Mafia and Public Spending: Evidence on the Fiscal Multiplier from a Quasi-experiment*

Antonio Acconcia† Giancarlo Corsetti‡ Saverio Simonelli§

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Abstract

We estimate the multiplier by relying on data on spending in infrastructure at provincial level in Italy, and an instrument identifying changes that are large and exogenous to local cyclical conditions. Such instrument is derived from an Italian law mandating the interruption of public work on evidence of mafia infiltration of city councils — thus, in contrast to other studies, our results are driven by episodes of contraction in spending. Basing IV estimates on local data allows us to address common problems in time series analysis, such as the risk of confounding the effects of fiscal and monetary measures. Our point estimates of the multiplier are in the range 1.2-1.4 on impact — with confidence intervals however including 1. Under the additional assumption that lagged spending is exogenous to current value added, the overall multiplier, with dynamic effects, rise to 1.8-2.

Keywords: Government Spending, Multiplier, Instrumental Variables.

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†University of Naples Federico II (Department of Economics) and CSEF. E-mail: antonio.acconcia@unina.it

‡Cambridge University, Rome III and CEPR. Email: giancarlo.corsetti@gmail.com

§University of Naples Federico II (Department of Economics), EUI and CSEF. E-mail: savsimon@unina.it

1 Introduction

The widespread resort to fiscal policy as a key instrument for output stabilization during the global crisis and the ensuing need to consolidate deficits via spending and tax adjustment has revitalized the debate on the multiplier. As this is typically estimated by tracing the effects on economic activity of exogenous fiscal impulses, much of the debate has focused on well-known issues in identifying exogenous innovations in spending or taxation, as distinct from variations that are systematically related to the business cycle. Failure to draw a sharp distinction in this dimension means that reverse causation, from output to spending and taxes, may spuriously raise, or lower, estimates of the multiplier (see e.g. Barro and Redlick 2010 and Ramey 2009). Even if this issue could be satisfactorily addressed, however, the literature has also made increasingly clear that the response of private expenditure to, say, an expansion in government spending is bound to depend crucially on the overall policy mix, reflecting possible constraints on monetary policy and the way current budget deficits are expected to be consolidated in the future, via spending cuts or tax hikes (see e.g. Christiano et al. 2010, Corsetti et al. 2009, Leeper et al. 2009, and Woodford 2010 among others), as well as on the state of the economy. Since these determinants of fiscal transmission may vary not only across countries, but also through time, there is no presumption that the macroeconomic transmission of any given fiscal innovation in the short run been adequately captured by a single (unconditional) impulse response.

In this paper, we propose a quasi-experiment setting for an analysis of the multiplier of government spending which allow us to address these issues. First, we carry out our estimation by exploiting the dynamics of public investment spending at subnational level, focusing on a sample including all Italian provinces. To identify fiscal shocks, we rely on an instrument derived from a specific institutional arrangement which produces large, sudden and strictly temporary changes in spending, unrelated to cyclical conditions in the local economy. Namely, on evidence of mafia infiltration in a city council, an Italian law mandates the dismissal of the elected officials, with an immediate de-facto interruption of payments into investment projects — to be started again a few quarters later, after police investigation verifies that the firms contracted by the municipality are not connected to the mafia. In contrast to many other studies, our estimates of the multiplier are therefore driven exclusively by episodes of sharp (temporary) fiscal contractions. Second, given that we analyze local data, we can naturally control for the business cycle and the monetary stance at national level. Moreover, the fact that the shocks identified in our analysis are strictly temporary, combined with the characteristics of fiscal federalism in Italy detailed below, means that the change in spending is associated with no variation in taxes: this means that we can obtain a measure of the multiplier independent of issues related to budget adjustment. Overall, thus, our results can be expected to shed light on the transmission of spending impulses in a controlled environment, holding constant the monetary stance, the tax burden, as well as the general cyclical condition of the national economy as a whole.

The point estimates of the multiplier is in the range 1.2-1.4 on impact, a value that is not significantly different from 1 at standard confidence level. Sensitivity analysis enlarges the range of point estimates to 1-1.6, depending on the episodes of dismissals of city council included in the analysis. Under the maintained hypothesis that lagged values of spending are exogenous to current value added, commonly made by the literature, dynamic effects over two years brings the overall multiplier close to 2, and significantly above 1 at standard confidence levels in some specifications of our empirical model.

The contribution of our paper is best appreciated against the time series literature on the same issue. Studies based on structural vector-autoregression (SVAR) models identify multipliers by assuming that changes in government purchases, adjusted for cyclical components, are pre-determined within a quarter. The main empirical findings point to a positive value for the multiplier, although not necessarily high (see, for instance, Blanchard and Perotti 2002; Perotti 2004). The main issue is whether the shock thus identified can be plausibly considered exogenous with respect to the determination of income. Notably, Ramey (2009) shows that many of these shocks are anticipated by the private sector. Another strand of the literature, adopting a narrative approach, uses instead institutional information to date historical episodes when changes in taxes and spending can be reasonably considered exogenous to contemporaneous economic conditions. In this framework, spending multipliers are estimated to be lower than, or equal to 1, while tax multipliers — relating output to marginal income-tax rates or tax revenues — are found to be around -1 (see, for instance, Ramey 2009; Barro and Redlick 2010; Romer and Romer 2010; and Mertens and Ravn 2009, stressing anticipation effects).[1]

Relative to this literature, the use of data on fiscal variables at the local level, combined with institutional information, creates new possibilities in the design of identification strategies, but also raises new issues. As regards the new possibilities, the use of institutional information allows us to identify an exogenous source of variations in government spending at the local level that are large and implemented

[1] The literature on spending multipliers has mostly focused on US government purchases, especially war-related increases in military spending. Relevant episodes for identification are indeed the Korean War and World War II, associated with estimates of the multiplier around 0.7 — a figure which may nonetheless reflect special economic conditions affecting the fiscal transmission mechanism in time of war (Hall 2009). Defence spending is also the focus of recent work proposing a novel way of approaching the time series analysis of multipliers, by Fisher and Peters (2010). These authors identify government spending shocks with statistical innovations to the accumulated excess returns of large US military contractors and find a multiplier of 1.5. A smaller number of studies have analyzed the macro effects of non-defence spending, or estimated multipliers in countries other than the US, mainly because of the lack of satisfactory instruments. The results from the available VAR-based work tend to be quite heterogeneous, depending upon the type of spending, and the state of the economy — for instance, peacetime fiscal multipliers can be expected to be lower than those in periods of war, as also argued by Barro (2009); they are found to be much larger during financial crises (Corsetti et al 2009). By the same token, estimates of multipliers systematically vary with country characteristics and policy regimes, including openness to trade and the exchange rate regime (Corsetti et al. 2010; Ilzetzki et al. 2010).

independently of local cyclical conditions. This is the 1991 law mandating the compulsory direct administration of local municipalities on evidence of mafia infiltration. When a local government is dismissed because of its ties to the mafia, one of the first acts of the external administrators appointed by the central government consists of suspending financial flows into local public work and investment projects. In our sample, indeed, the average growth rate of spending at provincial level turns negative conditional on a municipality being placed under compulsory administration, with an average contraction of 20 percentage points — a contraction that is completely reversed over a two to three years horizon. The instrument we derive from this institutional setting is key to disentangling exogenous changes in spending programmes, and addressing the potential biases of OLS estimators.

Moreover, as already mentioned above, estimates of the multipliers from local public investment allow us to control for national, common components, whose variations are usually predictable and arguably endogenous. By the same token, we can control for the national monetary stance, common to all provinces, thus addressing issues in the role of monetary policy in determining the size of the multiplier effects (see Christiano et al. 2009; Woodford 2010). In these dimensions, quasi-experiment studies help clarify the specific circumstances and the structural and policy environment in which we evaluate the macroeconomic effects of a given change in fiscal inputs. A notable instance is provided by the budget/tax consequences of spending innovations. Even if the consequences of temporary spending shock on the tax burden are small by definition, expected budget adjustment that impinges on the intertemporal price of resources may nonetheless have strong effects on current economic activity, either lowering or raising the equilibrium multiplier (see e.g. Corsetti et al. 2010). Indeed, a key issue faced by time-series study is how to disentagle the contribution of different factors to the equilibrium outcome from identified fiscal shocks. Our quasi-experiment allows us to obtain estimates of the multiplier that capture exclusively the direct effect of the change in government spending. In bringing our estimates to bear in the current debate on fiscal policy, they should then be combined with estimates of the influence of related adjustment in tax, budget policy, and the monetary stance.[2]

As regards new issues raised by the our quasi-experimental setting, a key question concerns which lessons can be drawn from analyses of the fiscal transmission at local level, for the analysis of nation-wide fiscal policy. Italian provinces are obviously small, highly open economic systems. On the demand side, we can thus expect that a significant share of a change in local demand will 'leak' outside the local territory. Everything else equal, this may lead to an underestimation of the multiplier effects relative to national-level estimates. However, there is also a supply side to the story. In response to a contraction in local demand driving down activity in one area, factors of

[2] By no means this is a weakness of our study. As already mentioned, time series methods identifying average macroeconomic effects of fiscal shocks typically do not explicitly allow (and thus cannot account) for monetary and budget determinants of the equilibrium multiplier effects of the identified shock.

production may move, determining an increase in activity in the neighboring areas.[3] Estimate of the output elasticity to spending at the municipality level may thus overstate the strength of the multiplier. It will be important to provide evidence that these effects are contained, outside the geographical units of our analysis.[4]

A distinct feature specific to our study is that, while our econometric model does not explicitly allow for asymmetric effects of changes in spending depending on their sign, the instrument we use only records sharp fiscal contractions. In the literature, analyses of multipliers typically rely on episodes (such as wars) in which non-economic factors (geopolitical considerations) cause (military) spending to rise sharply for reasons unrelated to cyclical development. Sharp fiscal contractions, in contrast, are typically driven by economic/budget necessity, hence harder to consider exogenous to economic conditions. In the quasi-experiment we consider in this paper, however, we identify episodes of fiscal retrenchment which are sharp, temporary, and to a large extent unexpected and unrelated to economic considerations. The observation that the episodes of exogenous changes in fiscal policy driving our results are contractions, however, suggests caution in interpreting our results as reliable estimates of the multiplier for fiscal expansions.

Together with our paper, a small but significant body of the literature on the fiscal multiplier has recently turned to local data to measure the causal impact of government spending on the economy. While still in the spirit of the time-series approach of e.g. Barro and Redlick (2010), Nakamura and Steinsson (2010) focus on military procurement spending in the US, exploiting the variations in spending at state or regional level associated with variations in national military build-ups and draw-downs. Their estimate of the government spending multiplier is approximately 1.5. Fishback and Kachanovskaya (2010) use annual data for the 48 US continental states between 1930 and 1940 to estimate multipliers in a period when unemployment rates never fell below 10 percent and there was ample idle capacity. Their estimates range from 0.91 to 1.39. More closely related to our approach, Serrato and Wingender (2010) use the fact that a large number of federal spending programmes are sized according to estimates of local populations. Discontinuous changes in the methodology underlying these estimates typically lead to revisions in the population figures, which in turn justify variations in the allocation of federal spending. Using these fund reallocations across US counties, Serrato and Wingender (2010) estimate a local income multiplier

[3] The concern with correlated changes in economic activity through space discussed in the text should not be confused with concerns about the outcome of a different experiment, by which spending is 'transferred' from one province to another. In this experiment, obviously, activity will fall in the first province, increase in the other. In the aggregate of the two provinces, the outcome may be zero even if the multiplier is (symmetrically) large.

[4] Note that these issues would also arise also in analyses of the multiplier in small countries belonging to a monetary union — countries with size between Luxembourg and Belgium. Yet our setting in not exactly the same as that of a small open economy. A part from issues in identifying exogenous shocks to fiscal policy, as argued above in our analysis we are able to bypass issues in taxation and budget adjustment — which is impossible when dealing with sovereign states.

for government spending as high as 1.88. Similarly, Clemens and Miran (2010) build on differences in the balanced-budget requirements at state level. States with stricter rules are forced to enact large cuts to their budgets during years in which adverse shocks occur; states with weak rules are allowed to make up the difference over a few years. Their estimate of the spending multiplier is 1.7. This literature has thus already shown proof of the potential gains from exploiting large local data and institutional information.

The rest of the paper is organized as follows. Section 2 presents the empirical framework. Section 3 is devoted to the analysis of our instrument, starting with some institutional details on the laws targeting mafia connections. In section 4 we discuss our main results and in section 5 we conclude.

2 The empirical model

The two qualifying features of our study are that, first, we look at infrastructure investment projects at the local level in Italy, exploiting differences in the time series of public spending across provinces; second, that we rely on an instrument derived from institutional information (the interruption of such investment programmes mandated by the Italian government on evidence of mafia infiltration of local administrations). In this section, we lay out our empirical framework and discuss the advantages of using local spending data. In a later section, we discuss our instrument.

To carry out our study, we collect data at the provincial level in Italy. The Italian province is a geographic entity similar to a U.S. county, and contains several municipalities. This is an notable feature of our data set since, as explained below, we will make use of an instrument identifying shocks to spending occurring at municipality level, not at provincial level — although it is very plausible to assume that some of these shocks may have repercussions on spending on infrastructure beyond the territory of the municipality. Dictated by the availability of comparable data, our sample covers the whole country over the ten-year span between 1990 and 1999. During this periods, there were 95 provinces in Italy. Hence, we have 950 observations.

For each province, let y_i denote the per capita value added, so that its rate of growth is $Y_{i,t} = \frac{y_{i,t}-y_{i,t-1}}{y_{i,t-1}}$; similarly, let g_i denote the per capita infrastructure investment, so that its year-on-year change, as a ratio of lagged value added, is $G_{i,t} = \frac{g_{i,t}-g_{i,t-1}}{y_{i,t-1}}$. In line with recent work on the fiscal multiplier (see e.g. Barro and Redlick 2010), we estimate spending multiplier relating the growth of per capita value added in province $Y_{i,t}$ to the year-on-year change in per capita spending on infrastructure in the same province. The basic empirical model is

$$Y_{i,t} = \beta G_{i,t} + \alpha_i + \lambda_t + \gamma X_{i,t} + v_{i,t}, \tag{1}$$

where the coefficient β measures the contemporaneous government spending multiplier; α_i is a province fixed effect, λ_t is a year fixed effect — the fixed effects being coefficients on dummies for each individual province, while the year effects are coefficients on time dummies; X denotes covariates, discussed below.

In addition to controlling for province-specific features with fixed effects, we also control for possible heterogeneity across provinces and macro areas by including two proxies for unemployment in X. These are given by the hours of *Cassa Integrazione Guadagni*, activated when large firms enter a crisis state and need to suspend production, as well as the rate of employment at provincial level.[5] Unemployment rates vary substantially across macro areas, with marked differences between the North and the South.

To improve precision of our estimates, in some of our specifications we will include lagged values of our instruments (to be discussed below) and lags of local public spending in infrastructure. Now, seminal contributions to the literature, especially (but not exclusively) to the SVAR literature, have posited that lags of $G_{i,t}$ are pre-determined with respect to $Y_{i,t}$, as a key maintained assumption to identify multipliers.[6] Under the same identifying assumption, we could also interpret (significant) coefficients on these lags as shedding light on possible dynamic effects of spending, complementing our IV estimates of the impact multiplier. Importantly, we verify that investment changes and lagged output changes are uncorrelated, controlling for lagged investment.

As emphasized by Bertrand, Duflo, and Mullainathan (2004) as well as Angrist and Pischke (2009), in repeated cross-sections and panel data, it is of paramount importance to specify the nature of the error term $v_{i,t}$ correctly. Namely, if either the data set is characterized by correlation within groups at any t, or group means are serially correlated (or both), then inference neglecting these features of the data may be highly misleading. As regards the serial correlation problem, there seems to be no consensus yet on the best strategy to deal with it. Taking advantage of the panel nature our data set, we will include in some of our specifications up to two lags of the dependent variable. As will be clear from the tables, the lagged endogenous variable will not play any decisive role in our main results, since its inclusion among regressors has virtually no effect on our point estimates of β, nor on their statistical significance. Regarding the spatial correlation problem, whether and how the 95 Italian provinces should be considered as a cluster sample is far from straightforward. A way to address this issue is to recognize that Italian provinces are grouped in 20 administrative regions. It may well be that observations within a region are correlated as a result of an unobserved cluster effect due to common regional rules and policies — an assumption posited, for instance, by Guiso et al. (2004). In what follows, we allow for this possibility by basing our inference on standard errors robust to contemporaneous spatial correlation within regions.

A notable advantage of estimating equation (1) with local information consists in

[5] The *Cassa Integazione Guadagni* is the main unemployment benefit arrangement covering employees of private firms in Italy.

[6] Barro and Redlick (2010) use the lagged value of their defence spending variable as an instrumental variable.

the fact that, by including time dummies, one can control for national components in public investment and GDP common to all provinces. Aggregate variations in spending and output at national level are usually predictable and arguably endogenous to cyclical developments, and hence are a major concern in time series analysis, as they may lead to spurious estimates of the multiplier, due to reverse causation. These concerns are greatly attenuated in our framework. In this respect, we should also note that focusing on the infrastructure investment component of public spending already makes our spending measure less likely to be affected by current business cycle considerations relative to the other spending components. Investment changes are often driven by political factors reflecting the beliefs of the party in power, and usually motivated in terms of long-run goals.

By the same token, recent contributions have clarified that general equilibrium multiplier effects cannot be assessed independently of the mix of monetary and fiscal policy anticipated to prevail at both short and long-term horizons (Christiano et al. 2009, Corsetti et al. 2009, 2011 and Woodford 2010). In other words, multipliers are bound to be crucially affected by constraints on monetary policy, the inflationary stance of the central bank, as well as by the anticipation of fiscal measures (spending cuts or tax hikes) dictated by the need to consolidate the budget and stabilize debt in the medium and long run — e.g. multipliers are higher if monetary policy is accommodative, or current expansions are anticipated to cause spending to fall below trend in the future. The dependence of the macroeconomic transmission of fiscal policy on the overall policy mix is obviously a challenge to identification in time-series studies. In our contribution, the inclusion of controls for common national components via time dummies accounts for the effects of policy measures with common effects throughout the country — including changes in the monetary policy stance. Our estimates of the multipliers are to a large extent insulated from systematic contemporaneous interactions of fiscal and monetary policy at national level.

A related advantage of our data set is that, over the sample period under consideration, investment spending at local level in Italy is allocated by the central government. Local administrations had control over the realization of projects, but virtually no power to set local taxes. So, the public resources channelled by the central government into local investment projects were not matched by variations in the tax bill of the local residents. To a large extent, therefore, we can by-pass issues arising from the omission of tax changes (or debt) from the equations we estimate. Romer and Romer (2010), for instance, emphasize that occasionally aggregate spending and tax changes may become strongly correlated — typically reflecting emerging political concerns with the ongoing government deficit. To the extent that tax changes have a negative impact on output — these authors argue — the omission of this variable induces a downward bias in the estimate of the spending multiplier.[7]

In conclusion, it is worth stressing that, with the inclusion of fixed effects, our

[7] A linear model would not be suitable to account for this consideration, as spending expansions are only occasionally, matched by tax hikes in the same period

estimates do not rest on cross-sectional variation across provinces. Between effects across provinces may clearly induce a bias in our estimates of the multiplier, since province-specific characteristics can be expected to be correlated with the criteria chosen by the government to allocate infrastructure spending at local level. By way of example, it is possible that the government systematically allocate relatively large projects in lower-growth provinces, in an effort to spur local economic activity. If this is the case, OLS estimates of the multiplier not controlling for between effects (and not instrumented) would be spuriously low.

3 Instrumenting Changes in Public Spending

Despite the advantages of relying on local information described in the previous section, OLS estimators of spending multipliers are not shielded from some standard criticisms. First, variations in infrastructure spending are usually planned one or more years before they actually occur. As Ramey (2009) argues, a failure to account for the delay between announcement and realization of spending projects can affect empirical results for multipliers. Second, in our sample, the government may still have systematically allocated funds in response to local developments, in ways that are not accounted for by our controls for province-specific economic features. To address these problems, we need to find a good instrument for unexpected variations in public spending exogenous to local economic conditions. We proceed in the spirit of Angrist and Pischke (2010), who, among others, suggest relying on quasi-experimental design, exploiting institutional information. This is the strategy we adopt in our study.

3.1 The institutional setting: mafia infiltration and compulsory administration

We introduce our instrument by providing some background information on the way mafia-related crimes are treated by the Italian law. In view of the rising presence of organized crime in the Italian economy, in 1982 two articles were added to the penal code, expressly targeting mafia-type associations.[8] Articles 416-bis and 416-ter recognize that the use of intimidation, associative ties and *omertà* (condition of silence) is specific to mafia, to acquire direct or indirect control of otherwise legal economic activities, especially in the area of the provision of public services and public investment. To pursue their goals, mafia-type associations have specific interests in influencing the

[8] Historically, different mafia groups have been active in different regions: the Camorra in Campania, the 'Ndrangheta in Calabria, the Sacra Corona Unita (SCU) in Puglia, and the Mafia in Sicily. Each group consists of a number of mafia associations, the most 'famous' being the Cosa Nostra in Sicily and, recently, the Casalesi in Campania.

results of electoral competition and obtaining effective control over public tenders.[9] Public licenses and public work indeed create the ideal profit opportunities for the mafias at the local government level.

The potential size of profits for the mafia at the local government level is especially large in Italy, due to the type of fiscal federalism established in the mid 1970s with two basis laws — Law No. 281/1970 and Law No. 382/1975. On the spending side, these laws give the central government the power to budget the overall flow of resources accruing to local governments and decentralized public administrations. The latter in turn retain full control of these funds, including the power to select public projects, and the firms to carry them out. On the tax side, however, local governments are not responsible for raising tax revenues locally against their spending plans. Not surprisingly, local administrators have operated under a strong incentive to lobby for public funding from the central government: local spending has persistently grown well in excess of local output (see Cassese 1977 and 1983). Because of the sheer size of public works under the control of local administrations, these have become an extraordinarily lucrative business for the mafia and entrepreneurs winning public tenders thanks to their tie with mafia associations. Historically, the prospects for profits for mafias were further boosted by the large public funds targeted at reconstruction activities after the strong earthquake hitting vast regions in the south of Italy in 1980.

Profits accruing to organized crime from their control of public-works are estimated to be comparable to those from illegal activities such as extortion and drug dealing (see Relazione, 2000). It should be stressed, however, that influence and/or control of mobsters on individuals who formally operate according to the law does not necessarily translate into distortions in the construction sector markets. According to the Commissione Parlamentare di Inchiesta (2005) and the media, mafia collusion may involve firms that are competitive nation-wide, thus operates with high standards of efficiency also in geographical areas and input markets outside the mafia-controlled regions.

The sheer proportion of the phenomenon of mafia infiltration has clearly been a key reason for introducing in the early 1990s, a number of tougher anti-mafia measures,[10] including the Law giving the central government the power to remove elected officials in a city council on evidence of their decisions being determined/controlled by the mafias (D.L. 31/05/1991 n. 164). Upon the removal of a city council, the central

[9] The rising influence of mafias on the legal economy via their relations with public officials, including political representatives, judges, local administrators and members of the police force is well understood. Already in the 1980s, Tommaso Buscetta, arguably the most famous *mafioso*, revealed to the prosecutor Giovanni Falcone important details about the strict links between Cosa Nostra and the Sicilian political system. Vito Ciancimino is an early example of an important politician convicted for being *mafioso*, and involved in several crimes. For evidence and historical facts about Italian mafias see Acconcia et al. (2009) and references within.

[10] Specifically, new laws hardened punishments for mafia mobsters while granting full or partial amnesty to whistleblowers providing the authorities with useful information on mafia crimes and connections (D.L. 13/05/1991 n. 152).

government appoints three non-elected, external commissioners, to rule the municipality for a period of 18 months. By 2008, the number of dismissed city councils was 172, mostly concentrated in the provinces of Naples, Palermo, Reggio Calabria, and Caserta (see Table 1).[11]

Considering all types of mafia criminal activities together, there is considerable variation in their intensity both across and within regions. There is a high concentration in Sicily, Campania, Calabria and Puglia, but the mafias' presence is also significant in northern regions like Piemonte and Lombardia.[12] According to many observers, differences across provinces and regions can hardly be explained by variations in the rate of deterrence. Rather, they mostly reflect the different pervasiveness of mafias across areas, sometimes due to mere historical accidents.

3.2 An instrument "one can't refuse"

When a local government is dismissed on evidence of mafia infiltration, one of the first acts by the external administrators appointed by the central government consists of suspending financial flows into local public work and investment projects, with the goal of stemming any direct or indirect financing of the mafia. Public work and projects are started again only after investigation and scrutiny of previous tender procedures and decisions to establish that the contractors are not effectively mafia associations.

On impact, the dismissal of elected administrators is thus associated with sizable fiscal retrenchment. The magnitude of the cut is apparent when comparing average spending growth in the provinces with compulsory administrations with the rest of Italy. In our sample, we have 109 cases of city councils put under compulsory administration. Aggregating them by province, we obtain 45 observations. Note that the aggregation of municipality-level information at provincial level tends to dilute the average changes in public investment due to compulsory administration of single city councils. In practice, however, infrastructure investment projects typically involve

[11] Anecdotal evidence on the influence of various mafias on public authorities is ample. By way of example, in 1998 the Court in Catania convicted a policeman on evidence that he worked for Cosa Nostra; in 2000, a judge who worked in the prosecutor's office in Messina (Sicily) and another retired trial judge were arrested on charges of collusion with the Sicilian mafia. Empirical evidence indeed unveils a positive and statistically significant correlation between the number of public officials convicted for bribes, and the number of people convicted for mafia association (Acconcia et al. 2009).

[12] Using data for convictions on the crime of mafia association as an indicator of relative intensity, for instance, we know that 90 per cent of the $5,443$ mobsters convicted in our sample period, were put on trial by courts in Southern regions — mainly Sicily, Campania, Calabria and Puglia. Yet, in the Campania region, only 239 mobsters were convicted in the judicial district of Salerno (corresponding to 24 convictions per $100,000$ inhabitants), against 1483 in the district of Naples (32 convictions per $100,000$ inhabitants). In the Calabria region, the number of convictions in Catanzaro and Reggio were, respectively, 204 and 343 (that is, 14 and 59 per $100,000$ inhabitants); in the region of Puglia, the corresponding number in Bari and Lecce is 142 and 534 (that is 6 and 30 per $100,000$ inhabitants). In the North many convictions were sentenced by courts in Piemonte and Lombardia.

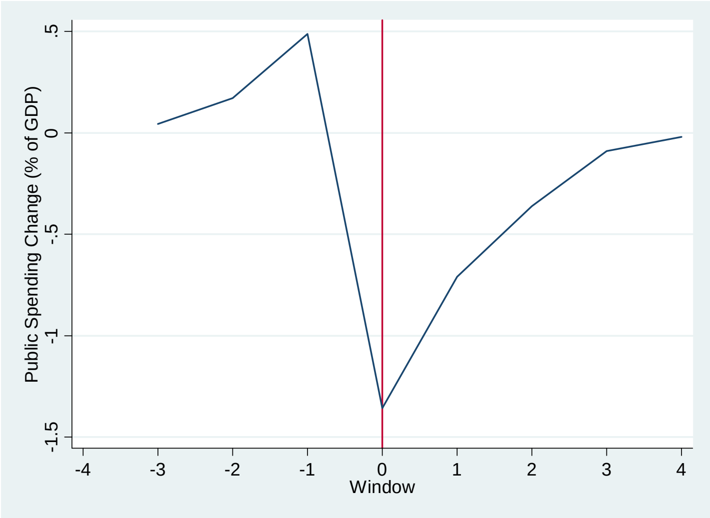

Figure 1: Public Spending Changes

more than one municipality. Even when the city under compulsory administration is small, the size of public investment affected by the freeze may end up being large at the provincial level — we will return on this point below.

Table 2 reports mean difference tests for the changes in public infrastructure investment using different metrics. In particular, we divide the observations in our sample into two groups: those relative to year-province after a municipality in the province is placed under compulsory administration by the central government (treatment group), and those in the rest of the sample (control group). Looking at our results for the whole sample (columns 1 and 3 of the Table), as expected, the mean change in investment spending is positive for the control group (second row of the table), and the difference from the treatment group is large and statistically significant. It is actually so large that the mean change in investment in the provinces with municipalities placed under compulsory administration is clearly negative. In terms of value added, this amounts to half a percentage point on average, when the whole sample is considered. In the table, we also redo the exercise focusing on the subsample of provinces with at least one case of compulsory administration (columns 2 and 4). The results in columns 2 and 4 of the table show that this difference in not driven by a systematically lower level of investment variation in the treatment group. It is also worth emphasizing that there are no systematic differences across the North-South divide.

A different perspective on the same piece of information is provided by Figure 1, which plots, for the provinces experiencing a first episode of city council dismissal in the early 1990s, the average rate of change in spending over a window of 8 years, centered around the year in which the city council is dismissed — marked by the vertical line over the 0 on x-axis. Specifically, the average growth rate is referred to the provinces of Napoli, Reggio Calabria, Palermo, Catania, Bari, Salerno and

Avellino, for the years 1991 and 1993. The contraction in spending at the time of the dismissal is apparent, so is its swift recovery over a 3 year horizon.

For comparison with the literature, the revenue effect of the 54 legislated exogenous tax changes identified by Romer and Romer (2009) amounts to -0.03% of GDP, with a standard deviation of 0.24. The largest quarterly change in taxes (a cut) amounts to nearly 2% of GDP. As regards defence spending, changes related to the Korean War were of the order of 0.5% in 1953 and -2.1% in 1954. Changes were more modest after 1954, the largest occurring during the Vietnam war (-1.2% in 1966 and 1.1% in 1967).[13] In size, the change in infrastructure investment underlying our estimate of the spending multiplier is comparable to the change in fiscal variables in related work on the same issue.

Two key features qualify our choice of instrument. First, that the dismissal of city councils typically follows criminal evidence unveiled by ongoing police investigations, sometimes conducted in areas that are geographically distant from the municipality, or focused on illegal activities unrelated to public works. The emergence of incriminating evidence leading to the dismissal decision follows a process that is to large extent independent of local cyclical conditions. This feature implies that the contraction in investment identified via our instrument is exogenous to time variation in local economic activity — once we control for province-specific effects. Second, based on policy investigation, the decision to dismiss a council and the implementation of such a decision occur quite rapidly.[14] Hence, anticipations of government-mandated cuts in spending from one year to another are unlikely to play a significant role in our sample. As further discussed below, our instrument will be defined as to account for likely differences in the impact of city council dismissals on the value added recorded in any given year, depending on when the dismissal takes place — whether early or late in the year.

A key condition for IV estimation is that our instrument has a clear effect on $G_{i,t}$, given the other exogenous regressors. For the instrument to be valid, however, we should also be confident that it is uncorrelated with the error term. That is, we need the exclusion restriction to be valid: the dismissal of the city council must affect value added at provincial level only to the extent that it leads to a (temporary but sharp) reduction in spending in infrastructure — so that our instrument affects GDP only through government spending. Arguably, a reason why this condition might fail is that, by calling attention to mafia activities in the local district, the dismissal of the city council may have economic consequences independently of the cut in public spending.

[13] See Barro and Redlick (2010).

[14] While according to the Law the dismissal of the city council should normally follow a formal decree by the President of the Republic, there are circumstances under which the local *Prefetto* can proceed immediately, without waiting for the legal definition of the procedure. This has indeed been common practice, especially in the initial phase. In this case, on average, the whole procedure would take about one month.

In assessing this problem, it is important to keep in mind that virtually all the episodes of city council dismissals in our sample occur in cities where the presence of the mafia, and its ties to local administrator, is public knowledge. In addition, while the police investigation may result into arrests during the period in which the city is run by external commissioners, the result is often no more than a dent in the widespread network of illegal connections of organized crime — unfortunately, as officially recognized, the presence of the commissioners did not produce any substantial progress towards the eradication of the mafia around the municipality. The information content of the news about city council dismissal primarily consists of its consequences for the flow of public spending over the horizon of the compulsory administration of the city.

Yet, it is possible that the mafia would have an incentive to relocate/downsize some of its business around the municipality, for fear that the whole area will be subject to intense police investigation. If this is the case, however, note that a relocation of mafia activities has in principle ambiguous effects on value added at provincial level. First, its impact may be quite contained if mafia simply sells existing activities which are formally legal, such as retail shops, and these continue to operate. Second, a lower intensity of some illegal activities, such as extortions, may actually reduce the burden of criminality on firms and households, with potentially positive effects. By the same token, the presence of an external commissioner may interfere with the activity of the local bureaucracy. Once again, however, one may envision positive and negative effects on the productivity of the local administration.

Although there is no ideal way to address such concerns, there are steps we can take to lessen them to some extent. Namely, if the transmission channel independently of spending is related to a rising intensity in policy investigation, it is plausible to expect that areas with a relatively heavy presence of the mafia would also be characterized by a relatively high number of mobsters arrested by the police. Under this maintained hypothesis, we can actually define controls for the possible channel by which city council dismissals affect the economy independent of the multiplier effects, mentioned above, as specified in the next section.

4 Empirical results

We are now ready to discuss the empirical results from our model. We start by providing some information about our measure of government spending and our instrument. From 1991 through to 1999, the median value of changes in investment $G_{i,t}$, in nominal terms per year-province, is 0.047% of the provincial GDP; the average value is 0.044% of GDP with a standard error of 0.038; thus, the average of investment changes is not statistically different from zero. Yet, observations with yearly changes of up to 0.5 or even 1 percent of GDP are common in our sample. The lowest and highest 5% percentiles record yearly changes up to -1.345 and 1.37 percentage points of GDP respectively; those relative to the 25% and 75% percentiles record spending cuts of

-0.26% of GDP and spending increases of 0.325% of GDP. The largest negative and positive changes are -8.52% and 10.65% respectively.

As regards our instrument, we first note that the dismissal of a city council on evidence of mafia infiltration can occur at any time during the year. The yearly flow of investment spending, and in turn its possible effect on the year-to-year change in local GDP, will crucially depend on how close the date of the city council dismissal is to the end of the calendar year. To account for this issue, we define multiple instruments as follows. Our first instrument, dubbed 'Council-dismissal-S1', equals the number of municipalities put under compulsory administration, provided that the official decree by which a city council is dismissed is formalized in the first semester of the year. To define our second instrument, 'Council-dismissal-S2', for each case of compulsory administration, we calculate the number of days between the dismissal of the city council and the year end, and average them over all municipalities in the same province$\times$year. For every province, 'Council-dismissal-S2' equals the number of municipalities put under compulsory administration in any given year, if the average number of days spent in such state is less than 180, and zero otherwise. We instrument $G_{i,t}$ entering 'Council-dismissal-S1' contemporaneously and 'Council-dismissal-S2' lagged one period. We should note here that results remain quite similar for alternative definitions of these variables, e.g., if we redefine them distinguishing the number of municipalities put under compulsory administration with an official decree formalized in the first or the second semester of the year. Two additional instruments can be derived by using two lags of a variable that simply records for each province$\times$year the number of municipality which are put under compulsory administration — dubbed 'Council-dismissal'.

To address concerns about the possibility that the dismissal of the city council affects value added independently of the contraction in public spending, our regression model includes five distinct variables accounting for the number of people reported to the judicial authority for (i) organized crime, (ii) extortion, (iii) mafia-related murders, (iv) corruption, as well as also (v) the number of corruption crimes reported to the judicial authority — all defined in per capita difference terms. We include these five variables both contemporaneously and lagged up to $t-2$. As explained above, we add these variables to our set of regressors under the maintained hypothesis that, if the disruption in economic activity is due to police investigation independently of the contraction in spending, this effect should be positively correlated with the investigation intensity, in turn proxied by the number of arrests.

4.1 Minimalist specification

Recalling that the two proxies for unemployment are specified in log-difference and in per-capita terms and entered lagged $t-1$ and $t-2$, our main results are as follows. As shown in the first column of Table 3, referring to the minimalist specification of our regression model, the impact multiplier is statistically different from zero, with a point estimate of 1.25: an exogenous cut in public infrastructure expenditure at local level as high as one per cent of local GDP determines a contemporaneous reduction in output of 1.25 per cent. This point estimate of the multiplier is somewhat lower compared to recent estimates of the multiplier exploiting cross-sectional variations across US states, larger than suggested by Barro and Redlick (2010) and others for the US economy as a whole.[15] It is again lower to the estimate by Giordano et al. (2007), looking at government consumption for the Italian economy as a whole. Nonetheless, it is worth stressing that we are not able to reject the null hypothesis $\beta \leqslant 1$ in favor of $\beta > 1$ at the standard confidence level. Thus, looking at the impact effect, we cannot exclude the possibility of changes in private demand compensating for the contraction in public spending.

The first stage regression confirms the results reported in Table 2: provinces under compulsory administration tend to have lower average investment. From the first stage regression, the coefficients of both instruments are estimated to be negative, as expected, and highly statistically significant. The value of the F-statistic, 9.5, with a p-value of 0.0001, suggests that we are not incurring a weak instrument issue. The Anderson-Rubin test rejects the null hypothesis, $\beta = 0$, at the 5% conventional level (p-value of 0.02). Finally, note that from the reduced form of the model (that is, regressing the dependent variable on covariates and the instrument), we see that provinces with municipalities under compulsory administration are indeed characterized by below-average output changes. The two coefficients of the instruments in the reduced form are negative; the coefficient attached to Council-dismissal-S2 is strongly significant (p-value of 0.02).

4.2 Enlarging the set of instruments and covariates

The second column of Table 3 reports estimates when we add to the set of instruments two lags of 'Council-dismissal' (at $t-2$ and $t-3$) recording the total number of municipalities put under the administration of external commissioners by province$\times$year. In other words, the total number of instruments is now 4. The main reason for doing so is that, since the compulsory administration is designed to last 18 months, it may have effects on three consecutive calendar years. Also, adding more instruments tends to increase the precision of estimates, and enlarge the set of variables for implementing overidentification tests.

Note that, relative to the first column of the table, in the second column the point estimate of the multiplier is 20% higher, while the t-ratio increases up to 2.58; as for the regression with 2 instruments only, the Hansen J statistic implies a p-value around 0.3, suggesting that the instruments are uncorrelated with the error term, and thus supporting our premise that the coefficient of the spending variable delivers an estimate of the spending multiplier. Also in this case, however, we cannot reject the null hypothesis that the multiplier is less than, or equal to 1.

[15] Note that the estimated multiplier in Barro and Redlick is about 1 when the unemployment rate gets to 11%, which is the average rate of unemployment in Italy during the 1990s.

As is well known, in relatively small samples, the gain in precision from adding more instruments might come at the cost of inducing some bias in the point estimates, often towards the OLS result, if instruments are weak (see Bound, Jaeger, and Baker 1995; Angrist and Pischke 2009). In principle, this may be the case in our overidentified models, as indicated by the reduction in the first-stage F-statistic — which nevertheless remains significant at the 1 per cent confidence level. However, we should stress that in our sample the OLS estimate of β is much below 1: the results from the Hansen J-test and the marked rise in the estimated value of β lends support to our model with 4 instruments.

The model in the third column of Table 3 makes a different use of the variable 'Council-dismissal'. Instead of using its lagged values at $t-2$ and $t-3$ as additional instruments, we include these lags as further controls. As apparent, the estimated coefficient for β (and its standard error) is lower relative to the model with 4 instruments in the second column, and very similar to the the one related to the minimalist specification in the first column. It seems of interest to note that, although these two lags of 'Council-dismissal' affect the point estimate and the standard error of β when added to the set of instruments, their coefficients are not different from zero when such lags are used as control variables. Thus, there is no evidence of a direct effect of the lagged values of our instrument on GDP.

In the table 4 under the headings R4 and R5, we deal with the potential problems from serially correlated errors by including two lags of the left-hand-side variable among the regressors. Only the first lag is significant, but marginally so. With the province and year fixed effects in place, the impact of adding these lags is negligible: there is hardly any change in the point estimate and the significance of β relative to Table 3.

4.3 Dynamics

In the two final columns of Table 4, we add two lags of public investment expenditure to the previous specifications. In this case, both the contemporaneous and the one-year lagged spending have statistically and economically significant coefficients — with the point estimate of the contemporaneous coefficient being twice as large.

A notable result is that the coefficient on contemporaneous public investment remains quite stable after the inclusion of these lags, indicating that we can disentangle the delayed effect of spending variations with some precision. For instance, in the model with the lags of the 'Council-dismissal' variable among the controls, adding lags of public spending raises the estimate of the impact multiplier from 1.43 to 1.52. Yet, it also causes the first lag of GDP growth to become significantly different from zero at the 5% confidence level. Thus, when considering the net effect of public spending on GDP growth (obtained by dividing the coefficient β by 1 minus the coefficient of $Y\,(t-1)$), the resulting estimate of the multiplier is 1.3, quite close to that of our minimalist model (without lagged spending).

However, if the lagged effect of spending is considered as part of the multiplier, then adding up the two coefficients relative to the contemporaneous and the one-year lagged investment change yields a combined estimate of the multiplier as high as 2.18 and 1.97, respectively, for models under the headings R6 and R7 (after correcting for the presence of lags of the dependent variable). In particular, when the model with four instruments is considered, the p-value for testing the null hypothesis that the overall multiplier is less than, or equal to 1, is 0.04.

A potential issue with our single-equation model, compared to a multiple-equation framework, is that our estimate of the effects of government investment does not take into account possible feedback from GDP to investment: strictly speaking, our results cannot be compared with results from SVAR models (a point stressed by Sims in his comments on Angrist and Pischke, 2010). However, our first-stage regression suggests that infrastructure investment does not react to GDP changes. For instance, in the first stage of the model in R7, the coefficients of the two GDP lags are not statistically different from zero. Point estimates for the first and second lag are, respectively, 0.03 (with a t-ratio of 1.21) and -0.01 (with a t-ratio of -0.41). In view of this evidence, we are confident that, in our case, the single equation 1 and the IV estimator correctly capture the short-run effects of public investment on output.

4.4 Sensitivity

4.4.1 The influence of individual provinces

The impact on the macroeconomic activity of a fiscal contraction, or even the sheer size of spending cuts, may be different across episodes of city council dismissals. It is plausible that some episodes exerts a stronger influence on our estimates, the same way in which some particular fiscal episodes of fiscal expansions — e.g. the US military build up during the Korean war — are recognized to be key in ascertain aggregate multiplier effects. We address this issue by analyzing the extent to which our results are sensitive to the exclusion of any particular province from the analysis.

In Tables 5 and 6, we report results for the most comprehensive specifications of our model excluding one of the following provinces in turn: Napoli, Caserta, Palermo, Catania, Salerno, Bari, Reggio Calabria. As shown in Table 1, these are the provinces with most episodes of city council dismissals. For comparison, in the first column we report again results relative to the whole sample too.

Observe that none of the provinces is a crucial driver of our estimates. Results do not vary dramatically across columns: sign and range of estimates of coefficients, as well as significance levels, are quite comparable. And yet in some cases — the exclusion of Caserta and Catania — the point estimates is apparently smaller. Accounting for this exercise, the range for our point estimates becomes larger — between 1 and 1.6.

4.4.2 Spillovers across borders

A key question is whether and to which extent the fall in local economic activity in response to cuts in local spending on infrastructure is compensated by an increase in economic activity in other areas, as production factors move in response to the localized fiscal shock. In addressing this question, it is worth stressing from the start that, by defining a province as our geographical unit of analysis, we are already measuring the effects of contractions in one area (the municipality) on the level of activity of greater areas (a province aggregates several municipalities). Part of the answer is already provided in our estimates.

And yet, variations in government investment within a province might also affect the GDP of neighboring provinces, in particular those which are part of the same region. To examine whether this is the case, as a first instance we consider the variable $SG_{i,t} = \frac{Sg_{i,t} - Sg_{i,t-1}}{Sy_{i,t-1}}$ and its first lag, where for each province i and year t, $Sg_{i,t}$ is the per capita investment across provinces which are part of the same region as i, excluding province i itself, and $Sy_{i,t-1}$ is defined accordingly. Results are shown in the first column of table 7, labelled S1. As can be seen, no evidence of spillovers is detected; the coefficient of the newly defined variables are both equal to zero.

In the second column, labelled S2, we enter $SG_{i,t-1}$ (in terms of deviation from the median value) interacted with $G_{i,t-1}$ (again as deviation from the median) to address whether the effect of local spending depends upon complementarity between spending in adjacent areas. Now the new coefficient is marginally significant with a positive sign.

Finally, in the last column we show results after aggregating either two or three adjacent provinces in a single area. Both coefficients of $G_{i,t}$ and $G_{i,t-1}$ tend to increase a bit providing further evidence that, if any, the spillover effect adds to the local effect of spending. Thus, after a drop in local public spending no evidence at all emerges of countervailing effect in adjacent areas.

4.4.3 Controlling for North-South differences

One potential issue is that mafia-related compulsory administrations are mainly in the South, where economics conditions are in general different respect to the North in many dimensions, including a stronger economic weight of public activities, and the presence of the mafias may be expected to be more pervasive. In our model, we already include provice fixed effects and other controls to account for these differences. Nonetheless, in Table 8, we verify further whether North-South variability is crucial for our results, by excluding observations from the North. Overall, after such exclusion, the impact multiplier tends to be a bit smaller, 1.25 — with the proportion between the lagged and the contemporaneous effect still at 1 to 2. The estimates remains statistically significant at the 5% level, and thus not significantly different from 1 at standard confidence levels.

4.4.4 National business cycle and fiscal-monetary mix

As already mentioned, a key problem in estimating public spending multipliers, especially on aggregate data, is that movements in government purchases are likely to be endogenous with respect to GDP, and depend on the interaction between monetary and fiscal policy, including budget policies governing the adjustment of future taxes and revenues to contemporaneous shocks. To the extent that the model fails to capture the systematic pro- or anti-cyclical component of spending, for instance, reverse causation translates into spurious estimates of the multiplier effects. By the same token, the multiplier effects may be mismeasured if the monetary stance is not appropriately controlled.

To shed light on these issues, in the third column of Table 8 we report estimates obtained by dropping the calendar year dummies from our set of regressors. The idea is that there could be common factors affecting all provinces in any period of time. Removing the time dummies indeed raises the impact multiplier, but not dramatically so. The multiplier becomes 1.81 in the dynamic specification, but its value remains not significantly different from one. The coefficient capturing the delayed effect of spending is instead not affected.

Finally, for the sake of comparison, we also include OLS estimates of the contemporaneous and the one-year lagged public investment spending — last column of Table 8. Both coefficients are statistically significant, but small in economic magnitude. The OLS coefficient of the contemporaneous multiplier is about 0.2, that is, six times lower than the corresponding IV estimate of the basic model — a comparable result is reported by Serrato and Wingender (2010). As already mentioned, a low OLS estimate may at least in part reflect a systematic policy of fund allocation towards the provinces with lower long-run growth pursued by the central government. In addition, however, there are usually long lags between the announcement of the fund allocation, and the implementation of the projects, which tend to be multi-year. Anticipation effects can also be expected to weigh on the low OLS estimates.

5 Conclusions

We have derived an estimate of the government spending multiplier using spending in infrastructure at provincial level in Italy, instrumented with the large cuts in the financing of investment projects at local level mandated by the government on evidence of mafia infiltration in a local city council — arguably exogenous to local cyclical conditions. An important question is how these estimates should be interpreted in the context of the long-standing debate on the size of the multiplier. We have emphasized that our study is designed so to be less exposed than time series analyses at the aggregate level, to the risk of deriving biased estimates of multipliers as a result of reverse causation — whereas public expenditure may indeed systematically vary in a pro- or anti-cyclical fashion. In contrast, we derive our estimates of the multipliers controlling for common components arguably capturing not only the cycle, but also the national monetary stance, in an institutional environment in which local public spending is allocated by the central government, with virtually no consequences for the level of local taxation. In light of these observations, our empirical estimates could in principle be read as approximating the macroeconomic transmission of (negative) changes in spending, which are matched neither by changes in monetary stance, nor by changes in the tax burden — arguably corresponding to the textbook exercise in conventional introductions to the theory of fiscal stabilization.

In contrast to most of the literature estimating fiscal multipliers, the instrument we use to identify exogenous variations in government spending de facto defines multipliers associated with sharp fiscal retrenchment. In most studies, especially on U.S. data, the variations in fiscal stance which are identified as exogenous to cyclical conditions typically consist of expansions in military spending arguably dictated by non-economic events. Episodes of sharp fiscal contractions usually do not qualify, since they are typically undertaken in reaction to a deteriorating debt and deficit outlook.

On impact, we cannot reject the hypothesis that local economic activity falls one to one with the interruption of local public spending. Sensitivity analysis points to a range of point estimates between 1.2 and 1.4. Dynamically, we detect a statistically significant, positive correlation of past spending with current output. Under the maintained hypothesis that lagged spending is exogenous to current output, this raises the point estimates of our multiplier to close to 2, including dynamic effects — a value that is even significantly larger than 1 in some specifications of our empirical model. Relative to other contributions, the design of our study however suggests strong caution in extrapolating our results to the case of spending expansions.

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Data appendix

Public investment in infrastructure includes spending on the following categories: Transport (roads and airports, railroads and other kinds of transportation, ports and rivers, telecommunications); Sanitation-Energy-Reclamation (hospitals, electric and hydroelectric plants, swamps, land reclamation, other categories); Buildings (public buildings and schools; public spending devoted to private buildings)**.** Data are at current prices. Source: ISTAT, *Annuario delle Opere Pubbliche*, (various issues). From 1986 to1999 ISTAT collected quarterly data on infrastructure investment at municipality level through the network of local statistical offices. The data were then aggregated at province level at yearly frequency. Since not all municipalities were included in the data collection, for each *year and province* ISTAT provides an index M_{it} useful to convert the sample data into the effective level of provincial investment. In particular, let $\widetilde{x}_{it}$ denote the level of investment for province i at time t aggregating information from the municipalities in the sample. Given the index M_{it}, the estimated overall public investment at provincial level is $x_{it} = \widetilde{x}_{it}/M_{it}$.

GDP. Total value added measured in millions of euro at current prices. Sources: Istituto Guglielmo Tagliacarne and ISTAT.

Cassa Integrazione Guadagni. "Cassa integazione guadagni" is the main unemployment benefit arrangement covering employees of private firms in Italy. Source: Istituto Guglielmo Tagliacarne.

Population. Source: ISTAT, *Statistiche Demografiche* (various issues).

Compulsory administration. Municipalities placed under the administration of external commissioners by the central government on evidence of ties between administrators and the mafias, either through the direct infiltration of mobsters among local bureaucrats or politicians or through indirect influence. Source: *Commissione parlamentare d'inchiesta sul fenomeno della criminalità organizzata mafiosa o similare. Technical Report* (various issues).

Mafia-type association. People reported by the police forces to the judicial authority because of mafia association (art. 416-bis of the Italian penal code). Source: ISTAT, Statistiche giudiziarie (various issues).

Extortion. People reported by the police forces to the judicial authority because of extortion. Source: ISTAT, Statistiche giudiziarie (various issues).

Murder. People reported by the police forces to the judicial authority because of murders related to the activity of mafia associations. Source: ISTAT, Statistiche giudiziarie (various issues).

Corruption. Crimes and people prosecuted relative to a broad measure of corruption, including embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements.

Table 1: Compulsory Administration and mafia

Napoli	44	Palermo	23	Reggio C.	23	Bari	5
Caserta	22	Catania	9	Catanzaro	7	Lecce	2
Salerno	5	Trapani	5	Vibo V.	5		
Avellino	3	Caltanisetta	5	Crotone	3		
Benevento	1	Agrigento	4				
		Messina	2				
		Ragusa	1				
Campania	75	Sicily	49	Calabria	38	Puglia	7

Note: The table reports the number of municipalities put under the administration of external commissioners because of relationships between elected administrators and the mafias. Time period 1991-2008.

Table 2: Investment Spending, Mean Difference Test

	Log-difference	Log-difference	Percent of GDP	Percent of GDP
Difference	-0.220***	-0.228**	-0.555**	-0.650*
	[-3.63]	[-3.21]	[-2.61]	[-2.45]
Control group	0.0584***	0.0666	0.120**	0.215
	[4.70]	[1.72]	[3.10]	[1.32]
N	950	180	950	180

Note: The table shows the results of mean difference tests relative to changes in public infrastructure investment. We divide the sample into two groups of observations: the treatment group consists of the year-province observations in which at least a municipality in the province is put under compulsory administration; the control group includes the rest of the sample. In the table, "Difference" reports a measure of variations in investment driven by compulsory administrations, that is the mean difference test by comparing investment variations across the two groups. In the second and fourth columns, we change the treatment and the control group, by restricting the former to provinces characterized by at least one case of local government dismissal during the sample period. Data are annual from 1990 to 1999 at Italian province level. The t-statistic is reported in brackets: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}\ p < 0.001$

Table 3: Investment Spending Multiplier

	R1	R2	R3
G(t)	1.25**	1.51***	1.37**
	[2.33]	[2.58]	[2.47]
Council-dismissal $(t-2)$			-0.22
			[-1.19]
Council-dismissal $(t-3)$			-0.06
			[-0.35]
time effects	YES	YES	YES
provincial fixed effects	YES	YES	YES
controls for mafia investigation	YES	YES	YES
unemployment rate proxies	YES	YES	YES
number of instruments	2	4	2
First stage F-test	9.46	4.27	8.43
(instruments validity)	(0.00)	(0.00)	(0.00)
N	950	950	950

Note: Data are annual from 1990 to 1999 at Italian province level. The dependent variable is the year-on-year change in per capita real Value Added divided by the previous year's per capita real Value Added. G(t) is the dated t year-on-year change in per capita real infrastructure investment (nominal spending divided by the GDP deflator) divided by the previous year's per capita real Value Added. Council-dismissal(t-2) and Council-dismissal(t-3) are the lagged values of number of municipalities put under compulsory administration for a given province at t. All estimated equations contain on the right-hand side year dummies, the first two lags of employment and the hours of 'cassa integrazione' both entered as per-capita lop-difference. Moreover, all equations contain the following set of variables (specified in log-difference, and in per capita terms, up to two lags): the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) mafia murders; the number of crimes and people prosecuted relative to corruption. Regarding the latter, data include embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements. In general, the spatial distribution of such variables reflects the province where the crime is effectively committed. Estimation is by two-stage least-squares. Standard errors clustered at the region level. The t-statistic is reported in squared brackets: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4: Investment Spending Multiplier (cont.)

	R4	R5	R6	R7
G(t)	1.58***	1.43***	1.70***	1.52***
	[2.79]	[2.72]	[3.24]	[2.75]
Council-dismissal $(t-2)$		-0.24		-0.12
		[-1.35]		[-0.60]
Council-dismissal $(t-3)$		-0.08		-0.06
		[-0.47]		[-0.35]
Y(t-1)	-0.13*	-0.13*	-0.17***	-0.16***
	[-1.84]	[-1.94]	[-2.67]	[-2.65]
Y(t-2)	0.00	-0.00	-0.01	-0.01
	[0.01]	[-0.03]	[-0.24]	[-0.27]
G(t-1)			0.85***	0.77***
			[3.44]	[2.98]
G(t-2)			0.22*	0.20*
			[1.85]	[1.70]
time effects	YES	YES	YES	YES
provincial fixed effects	YES	YES	YES	YES
controls for mafia investigation	YES	YES	YES	YES
unemployment rate proxies	YES	YES	YES	YES
number of instruments	4	2	4	2
First stage F-test	4.76	9.39	4.24	8.35
(instruments validity)	(0.00)	(0.00)	(0.00)	(0.00)
N	950	950	950	950

Note: Data are annual from 1990 to 1999 at Italian province level. The dependent variable is the year-on-year change in per capita real Value Added divided by the previous year's per capita real Value Added. G(t) is the dated t year-on-year change in per capita real infrastructure investment (nominal spending divided by the GDP deflator) divided by the previous year's per capita real Value Added. G(t-1) and G(t-2) are the lagged values of G. Council-dismissal(t-2) and Council-dismissal(t-3) are the lagged values of number of municipalities put under compulsory administration for a given province at t. All estimated equations contain on the right-hand side year dummies, two lags of the dependent variable, the first two lags of employment and the hours of 'cassa integrazione' both entered as per-capita lop-difference. Moreover, all equations contain the following set of variables (specified in log-difference, and in per capita terms, up to two lags): the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) mafia murders; the number of crimes and people prosecuted relative to corruption. Regarding the latter, data include embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements. In general, the spatial distribution of such variables reflects the province where the crime is effectively committed. Estimation is by two-stage least-squares. Standard errors clustered at the region level. The t-statistic is reported in squared brackets: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table 5: Investment Spending Multiplier: dropping provinces

	ITA	NA	CE	PA	CT	SA	BA	RC
G(t)	1.70***	1.65***	1.33***	1.64***	1.39**	1.69***	1.64***	1.65***
	[3.24]	[3.48]	[2.98]	[3.07]	[2.29]	[3.23]	[3.25]	[3.16]
Y(t-1)	-0.17***	-0.17***	-0.16***	-0.17***	-0.16***	-0.16***	-0.16***	-0.14**
	[-2.67]	[-2.68]	[-2.78]	[-2.65]	[-2.72]	[-2.58]	[-2.60]	[-2.23]
Y(t-2)	-0.01	-0.01	-0.02	-0.01	-0.02	-0.01	-0.01	-0.03
	[-0.24]	[-0.20]	[-0.41]	[-0.24]	[-0.37]	[-0.25]	[-0.15]	[-0.58]
G(t-1)	0.85***	0.83***	0.70***	0.83***	0.73***	0.85***	0.83***	0.84***
	[3.44]	[3.61]	[3.22]	[3.46]	[2.60]	[3.43]	[3.46]	[3.47]
G(t-2)	0.22*	0.22*	0.17	0.21*	0.18	0.22*	0.21*	0.21*
	[1.85]	[1.89]	[1.63]	[1.77]	[1.52]	[1.87]	[1.82]	[1.82]
time effects	YES	YES	YES	YES	YES	YES	YES	YES
provincial fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
controls mafia	YES	YES	YES	YES	YES	YES	YES	YES
unemp. rate	YES	YES	YES	YES	YES	YES	YES	YES
number of instruments	4	4	4	4	4	4	4	4
First stage F-test	4.24	5.41	7.70	3.30	3.92	3.25	4.21	3.45
(instruments validity)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)
N	950	940	940	940	940	940	940	940

Note: Data are annual from 1990 to 1999 at Italian province level. The dependent variable is the year-on-year change in per capita real Value Added divided by the previous year's per capita real Value Added. G(t) is the dated t year-on-year change in per capita real infrastructure investment (nominal spending divided by the GDP deflator) divided by the previous year's per capita real Value Added. G(t-1) and G(t-2) are the lagged values of G. All estimated equations contain on the right-hand side year dummies, two lags of the dependent variable, the first two lags of employment and the hours of 'cassa integrazione' both entered as per-capita lop-difference. Moreover, all equations contain the following set of variables (specified in log-difference, and in per capita terms, up to two lags): the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) mafia murders; the number of crimes and people prosecuted relative to corruption. Regarding the latter, data include embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements. In general, the spatial distribution of such variables reflects the province where the crime is effectively committed. Estimation is by two-stage least-squares. Standard errors clustered at the region level. The t-statistic is reported in squared brackets: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table 6: Investment Spending Multiplier: dropping provinces

	ITA	NA	CE	PA	CT	SA	BA	RC
G(t)	1.52***	1.59***	1.12**	1.50***	1.31**	1.52***	1.51***	1.34***
	[2.75]	[2.80]	[2.57]	[2.72]	[2.08]	[2.70]	[2.78]	[2.64]
Y(t-1)	-0.16***	-0.16***	-0.16***	-0.17***	-0.16***	-0.16***	-0.16***	-0.13**
	[-2.65]	[-2.65]	[-2.71]	[-2.61]	[-2.69]	[-2.58]	[-2.59]	[-2.15]
Y(t-2)	-0.01	-0.01	-0.02	-0.01	-0.02	-0.02	-0.01	-0.04
	[-0.27]	[-0.22]	[-0.40]	[-0.26]	[-0.38]	[-0.29]	[-0.18]	[-0.74]
Council-dismissal $(t-2)$	-0.12	-0.01	-0.24	-0.08	-0.05	-0.09	-0.09	-0.24
	[-0.60]	[-0.02]	[-1.27]	[-0.38]	[-0.28]	[-0.44]	[-0.43]	[-1.43]
Council-dismissal $(t-3)$	-0.06	-0.08	-0.11	-0.03	-0.01	-0.08	-0.04	-0.03
	[-0.35]	[-0.31]	[-0.59]	[-0.19]	[-0.05]	[-0.46]	[-0.24]	[-0.18]
G(t-1)	0.77***	0.81***	0.61***	0.77***	0.70**	0.78***	0.77***	0.71***
	[2.98]	[2.96]	[2.89]	[3.06]	[2.38]	[2.92]	[2.99]	[2.99]
G(t-2)	0.20*	0.21*	0.14	0.19*	0.17	0.20*	0.19*	0.17
	[1.70]	[1.73]	[1.41]	[1.69]	[1.43]	[1.71]	[1.67]	[1.58]
time effects	YES	YES	YES	YES	YES	YES	YES	YES
provincial fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
controls mafia	YES	YES	YES	YES	YES	YES	YES	YES
unemp. proxies	YES	YES	YES	YES	YES	YES	YES	YES
number of instruments	2	2	2	2	2	2	2	2
First stage F-test	8.35	9.61	14.69	6.59	7.40	6.35	8.25	6.85
(instruments validity)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	950	940	940	940	940	940	940	940

Note: Data are annual from 1990 to 1999 at Italian province level. The dependent variable is the year-on-year change in per capita real Value Added divided by the previous year's per capita real Value Added. G(t) is the dated t year-on-year change in per capita real infrastructure investment (nominal spending divided by the GDP deflator) divided by the previous year's per capita real Value Added. G(t-1) and G(t-2) are the lagged values of G. Council-dismissal(t-2) and Council-dismissal(t-3) are the lagged values of number of municipalities put under compulsory administration for a given province at t. All estimated equations contain on the right-hand side year dummies, two lags of the dependent variable, the first two lags of employment and the hours of 'cassa integrazione' both entered as per-capita lop-difference. Moreover, all equations contain the following set of variables (specified in log-difference, and in per capita terms, up to two lags): the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) mafia murders; the number of crimes and people prosecuted relative to corruption. Regarding the latter, data include embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements. In general, the spatial distribution of such variables reflects the province where the crime is effectively committed. Estimation is by two-stage least-squares. Standard errors clustered at the region level. The t-statistic is reported in squared brackets: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table 7: Public Spending Multiplier: Spillover

	S1	S2	S3
G(t)	1.45**	1.43***	1.83***
	[2.22]	[2.74]	[3.23]
Y(t-1)	-0.17***	-0.17***	-0.19***
	[-2.82]	[-2.70]	[-2.87]
Y(t-2)	-0.01	-0.01	-0.00
	[-0.22]	[-0.19]	[-0.08]
Council-dismissal(t-2)	-0.12	-0.14	-0.10
	[-0.60]	[-0.72]	[-0.52]
Council-dismissal(t-3)	-0.06	-0.06	-0.13
	[-0.33]	[-0.35]	[-1.02]
G(t-1)	0.73**	0.72***	0.98***
	[2.43]	[3.08]	[3.39]
G(t-2)	0.18	0.21*	0.23
	[1.54]	[1.88]	[1.27]
SPILL(t)	0.08		
	[0.31]		
SPILL(t-1)	0.25		
	[1.10]		
SPILL(t-1)*G(t-1)		0.20**	
		[2.03]	
time effects	YES	YES	YES
provincial fixed effects	YES	YES	YES
controls for mafia investigation	YES	YES	YES
unemployment rate proxies	YES	YES	YES
number of instruments	2	2	2
First stage F-test	5.81	8.32	14.26
(instruments validity)	(0.00)	(0.00)	(0.00)
N	950	950	410

Note: Data are annual from 1990 to 1999 at Italian province level. The dependent variable is the year-on-year change in per capita real Value Added divided by the previous year's per capita real Value Added. G(t) is the dated t year-on-year change in per capita real infrastructure investment (nominal spending divided by the GDP deflator) divided by the previous year's per capita real Value Added. G(t-1) and G(t-2) are the lagged values of G. Council-dismissal(t-2) and Council-dismissal(t-3) are the lagged values of number of municipalities put under compulsory administration for a given province at t. All estimated equations contain on the right-hand side year dummies, two lags of the dependent variable, the first two lags of employment and the hours of 'cassa integrazione' both entered as per-capita lop-difference. Moreover, all equations contain the following set of variables (specified in log-difference, and in per capita terms, up to two lags): the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) mafia murders; the number of crimes and people prosecuted relative to corruption. Regarding the latter, data include embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements. In general, the spatial distribution of such variables reflects the province where the crime is effectively committed. Estimation is by two-stage least-squares. Standard errors clustered at the region level. The t-statistic is reported in squared brackets: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table 8: Public Spending Multiplier: Further results

	Drop North	Drop Crimes	Drop λ_t	Drop α_i	OLS
G(t)	1.25**	1.23**	1.81***	1.73***	0.20***
	[2.09]	[2.49]	[3.01]	[2.81]	[3.15]
Y(t-1)	-0.26***	-0.14**	-0.11*	-0.07	-0.12**
	[-3.72]	[-2.39]	[-1.77]	[-1.11]	[-2.14]
Y(t-2)	-0.00	-0.02	0.06	0.06	-0.03
	[-0.03]	[-0.29]	[0.97]	[0.99]	[-0.56]
Council-dismissal(t-2)	-0.07	-0.14	-0.01	-0.14	-0.24
	[-0.41]	[-0.72]	[-0.08]	[-0.66]	[-1.63]
Council-dismissal(t-2)	0.10	-0.12	0.05	-0.08	-0.12
	[0.64]	[-0.77]	[0.22]	[-0.44]	[-0.92]
G(t-1)	0.63**	0.65***	0.76**	0.78***	0.23***
	[2.35]	[2.85]	[2.43]	[2.84]	[3.29]
G(t-2)	0.13	0.15	0.13	0.14	0.03
	[1.23]	[1.61]	[1.04]	[1.32]	[0.47]
time effects	YES	YES	NO	YES	YES
provincial fixed effects	YES	YES	YES	NO	YES
controls for mafia investigation	YES	NO	YES	YES	YES
unemployment rate proxies	YES	YES	YES	YES	YES
number of instruments	2	2	2	2	
First stage F-test	6.90	8.33	9.44	8.93	
(instruments validity)	(0.00)	(0.00)	(0.00)	(0.00)	
N	540	950	950	950	950

Note: Data are annual from 1990 to 1999 at Italian province level. The dependent variable is the year-on-year change in per capita real Value Added divided by the previous year's per capita real Value Added. G(t) is the dated t year-on-year change in per capita real infrastructure investment (nominal spending divided by the GDP deflator) divided by the previous year's per capita real Value Added. G(t-1) and G(t-2) are the lagged values of G. Council-dismissal(t-2) and Council-dismissal(t-3) are the lagged values of number of municipalities put under compulsory administration for a given province at t. All estimated equations contain on the right-hand side year dummies, two lags of the dependent variable, the first two lags of employment and the hours of 'cassa integrazione' both entered as per-capita lop-difference. Moreover, all equations contain the following set of variables (specified in log-difference, and in per capita terms, up to two lags): the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) mafia murders; the number of crimes and people prosecuted relative to corruption. Regarding the latter, data include embezzlement, misappropriation of yield to the damage of government, extortion and bribery agreements. In general, the spatial distribution of such variables reflects the province where the crime is effectively committed. Estimation is by two-stage least-squares. Standard errors clustered at the region level. The t-statistic is reported in squared brackets: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$