Fighting Corruption with Fiscal Rules

Gianmarco Daniele[1], Tommaso Giommoni[1], and Tommaso Orlando[2]

[1]Bocconi University

[2]Bank of Italy

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PRELIMINARY AND INCOMPLETE: PLEASE DO NOT CIRCULATE.

Abstract

Can fiscal rules work as an anti-corruption tool? In this paper, we study how the introduction of a policy, i.e. the Domestic Stability Pact, that limits deficit accumulation may discipline local governments, leading to a reduction in corruption. We exploit its extension to Italian municipalities below 5,000 inhabitants after 2013, in a local Difference-in-Differences approach. Our findings show that the DSP leads to a substantial decrease in both corruption charges per capita and corruption charges over total capital expenditures. These effects are driven by a reduction in inefficient capital expenditures and they emerge only in areas in which the DSP has been fully enforced. We then estimate the effects on local welfare, finding a null effect in the short-medium term. Overall, these findings suggest that fiscal rules might lead local governments to efficiently curb expenditure, and in turn corruption.

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

1 Introduction

In the last decades, fiscal rules have been introduced by national and supra-national governments to promote fiscal sustainability, constraining politicians' spending bias by imposing the reduction of deficits and public debt. This paper focuses on a positive unintended effect of fiscal rules, showing that they might work as an effective anti-corruption policy tool. Corruption is considered a major obstacle to economic development (Rose-Ackerman, 1999), as well as a main cause of distrust towards democratic institutions (Aassve et al, 2018). While we collected a large knowledge on the mostly detrimental effects of corruption (Olken and Pande, 2012), we still miss an agreement on how best to fight it.

A common feature of anti-corruption policies is the provision of an anti-corruption authority, which, in turn, designs policies aimed at curbing corruption, as regulations to promote competition or transparency, or audits of bureaucrats' and politicians' performances. The implementation of such policies requires relevant costs, as investments in new technologies, auditors' wages and training, and the design of specific regulations, which usually involve additional costs for the monitored agents. A crucial advantage of our setting is the test of a policy, the Domestic Stability Pact (henceforth "DSP"; *Patto di stabilità interno*, in Italian), which reduces corruption without involving any additional implementation cost.

The DSP is a set of fiscal rules adopted by the Italian national government, which constrains public spending at the local level. The national government sets numerical limits on budgetary aggregates, as well as sanctions for local governments which overspend their target. Similar policies are common in decentralized countries, in which local governments receive transfers from the national government, and they may not entirely internalize the cost of spending. In the last decades, fiscal rules have become more and more common: this is due to the substantial increase in public debts which threatens fiscal sustainability of an increasingly number of countries. The IMF currently lists 96 countries, which have adopted local, national or supra-national fiscal rules (Lledó, Victor, et al.,2017).[1]

[1] According to the IMF, European Union countries are the most regulated ones with three or more fiscal rules in use (Lledó, Victor, et al.,2017).

Our test relies on the introduction of the DSP for Italian municipalities with a population below 5,000: while the DSP was already enforced for cities above 5,000 inhabitants, cities below this threshold entered the DSP in 2013. Using data in the period 2004-2015, we employ a Difference-in-Differences estimation to test whether the introduction of the DSP affects corruption charges and budgetary outcomes. However, as cities of different sizes are subject to distinct policies, we restrict the sample to municipalities with a population close to the 5,000 threshold. In other words, we employ a "local" version of a Difference-in-Differences, testing the robustness of our estimates on many population bandwidths.

Importantly, we do not expect the DSP to uniformly affect Italian municipalities. In our period of interest, European transfers dis-proportionally reached some Italian regions, that were eligible for the European Convergence funds, whose GDP per capita is below 75% of the EU average (hereafter called "high EU funds regions"). As these transfers were not included in the formula of the DSP, municipalities in these areas were substantially less constrained by fiscal rules.

In line with this framework, we find that fiscal rules decrease corruption charges only in municipalities receiving standard flows of European transfers (hereafter called "low EU funds regions"), in which the DSP was fully enforced. The effect is sizeable and corresponds to 22% of a standard deviation. Conversely, we do not observe any change in municipalities located in high EU funds regions, where fiscal rules do not constrain spending on public investments.

The drop in corruption charges is linked to budgetary changes. Municipalities in low EU funds regions reduce public investments (i.e. capital and public procurement expenditures), which represent discretionary types of spending more likely to be affected by mismanagement and rent seeking (Hessami, 2014; Mauro, 1998; Liu and Mikesell, 2014). We validate this channel showing that the DSP does not affect public investments in high EU funds municipalities. Moreover, the analysis on public procurement allows us to exclude that local politicians are strategically shifting expenditures towards discretionary types of procurement, in which it would be easier to hide corruption practices.

Our results are not due to a mechanical decrease in public expenditure. First, local politicians have a set of choices in order to respect the DSP. For instance, they might

decide to increase local taxes, as shown by Marattin et al. (2019) or reduce current expenditure. Instead, they cut the most likely rent-seeking affected areas, i.e. capital and procurement expenditures. In line with this interpretation, we also observe a decrease in corruption charges per Euro invested in capital expenditures, which highlights an improvement in the quality of local public expenditure. Accountability mechanisms might provide a rationalization of such findings: increasing local taxes might be relatively more costly than decreasing inefficient expenditures for career motivated politicians. We provide several tests in line with a political accountability explanation of our findings.

Overall, these findings might imply a trade-off between a beneficial effect of fiscal rules on rent-seeking and a drop in potentially welfare enhancing public investments. To investigate the overall welfare effects of fiscal rules, we test whether the introduction of the DSP affected local GDP, at the municipality and at the provincial level, in the next three-four years. Exploiting data on individuals' declared income, we do not find any change in local GDP after the introduction of the DSP, which suggests an efficiency gain for local public finance.

This paper combines two different strands of literature. First, we relate to the literature on anti-corruption policies, which has mostly focused on bureaucrats and politicians' audits. These studies focus on the local effects of anti-corruption audits promoted by the national government in Brazilian (Avis et al., 2016; Ferraz and Finan, 2011; Zamboni, et al., 2018; Colonnelli and Prem, 2017), Mexican (Larreguy et al., 2015) and Puerto Rican municipalities (Bobonis et al., 2016). This literature has also examined the effect of auditing public infrastructure in Argentina and Indonesia (Di Tella and Schargrodsky, 2003; Olken, 2007).

Second, we contribute to the literature on fiscal rules. Early studies focus on cross-country comparisons (Alesina et al., 1996; Hallerberg and Von Hagen, 1999; Debrun et al., 2008), while more recent ones exploit within country variation (Grembi et al., 2016; Coviello et al., 2016; Gamalerio, 2017). Overall, previous evaluations show that fiscal rules can be effective in reducing deficits, and to a less extend in curbing public debts (Heinemann et al., 2018).

The main contribution of this paper is to show that fiscal rules might turn into an effective anti-corruption tool: we suggest that, as the DSP does not involve additional

bureaucratic costs and there is little scope for manipulation, it represents a potentially cheap intervention to curb corruption. Highlighting the unintended positive effects of fiscal rules might be of interest for international organizations and governments enforcing budgetary constraints. Our findings are also timely relevant as we shade 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. Nevertheless, as our analysis of the welfare effects is limited to the short-medium run (up to four years after the intervention), we cannot consider potentially adverse long-run consequences.

This paper also relates to the literature analysing the impact of governmental transfers on local corruption and rent seeking. These studies show that transfers may amplify corruption at the local level: De Angelis et al. (2018) focus on Southern Italian regions and they show that large transfers from the European Union lead to an increase in corruption, Brollo et al. (2013) reach similar conclusions for Brazilian municipalities. Furthermore, Barone, Narciso (2015) document that the presence of organized crime raises the probability of obtaining funding and this ultimately leads to higher levels of corruption. We validate the idea that European funds might foster rent-seeking and we also show that these transfers may offset the beneficial effects of fiscal rules on corruption.

Furthermore, 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). For the best of our knowledge, this is the first paper causally estimating the effects of fiscal rules on economic development: our null findings support the idea of a trade-off for economic growth between higher public spending and corruption reduction. The fact that European funds deter 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 good-enough institutions.

The paper is organized as follows. Section 2 presents the institutional background and illustrates the policy. Section 3 describes the dataset. Section 4 discusses the empirical strategy and section 5 shows our results. Section 6 presents some robustness checks and Section 7 concludes.

2 Institutional background

Our analysis of the impact of fiscal rules on corruption is based on the so-called Domestic Stability Pact. The DSP consists of a set of budgetary policies that are in force, with variable scope and extension, between 1999 and 2015 (after 2015 fiscal rules at the local level have been relaxed). It represents the national counterpart to the EU-wide Stability and Growth Pact (which are operational since 1998 and 1999). The DSP aims at regulating the expenditure of local governments in Italy (regions, provinces, and municipalities), so to constraint national public spending within the bounds set by Italy's adherence to the European Stability and Growth Pact.[2]

In this paper, we focus on the effects of the DSP to municipalities, the smallest administrative units in Italy. Our identification of the effects of fiscal rules on corruption is based on the extension of DSP to municipalities with population in the 1,000-5,000 range, that occurred in 2013.[3] Before that, since 2001 only municipalities with more than 5,000 inhabitants had been subject to the policy Grembi et al. (2016).[4]

In the period we consider (2004-2014), the DSP imposes restrictions on accrual-based current expenditure and actual capital expenditure (for details, see Bonfatti and Forni, 2017, and Chiades and Mengotto, 2013). The DSP establishes, for each municipality and year, that the overall budget balance has to be proportional to a (moving) average of balances obtained in previous years in the same municipality.[5] The operative details of this rule (including exceptions for specific expenditure items and the way reference surpluses had to be computed) are subject to changes across years, but such changes are uniformly applied to all involved municipalities.

Lack of adherence to the financial limits imposed by the DSP results in a number of sanctions being imposed on municipalities. These include caps on programmed expendi-

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

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

[4] The DSP is uniformly applied to ordinary-statute regions (15 out ot 20), as well as to Sicily and Sardinia, which have limited autonomy in terms of public finance despite having a special statute. In fact, the three remaining special-statute regions also endorsed a form of the DSP, and both Friuli Venezia-Giulia and the autonomous province of Trento were subject to a similar extension of the lower population limit from 5,000 to 1,000 in 2013. Our main results are robust to variations of the sample that keep account of municipalities that were exempt from applying the DSP. Specifically, Table 16, in the Online Appendix, reports our main findings on corruption charges, excluding from the sample Italian special regions.

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

ture, decreased transfers from the central budget, limits to hiring and to the subscription of new debt contracts, and reductions of local politicians' wage. According to evidence recorded by the national government, non-compliance is limited to a few cases. Indeed, the overall public finance goals of the DSP have been attained in every region.[6]

Among specific items that are excluded from the restrictions imposed by the DSP, EU funds for investment programs play a particularly relevant role in our paper. Indeed, Italy is an important recipient of European funds, especially through the European Territorial Cooperation programs. In particular, six out of twenty Italian regions are included in the group of "convergence regions", with a GDP level which is lower than the 75% of the European average: in Italy "convergence regions" are located in the South and in the Islands and include Apulia, Basilicata, Calabria, Campania, Sardinia and Sicily. These high EU funds regions receive a disproportionately high amount of transfers, which in fact make DSP restrictions on capital expenditure less binding for their municipalities. Differently, low EU funds regions receive a standard amount of funds from the European Union. Importantly, the division between "high" and "low" EU funds regions is exogenously determined at the European level by the above mentioned GDP rule.

3 Data

3.1 Corruption data

This paper relies on three main data sources, dealing respectively with corruption, local public finance, and the procurement of public works. Table 1 shows descriptive statistics of the main variables of the dataset.

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

[6] For instance, the Ministry of the Interior, with a decree issued in September 2015, notifies that only 60 municipalities were to receive sanctions for non-compliance with the DSP.

of alleged offence.[7] Three important remarks apply to such data: first, investigations occur at the beginning of the prosecution process, so they represent alleged offenses rather than verified crimes; second, each investigation may involve several individuals in the role of (alleged) perpetrators, but we only observe the total number of investigations, rather than the number of people involved; third, if one investigation concerns alleged offenses 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.

Table 1: Descriptive statistics

	Average value	Standard deviation
Panel A: corruption charge		
Crime PC	.015	.174
Panel B: public finance		
Total fees	275.4	217.0
Total taxes	109.4	85.9
Total special taxes	66.0	128.4
Total fiscal revenues	452.7	313.7
Current expenditures	887.6	597.4
Capital expenditures	585.1	1,337.2
Total expenditures	1,751.5	1,906.1
Deficit	4,692.4	1,370,604
Debt	80.2	266.6
Panel C: local procurement		
Total amount	1,212.0	5,299.7
Average amount	640.0	3161.7
Number of tenders	.0006	.0019
Panel D: local politics		
Electoral turnout	.760	.0949139
Education of the mayor	4.314	.711
Age of the mayor	49.4	9.9
Gender of the mayor	.109	.312
Panel E: local GDP		
Municipal GDP	85,500	650,000
N	96,898	

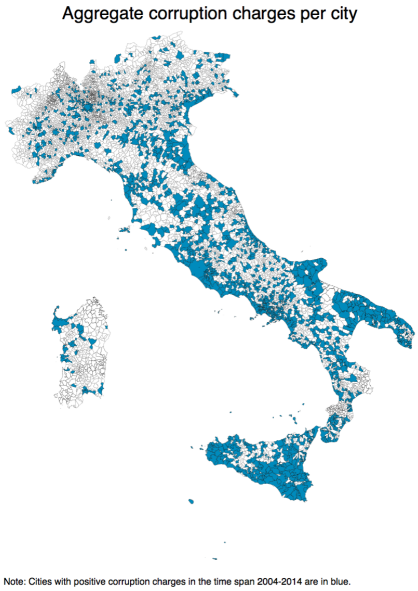
Note: Crime PC is expressed in per thousand terms, All amounts in Panel B and C are in per-capita terms and expressed in Euro. Amount in Panel C are divided over thousand. In Panel D education of the mayor varies in the range 1-6 (1 indicates no education and 6 master or PhD), Age is expressed in years and gender is a dummy equal to 1 in case of a female mayor. Amounts in Panel E are in Euros and are divided over thousand

In this paper we aggregate the number of crimes pertaining to corruptive phenom-

[7] SDI information has been used by several authors as a measure of the diffusion of corruption-related crimes in Italy: see, for instance, Giacomelli and Tonello, 2015, Mocetti and Orlando, 2017, and De Angelis et al., 2018. Unfortunately, SDI data are not publicly available after 2014.

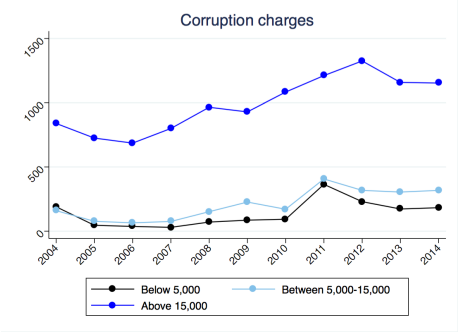
ena (i.e., bribery, graft, and malfeasance/resource embezzlement) to construct a time-varying index of corruption at the municipal level. The average number of corruption episodes reported in the SDI per municipality is 0.16. The average cumulative number of corruption episodes investigated between 2004 and 2014 per municipality is 1.81. As represented in Figure 1, more than three out of four municipalities display no corruption episodes in the period under consideration. Figure 1 highlights that corruption is spread across all Italian regions, although it appears more common in Southern municipalities. Overall, the average number of episodes recorded each year is around 1,300. Furthermore, the phenomenon affects cities of any size: Figure 2 plots the aggregate number of corruption crime for large, medium and small Italian cities over time and it shows that the phenomenon is widespread across municipalities with different dimensions.

Figure 1: Corruption crimes in Italian cities



Notes: The map shows Italian cities with at least one criminal charge registered in the SDI dataset (highlighted in light blue) in the time span in analysis, 2004-2014. The other cities in grey are those where no charges have been registered.

Figure 2: Evolution of corruption crime



Notes: The plot shows the aggregate amount of corruption charges registered in Italian cities over time, divided by population size: small cities (with population lower than 5,000 inhabitants), medium cities (with population between 5,000 and 15,000 inhabitants) and large cities (with population higher than 15,000 inhabitants).

3.2 Other municipal data

Data on municipal public finance come from the Italian Ministry of Internal Affairs (*Ministero dell'Interno*) which collects balance sheets of Italian municipalities (*Certificati consuntivi*). These data contain detailed information on revenues, expenditures, transfers, deficit and debt of cities in the time span 2004-2015. Moreover, we complement this dataset with information on European funds, drawn from the portal OpenCoesione.gov, an open government project managed by the Department for Cohesion Policy at the Presidency of the Council of Ministers.[8] OpenCoesione publishes data on all projects covered by the Cohesion Policy, financed by European Structural Funds with a national co-financing requirement. The data consist of the EU multi-year budget for 2007–2013, which includes funds that could be spent up to the end of 2015. Cohesion Funds are a crucial component of the EU's regional policy, which aims to reduce disparities in economic development across regions. The 2007–2013 budget is 347 billion euros, complemented by national and private co-financing of about 160 billion euros.[9] The total Italian expenditure certified to the EU was 46.2 billion euros.[10] As already discussed, "high EU funds" regions receive a dis-proportionately high amount of these transfers compared to "low EU funds" ones. This emerges clearly from Figure 3 which plots the aggregate amount of European funds spent by these two areas over time, in

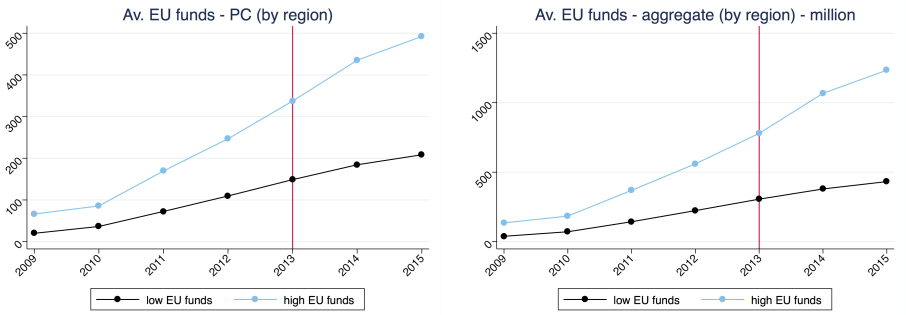
[8] See `www.opencoesione.gov.it/en/`

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

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

per-capita terms (left plot) and in aggregate terms (right plot): a region with low EU funds spend, on average, 228 millions of Euros per year, compared to the 618 millions spent by a region with high EU finds (these amounts correspond to a per-capita expenditure of 111.4 and 261.9 Euros in the two areas). Interestingly, the gap between the two areas widens after 2013. Moreover, Figure 4 shows the geographical distribution of transfers from the European Union (aggregate in the period 2007-2013) and similar conclusions emerge.

Figure 3: Evolution of European funds over time



Notes: These plots show the amounts of European funds used over time by low EU funds and high EU funds regions, per-capita (left plot) and in aggregate terms (rigth plot).

Figure 4: Distribution of European funds across regions



Notes: The map shows the aggregate amount of European funds spent by Italian regions in the time span 2007-2013.

Information on public procurement is drawn from data collected by Telemat, a private firm. Available data cover a large majority of public works contracts tendered by Italian municipalities between 2009 and 2016, but information is essentially limited to the award stage, starting when the tender is publicized and ending when the contract is assigned to one or more winning firms. Overall, around 115,000 tenders are recorded in the dataset. The median size (tender base price) is around 140,000 €. In this paper, we mainly use the number and size of tenders to inspect the local effects of DSP on municipal capital expenditure. The median value of public works tendered by Italian municipalities over the whole period 2009-2016 is close to 7 mln €. On average, each municipality managed 3.7 contracts per year, with an average total value of around 400,000 €. Around 18% of all contracts have base price smaller than 40,000 €, which is the threshold below which direct assignment of contracts (i.e., without any competitive tendering process) is allowed by law.

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 of Euros and the average variation is almost ten times larger. The source of this data is the Italian Ministry of Economics and Finance (*Ministero dell'Economia e delle Finanze*). Finally, we complement our analysis with political data on local elections and local politicians provided by the Italian Ministry of Interior.

4 Empirical analysis

4.1 Identification strategy

4.1.1 A local diff-in-diff

The goal of this paper is to estimate the causal impact of introducing the domestic stability pact on corruption in Italian cities. The ideal experiment would consist of adopting fiscal rules into a random set of municipalities, the treatment group, and of studying how corruption complaints evolve in these localities, compared to those which are not constrained.

To study this relation in our setting we exploit the enforcement of the DSP. First,

we cannot simply focus on the population threshold of 5,000 inhabitants, above which the policy applies until 2013, and compare cities that are subject to fiscal rules with those that are not. This comparison may, indeed, provide confounded estimates as there is another policy, the salary of the mayor, that changes sharply around this cutoff (Gagliarducci and Nannicini, 2013). Therefore, we rely on the change in the DSP taking place in 2013: this intervention reduced the population threshold including cities between 5,000 and 1,000 inhabitants, extending the coverage of these rules to 3,751 new cities. Hence, we exploit this policy intervention to test our hypothesis with a *local Differences-in-Differences* methodology. We compare cities around the 5,000 inhabitants threshold before and after 2013 and we limit the sample to cities in a neighbourhood of this population cutoff, in order to raise comparability between treatment and control group: such exercise tests for the introduction of fiscal rules in the treatment group that consists of cities with less than 5,000 inhabitants, which start being subject to the policy from 2013 on.

This *local Differences-in-Differences* allows us to exploit the change of the policy over time 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 cities in a neighbourhood of the 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[11].

4.1.2 Estimation assumptions

The key identifying assumption of this identification strategy requires that there are not other interventions, simultaneous to the DSP reform, affecting differently cities around the threshold. Moreover, trends in corruption between treatment 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 the results section.

[11] However, our results emerge also applying a Difference-in-Discontinuity technique. We report those findings among our robustness tests

The only other policy that changes at the 5,000 threshold in 2013 is the introduction of double preference voting conditioned on gender (Law 215/2012) in municipalities above the 5,000 threshold: voters can cast a vote for two candidates (instead of one), if they are of different gender. The aim of this policy is to increase the share of female politicians in local governments. However, in 2014, a reinforced version of this gender quota policy has been extended to municipalities above 3,000 inhabitants (Law 56/2014), which states that elected politicians of each gender cannot have less than 40% of municipal seats. In other words, for our purpose, only municipalities casting a vote in 2013 have been simultaneously treated with another policy at the 5,000 threshold, while from 2014, the gender quota threshold moves to 3,000. Therefore, in municipalities casting a vote in 2013, this policy could bias our results if female politicians have different attitudes towards corruption, as shown, for instance, by Brollo and Troiano, (2016) in the Brazilian context.

To reduce this possible bias, we control for the share of elected female politicians in all our specifications. As a stricter robustness test, in the Online Appendix (Table 17), we replicate our findings dropping from the sample all municipalities which had elections in 2013. Our findings are completely unaffected in both cases.

Lastly, this policy change allows, in principle, to study also the 1,000 threshold, as cities below this threshold were never included in the DSP. However, this is not feasible for two reasons: i) the diffusion of another policy, i.e. "unions of municipalties" (*Unioni di Comuni*) among very small municipalities, which implies that very small municipalities can jointly manage some of their powers without being subject to the DSP; ii) the very low number of corruption charges in municipalities around the 1,000 threshold, which limits the power of our analysis. We provide further explanations and these additional analysis in the robustness checks section.

4.1.3 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 could be that judges are now less willing to start investigations knowing that politicians are constrained by the DSP. This is unlikely as there is not any direct link between the DSP and anti-corruption policymaking: in this light, we have not been able to find any official document or media report linking the DSP to anti-corruption policymaking. Moreover, the DSP was already enforced before 2013 in many Italian cities, including the ones in which courts are located. Therefore, this hypothetical bias should have affected judges since the introduction of the DSP in 1999.

A second concern is that the DSP might lead politicians to decrease spending on local police, which, in turn, would have less resources to fight corruption: this concern is not plausible simply because the municipal police is not in charge of anti-corruption investigations. Moreover, in a robustness test, we show that mayors have not decreased expenditures on local police following the DSP introduction.

Third, some companies losing business due to the decreased spending on public procurement (linked to the introduction of the DSP) might react pressing charges against their competitors. To avoid this additional possible bias we not include in our analysis corruption investigations related to collusion among companies. Second, 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 our careful analysis of the municipal budget changes, which represents, according to us, the mechanism actually explaining the effects of the DSP on corruption.

4.2 Specification

The dependent variable of the analysis varies at the municipal/year level. The set of dependent variables include the measure of criminal complaints, the set of public finance and procurement indicators. The estimated empirical model is as follows:

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

Where y_{it} is the dependent variable (for instance the indicator of criminal complaints) in city i, in year t. S_{it} indicates the treatment group, dummy indicating cities below

the 5,000 inhabitants threshold (with population between 1,000 and 5,000), and it relies on two available population census (conducted in 2001 and 2011), T_t indicates the post reform period, a dummy equal to one after 2013. $P_i^* = P_i - P_c$ captures the distance from the population threshold, $P_c = 5,000$, for city i: the population of city i, P_i, is the forcing variable of the analysis and it is based on the value recorded in the last census of 2011. The fact that the forcing variable 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 Diff-in-Diff 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[12]. Moreover, we control for the distance to the 5,000 threshold differential between pre/post reform period, $P_i^* \cdot T_t$.

X_{it}, is a vector of controls including age, education and gender of the mayor, share of female politicians and city population. Moreover 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 cities included in the analysis is restricted to those in a distance h to the 5,000 threshold, $P_i \in [P_c - h; P_c + h]$. We do not arbitrarily select h, we rather test the sensitivity of our results using multiple bandwidths h, in the vein of the Regression Discontinuity Design methodology.

5 Results

5.1 Effect on corruption complaints

In this section we study the impact of introducing fiscal rules on corruption at the local level, measured with the number of criminal complaints, expressed in per-capita terms per thousand and standardized[13]. As already discussed in section 3, this policy is likely to have a different impact in regions receiving a dis-proportionately high amount of

[12] We estimate an alternative empirical model in order to study the dynamic effect of the treatment and to evaluate pre-trends. The model is as follows:

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

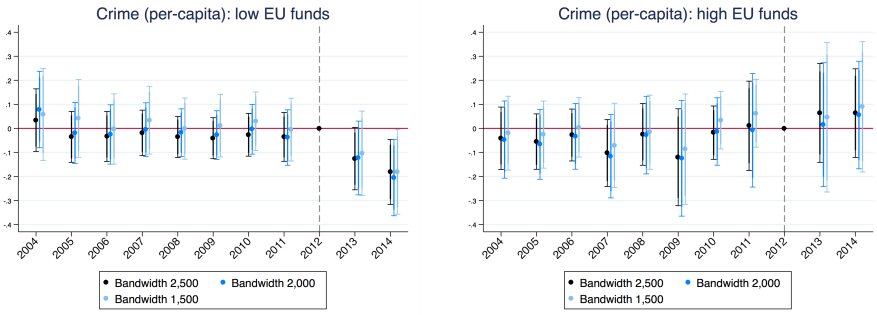
The local Diff-in-Diff estimator is the interaction term $S_{it} \cdot \delta_t$ which compares treated and control cities every year, using 2012, the last year before the reform, as the benchmark year. All the other terms are similar to model 1.

[13] In particular, the indicator for corruption has been rescaled to have a mean of zero and a standard deviation of one.

European funds, whose GDP is lower than the 75% of the European average. For this reason we conduct all the analyses differentiating between regions receiving the standard amount of European funds, "low EU funds" regions, and those obtaining large amounts, "high EU funds" regions.

Figure 5 shows the main graphical outcomes for equation 2. From Figure 5 emerges that corruption significantly decreases in the group of cities where the DSP is extended in 2013. However, the effect only emerges in those regions that receive low amounts of European funds, while there is no clear impact for regions receiving high amounts of these funds. On the one hand, the sub-figure on the left shows the effect of the policy for municipalities in low EU funds regions: corruption consistently drops in the group of treated cities after the extension of the policy in 2013. The average reduction is sizeable as it corresponds to a drop between 0.15 and 0.22 dependent variable standard deviations. Moreover, there are no differences between treatment and control groups prior 2013, which suggests that local trends in corruption are parallel before 2013, excluding the incidence of pre-trends in corruption complaints. On the other hand, the sub-figure on the right focuses on high EU funds regions and it shows that the DSP does not have any impact on the level of corruption complaints in this area, as this variable is never different between treated and control municipalities. Furthermore, the graph presents the main outcomes with three different population bandwidths: 1,500, 2,000 and 2,500 inhabitants. The results are very similar across the three bandwidths.

Figure 5: Effect of the DSP on criminal complaints



Notes: This plot shows the outputs of the local Diff-in-Diff estimation, according to the model 2. The dependent variable is the number of criminal complaints, expressed in per-capita terms and standardized. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds.

As a second analysis we study how this result is sensitive to the choice of the population bandwidth: in particular, we plot the Difference-in-Differences coefficient, according to model 1, varying the population bandwidth in the range 1,000-4,000.[14] Left plot of Figure 6 shows the results for regions with low EU funds. The effect of the policy turns out to be always negative and sizeable, moreover the magnitude of the coefficient is rather stable as the sample widens and the effect is almost everywhere statistically significant. This output suggests that this relation does not depend on the sample of cities included and it is robust to many different population bandwidths. Moreover, right plot of Figure 6 shows the same relation for high EU funds regions confirming that fiscal rules do not impact on corruption in this area[15].

To validate the mediating role of the European funds, we consider the entire sample and we analyse the differential impact of fiscal rules depending on the amount of funds received by the region where each city is located.[16] Table 2 shows the results of this triple-differences analysis (also in the case, we run this test using a 2,000 inhabitants bandwidth): *Stability pact* captures the diff-in-diff estimator while the interaction term *Stability pact*EU funds*, the triple-diff estimator, represents the differential impact of the policy depending on transfers received by each region. European funds are measured as the total amount of regional transfers, per-capita, spent in the time span 2009-2015 (column 1) and in the time span 2013-2015 (column 2).[17]

Results are similar in both columns. On the one hand, the diff-in-diff indicator is always negative and significant, confirming that the introduction of the DSP reduces corruption. On the other hand, the amount of funds spent locally seems to modify the effect of the policy: the beneficial impact of DSP on corruption weakens as regional transfers raises. This result suggests that European transfers offset the positive impact of fiscal rules on criminal complaints and may, under specific circumstances, promote

[14] In table 1, in the Online Appendix, we report the full results of one these tests, using a 2,000 inhabitants bandwidth.

[15] Furthermore, in Table 13 instead of using the standard corruption measure as dependent variable, we look at the three main types of corruption charges included in our dataset, such as graft (*concussione*), malfeasance (*abuso d'ufficio*) and embezzlement (*peculato*). Our findings are mostly driven by a reduction in malfeasance charges.

[16] We prefer the regional to the municipal allocation, as EU funds allocation at the city level would be endogenous: each region is in charge of allocating funds among local governments and mayors in cities with the DSP might have differential incentives to apply for EU funds.

[17] Note that we are focusing on the date in which EU funds are spent. Conversely, all EU funds in our analysis, i.e. those belonging to the 2007-2013 multi-year budget, were assigned to each region by the end of 2013.

corruption. This output is coherent with findings of De Angelis et al. (2018) showing that transfers from the European Union increase corruption in Southern Italy, and it relates to the literature studying the link between governmental transfers and the incidence of corruption (Brollo et al. 2013, Barone, Narciso 2015).

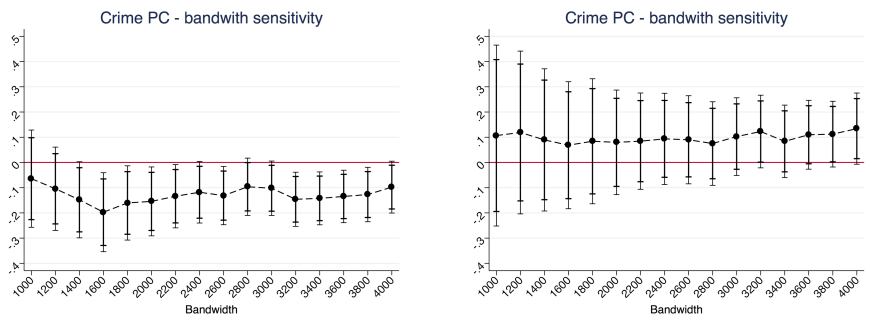
Table 2: European funds

Dep. Var.: Crime (PC)	EU funds PC	EU funds PC - post2013
	(1)	(2)
Stability pact	-0.0177	-0.0184
	(0.00976)*	(0.00973)*
Stability pact*EU funds (euro PC)	0.0000112	0.0000156
	(0.00000621)*	(0.00000824)*
Observations	18,506	18,506
Adjusted R^2	0.010	0.010
City, Year FE	Yes	Yes
Controls	Yes	Yes
Bandwidth	2,000	2,000

Notes: The dependent variable is criminal complaints per-capita. The sample includes all Italian municipalities. The interaction term variables are defined as follows: "EU funds PC" measures the total amount of regional European funds spent in the time span 2009-2015 in column (1) and the total amount spent starting from 2013. Both these variables are expressed in Euro per-capita. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

Figure 6: Effect of the DSP on criminal complaints - sensitivity analysis [low EU funds on the left/high EU funds on the right]



Notes: This plot shows the sensitivity analysis of the local Diff-in-Diff, according to the model 1. The dependent variable is the number of criminal complaints, expressed in per-capita terms and standardized. Each point represents the local Diff-in-Diff estimator for a distinct analysis conducted with the corresponding population bandwidth.

5.2 Effect on local public finance

In this section we study the impact of the DSP on local public finance. The main aim of this analysis is to explain why corruption is decreasing: we show that cities experiencing a drop in corruption are simultaneously decreasing capital expenditures. Figure 7 includes the outputs of this analysis (equation 2), all these variables are expressed in per-capita terms and the outputs are presented with three different bandwidths: 1,500, 2,000 and 2,500 inhabitants (all these results for the bandwidth 2,000 are reported in Table 14 in the Online Appendix).

The first two sub-figures study the impact on total public expenditure: the introduction of fiscal rules leads to an immediate and consistent reduction in local spending. The effect is visible in both groups of regions but the reduction is only statistically significant in regions with low EU funds, where the average effect corresponds to a reduction of 6.5% of the dependent variable standard deviation.

The sub-figures in the second and third rows of Figure 7 show the impact of fiscal rules on current and capital expenditures. On the one hand, fiscal rules reduce current expenditures without consistent differences between the two groups of regions, but the yearly coefficients are not statistically different from zero. On the other hand, there is a large reduction in capital expenditures, only in regions with low EU funds: the average reduction in this area corresponds to a 17% of the dependent variable standard

deviation. As explained in section 2, the lack of a significant reduction in investments in high EU funds regions is due to the sizeable European transfers received by these cities, which are exempted from the DSP. Cities located in these regions, indeed, are less constrained by fiscal rules in terms of investment spending and they can afford to maintain per capita expenditures almost unchanged.

This result suggests that the differential effect of fiscal rules on capital expenditures may explain why corruption drops only in low EU funds regions: cities in these areas experience a drop in corruption driven by the reduction in capital expenditures, differently from municipalities in high EU funds regions, which do not cut public investments and do not experience any change in corruption complaints. This interpretation is, indeed, coherent with the large empirical literature suggesting that capital spending is the component of the budget where corruption is more likely to take place (Hessami 2014).

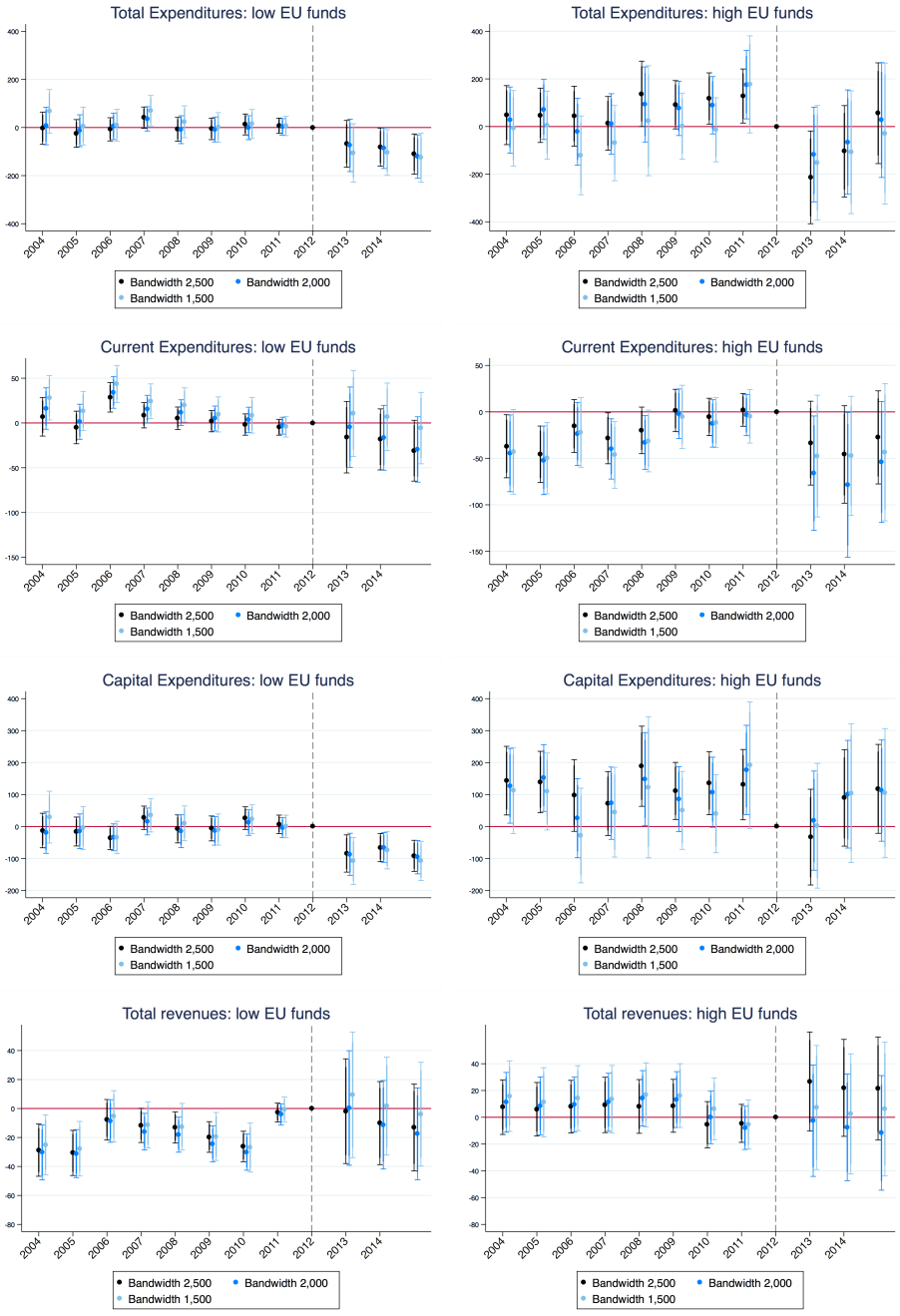
Moreover, the figures in the fourth row show that the impact on total fiscal revenues is rather weak for both groups of cities. These outcomes suggest that the immediate impact of fiscal rules is a reduction in the level of spending, rather than an increase in fiscal revenues and this may be due to the high political cost of raising taxes that local governments face[18].

Finally, Figure 8 shows the Difference-in-Differences coefficient for this set of public finance variables according to equation 1, varying the population bandwidth in the range 1,000-4,000 inhabitants: the main results of this analysis are robust to many different bandwidth specifications and the coefficient is rather stable as the sample widens.[19]

[18] We also study the impact of the DSP on other important components of the budget such as the local surplus and public debt. On the one hand, the main results suggest that this policy did not affect significantly the amount of surplus accumulated, on the other hand there is weak evidence that the DSP leads to a reduction of local debt. Graphical outcomes are shown in Figure , contained in the Online Appendix 2.

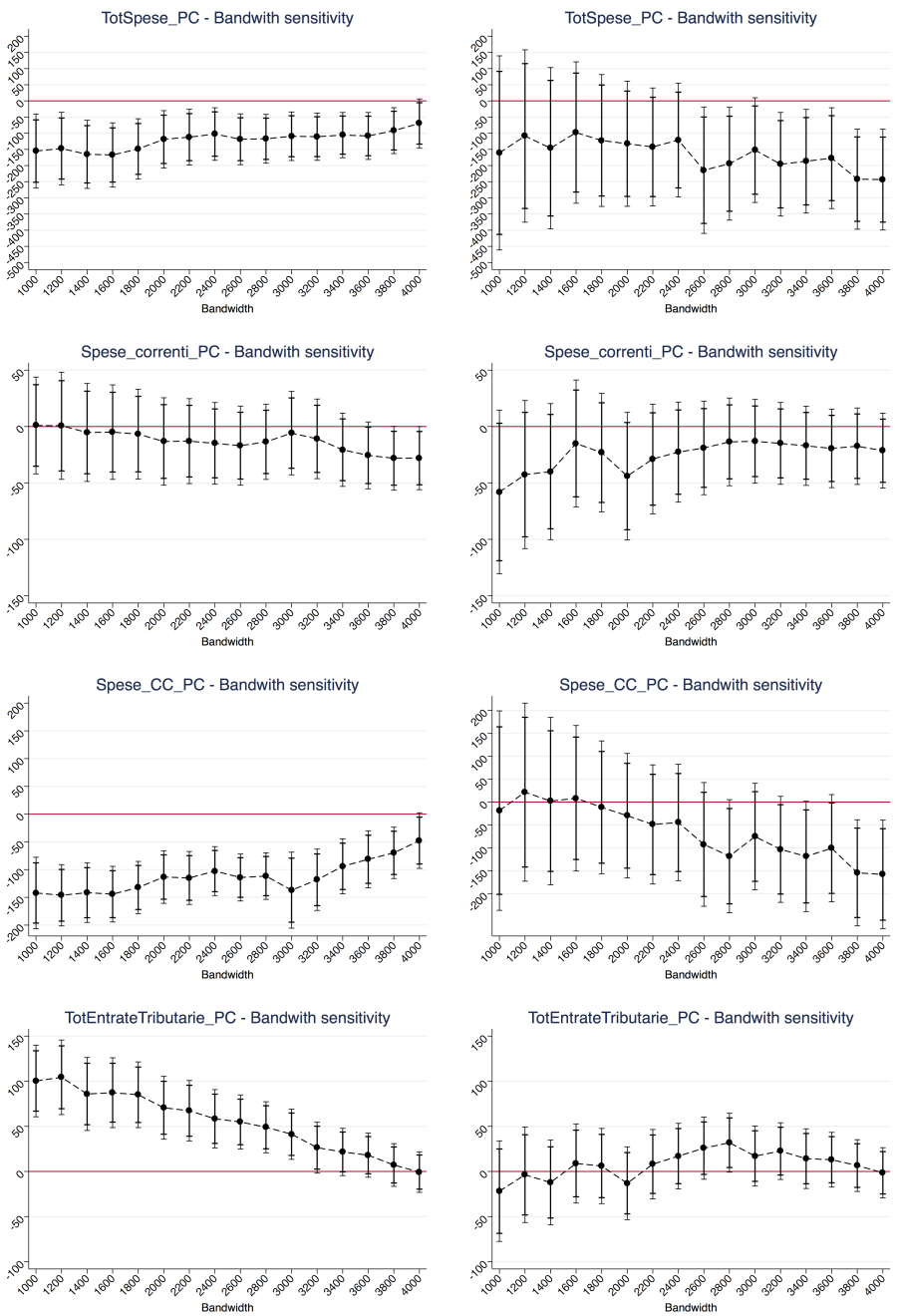
[19] Figure 8 shows an increase in local revenues among cities in low EU funds regions. However, as visible in 7, this effect seems to be driven by a positive pre-trend in this variable starting in 2010.

Figure 7: Effect of the DSP on local public finance



Notes: This plot shows the outputs of the local Diff-in-Diff estimation, according to the model 2. The dependent variables include the main public finance indicators, expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds.

Figure 8: Effect of the DSP on local public finance - sensitivity analysis [low EU funds on the left/high EU funds on the right]



Notes: This plot shows the sensitivity analysis of the local Diff-in-Diff, according to the model 1. The dependent variables include the main public finance indicators, expressed in per-capita terms. Each point represents the local Diff-in-Diff estimator for a distinct analysis conducted with the corresponding population bandwidth.

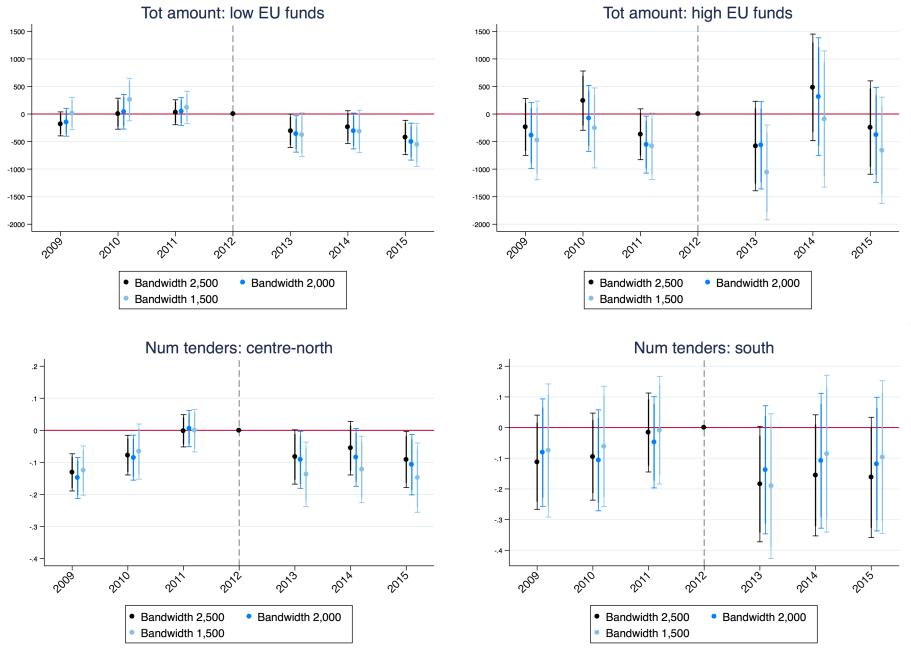
5.3 Effect on public procurement

In this section, we focus on procurement outcomes to provide further evidence that the reduction in corruption is mainly driven by the drop in public investments in low EU funds regions. Procurement expenditure is an important component of local public spending, in which local politicians have large discretionary powers: this explains why this part of the budget may be an important source of corruption and rent-seeking.

Figure 9 shows main results for this analysis. As in previous analysis, all the variables are expressed in pre-capita terms. The first row shows the impact of fiscal rules on the total amount spent in procurement every year, in the two groups of regions: consistently with previous results, the impact of the policy is an immediate and persistent drop in procurement expenditures only in low EU funds regions. This result is in line with our findings on capital expenditures, suggesting that cities in this area react to the policy reducing discretionary public investments. This effect is sizeable: the average reduction induced by the policy corresponds to a 8.3% drop, compared to the dependent variable standard deviation. The second row shows the impact of fiscal rules on the number of tenders opened every year. The policy leads to a weak reduction in both areas of the country.

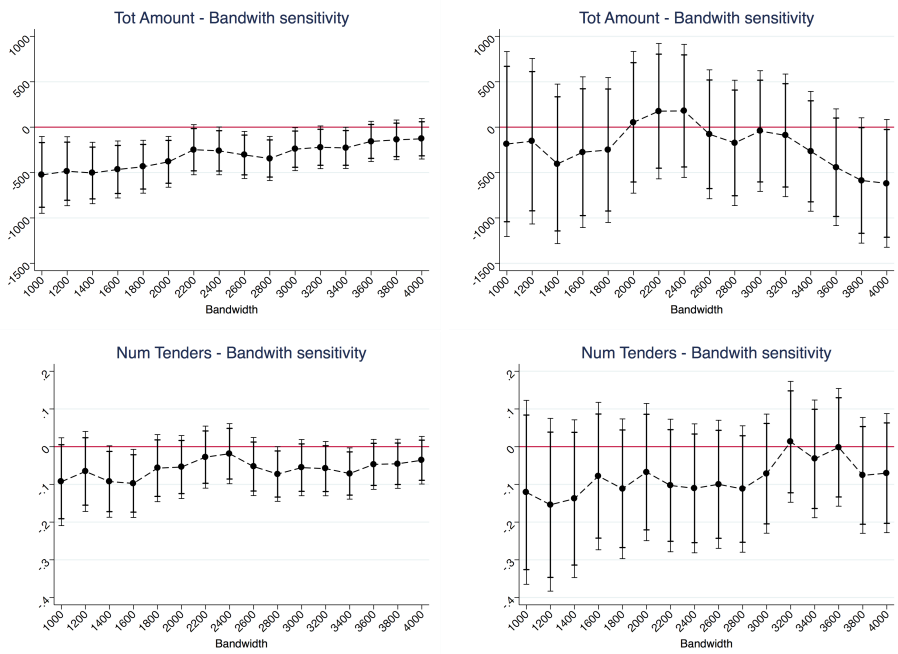
As in the previous analyses, Figure 10 plots the Difference-in-Differences coefficients for different bandwidth specifications in the range 1,000-4,000: the main results are confirmed.

Figure 9: Effect of the DSP on public procurement



Notes: This plot shows the outputs of the local Diff-in-Diff estimation, according to the model 2. The dependent variables include the main procurement indicators, expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds.

Figure 10: Effect of the DSP on public procurement - sensitivity analysis [low EU funds on the left/high EU funds on the right]



Notes: This plot shows the sensitivity analysis of the local Diff-in-Diff, according to the model 1. The dependent variables include the main procurement indicators, expressed in per-capita terms. Each point represents the local Diff-in-Diff estimator for a distinct analysis conducted with the corresponding population bandwidth.

5.4 Impact on quality 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: politicians cut capital expenditures, i.e. discretionary spending, which are more affected by corruption. A further question is whether within capital expenditures, they are specifically targeting the most inefficient types of spending. In other words, a drop in corruption may mechanically be due to the investments cuts or, alternatively, it may also be due to politicians reducing the most inefficient public investments.

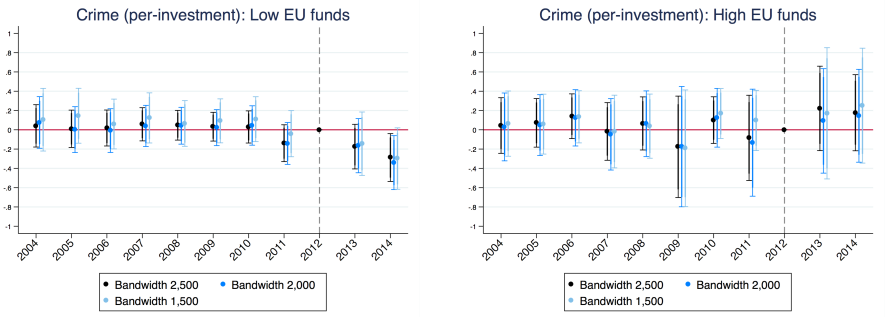
To test this hypothesis, we replicate our main analysis introducing as a dependent variable the ratio between the standardized number of corruption charges (per-capita) and the yearly spending on capital expenditures (expressed in logarithm). Figures 11 and 12 display this test. We find a decrease in corruption per Euro invested on capital

expenditures in low EU-funds areas, while a similar trend does not arise for high EU-funds areas. This implies that the Domestic Stability Pact leads to an improvement in the quality of public spending, in terms of the share of capital spending affected by corruption. In other words, this test suggests that local politicians are not just reducing public investments, indeed they are cutting the least efficient ones.

An alternative explanation could be that politicians are strategically shifting rent-seeking to spending areas less likely to be observed by law enforcement officials: a displacement effect might then explain the above findings. First, this explanation should be at work both in high and low EU-funds areas as the DSP is officially enforced in both areas. Conversely, we observe a decrease in corruption only in low EU-funds areas. Therefore, such displacement effect is difficult to reconcile with our findings.

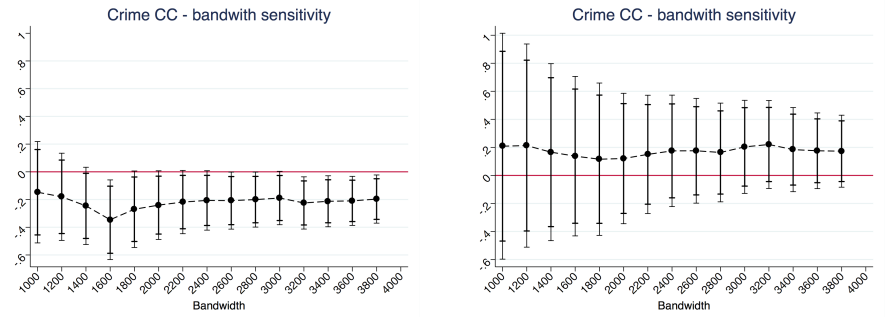
Second, we directly test for a plausible strategy to displace rent-seeking, looking at the share of public procurement assigned below 40,000 Euros. This is an important threshold as below this amount, politicians can direct assign contracts, which is a less transparent and less competitive procedure. Table 3 replicates our main estimation introducing as dependent variables: i) the percentage of tenders with an amount lower than 40,000 Euros; ii) the percentage of total amount in tenders whose amount is smaller than 40,000 Euros iii) and the overall tendered amount (per-capita) lower than 40,000 Euros. Overall, we find a decrease in the total tendered amount below this threshold, while we do not find a significant decrease in the number and the percentage of total amount of tenders below 40,000 Euros. These findings show that politicians are not resorting to less transparent tenders to potentially hide rent-seeking.

Figure 11: Effect of the DSP on criminal complaints over investments



Notes: This plot shows the outputs of the local Diff-in-Diff estimation, according to the model 2. The dependent variable is the number of criminal complaints, expressed per Euro invested and standardized. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds.

Figure 12: Effect of the DSP on criminal complaints over investments - sensitivity analysis [low EU funds on the left/high EU funds on the right]



Notes: This plot shows the sensitivity analysis of the local Diff-in-Diff, according to the model 1. The dependent variable is the number of criminal complaints, expressed per Euro invested and standardized. Each point represents the local Diff-in-Diff estimator for a distinct analysis conducted with the corresponding population bandwidth.

5.5 Impact on local welfare

5.5.1 Municipal GDP

Our analysis suggests that fiscal rules might tackle corruption by reducing inefficient capital expenditures. Nevertheless, a substantial reduction in public investments is likely to deter local economic growth. In an attempt to test the overall welfare effect of fiscal rules, we test whether the Domestic Stability Pact affects the municipal GDP, which is

Table 3: Modification in the distribution of tenders amount

	Perc. tenders <40K	Perc. amount <40K	Amount PC <40K
	(1)	(2)	(3)
Stability pact	-0.0159	-0.00689	-6.015
	(0.0211)	(0.0196)	(2.355)**
Observations	8,291	8,291	8,291
Adjusted R^2	0.003	0.003	0.005
Entity, Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Bandwidth	2,000	2,000	2,000

Notes: The dependent variables are as follows: "Perc. tenders <40K" captures the percentage of tenders with an amount lower than 40,000 Euros, "Perc. amount <40K" measures the percentage of total amount in tenders whose amount is smaller than 40,000 Euros, "Amount PC <40K" captures the overall tendered amount (per-capita) lower than 40,000 Euros. The sample in analysis only includes Low EU funds regions. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

measured by individuals income, as declared to the Italian fiscal agency, expressed in per-capita terms. Table 4 reports these estimates: the four panels report the effect of the DSP on local GDP in the same year (columns 1 and 2), in the next year (columns 3 and 4), in the next two years (columns 5 and 6) and in the next three years (columns 7 and 8). Overall, we find a null effect of fiscal rules on local GDP both in low and high EU funds areas. On the one hand, a reduction in local public investments might deter economic growth; on the other hand, a drop in inefficient spending and rent-seeking might have the reverse effect: the two effects seem to cancel each other out. An important limitation of this analysis is that we are not able to track the long term growth effects as data on local GDP stop in 2017.

As a complementary test, we investigate the effect of the DSP on inequality. Our data include individuals income grouped into seven brackets. Lack of individual data prevents us from calculate a Gini index. Instead, we look at the difference between the average income declared in the top income bracket and the bottom income brackets. The top bracket includes taxpayers with an income between 75,000-120,000 Euros[20], the two bottom income brackets include taxpayers with an income between 0-10,000 and 0-15,000 Euros. We report these test in Table 5. We do not find any significant effect of fiscal rules on this measure of inequality, which implies that, on average, income differences between top and bottom earners have not changed.

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

Table 4: Effect of the DSP on municipal GDP

Dep. var.: local GDP per-capita	Contemporaneous		After one year		After two years		After three years	
	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stability pact	233.4	-10.09	185.3	-14.78	-35.63	11.52	47.65	50.27
	(193.5)	(56.98)	(360.7)	(52.83)	(134.7)	(52.39)	(105.0)	(62.74)
Observations	14,423	5,688	14,394	5,688	14,362	5,688	13,136	5,234
Adjusted R^2	0.299	0.886	0.282	0.912	0.246	0.909	0.592	0.887
Entity, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000	2,000	2,000

Notes: The dependent variable is the local GDP measured as the aggregate income declared, expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of the DSP on municipal inequality

	Difference in av. revenues between last and first bracket (in logarithm)			
	first bracket 0-15,000		first bracket 0-10,000	
	LFRs	HRFs	LFRs	HFRs
	(1)	(2)	(3)	(4)
Stability pact	0.00326	0.000756	0.00369	0.000594
	(0.0120)	(0.0270)	(0.0123)	(0.0274)
Observations	14408	5148	14408	5148
Adjusted R^2	0.013	0.014	0.013	0.014
Entity, year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwith	2,000	2,000	2,000	2,000

Notes: The dependent variable is the difference between the average income declared in the last income bracket and the first income bracket. The last bracket included taxpayers with an income between 75,000-120,000 Euros, the first income bracket includes taxpayers with an income between 0-15,000 Euros (columns 1-2) and 0-10,000 Euros (columns 3-4). LFRs regions are those that receive standard funds from the European Union, HFRs regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.5.2 Province Welfare

As a complementary test, we consider provinces, instead of municipalities, as a unit of analysis. Specifically, we can exploit the fact that after 2013, there is an increase in the share of municipalities included in the DSP across all 110 Italian provinces. Such increase is heterogeneous across provinces, as each province has a different share of municipalities with population between 1,000 and 5,000 inhabitants.[21] Therefore, to

[21] The variation in the share of municipalities with population between 1,000 and 5,000 inhabitants is massive: there are provinces with a very high share of municipalities with population in this range (*e.g.*

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 cities exposed to the DSP across provinces.

First, we create a dummy *Post 2013*, equals to 1 after 2013, and a continuous time invariant variable (*Share*), going from 0 to 1, which measures the share of municipalities between 1,000 and 5,000 inhabitants. We conduct the standard (local) Difference-in-Difference analysis studying the interaction between the indicators *Post 2013* and *Share*. We consider some macro-level indicators, which are available at the provincial level, i.e. GDP, the total number of employed individuals (in log), the total number of firms and of new patents. We are interested in the interaction term *Share*Post 2013*, which captures the impact of the adoption of fiscal rules on those provincial level outcomes.

This specification allows testing for geographical spill-overs of the DSP, as the effects on welfare might spread to the local economy beyond the municipal borders. This is likely to be the case, as for instance, firms applying to public procurement in a municipality are likely to be based in the same province but not necessarily in the same municipality.

Table 6 reports this test. Also in the case, we do not find an effect of fiscal rules on different measures of local welfare.

Terni, with 78% of municipalities in this interval), which are strongly affected by the 2013 law change; conversely, there are provinces with a very low share of municipalities involved in the policy change (*e.g.* Barletta-Andria-Trani with 0% of municipalities in this interval), which are weakly affected or not affected at all by this policy. Figure 16 plots the distribution of the fraction of treated cities for Italian provinces, showing the large heterogeneity in this measure.

Table 6: Effect of the DSP on province Economic indicators

	Aggregate		PC	
	LFRs	HFRs	LFRs	HFRs
	(1)	(2)	(3)	(4)
Panel A: Municipal GDP				
Stability pact	-738.8	286.1	-0.000538	0.000985
	(802.4)	(305.7)	(0.00130)	(0.000874)
Observations	900	420	876	360
Adjusted R^2	0.291	0.263	0.339	0.327
Panel B: Employment (per thousands)				
Stability pact	-6.735	7.610	-0.0154	0.00161
	(6.919)	(15.13)	(0.0143)	(0.0112)
Observations	876	420	852	360
Adjusted R^2	0.324	0.315	0.440	0.593
Panel C: Number fo firms				
Stability pact	83.26	-246.9	0.00121	0.0000546
	(623.0)	(363.0)	(0.00131)	(0.00101)
Observations	600	280	584	240
Adjusted R^2	0.361	0.465	0.692	0.715
Panel D: Number of p atents				
Stability pact	8.552	1.649	0.0000444	0.0000113
	(10.39)	(1.710)	(0.0000410)	(0.00000929)
Observations	843	227	819	209
Adjusted R^2	0.019	0.089	0.000	0.081
Entity, year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,000	2,000	2,000	2,000

Notes: This analysis is conducted at the province level. The treatment is the fraction of cities in a province with population between 1,000 and 5,000 after 2013. The dependent variables are drawn from Eurostat. LFRs regions are those that receive standard funds from the European Union, HFRs regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes province and year fixed effects, average characteristics of mayors in the province (age, education and gender) and total population. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.6 Mechanism

5.6.1 Accountability and monitoring

As anticipated in the introduction, a plausible explanation for why local politicians are keen to reduce rent-seeking is political accountability. Instead of reducing inefficient spending, they might, for instance, respect the DSP by increasing local taxes, as shown

by Marattin et al. (2019). To validate the accountability channel, we provide some suggestive evidence based on electoral incentives.

First, we look at whether local politicians are more likely to reduce corruption during the electoral year. Second, we exploit the fact that Italian mayors have a two-terms limit to detect whether not-term limit mayors react differently, as they are eligible for re-election. Table 7 shows that mayors are more likely to reduce corruption during electoral years. However, they are not more likely to reduce capital expenditures in electoral periods. This interesting result suggests that, in electoral years, mayors aim at reducing corruption without additional cuts to public spending. The 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 the literature (Repetto 2018, Giommoni 2019). Conversely, we do not find a differential behavior between first and second term mayors. Finally, in columns 5 and 6, we test an additional channel, i.e. politicians' ability. Previous studies provide some evidence in line with the idea that competence matters also in the political arena. Specifically, we differentiate between mayors holding/not holding a university degree. Column 5 shows that educated mayors are much more likely to reduce corruption. Conversely, they do not reduce capital expenditures more than other mayors. This suggests that they might be relatively more efficient, as they are able to reduce corruption more than other mayors, without additionally cutting public investments[22].

[22] An additional test that we perform is whether mayors in treated cities, where fiscal rules are adopted, face any modifications in their probability to be re-elected which may change as a consequence of lower levels of corruption. To conduct this analysis we limit the sample to electoral years and we exclude term limited mayors, that cannot run again for the office. Main results are contained in Table 18, in the Online Appendix, and suggest that the policy does not have any significant impact on re-election probability of the incumbent, despite the positive sign for cities receiving low EU funds and the negative sign for the other group of cities.

Table 7: Accountability Vs. Ability

Sample: Low EU funds regions	Election year		Mayor term limited		Mayor high education	
Dep. var.:	Crime PC	Capital exp. PC	Crime PC	Capital exp. PC	Crime PC	Capital exp. PC
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact	-0.00778	-82.96	-0.0135	-74.08	0.0110	-121.7
	(0.00534)	(26.57)***	(0.00580)**	(24.39)***	(0.00948)	(43.97)***
Stability pact*interaction	-0.0133	21.10	0.00553	-5.657	-0.0250	50.18
	(0.00531)**	(38.38)	(0.00529)	(24.97)	(0.00988)**	(44.44)
Observations	13,268	13,210	13,268	14,347	13,268	14,347
Adjusted R^2	0.010	0.078	0.010	0.080	0.011	0.080
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the number of criminal complaints (expressed in per-capita terms, standardized) and capital expenditures per-capita. The sample in analysis only includes Low EU funds regions. Election year captures the years when a local election takes place, term limit is a dummy equal to one when the mayor is term limited and a mayor is coded as highly educated if she/he obtained at least a university certificate. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.6.2 Intensity in the application of the DSP

As explained in Section 2, the DSP was not uniformly applied to all municipalities, as the exact target was determined by a formula taking into account the historical levels of public spending (see Section 2 for more details). Intuitively, we should expect a stronger effect of the DSP in cities subject to more stringent fiscal rules. In Table 8, we take into account the median level of surplus set as DSP target, and we differentiate between cities below/above this median value. In line with our expectations, we find a stronger decrease in capital expenditures in low EU-funds areas (columns 1 and 2), especially for cities ruled by more stringent fiscal rules (column 2). In columns 4 to 8, we focus on current expenditures: in this case, we observe a decrease in current expenditures for cities with more stringent fiscal rules both in low and high EU-funds areas. This is in line with our main findings, which highlight a general decrease in current expenditures both in low and high EU-funds regions.

In line with our expectations, we find a stronger decrease in corruption for cities in low EU-funds areas ruled by more stringent DSP targets. A complementary approach would be to look at municipalities, which are actually respecting the DSP. In this case, we document a strong decrease in corruption driven by municipalities in low EU-areas,

which are complying with the DSP.

In Table 9, we replicate a similar analysis focusing on corruption charges. Also in this case, our effects are driven by cities in low-EU funds regions experiencing more stringent fiscal rules (columns 1 to 4). In the right panel of Table 9, we run a complementary test, in which we split cities depending on whether they comply with the DSP: the drop in corruption is completely driven by cities in low EU-funds areas complying with fiscal rules.

Finally, in line with the idea that stricter fiscal rules push politicians to improve the municipal surplus, Figure 13 shows the correlation at the city level between the fiscal rules target (horizontal axis) and the realized surplus (vertical axis).

Overall, this set of specifications highlights that i) stricter fiscal rules lead to stronger changes in public spending, and in turn, in corruption levels; ii) fiscal rules compliance is crucial to then expect an effect on budgetary and rent seeking outcomes.

Table 8: Stability pact surplus goal - Expenditures

Dep. var.:	Capital expenditures (PC)				Current expenditures (PC)			
	Low EU funds		High EU funds		Low EU funds		High EU funds	
	Low surplus (Below median)	High surplus (Above median)	Low surplus (Below median)	High surplus (Above median)	Low surplus (Below median)	High surplus (Above median)	Low surplus (Below median)	High surplus (Above median)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stability pact	-58.78	-158.0	56.63	-111.4	-9.439	-80.70	-4.102	-89.90
	(18.64)***	(54.90)***	(110.3)	(84.03)	(12.25)	(32.93)**	(20.43)	(50.76)*
Observations	8697	5635	2816	2790	8697	5635	2816	2790
Adjusted R^2	0.118	0.073	0.037	0.017	0.107	0.177	0.201	0.224
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000	2,000	2,000
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

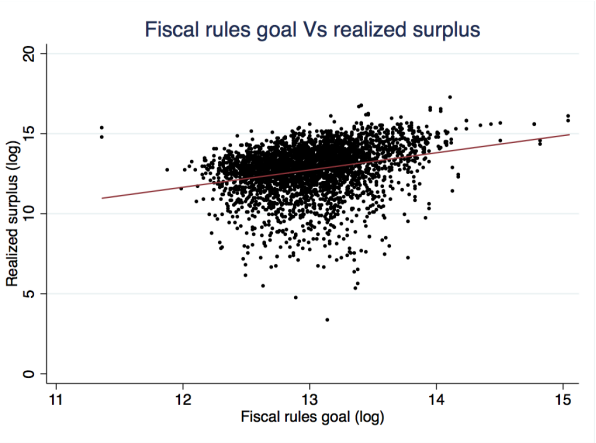
Notes: Cities in the category "Low/High surplus" are those required to accumulate a level of surplus below/above the median value to comply with the DSP. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Stability pact surplus goal - Criminal complaints

Dep. var.: Crime (PC)	Stability pact goal				SP goal respected (at least 1 year)			
	Low EU funds		High EU funds		Low EU funds		High EU funds	
	Low surplus (Below median)	High surplus (Above median)	Low surplus (Below median)	High surplus (Above median)	DSP goal Not respected	DSP goal Respected	DSP goal Not respected	DSP goal Respected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stability pact	-0.00751	-0.0188	0.0341	0.00650	0.00566	-0.0232	0.0459	0.00958
	(0.00584)	(0.0107)*	(0.0539)	(0.0324)	(0.00812)	(0.00681)***	(0.0611)	(0.0274)
Observations	7972	5273	2610	2624	5201	8067	2326	2912
Adjusted R^2	0.013	0.010	0.027	0.012	0.014	0.010	0.032	0.015
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000	2,000	2,000
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Cities in the category "Low/High surplus" are those required to accumulate a level of surplus below/above the median value to comply with the DSP. Cities in the category "DSP goal respected/not respected" are those that complied with the DSP (at least 1 year), accumulating the required amount of surplus. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 13: Stability pact - realized and targeted surplus



Notes: The plot shows the relation between the amount of surplus that the DSP requires to accumulate in logharitm ("Fiscal rules goal") and the surplus actually accumulated by Italian cities in logharitm ("Realized surplus"). The sample of cities included are those with population between 3,000 and 5,000 inhabitants and the years covered are 2013-2015.

6 Robustness checks

6.1 Effect of the DSP on criminal complaints - 1,000 inhabitants threshold

As already highlighted, the DSP is enforced after 2013 for cities below 5,000 and above 1,000. Nevertheless, we only exploit the 5,000 threshold. Ideally, we could also exploit

the 1,000 threshold comparing cities right below/above this threshold. Unfortunately, the 1,000 threshold cannot be included in our analysis for two reasons. First, about 38% of municipalities below 2,000, i.e. those that could be included in a test based on the 1,000 threshold, joined an "union of municipalities" (*Unioni di Comuni*). Those unions are in charge of all public services and administrative functions that were previously attributed to the single municipalities, particularly their budgets (Law 148, September 2011). Importantly, such unions of municipalities are exempted from the DSP, therefore for many cities close to the 1,000 threshold there has not been any policy change related to the DSP in 2013. Second, there is little variation in our dependent variable when considering very small cities. When considering cities in our main sample, e.g. municipalities with a population between 3,000 and 7,000, we observe an average of 0,07 corruption charges per year. Conversely, there are only 0,007 corruption charges per year in municipalities with a population below 1,000.

In Table 10, 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 and on corruption charges when considering the 1,000 threshold.

Table 10: Effect of the DSP on criminal complaints - 1,000 inhabitants threshold

Dep. var.:	Crime (PC)		Total revenues		Total expenditures		Current expenditures		Capital expenditures	
	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)	(5)	(6)				
Stability pact	0.00502	-0.0639	28.36	32.25	35.31	-20.13	-31.63	0.750	63.75	-97.79
	(0.00804)	(0.0854)	(17.09)*	(27.33)	(127.2)	(297.4)	(30.24)	(31.49)	(105.2)	(259.2)
Observations	16,450	4,419	17,684	4,785	17,704	4,785	17,704	4,785	17,704	4,785
Adjusted R^2	0.005	0.001	0.602	0.726	0.003	0.071	0.103	0.151	0.009	0.029
Bandwidth	500	500	500	500	500	500	500	500	500	500
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

6.2 Effect of the DSP on local police expenditures

An alternative explanation for our findings could be that the DSP pushes local politicians to cut spending on local police, in turn, reducing the probability of catching corrupt officials. This explanation is unlikely for two reasons: i) the main one is that the

municipal police is not in charge of crimes related to corruption, which are investigated by a specific brunch of the national police; ii) in Table 11, we test whether the DSP leaded to a decrease in spending on local police: we do not find any significant change in spending on local police both in high and low EU-funds municipalities.

Table 11: Effect of the DSP on local police expenditures

	Dep. var.: Local police expenditures (PC)	
	Low EU funds	High EU funds
	(1)	(2)
Stability pact	-1.723	0.552
	(1.234)	(1.703)
Observations	13,184	5,097
Adjusted R^2	0.042	0.039
Entity, year FE	Yes	Yes
Controls	Yes	Yes
Bandwidth	2,000	2,000

Notes: The dependent variable is the expenditures in local police, expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.3 Analysis with a *Difference-in-Discontinuity* methodology

In this section we conduct the main analysis on crime using the *Difference-in-Discontinuity* methodology in order to check whether main results are also robust to the application of this methods. This methodology has been used in many recent studies (Lalive, 2008; Campa, 2011; Leonardi and Pica, 2013; Casas-Arce and Saiz, 2015; Grembi et al., 2016) and it is based on the comparison of the outcomes before and after the reform for cities around the population threshold of application[23]. Columns (5-6) of Table 16 show this

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

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

Where P_i^*, S_{it} and T_t are defined as in model 1, and η_t represent year fixed effects and ξ_p province fixed effects. The Diff-in-Disc 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 (LLR) 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. (2014a, 2014b).

test with and without the inclusion of the standard set of controls. The negative effect of the reform on crime emerges also in this case and the magnitude of the estimate is not so different than the one estimated with the local Diff-in-Diff methodology. As explained in the empirical strategy section, we decide to rely on a local *Difference-in-Differences* methodology: as 1) we deem it more precise as it allows for the inclusion of city fixed effects, controlling for time invariant city characteristics; 2) and it facilitates a more standard evaluation of the absence of pre-trends effects.

6.4 Other tests

In this section we want to briefly recall three additional robustness tests conducted and already discussed in section 2 and 4.

First, the application ot the DSP was completely enforced for Italian ordinary regions but only some among special regions adhere to this policy. In order to check the robustness of our findings we run the main specification excluding from the sample the set of special regions. Results are contained in columns 1-2 of Table 16 (in the Online Appendix), and 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 has been adopted in 2013 to cities with more than 5,000 inhabitants and it represents a double preference voting, conditioned on gender. However, a reinforced version of this policy has been extended in 2014 to cities above 3,000 inhabitants, which states that elected politicians of each gender cannot have less than 40% of municipal seats. This policy, therefore, could bias our results only for cities that vote in 2013. In order to check for the robustness of our findings, we estimate the main model excluding from the analysis the sample of cities casting a ballot in 2013. Main results, shown in columns 3-4 of Table 16 (in the Online Appendix), suggests that this policy overlap is not a problem for our results.

Third, one possible concern may be that the DSP could affect the propensity of firms to charge their own competitors due to the reduction in procurement spending. In order to rule out this confounding story, we plot the total number of appeals to courts over time for low and high EU funds regions: Figure 15 show that trends in accusations

are parallel. This suggests that the introduction of fiscal rules in low EU funds regions did not modify the charging behaviour there, compared to the other area, the high EU regions, where the DSP has been only weakly enforced.

7 Concluding remarks

In this paper, we study the impact of introducing fiscal rules on the incidence of corruption charges and we show that this policy leads to a significant reduction of corruption related crimes. This result suggests that fiscal rules might work as a tool against corruption, in addition to being an effective policy to contain public deficit. We rely on the introduction of a specific set of fiscal rules, i.e. the Domestic Stability Pact (DSP) among Italian municipalities with a population below 5,000 in the period 2004-2014 and we employ a local Difference-in-Differences estimation to test whether the introduction of the DSP affects corruption charges and budgetary outcomes. Our findings show that the DSP leads to a substantial decrease in both corruption charges per capita and corruption charges over total capital expenditures. These effects are driven by a reduction in inefficient capital expenditures and they emerge only in areas in which the DSP has been fully enforced. Specifically, we do not observe a reduction in corruption among municipalities eligible to receive extra transfers from the European Union, which are not subject to the DSP: in these municipalities there is not any reduction in (inefficient) capital expenditures. Finally, local welfare is not significantly affected: we suggest that the decrease in rent-seeking and the drop in public investments lead to opposite welfare effects which cancel each other out.

Our findings are timely relevant as we shade 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 are of interest for governments and international organizations committed to enforce fiscal rules and/or anti-corruption policies.

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8 Appendix 1: Tables

Table 12: Effect of the DSP on criminal complaints - Local Diff-in-Diff estimator

	Dep. var.: Crime (per-capita)	
Panel A: without controls	Low EU funds	High EU funds
	(1)	(2)
Stability pact	-0.224	0.0803
	(0.0991)**	(0.102)
Observations	13,685	5,597
Adjusted R^2	0.008	0.019
Entity, year FE	Yes	Yes
Controls	No	No
Bandwidth	2,000	2,000
Panel B: with controls		
Stability pact	-0.155	0.0797
	(0.0697)**	(0.106)
Observations	13,268	5,238
Adjusted R^2	0.010	0.021
Entity, year FE	Yes	Yes
Controls	Yes	Yes
Bandwidth	2,000	2,000

Notes: The dependent variable is the number of criminal complaints, expressed in per-capita terms and standardized. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Effect of the DSP on single corruption crime

Dep. var.:	Graft (*concussione*)		Malfeasance (*abuso d'ufficio*)		Embezzlement (*peculato*)	
	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact	-0.000297	-0.00396	-0.00989	0.0349	-0.00197	-0.00152
	(0.000865)	(0.00294)	(0.00409)**	(0.0293)	(0.00188)	(0.00924)
Observations	13268	5238	13268	5238	13268	5238
Adjusted R^2	0.001	0.004	0.006	0.022	0.005	0.002
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000
City, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

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

Table 14: Effect of the DSP on local public finance - Local Diff-in-Diff estimator

	Total expenditures		Current expenditures		Capital expenditures		Total revenues	
Panel A: without controls	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Stability pact	-147.3	-154.5	-48.49	-48.21	-90.17	-53.74	-15.50	-22.26
	(37.82)***	(94.24)	(16.48)***	(25.56)*	(22.98)***	(64.53)	(14.76)	(18.45)
Observations	14810	5980	14810	5980	14810	5980	14802	5982
Adjusted R^2	0.021	0.058	0.083	0.195	0.080	0.020	0.463	0.750
Entity, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000	2,000	2,000
Panel B: with controls								
Stability pact	-95.32	-114.0	-23.87	-43.74	-75.88	-23.49	5.273	-12.95
	(44.27)**	(98.03)	(19.22)	(28.45)	(23.51)***	(69.32)	(16.94)	(19.14)
Observations	14347	5611	14347	5611	14347	5611	14339	5613
Adjusted R^2	0.043	0.061	0.157	0.196	0.080	0.021	0.509	0.752
Entity, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000	2,000	2,000

Notes: The dependent variables are the main public finance indicators, expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: Effect of the DSP on procurement - Local Diff-in-Diff estimator

	Number of tenders		Total amount	
Panel A: without controls	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)
Stability pact	-0.0537	-0.0691	-390.9	-101.8
	(0.0406)	(0.0888)	(147.5)***	(374.6)
Observations	8619	3471	8619	3471
Adjusted R^2	0.052	0.026	0.024	0.024
Entity, year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Bandwidth	2,000	2,000	2,000	2,000
Panel B: with controls				
Stability pact	-0.0540	-0.0673	-382.1	53.08
	(0.0426)	(0.0926)	(142.5)***	(397.3)
Observations	8291	3261	8291	3261
Adjusted R^2	0.056	0.029	0.022	0.027
Entity, year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,000	2,000	2,000	2,000

Notes: The dependent variables are the main procurement indicators, expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Robustness checks

	Excluding special regions		Diff-in-Disc estimation (Grembi et al. 2016)	
Panel A: without controls	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)
Stability pact	-0.232	0.144	-0.142	0.0160
	(0.109)**	(0.143)	(0.0674)**	(0.108)
Observations	12535	3808	11241	4453
Adjusted R^2	0.008	0.020	0.049	0.027
Entity, year FE	Yes	Yes	Yes	Yes
Controls	No	No	No	No
Bandwidth	2,000	2,000	1707.34	1707.34
Panel A: with controls				
Stability pact	-0.151	0.134	-0.147	0.0273
	(0.0743)**	(0.147)	(0.0700)**	(0.111)
Observations	12143	3568	10924	4171
Adjusted R^2	0.011	0.023	0.049	0.026
Entity, year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Bandwidth	2,000	2,000	1707.34	1707.34

Notes: The dependent variable is criminal complaints per-capita. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. In columns (1-2) Italian special regions have been excluded from the sample. In columns (3-4) the analysis is structures ad a Diff-in-Disc estimation, following Grembi et al. (2016), see section 6.3 for details. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 17: Impact of gender quotas

Dep. var.	Crime (per-capita)		Capital expenditures		Procurement exp. - total amount	
Panel A: without controls	Low EU funds	High EU funds	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)	(5)	(6)
Stability pact	-0.245	0.111	-81.18	-70.21	-419.0	-180.8
	(0.105)**	(0.116)	(23.85)***	(74.97)	(151.5)***	(413.0)
Observations	13,525	5,427	14,573	5,738	8,388	3,211
Adjusted R^2	0.008	0.019	0.082	0.020	0.023	0.023
Entity, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000
Panel A: with controls						
Stability pact	-0.161	0.114	-64.91	-39.32	-407.9	-40.21
	(0.0729)**	(0.121)	(24.35)***	(80.18)	(145.9)***	(436.3)
Observations	13,112	5,070	14,116	5,372	8,066	3,004
Adjusted R^2	0.010	0.021	0.083	0.020	0.022	0.025
Entity, year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	2,000	2,000	2,000	2,000	2,000	2,000

Notes: Cities that vote in 2013 are excluded from this analysis as they were subject to the introduction of gender quotas that have been expanded in 2014. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

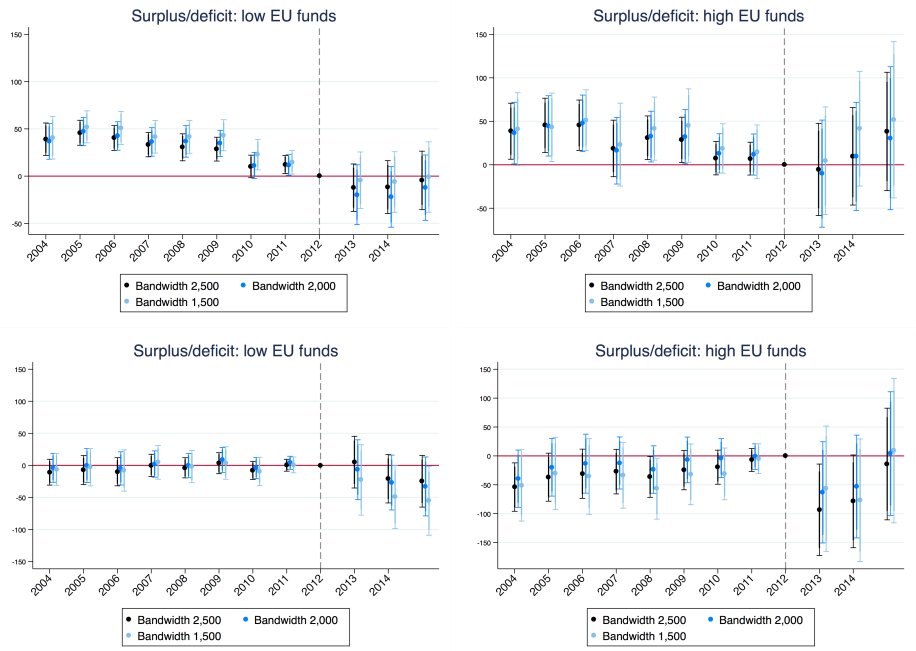
Table 18: Effect of the DSP on re-election probability of mayors

	The incumbent is re-elected			
	Low EU funds	High EU funds	Low EU funds	High EU funds
	(1)	(2)	(3)	(4)
Stability pact	0.0264	-0.185	0.0149	-0.181
	(0.143)	(0.210)	(0.147)	(0.234)
Observations	2587	1013	2432	878
Adjusted R^2	0.206	0.126	0.260	0.156
Entity, year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Bandwidth	2,000	2,000	2,000	2,000

Notes: The dependent variables is a dummy equal to one when the past mayor is re-elected. The sample is limited to electoral years and cities with term limited mayors have been excluded. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds. The specification includes city and year fixed effects, the distance from the population threshold after the reform, characteristics of the mayor (age, education and gender) and population of the city. Robust standard errors clustered at the municipality level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

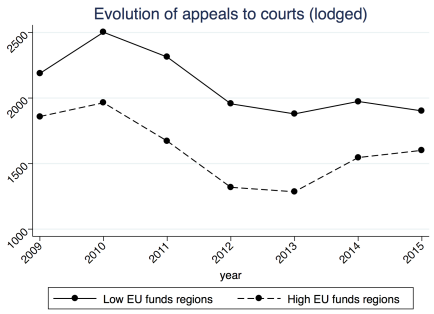
9 Appendix 2: Additional figures

Figure 14: Effect of the DSP on local public finance - Additional variables



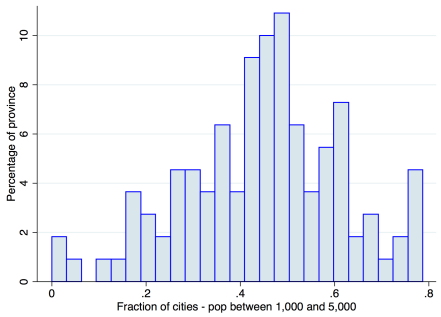
Notes: This plot shows the outputs of the local Diff-in-Diff estimation, according to the model 2. The dependent variables include surplus and debt, both expressed in per-capita terms. Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds.

Figure 15: Appeals to courts over time



Notes: This plot shows the total number of appeals to course, lodged, over time (source: Italian Ministry of Justice). Low EU funds regions are those that receive standard funds from the European Union, high EU funds regions are those with GDP lower than 75% of European average ("convergence regions") which receive large European funds.

Figure 16: Fraction of cities where DSP is extended



Notes: This plot shows the distribution of fraction of cities over all cities in Italian provicnces where fiscal rules have been extended in 2013. These are cities with population between 1,000 and 5,000 inhabitants. Population values refer to the 2011 Census.