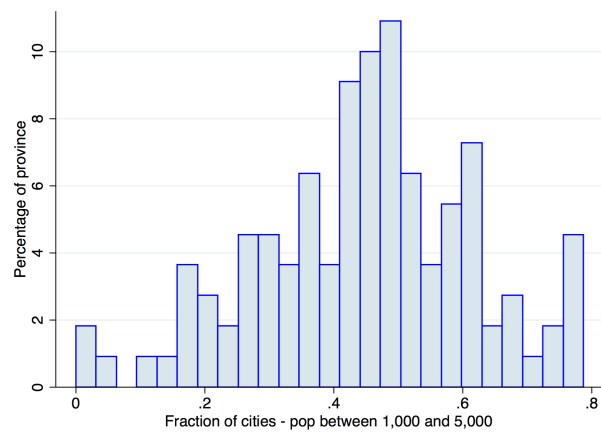
The plots show the sensitivity analysis of the local DID for cities in HFRs, according to Model 1. Each point represents the local DID estimator for a distinct analysis conducted with the corresponding population bandwidth, along with the relevant 95% and 90% confidence intervals. The dependent variables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

Figure 12: Geographical distribution of treatment and control



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

Figure 13: Share of municipalities where DSP is extended across provinces



This plot shows the share of municipalities across Italian provinces where fiscal rules were extended in 2013 (i.e. towns with a population of 1,000–5,000). Population values refer to the 2011 Census.

Tables

Table 1: Descriptive statistics

	All cities	LFRs	HFRs
Panel A: corruption			
Corruption (PC)	0.015	.0088	.032
Corruption (exp.)	0.010	.0066	.021
Panel B: public finance (€ per capita)			
Real estate tax rate	.485	.489	.474
Current expenditures	887.6	900.2	850.3
Capital expenditures	585.1	568.5	634.4
Panel C: local procurement (€ per capita)			
Total amount	1,212.0	785.2	2,419.9
Panel D: local politics			
Electoral turnout	0.760	.770	.730
Term limit	.332	.347	.291
Mayor university degree (1 = univ. degree or above)	0.845	.822	.912
Age of the mayor (years)	49.4	49.6	48.9
Gender of the mayor (1 = female)	0.109	.128	.058
Av. education councillors (1 = univ. degree or above)	.537	.487	.677
Councillors' age (Av.)	44.39	44.84	43.11
Proportion female councillors (1 = female)	.202	.219	.153
Incumbent re-election	.419	.434	.372
Panel E: local growth			
Municipal GDP (millions of €)	85.5	91.6	68.2
Province GDP (millions of €)	14,407.2	16,670.4	9,557.5
Province employment (per-capita)	.407	.437	.335
Province N firms (per-capita)	.076	.083	.059
Panel F: local Services			
N children kindergartens	28.6	35.2	10.0
kindergartens requests / N spots	73.7	1.2	479.8
School canteen meals	922.4	1,083.3	468.2
N canteens requests / N spots	.994	.996	.989
N employees schools	2.9	3.4	1.9
Sport facilities	5.7	5.0	7.6
Roads (km)	25.8	22.0	36.8
Street lightening (share)	.571	.569	.577
Waste management share	.903	.904	.897
N public employees	9.5	8.4	12.5
Urban planning approval	.755	.821	.569

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

Table 2: Impact of DSP on corruption charges

	Corruption (PC)		Capital spending (PC)		Corruption (PC)	
			(top 50)	(top 20)	(top 50)	(top 20)
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact (S^*T)	-0.0750 (0.0394)*	-0.0855 (0.0402)**	-129.4 (20.22)***	-229.2 (53.66)***	-0.0923 (0.0633)	-0.305 (0.185)*
Observations	17992	17481	9405	3749	8759	3513
Adjusted R^2	0.010	0.011	0.107	0.110	0.012	0.017
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), (2), (5) and (6), and the amount of capital spending per-capita in columns (3) and (4). The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). In columns (3-6), municipalities are required to accumulate a level of surplus above the top 50% or 20% of the variable distribution. Robust standard errors clustered at the municipal level are in parentheses:

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Impact of DSP on public finance and procurement

	Real estate	Current	Capital	Procurement
	tax rate	spending (PC)	spending (PC)	spending (PC)
	(1)	(2)	(3)	(4)
Stability pact (S^*T)	-0.00516 (0.00410)	-17.32 (6.984)**	-54.95 (11.19)***	-227.0 (65.76)***
Observations	19,319	18,890	18,890	10,954
Adjusted R^2	0.441	0.130	0.091	0.023
City, year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500

The dependent variables are the public finance and procurement measures expressed in per-capita terms. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Impact on corruption-proofness of spending

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

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

Table 5: Impact on local welfare

	Municipal welfare	Municipal income inequality		Province welfare		
	Municipal GDP (PC)	Measure 1	Measure 2	GDP (PC)	Employment (PC)	N firms (PC)
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact (S^*T)	-54.48	0.00727	0.00730	-0.00132	-0.0129	-0.00115
	(84.84)	(0.00533)	(0.00545)	(0.00134)	(0.0129)	(0.00173)
Observations	19399	19314	19314	900	852	600
Adjusted R^2	0.146	0.013	0.013	0.381	0.433	0.742
City (or province), year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	-	-	-

The dependent variables are the following. Column (1): local GDP measured as the sum of individuals income, expressed in per capita terms. Columns (2) and (3): the difference between the average income declared in the last and first income brackets. The last bracket included taxpayers with an income of 75,000–120,000 euros, and the first income bracket includes taxpayers with an income of 0–15,000 euros (Measure 1 in column 2) or 0–10,000 euros (Measure 2 in columns 3). Columns (4-6) show the analysis at the province level: the treatment is the fraction of municipalities in a province with a population of 1,000–5,000 after 2013. The dependent variables are drawn from Eurostat. Province and year fixed effects are included. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Accountability vs. ability

Dependent variable:	Election year		Mayor term limited		Mayor high education	
	Corruption PC	Capital exp. PC	Corruption PC	Capital exp. PC	Corruption PC	Capital exp. PC
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact ($S*T$)	-0.0429 (0.0406)	-57.78 (15.84)***	-0.125 (0.0529)**	-78.37 (21.41)***	0.0962 (0.0983)	-95.08 (33.18)***
Stability pact*interaction	-0.142 (0.0636)**	40.63 (25.69)	0.115 (0.0636)*	-9.801 (20.67)	-0.199 (0.0987)**	44.90 (35.28)
Observations	17481	17393	17481	18890	17481	18890
Adjusted R^2	0.012	0.087	0.012	0.090	0.012	0.091
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	2,500	2,500	2,500	2,500	2,500

The dependent variables are corruption investigations per 1,000 inhabitants (standardized) and capital expenditures per capita. *Interaction* is a term which represents *Election year* (columns 1-2), *Mayor term limited* (columns 3-4), which is a dummy equal to one if the mayor is not eligible for reelection, and *Mayor high education* (columns 5-6), which is a dummy if she/he obtained a university degree. The specification includes municipality and year fixed effects, the distance from the population threshold and the characteristics of mayor and municipal councillors (age, education and gender). Robust standard errors clustered at the municipal level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of the DSP on corruption – 1,000 inhabitant threshold

	Corruption (PC)	Real estate tax rate	Current spending (PC)	Capital spending (PC)
	(1)	(2)	(3)	(4)
Stability pact ($S*T$)	0.00310 (0.0278)	0.00684 (0.00411)*	-4.448 (10.55)	-29.77 (29.48)
Observations	16450	18028	17704	17704
Adjusted R^2	0.005	0.554	0.140	0.025
City, Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	1,000	1,000	1,000	1,000

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

Table 8: Robustness checks

	Police spending	Diff-in-Disc	Excluding special regions	Gender quotas		Neighbour cities	Re-election probability
	Police spending (PC)	Corruption (PC)	Corruption (PC)	Corruption (PC)	Capital spending (PD)	Corruption (PC)	Incumbent re-elected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stability pact (S^*T)	-1.124 (0.769)	-0.151 (0.0698)**	-0.0960 (0.0431)**	-0.0807 (0.0369)**	-64.70 (13.48)***	-0.0275 (0.0479)	0.0267 (0.0718)
Observations	17,364	10,924	15,806	17,381	17,772	7,671	3,304
Adjusted R^2	0.027	0.049	0.011	0.011	0.70	0.011	0.255
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,500	1,707	2,500	2,500	2,500	-	2,500

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

Table 9: Effect of DSP on single corruption charges

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

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

Table 10: Effect of DSP in HFRs

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

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

Table 11: European funds

	(1)
	Corruption (PC)
Stability pact ($T*S$)	-0.0173 (0.00580)***
Post-reform (T)*EU funds	-0.0128 (0.0223)
Treatment group (S)*EU funds	-0.0317 (0.0245)
Stability pact ($T*S$)*EU funds	0.0528 (0.0269)*
Observations	23,881
Adjusted R^2	0.012
City, Year FE	Yes
Controls	Yes
Bandwidth	2,500

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

Table 12: Data sources

Data	Source
Corruption data	SDI (<i>Sistema d'indagine</i>) - Italian Ministry of the Interior
Municipal balance sheets	<i>Certificati consuntivi</i> - 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	Italian Ministry of the Economy https://www1.finanze.gov.it/finanze3/pagina_dichiarazioni/dichiarazioni.php
Data on local elections	<i>Archivio storico delle elezioni</i> - Italian Ministry of the Interior https://elezionistorico.interno.gov.it/
Data on local governments	<i>Anagrafe degli amministratori locali e regionali</i> - Italian Ministry of the Interior https://dait.interno.gov.it/elezioni/anagrafe-amministratori
Data on provincial welfare	<i>Eurostat</i> https://ec.europa.eu/eurostat/web/rural-development/data
Municipal services	<i>Certificati consuntivi</i> - Italian Ministry of the Interior https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4