

Fiscal rules and the reliability of budgetary plans: evidence from local governments

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**PRELIMINARY VERSION, PLEASE DO NOT QUOTE WITHOUT
PERMISSION**

Abstract. – Notoriously, fiscal plans often differ markedly from fiscal outcomes. In the present paper, we aim at assessing the determinants of these implementation errors. In particular, we study to what extent they are reduced by the imposition of numerical fiscal rules. We use a unique data set including a wide sample of Italian municipalities, for which we have access to both the initial budget (which records planned revenues and expenditures) and the year-end budget (which records *actual* revenues and expenditures). To capture the causal effect of budgetary rules we exploit a reform which introduced stricter rules concerning capital expenditures in a subset of Italian municipalities (those with more than 5000 residents) starting from 2005. This reform gives us a natural control group (municipalities with less than 5000 residents). Using a difference-in-differences estimation strategy we find that the introduction of the rule reduced the gap between planned and realized capital expenditures.

JEL: H68, H72, H83.

KEYWORDS: budget rules, budget execution, local public finance

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1. Introduction¹

It is quite common, in the public sector as well as in private enterprises, that planned budgets turn out to be different from realized budgetary figures. In the public sector, in particular, budgetary slippages can be sizable and have momentous effects. A notorious recent example is Greece: when in October 2009 the government acknowledged that the deficit for 2009 would be 3% of GDP higher than what was previously planned, this spurred a sovereign debt crisis that ultimately led to a default, and spread tensions to other euro area countries as well.

Even if, in most cases, consequences are not so dramatic, it is clear that reliable public budgets are important to anchor economic agents' expectations (IMF, 2009). Furthermore, they are key to assess policy-makers' behaviour and keep them accountable. For this aspect, as well, the Greek example is typical: budgetary problems become apparent only after a general political election took place, so during the campaign the voters could not judge the incumbent government for its true fiscal record.

The present paper aims at understanding better what determines budget implementation errors (the gap between planned and realized outturns) and how they can be reduced. We focus in particular on the effectiveness of fiscal rules and on the importance of cultural characteristics of the polity.

We exploit a unique dataset including *ex ante* budget plans as well as *ex post* budget outcomes of a wide sample of Italian municipalities. We assess the effects of a change in the budget rule involving municipalities with more than five thousand residents, called *Patto di Stabilità Interno* (Domestic Stability Pact, henceforth DSP): starting from 2005, the budget aggregate relevant for the compliance of the DSP included capital expenditures.

To give a preview of our results, in our sample there is evidence of systematic and sizable overshooting of planned capital expenditures with respect to realized outcomes. The former is higher than the latter in about 2/3 of the municipalities, with planned expenditures about 40% higher than realized expenditures on average (56% if one considers only municipalities in which the difference is positive). In the municipalities subject to the new more stringent rules, implementation errors were

¹ We thank Guido de Blasio, Guglielmo Barone, Emilio Vadalà and seminar participants at the Bank of Italy for useful suggestions. We keep responsibility for all remaining errors. The views expressed in the paper do not necessarily reflect those of the Bank of Italy.

significantly reduced. In these municipalities, planned expenditures were on average about 330 euros per capita higher than realized before the reform; after the reform this gap went down to 220 euros. Furthermore, the effect of the rule is much stronger in high-civiness municipalities, pointing to a complementarity between formal and informal rules.

What is the mechanism through which the reform achieved this effect? While we are aware of no formal theoretical model explaining the political economy of budget execution, we believe that our results can be rationalized as follows. It is often argued that voters overestimate the effects of public spending and underestimate the cost of taxation and/or of deficits (for a classical exposition of this “fiscal illusion” see Buchanan and Wagner, 1977). Moreover, as emphasized by Beetsma et al. (2009), “*while fiscal plans draw a lot of attention, (...) implementation receives much less attention*”. If these two assumptions hold true, policy-makers have an incentive to “plan to cheat”². That is, they promise an amount of expenditures higher than what they will actually want to put in place because this behaviour allows them to present budgets that are acceptable to voters, and at the same time preserves fiscal discipline. 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 ex ante budget is no more in place. Against this background, imposing a provision like the one included in the DSP since 2005 contributes to bring plans in line with outcomes for two reasons. First, the appetite of voters for high capital spending is reduced, as this now entails a cost in terms of reduced current expenditures and/or higher taxes. Second, the possibility to cheat the voters with generous fiscal plans would be reduced as well: promising high expenditures would not be credible, as it would be patently in contrast with the fiscal rule.

Econometric research on budget execution has been scant, notwithstanding the practical importance of the issue.

Among the very few papers on the issue, both Beetsma et al, (2009) and Von Hagen (2010) find that implementation errors are smaller in countries that adopt

² The expression is taken by Beetsma et al. (2009).

numerical fiscal rules.³ Pina, and Venes (2011) find similar results, but only if numerical *expenditure* rules are in place.⁴ All the three papers consider a small panel of European countries. They discuss only national-level rules, with the exception of Pina and Venes (2011) which study the impact of a supra-national rule (the Stability and Growth Pact, which is enforced by the EU authorities) and find that the introduction of such rule increases budget implementation errors. A similar conclusion about the European Stability and Growth Pact is drawn by Frankel (2011), which studies a slightly larger panel of countries (33). Finally, we should mention Beetsma et al. (2013), which focuses on the budget implementation performance of the Dutch central government since the end of the second world war. They show that in the last part of their sample, in which budgetary institutions were significantly reformed, implementation has also significantly improved.

With respect to the above-mentioned papers, the present contribution constitutes an improvement along several dimensions. First, resorting to local government data, we are able to increase enormously the sample size. Second, as we consider within-country data, we reduce significantly the problems related to omitted variables and more generally we do not need to make the strong assumption of cross-country homogeneity, as we consider units which substantially share the same set of rules and institutions. Third, our set up provides us with a natural control group (the municipalities not subject to the DSP), which can be used as a benchmark to assess the causal impact of the reforms. This addresses the main empirical problem in the literature, namely the suspect that differences in rules simply reflect differences in the underlying fiscal preferences of the citizens (Poterba, 1997). Finally, we go beyond previous contributions by considering culture as a further explanatory variable. Indeed, several contributions suggest that cultural traits have an impact on public policies. La Porta et al. (1997), La Porta et al. (1999) and Knack (2002), and Giordano and Tommasino (2013) look at the overall quality of public action. Algan et al. (2011) and Camussi et al. (2013) focus on the composition of expenditures.

³ The latter also emphasizes the role of budgetary procedures, and in particular notices that errors are smaller if the governing coalition agrees, at the government formation stage, on a politically binding multi-year fiscal program.

⁴ As opposed to rules constraining the budget balance.

More generally, our paper adds to the literature on the effects of fiscal rules on fiscal outcomes (see e.g. the early paper by Poterba 1994, and the more recent contribution by Debrun et al., 2009; useful surveys can be found in Poterba, 1997, IMF, 2009 and Wyplosz, 2012). In particular, the closest paper is the one by Grembi et al. (2012), which also considers Italian municipalities, but focus on a different change in the DSP (the exemption from the rules which benefited the municipalities with less than 5000 residents and which entered into force in 2001), and looks only at budget outcomes, disregarding budget plans.

Also related is the fast-growing literature on the economic and political effects of culture (on this already ample field, the authoritative survey by Guiso et al., 2011 provides perhaps the best starting point).

The rest of the paper is structured as follows. In Section 2 we review the institutional framework in which Italian municipalities operate, and describe the change in the rules which we exploit in the empirical analysis; in Section 3 we spell-out our empirical strategy; in Section 4 we present our dataset; in Sections 5 we show our main results, together with some validity tests; Section 6 concludes, pointing to some avenues for further research and to possible policy implications of our analysis.

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 illumination, waste disposal, urban road maintenance, local transports, etc) and for social services at large (such as assistance to poor people, retirement homes, 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 10 per cent (4 per cent of Italian GDP), 25 per cent of which are

welfare-related; this share rises to 30 per cent if we consider only capital expenditure, and to slightly less than 50 per cent if we focus on investment.

The about 8,100 Italian 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 current 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.⁵

The Mayor and the City council are both elected directly by the population. Elections are held every five years, usually between April and June.

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 rules are meant to increase fiscal responsibility of local governments, ensuring that they contribute to the attainment of the fiscal targets for the General Government imposed by the European-level public finance framework (the European Stability and Growth Pact entered into force in the second half of 1998). Non-compliant municipalities incur on several sanctions, such as stricter constraints on indebtedness, intermediate consumption and personnel hiring.

Rules were subject to several revisions since their first introduction. For our purposes, the major changes were:

- After 2001, all municipalities below 5,000 were exempted from the DSP;
- After 2002, the five autonomous regions (*Regioni a Statuto Speciale*) were allowed to set their own fiscal rules for municipal governments. Two of them, however (Sicily and Sardinia), chose not to derogate the DSP, so they are the only autonomous regions that we include in the analysis;
- In 2005, the DSP changed from a “golden rule” (the requirement of a balanced budget *excluding* capital expenditure) to a stricter “balanced-budget rule” (capital expenditures could not be detracted anymore from overall expenditure). The effects of this change, as we will explain in the next Section, represent the focus of the present study.

⁵ The allocation of these grants across municipalities reflects demographic, socio-economic and fiscal indicators.

After 2005, the rules remained relatively unchanged until the end of 2007.⁶

3. Empirical strategy

As we remarked above, starting from 2005 capital expenditures have been included in the definition of budget balance, which is relevant for DSP compliance (which in turn only constrains municipalities with more than 5000 residents). This implies that increased capital expenditures cannot be financed with deficits anymore, but only by an increase in revenues or decrease in current expenditures.

We expect the increased opportunity cost of capital expenditures due to the reform to reduce the incentive to promise overly generous capital expenditures programmes, which have to be revised ex post. To assess empirically the effect of the new rules on the size of budgetary slippages, we use a difference-in-differences approach. We observe Municipal budgets both before and after the reform, so we can compare the change in the budget implementation error observed in the municipalities subject to the pact (the treatment group) with the one observed in the other municipalities (the control group).

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

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

Where DSP_i is a dummy that takes value of one if the municipality is subject to the pact and zero otherwise, T_t is a time dummy, and X_{it} is a vector of controls. Our coefficient of interest is represented of course by β_1 .

One difficulty is that treated and control groups may differ in some unobservable way. To address this problem, we consider budgetary variables in per capita terms, we control for several variables that may influence budgetary outcomes (among which, population itself), and we restrict the analysis to Municipalities which are not too far away from the 5,000-residents threshold. In particular, we do consider neither

⁶ In 2008 the requirements of the DGP were imposed not only ex post but also ex ante, so they constrained also the provisional municipal budgets. In 2009, Italian Regions were allowed to create an internal “market” between Municipalities and the Region, to exchange surpluses and deficits to fulfil the DSP requirements (Regionalizzazione verticale). This policy was first implemented on a voluntary base by six Regions: Piemonte, Lombardia, Umbria, Liguria, Toscana and Emilia Romagna.

municipalities with less than 2,000 nor those with more than 20,000 inhabitants. This leaves us with a treatment and a control group of about the same size. As an alternative, we consider a symmetric interval around the population threshold, considering the municipalities between 2,000 and 8,000 residents.⁷

Another difficulty is that other changes in the rules may confound the effect of the 2005 change (as we remarked above, the DSP has been amended several times). To avoid this difficulty, we focus most of our analysis on the two years 2004 and 2005. However, luckily for our purposes, between 2003 and 2006, there were no other significant changes to the DSP, so in some sensitivity analyses we include also these two years.

4. Data

In this Section, we briefly describe the data used in our empirical exercise. Table 1 provides descriptive statistics.

4.1) *Realized and planned municipal expenditures*

Budgetary data for municipalities come from the Budget of Italian municipalities, published by the Ministry of Interior.

Italian Municipalities are required by the end of April to present their provisional budget (*Certificati di Conto Preventivo*) in which are defined committed expenditures and revenues. Such plans may be subsequently revised until November 30th. By the end of June of the next year, Municipalities publish the year financial statement (*Certificati di Conto Consuntivo*).

The difference between final outturns, as recorded in the *Certificati di Conto Consuntivo*, and planned outturns, which can be found in the *Certificati di Conto Preventivo* represents our dependent variable.

⁷ In this reduced sample, municipalities below the threshold are about twice as many as the municipalities above the threshold.

Figure 1 shows the dynamics of provisional and final per capita current expenditures in our sample of Italian Municipalities.⁸ The figure on the left refers to Municipalities below 5,000 inhabitants; the one on the right refers to Municipalities with more than 5,000 inhabitants (DSP municipalities). Both figures show how provisional current expenditures closely track final ones for both types of Municipalities. Current expenditures are in fact more rigid and less easy to manipulate.

Conversely, capital expenditures are more subject to fluctuations. This can be seen in figure 2 which plots the dynamics of per capita capital expenditures for the two types of Municipalities. More importantly for our purposes, though provisional expenditures are higher than final ones for both types of Municipalities, the gap is smaller starting from 2005 (when capital expenditures were included in the pact for the first time) for DSP municipalities, while no such tendency emerges for municipalities not subject to the DSP. This can be seen in a clearer way in figure 3, which shows the implementation error in capital expenditures in the two groups: between 2001 and 2004, budget execution in DSP municipalities was slightly more precise than in non-DSP municipalities, and in any case the two trends were roughly parallel; after the reform implementation improved only in DSP municipalities. As a consequence, plans in DSP municipalities become in 2006 and 2007 more accurate than in non DSP municipalities.

4.2) *Civiness*

In Section 5.2 we will investigate whether civiness has an impact on the accuracy of budgetary implementation. We measure civiness as the average level of trust, which is one of the measures more often used in previous studies. In particular, our measure is taken from the survey '*Aspetti della vita quotidiana*' conducted by the Italian National Statistical Institute (ISTAT). The survey is conducted from 1993 on a yearly basis on a representative sample of the Italian population (in 2010 around 24 thousand households were interviewed, corresponding to more than 54 thousands individuals). The survey includes questions on many different aspects of the everyday life of Italians citizens, including political and social participation, public services satisfaction, health, lifestyle and so on. Starting from the 2010 wave, specific questions on trust and beliefs on trustworthiness of others were included in the questionnaire.

⁸ Data from *Certificati di conto preventivo* and from *Certificati di conto preventivo* may be subject to imputation errors and miscoding. To avoid that results are influenced by extreme values, we ignore the observations characterized by unrealistically low and/or high planned capital expenditures (in particular, we drop the observations below the 5% percentile and/or above the 95% percentile).

We use a trust question commonly used in the literature and included also in other surveys such as in the US General Social Survey (GSS) and in the WVS: "Generally speaking, would you say that most people can be trusted or that you have to be very careful in dealing with people?". Answers range from 1 "Yes" to 0 "No"⁹. We average at the provincial level individual answers to get an indicator of "generalized trust". Figure 4 shows the distribution of our trust variable across Italian provinces: higher levels of trust are concentrated in the northern part of the country, but there is substantial geographical variability even within the North.

4.3) Other controls

In the econometric analysis, we control for several factors that might affect the degree of ex post accuracy of budgetary plans. In particular, we are concerned that implementation errors are not only due to politicians' strategic behaviour but can arise due to unexpected events. To account for this possibility, we use three indicators of the flexibility of the budget, which should capture how easy it is for the policy-maker to correct unexpected and unintended budgetary developments during the fiscal year. Our first indicator captures the degrees of freedom that the municipality enjoys on the revenue side and is given by the fraction of own revenues over total revenues (*revenue-flex*). Indeed, own revenues can be partly controlled by the government, by changing local tax rates or tariffs. The second indicator captures the degree of (in)flexibility of expenditures and is given by expenditure for public employees as a fraction of current non-interest expenditures (*exp_rigidity*). Indeed, expenditure for public employees cannot be easily changed in the short-run. The third indicator is given by the ratio between interest payments and current revenues and is a proxy of the burden of debt (*debt_burden*). Again, this captures the fact that the amount of interest payments is beyond the control of the government in the short run, and also the fact that a budgetary condition which is not sound can make more difficult to cushion adverse shocks by changing the budget balance.

Another reason that may hamper the ability to manage the budget during the year is political instability. To control for this factor, we include among the regressors a

⁹ The WVS-GSS question suffers from three shortcomings. First, the WVS-GSS formulation has been criticized because the respondents have the choice between trust and caution and not between trust and distrust or cautious and incautious behaviour (see e.g. Naef and Schupp, 2009). Second, as we discussed in the introduction (see the references in footnote 7), experimental evidence shows that people often interpret the question as if it was not about beliefs but about one's own trustworthiness. Third, the question is not clear about who is to be trusted (strangers or acquaintances).

dummy variable which is equal to one if a municipal election has taken place in the year. This variable could also capture the fact that incentives to manipulate fiscal plans are stronger in election periods.

Finally, we control for municipal taxable income and for the geographical position of the municipalities, through a dummy equal to one if it belongs to one of Southern Italy.

5. Results

5.1) *Main results*

Table 2 presents the main results for the difference-in-differences estimation on the period 2004-05. In particular, our baseline specification is provided by Column (2), and shows that the change in the rules significantly reduced the implementation errors in capital expenditures in municipalities subject to the pact. The reduction is also significant in economic terms, as it is about 50 euros per capita higher than the reduction observed in the control group. To put this number in perspective, consider that the average implementation error in 2004 was about 310 euros per capita. In Table 2, we also report results of a regression with a more parsimonious specification, in which controls are omitted (Column 1) and of a regression in which we only consider municipalities below 8000 residents (Column 3). In both cases, while the magnitude of the effect is reduced and the β_1 coefficient is less precisely estimated, the result is still there qualitatively.

An interesting question is what component of the implementation error drives the result. To answer the question we run two regressions, in both cases sticking to the specification in equation (1). In the first regression (Table 2, column 4), we consider planned capital expenditures as the dependent variable, while in the second we consider realized capital expenditures as the dependent variable. It appears that the introduction of the new rules decreased only planned expenditures.

Finally, we extend our sample including also the observations from 2003 (a pre-treatment year) and 2006 (a post-treatment year). Results (reported in Table 3) are in line – actually stronger - with those previously discussed.

5.2) *Validity tests*

The difference-in-differences approach relies on the assumption that, without the treatment, the change in the outcome variable for the treated population would have been the same as the change observed for the control group, conditional on the control variables (*common trend assumption*). The common trend assumption is not directly testable, as it relies on a counter-factual scenario. However, we can indirectly assess its plausibility. A simple eye-ball test in our case seems consistent with the assumption: indeed, looking at figure 3 it seems that, before the treatment, implementation errors in DSP and non DSP municipalities exhibited a very similar dynamics. However, in this section we try to assess the plausibility of our identifying assumption in more formal ways.

As a first check, we conduct two "placebo" experiments, testing whether a significant difference in the change in implementation errors between DSP and non-DSP municipalities appeared even in periods in which the treatment did not take place (in other words, we assess the effects of "mock reforms"). The evidence supports the common trend assumption. Indeed in Table 4 we show that the β_1 coefficient is not significant if we pretend that the treatment has taken place one year before (Column 1) or one year after (Column 2) of the actual treatment.

As a second "placebo" experiment, we rerun our equation (1) considering the actual treatment date, but changing the dependent variable from the implementation error on capital expenditures to the implementation error in current expenditures. As we discussed above, the former were not directly touched by the reform in the budgetary rules. Reassuringly, also in this case the treatment effect is absent (Table 4, Column 3).

5.3) *The role of civicness*

A final question is whether the effects of the change in the budgetary rules depend on the civicness of the municipality. To delve deeper into this issue we rerun our baseline regression twice, splitting our sample between high civicness and low civicness municipalities, defining as high (resp. low) civicness cities those with a level of trust above (resp. below) the median level. Results (shown in Table 5) are quite

interesting. They show that the effect of the new rules is magnified in high trust municipalities (Column 1 and 3), but it is no longer significant if one looks only at low-trust municipalities (Column 2 and 4).

6. Conclusions and avenues for further research

Our analysis shows that the introduction of a strict budget rule may partly reduce the distance between planned and realized budget items, but only if the cultural context in which the rule is imposed is sufficiently imbued with civic values and beliefs.

We are aware that our paper is just a first step toward explaining the budget execution performance of local governments. Our analysis could be extended along several dimensions. First, we plan to enrich our set of control variables, including other proxies of political instability, such as measures of the fragmentation of the government coalition. Second, we would like to consider also other dimensions (or proxies) of civicness, beyond trust, which have been considered in the previous literature (such as blood donation, referendum turnout, etc.). Third, we would like to look not only at the accuracy of accruals budgets but also at the accuracy of cash-based budgets; we would also like to study whether the observed budgetary slippage is already present in the revised budget (the *Assestamento*, released at the end of the year before the one which is the object of the budget).¹⁰ A third series of extensions could involve the estimation techniques, as we could resort to matching methods, which – contrary to the difference-in-differences estimator - allow for non-linear and heterogeneous treatment effects. Applying these methods is interesting also because they rely on identifying assumptions which are different from the common trend assumption, and because they minimize the risk that the results are driven by unobserved differences between the treated and the control group. One could also apply spatial econometrics techniques to take into account possible interactions among neighbouring municipalities.

Finally, it would be important to build a formal game-theoretic model to rationalize our findings along the lines discussed in the introduction.

¹⁰ On this issue, see Anessi-Pessina et al. (2012).

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FIGURES

Figure 1

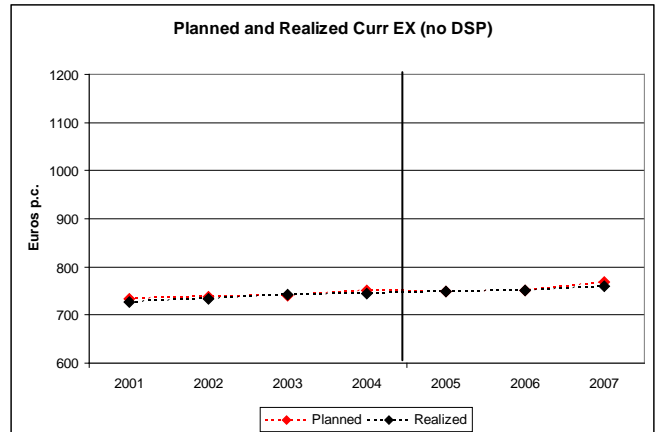
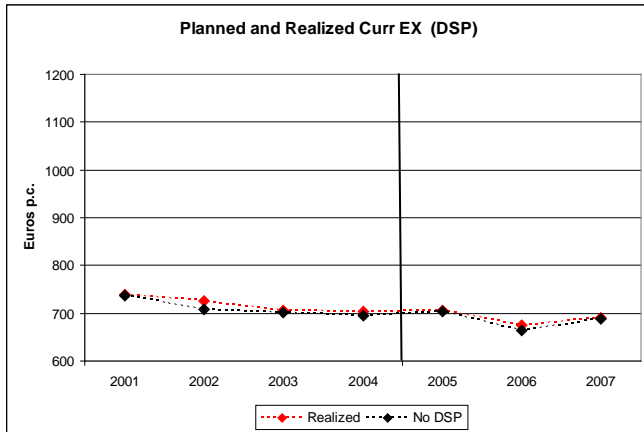


Figure 2

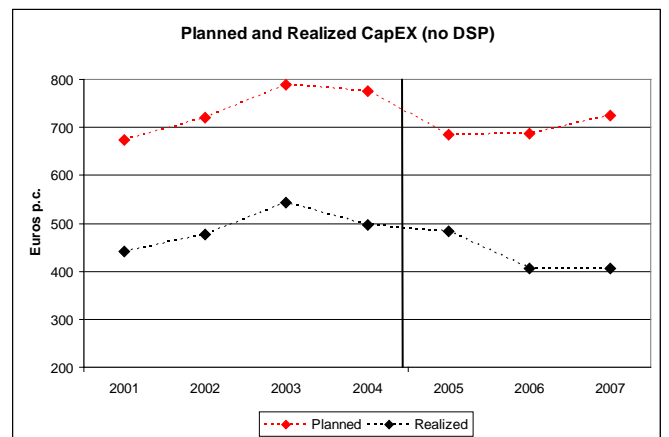
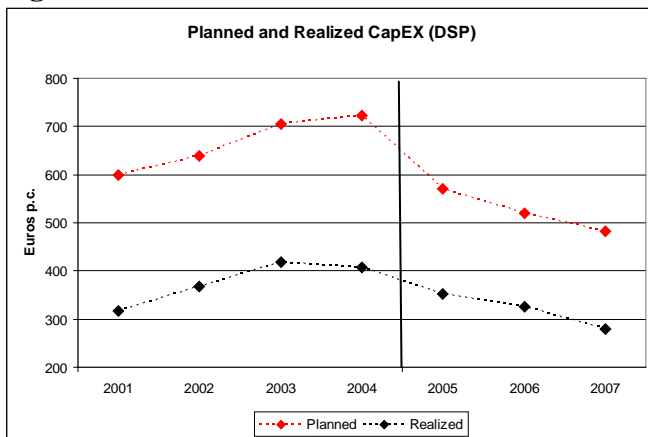


Figure 3

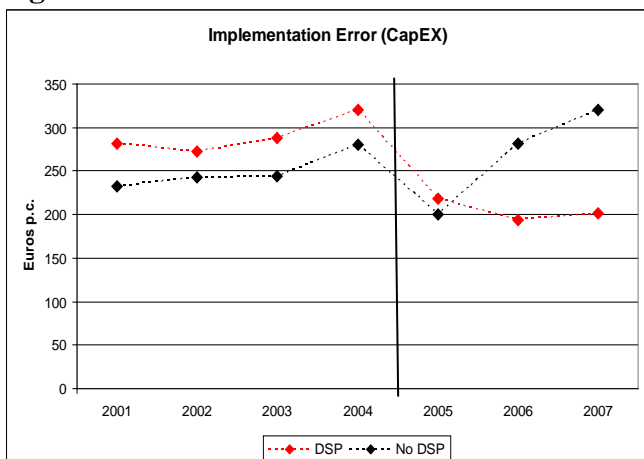
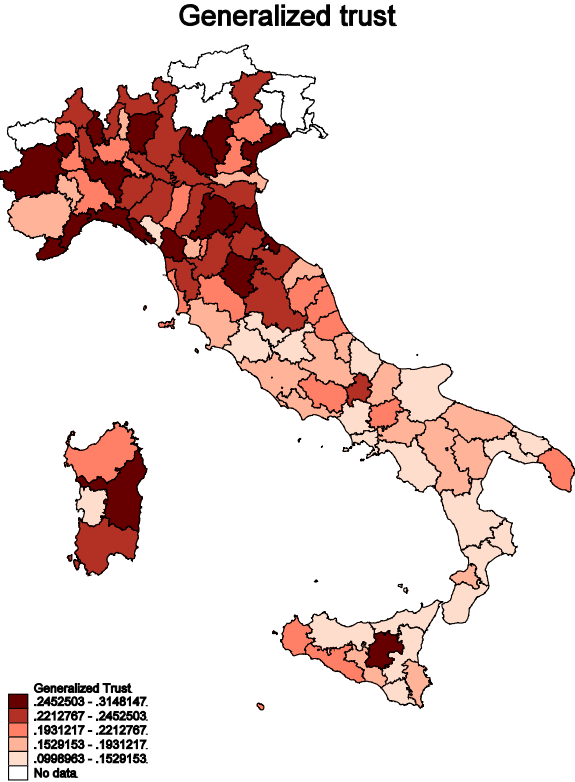


Figure 4



TABLES

Table 1: Descriptive statistics

	2004		2005	
	No DSP	DSP	No DSP	DSP
Realized capital expenditures ⁽¹⁾	1603.3 (1741.4)	3850.4 (3284.6)	1545.6 (1554.0)	3324.7 (3110.7)
Realized current expenditures ⁽¹⁾	2423.9 (1037.4)	6701.5 (3627.5)	2438.9 (1004.8)	6809.5 (3665.3)
Planned capital expenditures ⁽¹⁾	2537.7 (2334.3)	7070.5 (7080.4)	2204.4 (2107.7)	5469.0 (5943.7)
Planned current expenditures ⁽¹⁾	2448.8 (1023.6)	6777.8 (3629.3)	2444.4 (1010.6)	6841.0 (3936.6)
Implementation Error ⁽²⁾	0.286 (0.648)	0.334 (0.598)	0.210 (0.603)	0.221 (0.516)
<i>Revenue_flex</i>	0.567 (0.186)	0.605 (0.154)	0.583 (0.184)	0.632 (0.150)
<i>Exp_rigidity</i>	0.296 (0.078)	0.292 (0.062)	0.294 (0.075)	0.293 (0.061)
<i>Debt_burden</i>	0.052 (0.034)	0.048 (0.029)	0.052 (0.034)	0.048 (0.028)
Per capita taxable income ⁽¹⁾	20.0 (6.758)	21.6 (7.016)	19.3 (6.869)	20.6 (6.979)
D_South	0.315 (0.465)	0.323 (0.468)	0.313 (0.464)	0.320 (0.467)
Population	3270.5 (846.8)	9577.7 (3908.9)	3274.7 (846.2)	9624.5 (3954.7)
D_Elections	0.566 (0.506)	0.435 (0.506)	0.065 (0.248)	0.056 (0.230)
Trust	0.221 (0.062)	0.219 (0.060)	0.220 (0.062)	0.218 (0.059)
Observations	1668	1617	1662	1647

Notes: (1) thousands of euros; (2) euros per capita. Standard deviations in parentheses.

Table 2: Main results.

Dependent variable:	(1)	(2)	(3)	(4)	(5)
	Difference between planned and realized capital expenditures (euro per capita)			Planned capital expenditures (euro per capita)	Realized capital expenditures (euro per capita)
Years:			2004-2005		
Population:	[2,000; 20,000]	[2,000; 20,000]	[2,000; 8,000]	[2,000; 20,000]	[2,000; 20,000]
DSP	0.0482** (0.0217)	0.103** (0.0413)	0.157** (0.0638)	0.122*** (0.0427)	0.0186 (0.0259)
Dummy_2005	-0.08*** (0.0174)	-0.0806*** (0.0200)	-0.0686*** (0.0214)	-0.106*** (0.0191)	-0.0259* (0.0134)
DSP * D_2005	-0.0373* (0.0225)	-0.0491** (0.0219)	-0.0437* (0.0271)	-0.0665*** (0.0210)	-0.0174 (0.0135)
<i>Revenue_flex</i>		0.444*** (0.0643)	0.499*** (0.0759)	-0.916*** (0.0651)	-1.360*** (0.0471)
<i>Exp_rigidity</i>		0.00954 (0.156)	0.188 (0.183)	-0.386** (0.164)	-0.395*** (0.0932)
<i>Debt_burden</i>		1.496*** (0.322)	1.450*** (0.377)	-0.334 (0.327)	-1.830*** (0.186)
Per capita taxable income		-0.0122*** (0.00205)	-0.0129*** (0.00250)	-0.0129*** (0.00224)	-0.000729 (0.00124)
South		0.160*** (0.0342)	0.115*** (0.0414)	0.185*** (0.0358)	0.0255 (0.0190)
Dummy_election		0.0200 (0.0181)	0.0466** (0.0224)	-0.0500*** (0.0189)	-0.0701*** (0.0106)
Constant	0.286*** (0.0159)	0.210* (0.109)	-0.122 (0.332)	1.868*** (0.114)	1.658*** (0.0704)
Obs	6594	6593	4815	6593	6593
R2	0.007	0.094	0.084	0.169	0.398

Note: ***: significant at 1%; ** significant at 5%; * significant at 10%. OLS estimates. Standard errors (in parentheses) are clustered at the municipal level. All regressions include a 3rd order polynomial of population.

Table 3: Further results.

	(1)	(2)	(3)
Dependent variable:	Difference between planned and realized capital expenditures (euro per capita)		
Years:	2003-2006		
Population:	[2,000; 20,000]	[2,000; 20,000]	[2,000; 8,000]
DSP	0.0426** (0.0186)	0.0730** (0.0339)	0.0988* (0.0511)
Dummy_2004	0.0326*** (0.0106)	0.0212* (0.0122)	0.00606 (0.0163)
Dummy_2005	-0.0204 (0.0156)	-0.0279* (0.0151)	0.00855 (0.0179)
Dummy_2006	0.00744 (0.0156)	-0.00767 (0.0153)	-0.0354*** (0.0121)
DSP * D_2005	-0.0832*** (0.0181)	-0.0822*** (0.0177)	-0.0773*** (0.0224)
<i>Revenue_flex</i>		0.379*** (0.0495)	0.385*** (0.0586)
<i>Exp_rigidity</i>		0.170 (0.125)	0.356** (0.147)
<i>Debt_burden</i>		1.964*** (0.265)	2.012*** (0.308)
Per capita taxable income	n.a.	n.a.	n.a.
South		0.278*** (0.0205)	0.227*** (0.0251)
Dummy_election		0.0265** (0.0126)	0.0489*** (0.0158)
Constant	0.254*** (0.0142)	-0.135* (0.0759)	-0.260 (0.268)
Obs	13121	13119	9610
R2	0.005	0.091	0.077

Note: ***: significant at 1%; ** significant ant 5%; * significant at 10%. OLS estimates. Standard errors (in parentheses) are clustered at the municipal level. All regressions include a 3rd order polynomial of population.

Table 4: Validity tests.

	(1)	(2)	(3)
Dependent variable:	Difference between planned and realized capital expenditures (euro per capita)		Difference between planned and realized <u>current</u> expenditures (euro per capita)
Years:	2002-03	2003-04	2004-05
Population:		[2,000; 20,000]	
DSP	0.0179 (0.0408)	0.0186 (0.0426)	-0.001 (0.004)
Dummy_2005	0.0417*** (0.0157)	0.0111 (0.0211)	-0.007** (0.003)
DSP * D_2005	0.0112 (0.0213)	0.0212 (0.0213)	-0.000 (0.003)
<i>Revenue_flex</i>	0.420*** (0.0601)	0.418*** (0.0672)	0.0145** (0.007)
<i>Exp_rigidity</i>	0.0773 (0.142)	0.214 (0.175)	9.75e-05*** (3.44e-05)
<i>Debt_burden</i>	2.416*** (0.363)	2.603*** (0.366)	-0.0251 (0.0421)
Per capita taxable income	n.a.	n.a.	0.000 (0.000)
South	0.274*** (0.0244)	0.295*** (0.0280)	0.008** (0.004)
Dummy_election	0.0425* (0.0254)	0.0277 (0.0199)	-0.0016 (0.0022)
Constant	-0.185** (0.0874)	-0.264** (0.104)	-0.038** (0.019)
Obs	6280	6532	6593
R2	0.103	0.099	0.012

Note: ***: significant at 1%; ** significant ant 5%; * significant at 10%. OLS estimates. Standard errors (in parentheses) are clustered at the municipal level. All regressions include a 3rd order polynomial of population.

Table 5: The role of civicness

	(1)	(2)	(3)	(4)
	High trust	Low trust	High trust	Low trust
Dependent variable:	Difference between planned and realized capital expenditures (euro per capita)			
Years:	2004-2005			
Population:	[2,000; 20,000]		[2,000; 8,000]	
DSP	0.197*** (0.0659)	-0.00539 (0.0473)	0.246** (0.103)	0.0563 (0.0736)
Dummy_2005	-0.0467 (0.0291)	-0.132*** (0.0256)	-0.0377 (0.0304)	-0.128*** (0.0283)
DSP * D_2005	-0.112*** (0.0345)	0.0268 (0.0253)	-0.0922** (0.0431)	0.0197 (0.0304)
<i>Revenue_flex</i>	0.390*** (0.0884)	0.558*** (0.0833)	0.449*** (0.104)	0.628*** (0.0979)
<i>Exp_rigidity</i>	-0.0839 (0.232)	0.0685 (0.186)	0.0987 (0.277)	0.159 (0.212)
<i>Debt_burden</i>	1.420*** (0.438)	1.672*** (0.422)	1.233** (0.500)	1.889*** (0.506)
Per capita taxable income	-0.0105** (0.00423)	- (0.00212)	-0.0138** (0.00548)	- (0.00255)
South	0.160*** (0.0560)	0.130** (0.0641)	0.0946 (0.0694)	0.0729 (0.0660)
Dummy_election	0.0375 (0.0277)	-0.00815 (0.0223)	0.0697** (0.0334)	0.00107 (0.0285)
Constant	0.282 (0.184)	0.0880 (0.126)	0.0113 (0.523)	-0.258 (0.387)
Obs	3503	3090	2562	2253
R2	0.069	0.088	0.059	0.088

Note: ***: significant at 1%; ** significant ant 5%; * significant at 10%. OLS estimates. Standard errors (in parentheses) are clustered at the municipal level. All regressions include a 3rd order polynomial of population.