BANCA D'ITALIA

EUROSISTEMA

Temi di discussione

(Working Papers)

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Number 1360

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The views expressed in the articles are those of the authors and do not involve the responsibility of the Bank.

ISSN 1594-7939 (print)
ISSN 2281-3950 (online)

Printed by the Printing and Publishing Division of the Bank of Italy

FISCAL RULES AND THE RELIABILITY OF PUBLIC INVESTMENT PLANS: EVIDENCE FROM LOCAL GOVERNMENTS

by Anna Laura Mancini* and Pietro Tommasino**

Abstract

We document that Italian public administrations systematically overestimate capital expenditures, and that the introduction of a cap on this spending item improves the accuracy of their plans. Our analysis relies on a unique dataset including budgetary figures (both planned and realized) for all Italian municipalities, and exploits a national reform which introduced a limit on realized capital expenditures only for municipalities above a given population threshold (5,000 residents). One possible interpretation of our results is that by exploiting the imperfect knowledge of voters, policymakers benefit from promising overly ambitious investment plans. The introduction of expenditure limits makes these promises less credible, and helps to bring spending plans in line with reality. Furthermore, we find that capital revenue are also overestimated, and that the forecast accuracy of these revenues improves due to the reform. This is in line with our political-economy interpretation: as there is less room to inflate expenditures, politicians also have less incentive to indulge in window-dressing on the revenue side.

JEL Classification: E62, H62, H68.
Keywords: budget rules, budget execution, local public finance, official forecasts, public investment, fiscal transparency.
DOI: 10.32057/0.TD.2022.1360

Contents

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1. Introduction[1]

Reliable and accurate public budgets are crucial to anchor economic agents' expectations and to keep policy-makers accountable. Unfortunately, politicians do not have strong incentives to adopt the most transparent accounting practices. First, it is often argued that voters overestimate the effects of public spending and underestimate the cost of financing it through taxation and/or deficits – the so-called "fiscal illusion" phenomenon[2]. Second, as emphasized by Beetsma *et al*. (2009), "*while fiscal plans draw a lot of attention, (...) implementation receives much less attention*": cheating is usually discovered only later on, when chances are that the public attention has shifted to other policy issues, and/or the politician responsible for the budget is no more in place. If these two assumptions are true, policy-makers have an incentive to "plan to cheat"[3]. That is, they promise an amount of expenditures higher than what they will actually deliver, because this allows them to cater to the demands of the various groups of voters, and at the same time they present overoptimistic revenue forecasts, in order to preserve the appearance of fiscal discipline. Once the extra revenues hoped for by the government fail to materialize, budgeted investment expenditures are downsized or abandoned altogether.

In this context, caps on realized spending can contribute to more realistic ex ante spending plans. Indeed, politicians have less room to inflate planned expenditures, once there is a legal ceiling in place. Introducing the ceiling also makes the opportunity cost of spending more salient for voters; ultimately, over-optimistic spending plans have a smaller political pay-off.

In this paper, we provide evidence in favour of this theoretical intuition, exploiting a unique dataset including the ex-ante budget plans as well as ex-post budget outcomes of all the

[1] The views expressed in the paper are those of the authors and do not necessary reflect the position of the Bank of Italy. We thank Fabrizio Balassone, Guido de Blasio, Domenico Depalo, Vanni Mengotto, Paolo Naticchioni, Emilio Vadalà, Stefania Zotteri and seminar participants at the Bank of Italy Conference "*Lungo il sentiero stretto. I governi locali in tempo di crisi*" (Rome, December 4, 2017; https://www.bancaditalia.it/pubblicazioni/altri-atti-convegni/2017-lungo-sentiero-stretto/index.html), the 2021 ZEW Public Finance Conference (Mannheim, May 6, 2021), the 20th Journées Louis-André Gérard-Varet (Marseille, June 22, 2021), the XXXIII SIEP Web Conference (Bari, September 17, 2021) and at the Banca d'Italia – World Bank workshop on "Misurazione e Valutazione del funzionamento della Pubblica Amministrazione" (Rome, September 21, 2021). All remaining errors are our own responsibility.

[2] Early work on fiscal illusion – a concept first introduced in Puviani ([1903] 1973) and popularized by Buchanan (1967) – is surveyed by Dollery and Worthington (1996). Interestingly, the mechanisms behind the fiscal illusion phenomenon can be reframed in the context of the recent advances in behavioral economics (see e.g. Doring and Oehmke, 2020 and Schnellenbach and Schubert, 2015).

[3] The expression is borrowed from Beetsma *et al*. (2009).

about 8,000 Italian municipalities. We study the effects of the introduction in 2005 of a rule that constrains capital expenditures in municipalities with more than 5,000 residents. Capital expenditures are well-suited to test our political economy hypothesis, because they are more flexible than current expenditures, which include mostly pre-determined items (for example employees compensation or interest payments), and because of their high symbolic appeal for the voters. The ongoing provision of basic public services often goes unnoticed (unless in case of major mismanagements) and is taken almost for granted by most citizens. On the contrary, investment projects are not perceived by the electorate as business-as-usual, are often "sold" politically as a mean to change radically for the better the life of a community, and are ultimately a crucial determinant of local government's popularity.

Our analysis show that the municipalities subject to the new capital-spending rule significantly reduced their over-optimism in expenditure projections: planned capital expenditures decreased more than actual ones, in line with our a priori. Furthermore, in the new regime revenue projections are also more accurate (less over-optimistic).

While the literature on the effectiveness of fiscal rules on actual budgetary outcomes is large,[4] research on their impact on budget execution has been scant, notwithstanding the practical importance of the issue. Beetsma *et al*, (2009), Von Hagen (2010) and Giuriato *et al*. (2016) find that implementation errors are smaller in countries that adopt numerical fiscal rules. Pina and Venes (2011) find similar results, albeit only if numerical expenditure rules are in place. All four papers consider a small panel of European countries. Similar conclusions are drawn by Frankel (2011), which studies a slightly larger panel of countries (33).

To our knowledge, there are very few papers that – like ours – exploit within-country data to analyse the relationship between the stringency of fiscal rules and budgetary projection errors. In particular, Luechinger and Schaltenegger (2013) and Chatigny (2015) looking at the twenty-six Swiss Cantons over a period of approximately twenty years find evidence that the introduction of budgetary rules improves accuracy of official forecasts. Both papers use standard panel data techniques, as opposed to our quasi-experimental approach; the fact that Canton-level rules were introduced in a staggered way allows them to control for year- and Canton-fixed effects. Leaving technical identification issues aside (as we will discuss below, the introduction

[4] Concise overviews can be found in Burret and Feld (2014) and Wyplosz (2014). For a more recent and systematic assessment, see Heinemann *et al*. (2018). The effect of the Italian municipal fiscal rules on the municipal deficit and budget composition have been studied, respectively, by Grembi *et al*. (2016) and Venturini (2020). Daniele and Giommoni (2020) look at the effect of the DSP on corruption. See also Balassone et al., (2011).

of a fiscal rule and its degree of stringency might be both endogenous), the political-economy mechanism behind the results of the Swiss papers does not apply to the Italian municipalities' case. Fiscal forecasts in Swiss cantons are provided by the Canton's finance minister, which is politically independent and more fiscally conservative than the rest of the Government. This is why, according to both papers, finance ministers purposely introduce a pessimistic bias in deficit forecasts in order to discourage overspending by other Ministries. With the introduction of a fiscal rule, this behaviour becomes less useful; therefore forecast accuracy increases. In Italian municipalities, instead, the mayor's cabinet is more cohesive, and the official in charge of the budget is aligned with the interests of the mayor and of its political majority.

Using within-country data has several advantages. First, we are able to increase manifold the sample size with respect to cross-country studies. Second, we reduce significantly the need to control for institutional differences. Third, when it comes to the explanatory variables, measurement error is reduced: cross-country fiscal rules indices, including the one provided by the European Union and used by the above-mentioned studies, require comparing different institutional set-ups, which unavoidably requires some judgemental choice.

Finally, and most importantly, using within-country data addresses the well-known issue of the endogeneity of the rules (Poterba, 1997). Actually, the cross-country paper mentioned above either did not explicitly address the issue or adopted an instrumental variables approach, whereas our set up is such that one can plausibly consider the introduction of the new budget rules as an exogenous treatment. Our set up also provides us with a natural control group, the municipalities with less than 5,000 residents, which works as a benchmark to assess the causal impact of the reform and allows us to apply quasi-experimental microeconometric techniques.

At a late stage of our research, we learned of the paper by Picchio and Santolini (2020). While their data and methodology are similar to ours, they analyze two different reforms, which occurred in 2001 and 2002, respectively. In 2001 the exemption from a set of fiscal rules called the Domestic Stability Pact (see Section 2) for municipalities with less than 5,000 people was introduced; in 2002 more severe sanctions were introduced for non-compliant municipalities above 5,000 people. Picchio and Santolini (2020) find no effect in the former case. This is not surprising, as it is widely acknowledged that before the introduction of stricter sanctions the enforcement of local fiscal rules was (and was perceived by municipalities as) very loose (see Section 2). Concerning the 2002 reform, even if in those years the Domestic Stability Pact concerned the overall current account deficit (the difference between current revenues and current expenditures), Picchio and Santolini (2020) find a significant effect only on the accuracy

of current revenue forecasts. We study a policy change that is about capital expenditures, and we find both a direct effect on capital expenditure forecasts errors and an indirect effect on capital revenue forecast errors.

Our paper also contributes to the literature on the political and institutional determinants of official budgetary forecast errors. While we focus on the effect of fiscal rules, previous papers have highlighted the importance of the electoral cycle (Boylan, 2008; Bohn and Veiga, 2020), partisanship (Bischoff and Gohout, 2010; Chatagny, 2015; Jochimsen and Lehmann, 2017), fiscal autonomy (Boukari and Veiga, 2018), political fragmentation (Goeminne *et al*., 2008), independent forecasting agencies (Buettner and Kauder, 2010; Beetsma *et al.*, 2013). These contributions can indeed be framed within the broad literature on fiscal transparency, defined as the degree of "comprehensiveness, clarity, *reliability*, timeliness, and relevance of public reporting on the past, present and *future* state of public finances" (IMF; italics added).[5]

The rest of the paper proceeds as follows: Section 2 presents the institutional set-up; Section 3 describes the empirical strategy and Section 4 the data we used; in Section 5 we present and discuss our results; Section 6 concludes.

2. Institutional set-up

In the Italian institutional framework the sub-national sector comprises three levels of Government: Regions, Provinces and Municipalities. Regions are involved primarily in the provision of health services. Provinces perform some functions in the areas of road maintenance and natural environment. Municipalities are responsible for several local public services (such as public lights, waste disposal, urban road maintenance, local transports) and for social services at large (such as assistance to poor people and childcare). They also provide school-related services such as refectories and school buses, whereas education in the stricter sense (and therefore teachers' pay-rolls) is instead a Central Government's responsibility.

The share of General Government primary current expenditures accounted for by municipalities is about 7 per cent (3 per cent of Italian GDP), this share rises to 21 per cent if we consider capital expenditure, and to slightly less than 57 per cent if we focus on investment.

[5] The IMF definition and the latest version of the IMF fiscal transparency code can be found here: https://www.imf.org/en/Topics/fiscal-policies/fiscal-transparency. Alt (2019) and de Renzio and Wehner (2017) are two authoritative surveys of the academic literature. Rios et al. (2018), show that, in the case of Spanish municipalities, forecast accuracy is indeed positively correlated with other dimensions of fiscal transparency.

Municipalities are financed with a mix of transfers and own revenues. On average, municipal taxes account for a share of roughly 35 per cent of total revenue, another 25 per cent stems from other own revenues, such as the collection of tariffs and fees, while the remaining 40 per cent comes from transfers from higher levels of Government, which are largely unconditional.[6]

Capital revenues are mostly made of transfers from higher levels of governments, but they include also other revenue sources, which are less stable over time, such as concession fees and sales of public assets. Mainly for the latter components, capital revenues vary a lot over time and they are easy to be deliberately overestimated in case the municipal government needs an accounting shortcut to make room for desired new investment (see Table 1 for a stylized representation of a typical municipal budget). On the contrary, a share of current revenues does come from taxes imposed on the local community, so their increase entails a non-negligible political cost.

In 1999, the Italian Government introduced a set of rules that constrained all municipalities in terms of fiscal discipline, the so-called Domestic Stability Pact (DSP). The primary goal of the DSP was to make local administrators co-responsible with the central government in ensuring the compliance of the Stability and Growth Pact, which came fully into force in the same year. Over time, the DSP underwent substantial changes, making it difficult to plan and to comply with the rules.[7]

First, the penalty system changed significantly over the years. Initially, the only sanction for non-compliant municipalities was the threat of paying the quota that was directly imputable to the municipality of any fine that European authorities would impose to Italy for non-compliance with the Stability and Growth Pact. As it is well-known, no financial sanction has been ever levied against a EU country in accordance with the rules of the Stability and Growth Pact, given that this would require a politically very difficult agreement within the EU Council (for example, in 2005, the EU countries preferred to change the rules rather than sanctioning Germany and France). Furthermore, the process that leads to the actual imposition of a fine to the non-complying country after the detection of a breach of the fiscal rules involves several steps, so that the likelihood that the mayors that were pro-rata responsible for the Italian non-compliance are still in charge when the fine is eventually imposed is almost nil. To sum up, in

[6] The allocation of these grants across municipalities reflects demographic, socio-economic and fiscal indicators.

[7] Detailed accounts of the rules and their evolution can be found in Balassone and Zotteri (2001), Patrizii *et al.* (2006), Pignatti (2009), Valerio (2009), Monacelli *et al.* (2016).

its initial formulation the DSP had no teeth (Balassone *et al.* 2011). In 2000, it was decided that compliant municipalities would have access to lower interest rates on borrowing from the central government, but only in 2002 the central government introduced a new set of sanctions, which included stricter constraints on indebtedness, intermediate consumption and personnel hiring for non-compliant municipalities. This made the enforcement of the DSP rules definitely more effective.

Second, the set of municipalities subject to the DSP also changed over time. While, initially, all municipalities were included, since 2001 municipalities below 5,000 have been exempted from the DSP. Their exclusion was mainly due to the difficulty of monitoring their activities, given that they represent more than 70% of municipal administrations, and to the small impact they have on the overall national level of public spending. Starting from 2002, autonomous regions (*Regioni a Statuto Speciale*) were allowed to set their own fiscal rules for municipalities in their territories[8].

Third, the target variables of the DSP changed over time, too (see also Table 2 for a summary). From the start to 2004 the DSP was based on a deficit rule, albeit limited to the current account part of the budget (current expenditures minus current revenues), thus excluding investment. In 1999 and 2000, the target was a current account deficit not greater than that of the previous year. In 2001, there was a cap on the deficit with respect to the 1999 level. From 2002 to 2004, the constraint was with respect to the 2000 deficit (for 2002 only, an extra goal on the level of total expenditures was added). In 2005 and 2006, the DSP changed from a deficit rule to an expenditure rule: both capital and current expenditures had to remain at or below the average level of the previous years. As we argued in Section 1, this change reduced the importance of strategically-biased revenue forecasts because it excluded the possibility to finance higher expenditures through higher revenues. In 2007, the deficit rule was resumed, although with a different definition (this time, also capital revenues and expenditures were included, in line with the Maastricht definition for the general government). This choice intended to give back to local governments their autonomy on what measures to adopt between reducing expenditures and increasing revenues, to contain the deficit, and at the same time to align the target for local government to the one for the general Government. From 2008, other major changes were introduced. For this reason, our empirical analysis ends there.

[8] Sicilia and Sardegna decided not to use this possibility and to stick to the national rules. Nevertheless, we decided to exclude from our analysis municipalities that belongs to the autonomous regions.

As we have already mentioned, since 2001 the DSP only applies to municipalities with more than 5,000 people. However, fiscal rules are not the only policy varying with population size. The size and wages of the executive committee, the electoral rule and the wage of the mayor, the size of the city council and the possibility to have neighbourhood elected councils are all elements that vary at some population cut-off. In particular, at the DSP cut-off (5,000 inhabitants) the wage of the mayor and of the members of the executive committee show a sharp increase. Gagliarducci and Nannicini (2013) show that this wage increase attracts higher educated individuals into elective and administrative bodies, therefore improving their performance.

3. Identification strategy

Our analysis focuses on the change occurred after 2004 with the introduction of a cap on capital expenditures and a cap on current expenditures in substitution of the deficit rule.

Our focus is on the implementation error, computed as the difference between the forecast and the realised value for each variable of interest, for each municipality and year in our sample. It can therefore take both positive and negative values. Each dependent variable is expressed in 2010 euros. Results are qualitatively unchanged if the dependent variable is standardized by population size (as in Nannicini *et al*., 2016).

To identify the effect of the policy shift we start by using a difference-in-differences (*diff-in-diff*) approach. Our treated group corresponds to municipalities subject to DPS, which in 2005 faced the change in their budget constraints. Municipalities not subject to the DSP represent our control group.

The budget implementation error in municipality in year t (Y_{it}) is modelled as:

$$(1) \qquad Y_{it} = \alpha + \beta_1 DSP_i + \beta_2 T_t + \beta_3 DSP_i * T_t + \gamma X_{it} + \varepsilon_{it}$$

where T_t is a dummy equal to one for years 2005-07, DSP_i is a dummy equal to 1 if municipality *i* is subject to the pact and X_{it} are municipal controls. The coefficient of interest is represented by β_3.

However, as it is well-known, the estimated coefficient β_1 correctly identifies the causal effect we are interested in only under the quite demanding assumption that small cities are a good counterfactual for large cities. In other words, it is required that – absent the policy intervention

– the dependent variables of interest would have followed parallel trends in both groups. While, in the following Sections, we will provide evidence that in our context such a "parallel trends" assumption is plausible, it seems wise to present results obtained under a different – and arguably more plausible – set of identifying assumptions.

Notice that what would seem a natural alternative, namely a static Regression discontinuity design (RDD), would also be problematic. Indeed, as we have highlighted in Section 2, at the 5,000 inhabitants' threshold two changes happen: the kick in of the DSP fiscal rules and the jump in the wage of the mayor and of the members of the executive committee. Therefore, the static RDD coefficient would capture the effect of both changes, not being able to isolate the effect of the fiscal rule we want to study. To address this issue, we opt for a different estimation strategy, namely the difference-in-discontinuity (*diff-in-disc*) approach proposed by Grembi *et al.* (2016) and Eggers *et al.* (2018), which takes advantage of the time-dimension of our dataset. This estimator basically combines the *diff-in-diff* and the RDD approaches to isolate the effect of the policy that changes around the population threshold and over time (the DSP pact) from the one that jumps at the threshold but stays constant over time (the wages). Under the assumptions that: (i) without the DSP change, the effect of the confounding policy is constant over time in a neighbourhood of the 5,000 population threshold (which is basically a local and therefore milder version of the "parallel trends assumption") and (ii) potential outcomes are a continuous function of population at the threshold (which is the standard RDD requirement), Grembi *et al.* (2016) show that the local treatment effect of the policy of interest can be estimated through the following OLS regression:

$$(2)\quad Y_{it} = \alpha + \theta P_{it}^{*} + (\beta_1 DSP_i + \beta_2 T_t + \beta_3 DSP_i T_t) + P_{it}^{*}(\gamma_1 DSP_i + \gamma_2 T_t + \gamma_3 DSP_i T_t) + \vartheta X_{it} + \varepsilon_{it}.$$

T_t is a dummy equal to one for years 2005-07, DSP_i is a dummy equal to 1 if municipality i is subject to the pact and $P_{it}^{*} = (P_{it} - P_c)$ is population centred at the cut-off point (P_c=5,000). The easiest way to rationalize equation (2) is to notice that it nests equation (1), but is richer because it allows for population to influence the outcome not only indirectly though the discrete treatment ($DSP_i * T_t$) but also directly, in a continuous way. Furthermore, equation (2) allows the linear relationship between Y_{it} and P_i^{*} to have four different slopes, depending on whether the observation is before or after the treatment-period and whether it is above or below P_c. An alternative way to grasp the intuition is to notice that, if for exposition's sake we only consider

two time periods and assume that population and covariates are time-invariant, equation (2) is the standard way to estimate the treatment effect in a static RDD setting in which the dependent variable is ΔY_{it}(see e.g. Bagues and Campa, 2021).

As any RDD estimation, results are sensitive to the size of the chosen bandwidth h: a smaller h ensures consistency at the expenses of accuracy, while a broader bandwidth improves estimates precision but lowers internal consistency. For this reason, we present the robustness of our results to different bandwidths, computed either following the algorithm proposed by Calonico, Cattaneo and Titiunik (2014a, b) or the mean squared error approach. The coefficient of interest is represented by β_3.

Both in the *diff-in-diff* and in the *diff-in-disc* analysis, our preferred specifications include some control variables, in order to improve the efficiency of our estimators (Calonico *et al*., 2019) and address the risk of biases induced by possible sorting behaviour (Eggers et al., 2018). In particular, we include: a set of geographic dummies (for Central, Northern and Southern Italy), to capture well-known long-standing differences in the quality of local administrations and in the structure of the economic and the social environment; the time elapsed (in years) since the last local elections, as a proxy for the cyclical strength of political-economy distortions; average municipal income, as a proxy for the level of economic development. In some regressions, we also add two indicators that capture the fiscal autonomy of each municipality (Chiades and Mengotto, 2013): the fraction of local taxes and tariffs over total revenues and the ratio of interest expenditures over total revenues. In principle, a higher reliance on own resources (as opposed to transfers) should have a positive effect on the accuracy of budgetary plans, because voters should pay more attention on how the administration uses money coming from their own pockets. On the contrary, a high interest burden should have a negative effect on accuracy because it reduces the possibility to insulate expenditure plans from the effects of unforeseen contingencies.

Finally, it is worth underlining that the *diff-in-disc* estimator – contrary to the *diff-in-diff* – identifies the effect of the change in the DSP rules on the forecasting abilities of municipalities with more than (but not too far from) 5,000 inhabitants; further assumptions would be required to generalize the results to all municipalities. As it is often the case, milder identification assumptions imply a cost in terms of external validity.

4. Data

Budgetary data for municipalities come from the Budget of Italian municipalities, published yearly (with variable delays) by the Ministry of Interior. Italian municipalities are required by the end of previous year to present their provisional budget (*Certificati di Conto Preventivo*) in which planned expenditures and revenues are reported. Such plans may be subsequently revised by the 30th of November. By the end of June of the following year, municipalities publish the year financial statement (*Certificati di Conto Consuntivo*).

Since the dimension of the municipality affects the size and the types of expenditures, to ensure comparability between the treated and the control group, we restrict the analysis to municipalities that in the year 2001 had more than 3,000 but less than 8,000 inhabitants. Municipalities between 3,000 and 5,000 inhabitants are in fact not subject to DSP but quite similar for type of expenditures to municipalities with a population between 5,000 and 8,000 inhabitants. Indeed, very small and very large municipalities are likely to be very different from the others. For example, very large municipalities often externalize several services, while very small towns are much more likely to provide services jointly (Chiades and Mengotto, 2013).

Moreover, we restrict our observational period to years 2001 to 2007 to minimize the indirect effect of other major changes occurred in DSP rules later on.

Actually, 2007 can be considered a borderline year. On the one hand, the provisional budget is not yet constrained to be compliant with the rules (this crucial change will take place from 2008), and capital expenditures are still not protected by a "golden rule". On the other hand, in 2007 the budgetary indicator that constitutes the target of the DSP changes from expenditures to the overall balance (not just the current account balance, as it was the case before 2004). So in 2007 the rules give somewhat more flexibility concerning capital expenditures with respect to 2005 and 2006 (local governments could now increase capital expenditures by cutting capital expenditures or rising revenues). All in all, we prefer to keep 2007 in our sample, also because public administrations are quite slow to react to rule changes. However, results are mostly unchanged, albeit the coefficients of interest are in some cases less precisely estimated, if we exclude the 2007 data from our sample (table available on request).

On the contrary, the 2008 clearly is the start of a different regime. First, as we already mentioned, the DSP rules were imposed not only ex post (on the final outturn), but also on the provisional budget. Second, the balance in accrued terms was not relevant per se anymore.

Instead, what mattered was the *sum* of the deficit in accrued terms and the deficit in cash terms, so that, from example, a large accrual deficit could be compensated by a cash surplus.

Table 3 provides descriptive statistics. Municipalities above 5,000 inhabitants show higher expenditures and higher revenues with respect to those below the threshold, as expected given that they serve a bigger community. In particular, capital expenditures (the bulk of which – about 98% – consists of investment) are about €2.2 mln for small cities, as opposed to €3.1 mln for large cities before the reform; after the reform, capital expenditures decreased in both groups in the same proportion, at €1.8 mln and €2.6 mln, respectively. Before the reform, capital revenues were about €1.5 mln for small cities and €2.0 mln for large cities; after the reform, capital expenditures slightly decreased in both groups. Overall, the figures suggest that the magnitude of implementation slippages on investment is huge[9], especially if one considers that investment projects at the local level are characterized by an all-or-nothing nature: in most cases, you either complete the planned public work within the year or you don't even start it.

Figure 1 depicts the dynamics over time of forecasted and final expenditures and revenues. If we look at current expenditures, provisional expenditures fairly track final ones for both groups of municipalities. Current expenditures are in fact more rigid and less easy to manipulate. Conversely, for capital expenditures and capital revenues forecast errors are huge for both types of municipalities. However, large municipalities' planning ability seems to improve after 2005 – when they were subject to the caps on expenditures – especially in comparison to the planning performance of smaller municipalities. Before the reform, realized capital expenditures were just 49% and 47% of planned capital expenditures in small and large cities, respectively; after the reform, these percentages increased in the case of small cities (54%) and decreased in the case of large cities (45%). The figures concerning capital revenues are very similar.

5. Results

We are primarily interested in the effect of introducing the cap on capital expenditures on the difference between planned and realized capital expenditures and between planned and realized investment (investment are indeed by far the main component of capital expenditures).

[9] This stylized fact is confirmed, among others, by Repetto (2017), which also notes that the difference between plans and actual current expenditures is instead negligible.

As we discussed above, we also have reasons to expect that the new rule will have an impact on the accuracy in the forecast of capital revenues as well. Indeed, these revenues are not very politically costly to increase and easy to manipulate, therefore being optimistic about their size is a natural way to balance the budget in expected terms, vis-à-vis inflated capital expenditure plans.

We also run regressions using as dependent variables the difference between realized and planned values of the other main budgetary aggregates. We expect that these items should experience no significant change between the control and treated group during that period.

We start by estimating a parsimonious specification with neither geographical nor financial controls on the whole sample (column 1)and on a sub-sample obtained by trimming the extreme observations (those with the highest 1% and the lowest 1% values of the dependent variable) (column 2), to be sure that our result is not driven by outliers. We then add geographical controls, both without (column 3) and with (column 4) trimming and finally we also include financial controls (column 5 and 6, with and without trimming respectively).

The expected sign of β_3 is negative: the introduction of a cap on realized expenditures should increase the accuracy of budgetary plans as the cap makes clear that overly ambitious plans are unfeasible. .

Table 4 presents the results of our two strategies (*diff-in-diff* and *diff-in-disc*) for our two main variables of interests and Table 5 for the remaining items (current expenditures and current revenues). Finally, we also add a table (Table 6) in which the dependent variable is standardized by the municipal population, so it is expressed in euro per capita. This is mainly done for comparison's sake, as most papers consider municipal budgetary aggregates in per capita terms (see in particular Grembi et al., 2016 and Venturini, 2020) but we do not expect that this normalization will affect the results. First, we consider only municipalities which are very similar in terms of population; second, in most regressions we control for population.

5.1) Diff-in-diff results

The reform reduced the forecast error concerning capital expenditures, in line with our a priori (Table 4, top panel). The effects is significant both statistically and in economic terms. According to our preferred *diff-in-diff* estimate (i.e. the one with the full set of controls; Model

5), the introduction of the cap on investment reduced the forecast error on investment expenditures by almost €1 mln, or 35% of the pre-reform average error.[10]

The reform also increased the accuracy of capital revenues by about €730.000, about one third of the pre-reform average error. This finding corroborates our "political economy" interpretation of the forecast bias. As capital revenues are not related to municipal taxes, local governments do not pay a large cost in terms of consensus if they over-estimate this budget item. At the same time, this over-estimation makes possible to increase planned investment without increasing the planned deficit – which is likely to improve the reputation of the municipal government. The exogenously imposed fiscal rule puts a cap to the amount of budgeted investment spending, and therefore limits the possibility of pursuing this political strategy.

Looking at Table 5 (top panel), we find no significant effect on current expenditures and a statistically significant but quantitatively small effect on current revenues. As said, the policy change was minor and therefore we did not expect to find any significant change in those two dimensions.

To understand the channel behind the increase in the reliability of investment plans, we repeat our analyses using separately the planned and the realized levels of capital expenditures and capital revenues (Table 7, right panel). The impact of the changes in DSP rules goes mainly through a reduction of the planned expenditures and revenues.

To what extent can we trust our *diff-in-diff* estimates? We provide evidence about the reliability of the "parallel trend" assumption, on which the reliability of *diff-in-diff* rests, by means of a simple exercise. In Figure 2, we plot the treatment coefficients of a *diff-in-diff* regression in which the treatment is defined as being below 5,000 inhabitants in each year. For our variables of interest (investment and capital revenues), in the pre-reform period the coefficient for the relevant variables is not significantly different from zero, suggesting that the parallel-trend assumption is reasonable. The contrary is true for current revenues, especially in 2002 (a year in which a major change in the enforcement of the DSP took place, as we discussed above) casting doubts on the reliability of the results for this specific item. Another interesting takeaway of Figure 2 is that in the year 2006 (the first year with the new rules) the reform seems still quite ineffective. This is not surprising, as public administrations tend to be quite slow in

[10] Across *diff-in-diff* specifications, the point estimate is always statistically and economically significant and ranges from about €500.000 to €1 mln.

adjusting their behaviour to new rules (as we discussed before, this is also one of the reasons why we prefer to keep observations for 2007 in our sample).

We also perform falsification tests, looking at the impact of "fake" reforms on the sample of municipalities with less than 5,000 inhabitants, which therefore were actually not affected by the "actual" reform (see e.g. Bagues and Campa, 2020). In particular, we perform this placebo test for each 100 population intervals, by using equation (1), and find that never yield statistically significant results for investment and capital revenues (Figure 3). Also in this case, instead, current revenues fail to pass the falsification test.

5.2) Diff-in-disc results

Overall, we believe that available evidence provides a high degree of credibility to the *diff-in-diff* approach, at least in our specific setting. However, to be on the safe side, we also look at the results stemming from the *diff-in-disc* approach, which appears much less demanding in terms of identification assumptions.

The intuition behind the *diff-in-disc* results can be grasped by looking at descriptive graphs in which we map, for each municipality, the difference in outcomes between the post-reform and the pre-reform period as a function of the distance between actual population and the population threshold (Figure 4). Indeed, if we fit these data points with a third-order spline polynomial on each side of the origin, a discontinuity is visible in the case of investment and capital revenues but not in the case of current revenues and current expenditures.

Estimation results confirm this graphical evidence (Table 4 and Table 5, central and bottom panels). Notice also that significance is preserved irrespective of the algorithm used to choose the bandwidth.

Quantitatively, the effects estimated using the *diff-in-disc* are even more pronounced, being double in size with respect to the one estimated using the *diff-in-diff* technique. It should be reminded, however, that *diff-in-diff* estimates aim at identifying an average treatment effect, while *diff-in-disc* estimates capture the *local* effect of the treatment (i.e. the effect on the units just above the population threshold).

Our point estimates are negative for almost all bandwidth that are not too large to undermine our identification assumptions (Figure 5). They are imprecisely estimated for extremely small bandwidths because the observations are too few.

We perform several placebo experiments also in the case of the *diff-in-disc* approach. First, we rerun our *diff-in-disc* regressions by using false population threshold (Figure 6). Second, we used "fake" reform dates. Both falsification tests provide the expected results, namely, "fake" reforms do not appear to induce significant changes in our variables of interest (Table 8).

5.3) Heterogeneity and underlying mechanisms

While we tend to attribute the effects of the fiscal rules on planning accuracy to the reduction in the "planning to cheat" motive, in principle there might be other mechanisms at play, all mutually compatible. For example, another possible interpretation of our findings is that accuracy requires "effort"[11], and that the incentives to exert effort are in turn increased by tighter fiscal rules: in this sense, the new rules would alleviate a bureaucratic, as opposed to a political, agency problem.

Furthermore, policy-makers may be unable to implement their investment plans due to collective action problems. Investment plans are the outcome of an agreement among a set of powerful legislators, each of which has his/her own pet project (Williams, 2017).[12] These agreements are often written on water and reneged ex-post, which results in the cancellation of many of the original projects. Project "inflation" at the planning stage is arguably an increasing function of the degree of fragmentation of the governing coalition. Tighter fiscal rules may then mitigate the effect of fragmentation as they raise (and makes more salient) the opportunity cost of including in the initial budget projects which have a low completion probability to start with.

While it is difficult to discriminate between these different narratives (political agency, bureaucratic agency, fragmentation) in our empirical set up, looking at whether our results are heterogeneous across different subsamples can provide a better understanding of the reason(s) why the reform was effective.

We therefore perform three split-sample exercises, analysing separately municipalities with high (resp. low) percentage of informed citizens, high (resp. low) degree of political fragmentation and municipalities in which the mayor is (resp. is not) re-eligible. These

[11] Of course, "effort" is here a shorthand to capture the idea that increasing forecast accuracy is costly. For example, as documented by the recent literature on private firms' forecasts, it might be necessary to improve computing capacity, hire better-skilled personnel or improve management practices (Bloom *et al.*, 2020, 2021).

[12] Differently from Williams (2017), which has project-level data, our analysis is limited to budgetary data, so we cannot ascertain whether over-optimism of spending plans reflects the fact that certain projects are simply not started, delayed or interrupted. This would be a very interesting avenue for further research. Indeed, the Ministry of Infrastructure and Transport manages a database of unfinished public work ("Anagrafe delle opere incompiute").

exercises have several limits. First, the various sub-samples are small and the units within each of them are quite homogeneous, so the estimates lack precision (this is more problematic if one tries to use the diff-in-disc method; for this reason, we just look at diff-in-diff estimates). Second, some of the variables we use to split our original sample are imprecisely measured. We proxy voters' information with newspaper readership, which – while it is a very standard measure in the literature – is measured only at the province level and is partly outdated, as many people may get information online. We measure council fragmentation using the Herfindal index which is a well-established indicator. However, our data on the party composition of the city councils are noisy; in particular, sometimes they may not distinguish between different civic – i.e. non party – political groups ("*liste civiche*").

With this caveats in mind, running equation (1) on these different subsample produces interesting results. It turns out that the effect of the new fiscal rules: (i) is less strong, but still present, in less fragmented municipalities (Table 9, panel A); (ii) is stronger in less-informed environments (Table 9, panel B); (iii) is only present whenever the major is re-eligible (Table 9, panel C).

Findings (ii) and (iii) corroborate our political economy interpretation, as they show that tighter rules are more helpful when the political agency problem (i.e. the planning-to-cheat incentive) is more relevant, either because the voters are less informed (therefore more subject to fiscal illusion), or because the major is looking for re-election. In particular, if the "bureaucratic agency" was the *only* true hypothesis, major re-electability should not play a role, at least *prima facie*. Finally, finding (i) suggests that both the fragmentation mechanism and our planning-to-cheat motive might be simultaneously at play.

6. Conclusions

Improving the reliability of public investments projections should be a paramount policy concern: from a political viewpoint, it increases accountability, helping citizens to assess the choices and performances of incumbent politicians; from an economic viewpoint, it reduces the uncertainty faced by firms and therefore raises private investment and productivity.[13] Not surprisingly, the IMF transparency code underlines that "Economic and fiscal forecasts and budgets should be credible". To give a sense of the quantitative relevance of the issue at stake, if we projected our estimation results on the universe of the 8000 Italian municipalities, we would

[13] See e.g. Bloom (2009), Tanaka *et al.* (2020).

find that the aggregate planning error would amount to around 25 billion euros, equal to almost 60 per cent of the total public investment expenditure (or, equivalently, and to about 1.6% of GDP). Indeed, the data show that planning inaccuracy is much larger in large municipalities that – for econometric reasons that by now are familiar to the reader – are left out from our estimates. The aggregate size of the phenomenon is also relevant outside the time window that we consider (see Figure 7).

Our analysis has documented that public investment plans suffer from large and systematic over-optimism – almost half of planned expenditures are not realized ex post – and that a cap on capital spending contributes to a significant extent – actually, by about one third – to reduce this bias. Both findings can be rationalized if we assume that citizens suffer from fiscal illusion (i.e. that they tend to overestimate the net benefits of public expenditures) and at the same time pay more attention to plans than to the implementation phase. Indeed, these characteristics of the electorate provide a powerful incentive for rational self-interested policy-makers to introduce an upward bias in their investment forecasts. Introducing a cap on spending arguably makes such a strategy more difficult to pursue, as now even (at least some) inattentive citizens may question the credibility of ambitious investment plans are in contrast with a fiscal rule.

Our results are of course subject to several limitations. First, while the within-country approach enhances the internal validity of our econometric results, it might reduce their external validity because differences in national institutions could interact with fiscal rules.

Second, while we show that fiscal rules have improved fiscal transparency in one dimension – curbing official prediction errors – we cannot exclude that they may have reduced transparency in other dimensions, for example by increasing the manipulation of *off-balance sheet* items. We know both from theory (Milesi-Ferretti, 2004) and from cross-country (von Hagen and Wolff, 2006; Reischmann, 2016) and Italian (Balduzzi and Grembi, 2011) empirical evidence that this might indeed be the case.

Third, the effects of the DSP reform may increase over time, either because the new rules gain credibility (do not forget the poor record of the DSP in terms of stability of the norms and stringency of implementation) or because local governments need time to improve their planning capability. In this case, one could argue that our estimates represent a lower bound.

These caveats notwithstanding, our results have relevant policy implications.[14] In particular, they suggest that reliance on numerical fiscal rules to promote investment might be misplaced,

[14] Incidentally, these implications appear valid under all the three "narratives" discussed in Section 5.3 (political agency, bureaucratic agency, fragmentation).

at least in normal times. Before the reform we study in this paper, Italian municipalities were subject to a "golden rule" (i.e., capital expenditures were not constrained by the DSP).[15] However, we have shown that this lack of formal constraints only produced inflated capital budgets, with no tangible benefits in terms of actual investment.[16] This suggests that policy emphasis should shift from numerical to procedural rules and, more generally, to sound public investment governance practices (Basdevant *et al*., 2020; IMF, 2018; OECD, 2017). For example, the possibility to shift funds between current and capital spending projects should be limited; priority in financing should be granted to the completion of already approved plans before launching new projects (Williams, 2017); managerial autonomy should be promoted (Rasul and Rogger, 2018); projects should be accompanied by realistic cost estimates (Allen *et al*., 2020). Last but not least, disbursement of funds from higher levels of government should also be conditional to the actual advancement of investment projects.[17]

[15] For an introduction to "golden rules", cfr. Balassone and Franco (2000) and Blanchard and Giavazzi (2004).

[16] The qualification "in normal times" is important. In crisis times, when governments are obliged to severe fiscal consolidation strategies. The temptation of cutting down investments to be compliant with the rules is strong (as the experience of the 2009-2011 crisis in Europe has thought us). In this context, having a "golden rule" instead of a more standard balance-budget rule in place can be relevant to "protect" public investment (European Fiscal Board, 2020).

[17] Italian authorities have recently launched what is the most ambitious investment plan in decades, funded by the Next-Generation EU program (https://italiadomani.gov.it/en/home.html). European resources will be transferred to each country only if public administrations provide evidence that the planned investments and reforms are being implemented in a timely and efficient manner. An appropriate governance framework at the national level is a precondition for obtaining the EU funds.

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TABLES

Table 1: Structure of the Italian Municipal Budgets

Revenues	Expenditures
Title 1: Current Tax revenues	*Title 1:* Current expenditures
Title 2: Current transfers from other levels of Government	*Title 2*: Capital expenditures (of which Investments)
Title 3: Current non-tax revenues (fees)	*Title 3*: Loan repayments
Title 4: Capital Revenues (Disposal of fixed assets, capital transfers from other level of government and private entities)	
Title 5: Borrowing	

Table 2: changes in Domestic Stability Pact rules for municipalities with pop. above 5,000(1)

	2001	2002	2003	2004	2005	2006	2007	2008
Current Account Deficit	3% growth w.r.to 2000	2.5% growth w.r.to 2001	no growth w.r.to 2001	no growth w.r.to 2003				
Current Expenditures		6% growth w.r.to 2000			10% growth w.r.to 2001-03 average	-8% growth w.r.to 2004		
Capital Expenditures					10% growth w.r.to 2001-03 average	-8% growth w.r.to 2004		
Overall deficit (Including Capital Expenditures)							8% growth w.r.to 2003-05 average	
Overall accrual deficit+ Overall cash deficit								8% growth w.r.to 2003-05 average

(1) Municipalities with a population below 5,000 are exempted from the rules of the DSP.

Table 3: Descriptive statistics

	Years 2001-2004		Years 2005-2007	
	Below 5000	Above 5000	Below 5000	Above 5000
Forecasted				
Capital expenditures (title 2)	4267	5806	4006	4690
Investments (title 2)	4175	5704	3929	4536
Primary current expenditures (title 1)	2808	4589	2925	4588
Capital revenues (title 4)	3156	4142	3163	3679
Current revenues (title 1 and 3)	2265	3985	2598	4379
Realised				
Capital expenditures (title 2)	2156	3087	1841	2571
Investments (title 2)	2111	3015	1796	2473
Capital revenues (title 4)	1505	1982	1300	1796
Primary current expenditures (title 1)	2804	4.565	2910	4568
Current revenues (title 1 and 3)	2342	4162	2590	4371
Forecast error				
Capital expenditures (title 2)	2111	2718	2165	2120
Investments (title 2)	2064	2690	2133	2063
Capital revenues (title 4)	1649	2159	1864	1882
Primary current expenditures (title 1)	4	24	15	
Current revenues (title 1 and 3)	-77	-177	-8	21
Other controls				
Fiscal autonomy I (own revenues on total revenues) (%)	34.6	42.0	45.2	51.2
Fiscal autonomy II (interest exp. on total revenues) (%)	3.3	3.4	3.3	3.4
Average individual income	44.101	78.495	47.055	84.187
Population	3745	6318	3820	6522
North (%)	58.3	58.0	58.3	58.0
Center (%)	15.7	15.4	15.7	15.4
South (%)	26.0	26.5	26.0	26.5
Year of the legislature	2.1	2.0	1.9	2.0
Observations (n.)	2820	2312	2115	1734
		Memo items		
Forecasted (per capita values)				
Investments (title 2)	1.109	0.919	1.054	0.716
Prim. Curr. expenditures (title 1)	0.749	0.726	0.764	0.704
Capital revenues (title 4)	0.929	0.765	0.936	0.672
Current revenues (title 1 and 3)	0.604	0.633	0.678	0.674
Realised (per capita values)				
Investments (title 2)	0.558	0.481	0.471	0.381
Prim. Curr. expenditures (title 1)	0.748	0.723	0.759	0.701
Capital revenues (title 4)	0.489	0.408	0.426	0.376
Current revenues (title 1 and 3)	0.624	0.661	0.675	0.673

Expenditures and revenues are in thousands of 2012 Euros unless otherwise stated. Municipalities between 3,000 and 8,000 inhabitants in all regions but Valle d'Aosta, Trentino Alto Adige, Friuli Venezia Giulia, Sicilia and Sardegna. Years: 2001-2007.

Table 4: The effect of fiscal rules on forecasting errors

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
				Diff-in-diff			
Investment	-745.689***	-503.309***	-954.943***	-589.080***	-1038.192***	-641.079***	-710.221***
	[193.182]	[118.563]	[247.904]	[143.913]	[247.042]	[143.267]	[158.170]
Obs	8981	8803	7662	7516	7662	7516	7662
Capital Revenues	-577.394***	-321.708***	-771.277***	-382.904***	-832.547***	-418.303***	-521.073***
	[184.222]	[106.775]	[236.553]	[130.551]	[236.141]	[130.230]	[145.212]
Obs	8981	8803	7662	7519	7662	7519	7662
				Diff-in-disc – CCT bandwidth *(Exp.=712, Rev.=681)*			
Investment	-2368.677	-745.561	-4259.913**	-2539.236**	-4286.460**	-2566.652**	-2786.840**
	[1822.383]	[1101.776]	[2164.497]	[1218.605]	[2154.743]	[1210.508]	[1364.884]
Obs	1970	1936	1666	1637	1666	1637	1666
Capital Revenues	-2593.371	-664.455	-3808.707*	-2023.659*	-3781.985*	-1978.947*	-2263.042*
	[1758.543]	[1004.938]	[2081.183]	[1138.478]	[2074.155]	[1133.556]	[1269.944]
Obs	1869	1839	1582	1558	1582	1558	1582
				Diff-in-disc – MSE bandwidth *(Exp.=559, Rev.=502)*			
Investments	-2003.342	-763.808	-3722.392*	-3168.564**	-3830.170**	-3269.860**	-3109.072**
	[1772.039]	[1400.158]	[1915.562]	[1568.037]	[1847.531]	[1530.385]	[1541.764]
Obs	1425	1401	1200	1180	1200	1180	1200
Capital Revenues	-1698.371	303.380	-3649.846*	-1841.620	-3580.441*	-1778.338	-2607.675
	[1744.648]	[1195.769]	[2028.202]	[1411.075]	[2001.300]	[1393.425]	[1605.115]
Obs	1225	1204	1039	1021	1039	1021	1039
Geo controls			x	x	x	x	x
Financial controls					x	x	x
Trim. 1%		x		x		x	
Win. 1%							x

Thousands of Euros. Municipalities between 3,000 and 8,000 inhabitants. Years: 2001-2007. All monetary variable are in 2012 euros. Estimates of tightening fiscal rules for municipalities above 5,000 inhabitants. Geographical controls: population, centre and south dummies, average municipal income, years elapsed since the beginning of the legislature. Financial controls: importance of own resources, interest burden. Diff in disc: Local Linear estimation strategy with optimal bandwidth *h* chosen either using the Calonico *et al.* (2014a) or the mean squared error approach. In models 2, 4 and 6 the dependent variable was trimmed at 1% on both sides. In model 7 the dependent variable was winsorized at 1% level on both size. Standard errors are clustered at the municipality level.

Table 5: The effect of fiscal rules on forecasting errors – per capita level

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
				Diff-in-diff			
Investment	-0.135***	-0.067***	-0.200***	-0.076**	-0.218***	-0.086***	-0.117***
	[0.043]	[0.025]	[0.055]	[0.031]	[0.055]	[0.031]	[0.034]
Obs	8981	8803	7662	7515	7662	7515	7662
Capital Revenues	-0.122***	-0.054**	-0.179***	-0.055**	-0.193***	-0.062**	-0.092***
	[0.041]	[0.023]	[0.053]	[0.028]	[0.053]	[0.028]	[0.031]
Obs	8981	8803	7662	7517	7662	7517	7662
				Diff-in-disc – CCT bandwidth *(Exp.=708, Rev.=682)*			
Investment	-0.515	-0.075	-0.892*	-0.421*	-0.900*	-0.429*	-0.551*
	[0.384]	[0.217]	[0.467]	[0.240]	[0.466]	[0.238]	[0.287]
Obs	1957	1929	1656	1632	1656	1632	1656
Capital Revenues	-0.539	-0.089	-0.793*	-0.349	-0.785*	-0.340	-0.444*
	[0.369]	[0.206]	[0.445]	[0.231]	[0.444]	[0.230]	[0.262]
Obs	1871	1843	1584	1562	1584	1562	1584
				Diff-in-disc – MSE bandwidth *(Exp.=565, Rev.=502)*			
Investments	-0.360	-0.069	-0.772*	-0.621**	-0.793**	-0.644**	-0.631**
	[0.354]	[0.268]	[0.393]	[0.305]	[0.378]	[0.297]	[0.307]
Obs	1444	1424	1214	1197	1214	1197	1214
Capital Revenues	-0.495	-0.085	-0.761**	-0.490*	-0.742**	-0.471*	-0.579**
	[0.330]	[0.230]	[0.372]	[0.279]	[0.359]	[0.272]	[0.293]
Obs	1306	1286	1102	1087	1102	1087	1102
Geo controls		X	x	X	X	X	X
Financial controls					X	X	X
Trim. 1%		X		X		X	
Win. 1%							X

Thousands of Euros. Municipalities between 3,000 and 8,000 inhabitants. Years: 2001-2007. All monetary variable are in 2012 euros. Estimates of tightening fiscal rules for municipalities above 5,000 inhabitants. Geographical controls: population, centre and south dummies, average municipal income, years elapsed since the beginning of the legislature. Financial controls: importance of own resources, interest burden. Diff in disc: Local Linear estimation strategy with optimal bandwidth *h* chosen either using the Calonico *et al.* (2014a) or the mean squared error approach. In models 2, 4 and 6 the dependent variable was trimmed at 1% on both sides. In model 7 the dependent variable was winsorized at 1% level on both size. Standard errors are clustered at the municipality level.

Table 6: The effect of fiscal rules on forecasting errors – other dependent variables

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
				Diff-in-diff			
Current exp.	-11.849	-2.908	-37.266**	-11.661	-37.670**	-10.843	-18.275
	[14.512]	[7.766]	[18.152]	[9.766]	[18.155]	[9.752]	[11.191]
Obs	8981	8803	7662	7516	7662	7516	7662
Current revenues	102.370***	86.447***	28.632	42.196***	16.798	32.635**	30.718*
	[16.206]	[11.369]	[21.315]	[14.965]	[21.096]	[14.646]	[15.974]
Obs	8981	8803	7662	7512	7662	7512	7662
				Diff-in-disc – CCT bandwidth *(Exp.=818, Rev.=611)*			
Current exp.	20.604	43.150	-70.928	-11.833	-71.535	-13.571	-33.994
	[77.985]	[42.476]	[88.821]	[51.080]	[88.686]	[51.053]	[62.157]
Obs	2425	2381	2069	2030	2069	2030	2069
Current revenues	61.675	152.802***	61.675	152.802***	6.801	107.195**	94.192*
	[105.966]	[46.671]	[105.966]	[46.671]	[107.604]	[47.559]	[49.525]
Obs	3207	3148	3207	3148	3207	3148	3207
				Diff-in-disc – MSE bandwidth *(Exp.=610, Rev.=557)*			
Current exp.	-33.844	15.068	-120.900	-49.621	-120.294	-54.000	-93.074
	[100.244]	[56.781]	[113.774]	[65.695]	[112.983]	[65.973]	[79.738]
Obs	1591	1557	1340	1309	1340	1309	1340
Current revenues	54.881	189.314***	54.881	189.314***	-11.551	139.447***	108.028**
Financial controls					X	X	X
Trim. 1%		X		X		X	
Win. 1%							X

Thousands of Euros. Municipalities between 3,000 and 8,000 inhabitants. Years: 2001-2007. All monetary variable are in 2012 euros. Estimates of tightening fiscal rules for municipalities above 5,000 inhabitants. Geographical controls: population, centre and south dummies, average municipal income, years elapsed since the beginning of the legislature. Financial controls: importance of own resources, interest burden. Diff in disc: Local Linear estimation strategy with optimal bandwidth *h* chosen either using the Calonico *et al.* (2014a) or the mean squared error approach. In models 2, 4 and 6 the dependent variable was trimmed at 1% on both sides. In model 7 the dependent variable was winsorized at 1% level on both size. Standard errors are clustered at the municipality level.

Table 7: The effect of fiscal rules on levels

	Diff in Diff		Diff in disc			
			CCT		MSE	
	Forecasted	Realised	Forecasted	Realised	Forecasted	Realised
			Model 5			
Investment	-1328.925***	-290.734**	-4016.517*	-669.445	-3634.352*	74.529
	[258.956]	[128.540]	[2210.945]	[720.465]	[1960.045]	[996.747]
H			*691*	*697*	*562*	*512*
Obs	7662	7662	1610	1631	1205	1105
Capital Revenues	-807.699***	24.848	-2955.697	1014.122	-3197.349*	996.444
	[249.067]	[126.010]	[2374.650]	[803.896]	[1915.569]	[1038.431]
H			*615*	*604*	*528*	*478*
Obs	7662	9.242	1359	1309	1111	960
			Model 6			
Investment	-865.318***	-150.783**	-2138.874	-565.172	-2675.357	-235.002
	[157.122]	[66.811]	[1344.663]	[438.587]	[1627.042]	[599.861]
H			*691*	*697*	*562*	*512*
Obs	7514	9. 7525	1591	1603	1191	1091
Capital Revenues	-362.308**	97.793	-609.773	60.240	-1614.984	-256.570
	[145.668]	[65.345]	[1415.631]	[530.446]	[1571.454]	[700.883]
H			*615*	*604*	*528*	*478*
Obs	7523	7531	1343	1295	1100	950

Thousands of Euros. Municipalities between 3,000 and 8,000 inhabitants. Years: 2001-2007. All monetary variable are in 2010 euros. Estimates of tightening fiscal rules for municipalities above 5,000 inhabitants. Geographical controls: population, centre and south dummies, average municipal income, years elapsed since the beginning of the legislature. Financial controls: importance of own resources, interest burden. Diff in disc: Local Linear estimation strategy with optimal bandwidth *h* chosen either using the Calonico *et al.* (2014a) or the mean squared error approach. The dependent variable was trimmed at 1% on both sides of te threshold. Standard errors are clustered at the municipality level.

Table 8: Placebo analysis – different year of treatment

	2003		2004	
	CCT		CCT	MSE
Investment	-1304.420	-1706.437	-1938.694*	-2751.662*
	[1053.620]	[1304.367]	[1143.318]	[1458.772]
Obs	1637	1180	1637	1180
Capital revenues	-1645.760	-1141.343	-1753.203	-1518.102
	[1008.881]	[1142.360]	[1093.677]	[1272.242]
Obs	1558	1021	1558	1021
Current expenditures	-8.138	-36.571	-34.971	-79.446
	[47.491]	[61.924]	[47.481]	[61.765]
Obs	2030	1309	2030	1309
Current revenues	41.069	57.766	38.656	68.217
	[50.712]	[56.619]	[46.156]	[51.320]
Obs	3148	2523	3148	2523
Geographical controls	X	X	X	X
Financial controls	X	X	X	X
Trimming 1%	X	X	X	X

Thousands of Euros. Municipalities between 3.000 and 8.000 inhabitants. Years: 2001-2007. All monetary variable are in 2010 euros. Estimates of tightening fiscal rules for municipalities above 5,000 inhabitants. Geographical controls: population, centre and south dummies, average municipal income, years elapsed since the beginning of the legislature. Financial controls: importance of own resources, interest burden. Financial controls: fiscal autonomy and debt balance. Diff in disc: Local Linear estimation strategy with optimal bandwidth *h* chosen either using the Calonico *et al.* (2014a) or the mean squared error approach. The dependent variable was trimmed at 1% on both sides. Standard errors are clustered at the municipality level.

Table 9: Split-sample analysis

	Newspapers		Council frag.		Re-eligible major	
	High	Low	High	Low	Yes	No
Investments	-406.965***	-840.297***	-948.0***	-504.26**	-652.3***	-457.529
	[121.348]	[278.334]	[214.670]	[227.278]	[201.611]	[331.824]
Obs	4126	3390	3795	3721	4689	2823
Capital Revenues	-249.626**	-539.555**	-627.0***	-385.723*	-429.29**	-209.139
	[99.095]	[259.818]	[190.880]	[210.703]	[183.069]	[300.249]
Obs	4127	3392	3723	3796	4690	2825

Thousands of Euros. Municipalities between 3,000 and 8,000 inhabitants. Years: 2001-2007. All monetary variable are in 2012 euros. Estimates of tightening fiscal rules for municipalities above 5,000 inhabitants. Geographical controls: population, centre and south dummies, average municipal income, years elapsed since the beginning of the legislature. Financial controls: importance of own resources, interest burden. The dependent variable was trimmed at 1% on both sides. Standard errors are clustered at the municipality level. Municipalities are classified as: (1) "high newspaper reading" if the population ratio that reads a newspaper at least once a week is greater than the sample median (61,6 per cent); (2) "highly fragmented" if the Herfindahl index of the city council is lower than the sample median (0,66); (3) major not re-eligible" if the major is already in his second term.

FIGURES

Figure 1

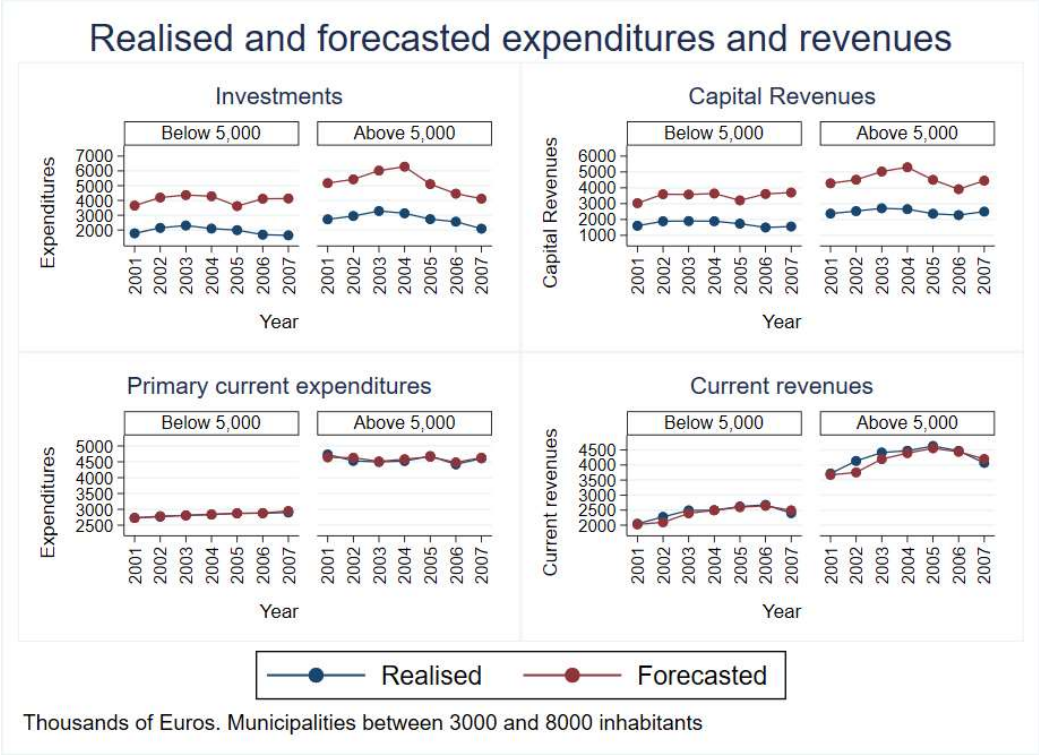


Figure 2

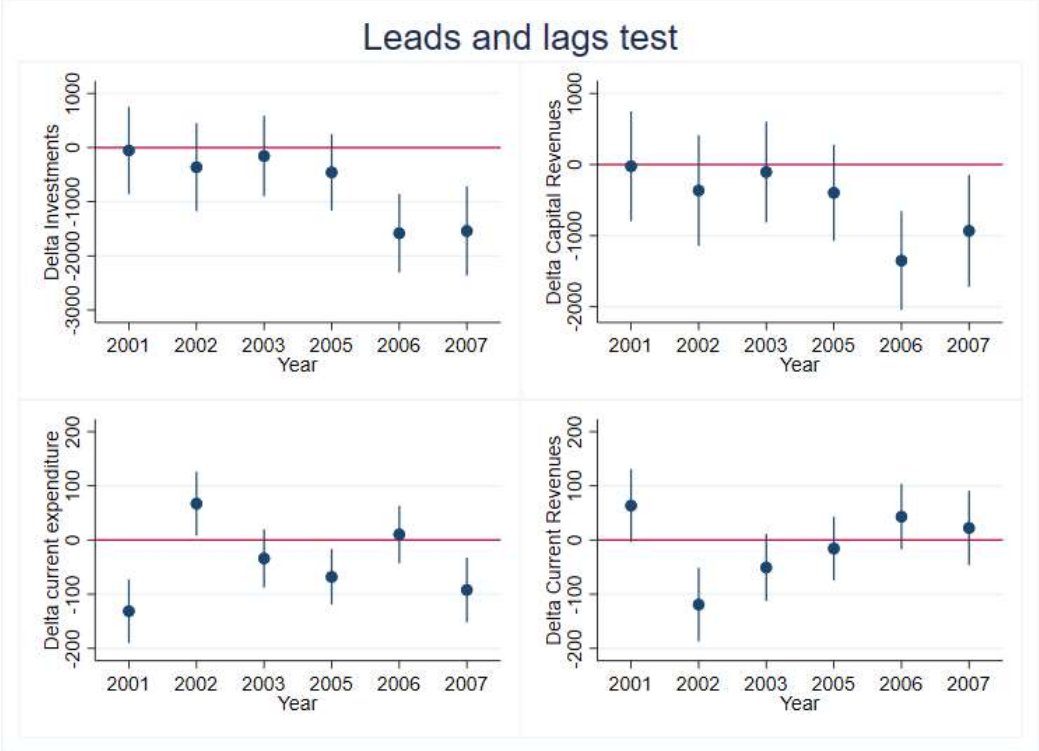
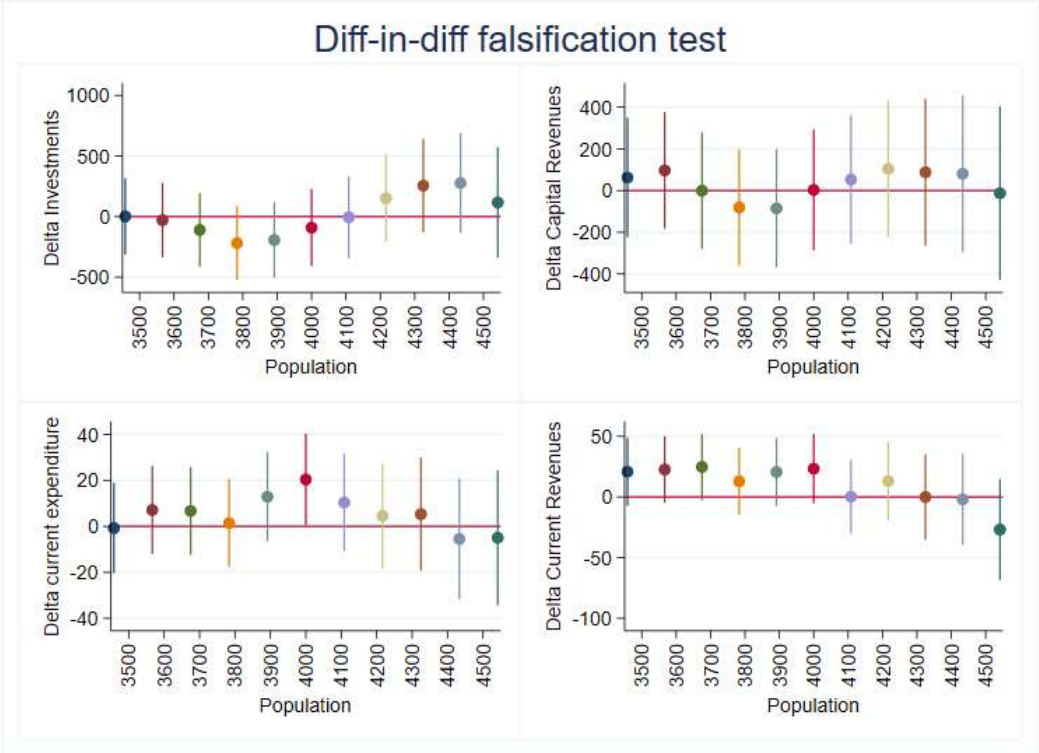
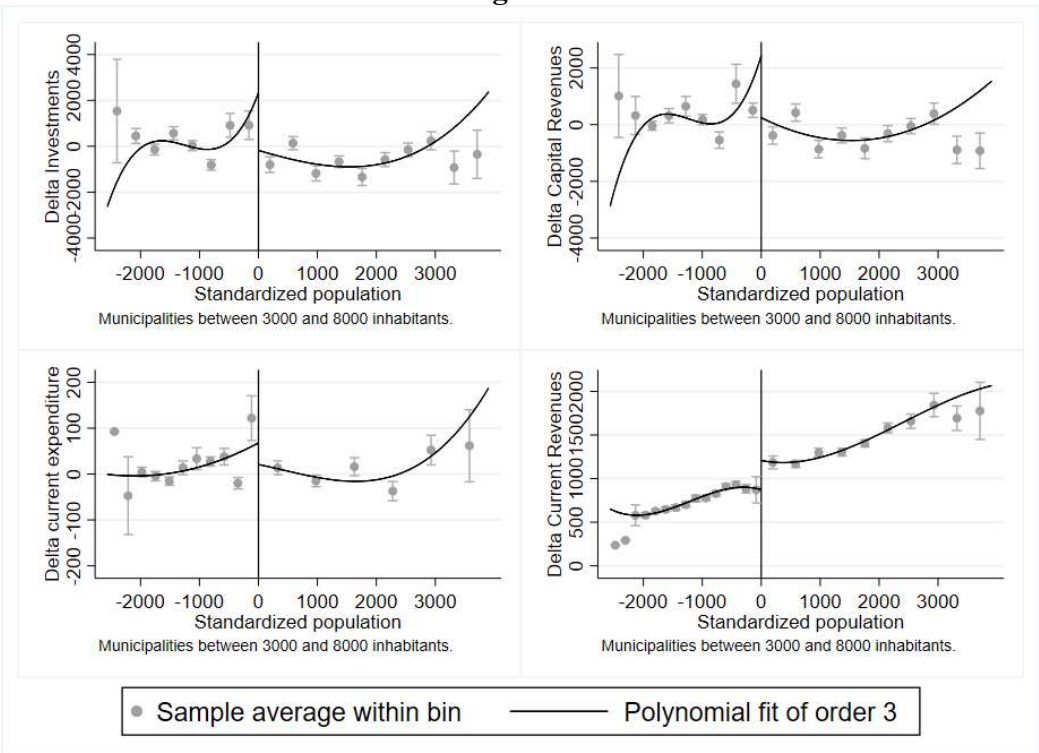


Figure 3



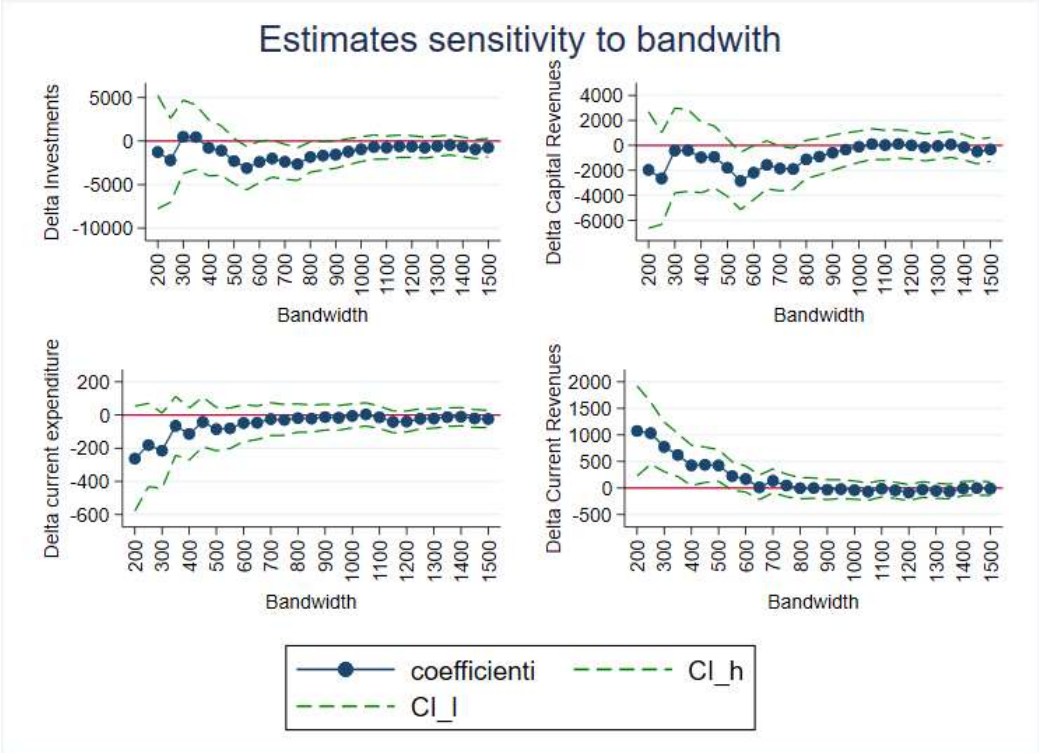
Notes. Coefficients in a difference-in-differences specification on municipalities with less than 5.000 inhabitants. The treatment is defined as being above different population thresholds (shown in the X axis). The regression includes town fixed effects as well as geographical and financial controls. Dependent variable trimmed at 1% on both sides. For each threshold, we report the point estimate and the 90% confidence interval.

Figure 4



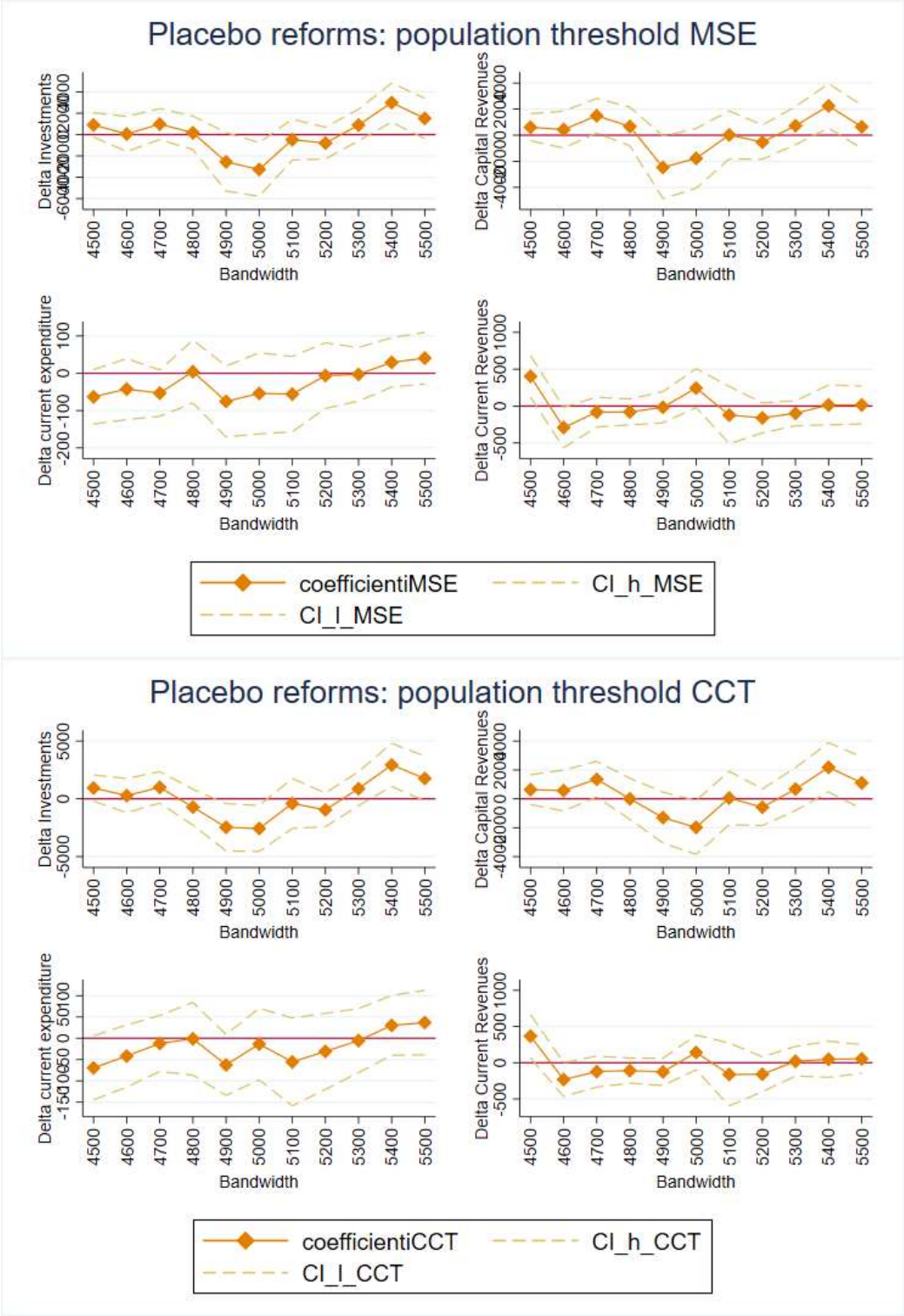
Notes. Global polynomial of 3rd order and local sample means with bin selected with IMSE-optimal evenly-spaced method using spacing estimators (confidence interval at 90%). Y-axis: difference between the forecast error before and after 2004. X-axis: legal population at 2004 normalized at the threshold = 5,000.

Figure 5



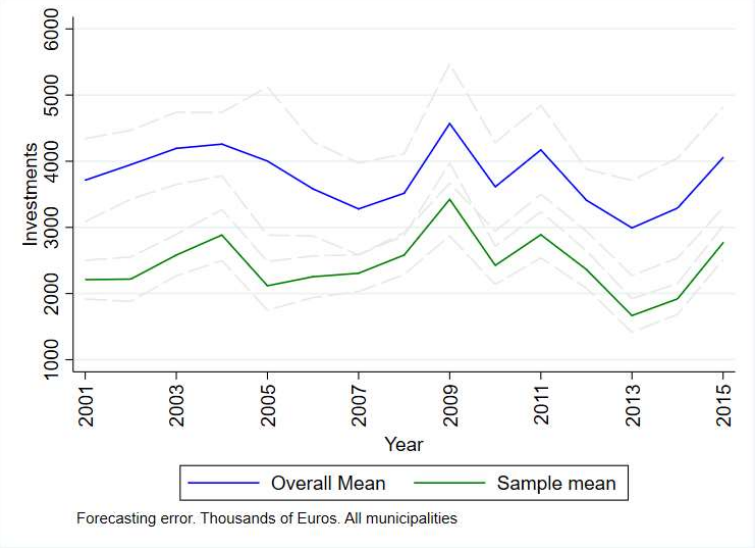
Notes. Diff-in-disc coefficients (estimated with controls) for different values of the bandwidth (with the confidence interval at 90%). Y-axis: Diff-in-disc estimates of the impact of the DSP for municipalities larger than 5,000 inhab. Method: local linear regression with triangular kernel. X-axis: Bandwidth (absolute values, inhabitants).

Figure 6



Notes. Diff-in-disc coefficients (estimated with controls) for different values of the population threshold (with the confidence interval at 90%) using two different bandwidths. Y-axis: Diff-in-disc estimates of the impact of the DSP for municipalities larger than the inhabitant level shown I the X axis. Method: local linear regression with triangular kernel. X-axis: inhabitants.

Figure 7


Investments
1000
2000
3000
4000
5000
6000
2001
2003
2005
2007
2009
2011
2013
2015
Year
Overall Mean
Sample mean
Forecasting error. Thousands of Euros. All municipalities

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