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ABSTRACT

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

We estimate the multiplier relying on differences in spending in infrastructure across Italian provinces and an instrument identifying investment changes that are large and exogenous to local cyclical conditions. We derive our instrument from the Law mandating the interruption of public work on evidence of mafia infiltration of city councils. Our IV estimates on cross sectional data allow us to address common problems in time series analysis, such as the risk of estimating spuriously high multipliers because of endogeneity and reverse causation, or the risk of confounding the effects of fiscal and monetary measures. Accounting for contemporaneous and lagged effects, and controlling for the direct impact of anti-mafia measures on output, our results suggest a multiplier as high as 1.4 on impact, and 2 including dynamic effects.

JEL Classification: C26, E62 and H54
Keywords: government spending, instrumental variables and multiplier

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1 Introduction

Endogeneity and omitted variables are key concerns in estimating the response of GDP to fiscal policy. Time series studies, for instance, face daunting difficulties in identifying innovations in government spending, 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, may spuriously raise the multipliers estimated (see e.g. Barro and Redlick 2010 and Ramey 2009). Similarly, the response of private expenditure to an expansion in government spending can be expected to depend on the overall policy mix, including budget consolidation policies: a bias may follow from the omission of monetary policy and debt/deficit variables from the empirical model (see e.g. Corsetti et al. 2009, Leeper et al. 2009, and Woodford 2010). In this paper, we provide an estimate of government spending multipliers which is to a large extent shielded from these issues. First, we carry out our estimation by exploiting cross-sectional information — we look at the cross-sectional variation in public investment spending across Italian provinces financed by the general government, controlling for both common cyclical movements and common policy impulses. Second, we rely on an instrument derived from a specific institutional arrangement which produces large and sudden changes in spending unrelated to cyclical conditions in the local economy: on evidence of mafia infiltration in a city council, an Italian law mandates the dismissal of the elected officials, with an immediate defacto interruption of payments into investment projects. Our point estimate of the multiplier is 1.4 on impact, with the 95% confidence interval containing 1 — hence we cannot reject the hypothesis of crowding out of private spending. Including the dynamic effects from spending, however, the overall multiplier is as high as 2.

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. Studies following a narrative approach instead use 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]

[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

The availability of cross-sectional data on fiscal variables at the local level raises new possibilities in the design of identification strategies, especially when combined with institutional information. In our analysis, we estimate multipliers from local public investment while controlling for national, common components, whose variations are usually predictable and arguably endogenous. Since the allocation of funding across Italian provinces during our sample years is decided at the central government level, once we control for the common national components, our results are less exposed to the criticism stressing endogeneity of spending as a cause of spuriously high multipliers in time series models. In this respect, it is actually possible that in our cross-sectional study OLS estimators may suffer from a *downward* bias. To the extent that the central government allocates resources for local investment spending with the goal of fostering growth in low-growth regions, the areas of the country with the worst economic prospects would systematically receive the highest amount of resources for local investment.

By the same token, our cross-sectional framework is less vulnerable to the criticism pointing to the omitted variable bias from ignoring the interaction of fiscal and monetary policy, or the budget implications of fiscal expansions. Using cross-sectional information, we naturally control for the national monetary stance, common to all provinces, thus addressing issues in the importance of monetary policy as key determinant of the size of the multiplier (see Christiano et al. 2009; Woodford 2010). Moreover, over our sample period, changes in local investment spending in Italy had virtually no consequences on the tax burden faced by the local population: tax rates are set almost exclusively at the national level.

In addition, a key piece of institutional information allows us to identify an exogenous source of variations in government spending at the local level that are large and implemented 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

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).

negative conditional on a municipality being placed under compulsory administration, with an average contraction of 20 percentage points. The instrument we derive from this piece of institutional information is key to disentangling exogenous changes in spending programmes, and addressing the potential biases of OLS estimators.

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 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 panel data sets 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 exploit cross-sectional variations in public spending (looking at infrastructure investment projects at the provincial level in Italy); second, that we can 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 cross-sectional variations. 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 being a geographic entity similar to a U.S. county, and containing several municipalities. Dictated by the availability of comparable data, our sample covers the whole country over the ten-year span between 1990 and 1999. 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

$$Y_{i,t} = \alpha + \beta G_{i,t} + \delta_2 D2_t + \cdots + \delta_T DT_t + v_{i,t}, \tag{1}$$

where the Ds are time dummies. The coefficient β measures the contemporaneous government spending multiplier. In our sample, there are 95 provinces distributed over 20 administrative regions. Because of this administrative structure, our data may be thought of as a cluster sample where each unit, the province, is part of a cluster, the region. Since observations within a region may be correlated as a result of an unobserved cluster effect, we base our inference on standard errors robust to spatial correlation within regions, as in Guiso et al. (2004).

A key advantage of estimating equation (1) with cross-sectional data to measure the effect of spending consists in the fact that one can control for national components in public investment and GDP common to all provinces by including time dummies. 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 spuriously lead to high estimate of multipliers, 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.

We nonetheless control for business cycle movements specific to provinces, by including two lags of the dependent variable and a proxy for unemployment (hours of *Cassa Integrazione Guadagni*).[2] Moreover, we also include two lags of $G_{i,t}$ — capturing the delayed effects of public investment. The identifying assumption is that the lags of $G_{i,t}$ are pre-determined with respect to $Y_{i,t}$, in line with the SVAR literature.[3] Importantly, we will show below that the cross-sectional distribution of

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[2] The *Cassa Integazione Guadagni* is the main unemployment benefit arrangement covering employees of private firms in Italy. We have also experimented with different proxies, such as the unemployment rate, without detecting any substantial difference in results. In general, many variables are likely to be serially correlated. Thus, allowing for lagged growth provides a way of controlling for variables omitted from the minimalist specification.

[3] Barro and Redlick (2010) use the lagged value of their defence spending variable as an instru-

investment changes and lagged output changes are uncorrelated, controlling for lagged investment.[4]

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 study, however, the inclusion of controls for common national components via time dummies also accounts for the effects of common policy measures, with homogeneous effects throughout the country — including changes in the monetary policy stance. Our cross-sectional estimates of the multipliers do not therefore reflect systematic contemporaneous interactions of fiscal and monetary policy at national level.

In addition, over the sample period under consideration, investment spending at local level in Italy is allocated by the central government. Local administrations have control over the realization of projects, but limited or no power to set local taxes. Hence the transfer of resources by the central government to finance local investment is not associated with variations in the tax bill faced by the residents in the municipality. This feature of our sample allows us to by-pass issues arising from the omission of tax changes (or debt) from the equation estimated. 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.[5] More in general, in our sample, variations in the level of local investment spending do not correspond to variations in the tax burden of local residents.

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mental variable.

[4] After including the time dummies and lagged business-cycle indicators, the omission of additional variables which may affect GDP growth but are orthogonal to the investment variable would not bias the estimate of β.

[5] 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

3 Instrumenting Changes in Public Spending

Despite the advantages of using cross-sectional 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 have systematically allocated funds with the goal of compensating differences in growth rates at local level. To the extent that the provinces with the worst economic prospects systematically received the highest amount of funds for investment projects, the OLS estimates would be biased downwards. To address these problems, we need to find a good instrument for unexpected variations in public spending exogenous to local economic conditions. In this respect, Angrist and Pischke (2010), among others, suggest relying on quasi-experimental design, exploiting institutional information. This is the strategy we adopt in our study. In what follows, we make the case for using the compulsory administration of local municipalities mandated by the Italian government on evidence of mafia infiltration to instrument differences in public investment changes at the local level.

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.[6] 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 results of electoral competition and obtaining effective control over public tenders.[7]

[6] 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.

[7] 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.

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.[8] Profits accruing to organized crime from these activities 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 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,[9] 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 their removal, the central government appoints three non-elected, external commissioners, to rule the municipality for a period of up to 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).[10]

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[8] Historically, the prospects for profits for mafias were boosted by the large public funds targeted at reconstruction activities after the strong earthquake hitting vast regions in the south of Italy in 1980.

[9] 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).

[10] 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

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.[11] 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 "you 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 43 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 more than one municipality. Even when the city under compulsory administration is small, the size of public investment affected by the freeze can end up being large at the provincial level.

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). We also compare

convicted for bribes, and the number of people convicted for mafia association (Acconcia et al. 2009).

[11]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.

results for the whole sample (columns 1 and 3 of the Table) with results for a subsample restricted to provinces with at least one case of compulsory administration (columns 2 and 4). 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 when the whole sample is considered. 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, similarly, there are no systematic differences across the North-South divide.

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).[12] 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, and is thus virtually independent of (local) cyclical conditions. This feature ensures that the contraction in investment identified via our instrument is exogenous to local economic activity. Second, based on policy investigation, the decision to dismiss a council and the implementation of such a decision occur quite rapidly.[13] Hence, anticipations of government-mandated cuts in spending from one year to another are unlikely to play a significant role in our sample.

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 for it. 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

[12]See Barro and Redlick (2010)

[13]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 just one month.

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 −1.344 and 1.372 percentage points of GDP respectively; those relative to the 25% and 75% percentiles record spending cuts of −0.246% of GDP and spending increases of 0.325% of GDP. The largest negative and positive changes are −8.518% and 10.645% respectively.

We instrument $G_{i,t}$ with the number of municipalities put under the administration of external commissioners in province i at time $t-1$. 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: 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. For instance, the mafia may relocate some of its business for fear that the whole area will be subject to intense police investigation. If this is the case, however, 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 mobster arrests by the police. Under this maintained hypothesis, we control for the direct channel of city council dismissals on the economy (independent of the multiplier effects) by including the number of people reported to the judicial authority for organized crime, extortion, mafia-related murders and corruption, and also the number of corruption crimes, in our set of regressors. All variables are specified in log-difference and in per-capita terms, up to two lags.

Our main results are as follows. Without allowing for lagged values of $G_{i,t}$, the impact multiplier is statistically different from zero, with a point estimate of 1.67 (see the first column of Table 3, under the heading R1): an exogenous one per cent (in terms of total GDP) cut in infrastructure investment determines a contemporaneous reduction in output of 1.67 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. It is instead larger than suggested by Barro and Redlick (2010) and others for the US economy as a whole — and similar to that reported by Giordano et al. (2007) looking at government consumption for the Italian economy as a whole.[14] As discussed above, OLS results relative to non-defence current spending may be expected to be upward biased. In this perspective, 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 some crowding out of private spending.

The first stage regression confirms the results reported in Table 2: provinces under

[14]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.

compulsory administration tend to have lower average investment changes. Regressing this variable on covariates and the instrument, the coefficient of the latter is estimated to be negative, as expected, and highly statistically significant. The value of the F-statistic, 12.7, with a p-value of 0.0005, suggests that we are not incurring a weak instrument issue. The Anderson-Rubin test rejects the null hypothesis, $\beta = 0$, at the 5 per cent conventional level (p-value of 0.01). 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 coefficient of the instrument in the reduced form is negative, with a p-value of 0.01, thus providing further support to the relevance of our result.

Dynamics The second column of Table 3 (under R2) reports results when we add two lags of public investment expenditure to the previous specification. Both the contemporaneous and the one-year lagged investment have statistically and economically significant coefficients, with the coefficient on the former being twice as large. Most importantly, however, note that the effect of contemporaneous investment is virtually unaffected by the inclusion of lags (it follows that we can disentangle the delayed effect of investment variations with some precision). The F-statistic related to the effect of the instrument on the causal variable raises to 14.4. Adding up the current and one year lagged effects, our estimate of the overall multiplier is as high as 2.2.[15] The p-value for testing the null hypothesis that the overall multiplier is less than, or equal to 1 is 0.051.

A potential issue with our single-equation model, compared to a multiple-equation framework, is that our estimate of the effect 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 clearly shows that infrastructure investment does not react to GDP changes; 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.

Enlarging the set of instruments Columns R3 and R4 of Table 3 report estimates for a refined and larger set of instruments. First, we note that the dismissal of a city council on evidence of mafia infiltration can occur at any time during the year. It follows that the size of the year-to-year change in investment spending associated with it may crucially depend on when the dismissal takes place, i.e. how close it is

[15] Dropping the GDP lags does not substantially alter the estimates of the other coefficients of interest.

to the end of the calendar year. Thus, for each case of compulsory administration, we calculate the number of days between the dismissal of the city council and the year end. Then we sum up over all municipalities in the same province and use this variable as an additional instrument. Second, as the economic consequences of compulsory administration may depend on the size of the municipality, we consider the population of the municipality under compulsory administration weighted by the number of days between the dismissal of the city council and the year end. In Column R3, we re-estimate the model adding as instrument one lag of these two variables. In column R4 we instead enter all three instrumental variables both contemporaneously and lagged up to two years. Hence, for the model under column R4, the number of instruments is 9.

In general, adding more instruments increases the precision of estimates and creates the possibility for a test of overidentification. As regards precision, including 3 instruments sharply reduces the p-values attached to the Anderson-Rubin test, from 0.010 to less than 0.001. The p-value is even lower when the number of instruments is 9. The t-ratios of the impact and lagged multipliers increase accordingly. Moreover, in both circumstances the Hansen J statistic implies a p-value around 0.4, suggesting that the instruments are uncorrelated with the error term. With 9 instruments, the values for the multipliers are very close to those with exact identification — 1.42 instead of 1.53 for the impact effect and 0.68 instead of 0.72 for the delayed effect — while estimates are a little bit higher in column R3. As before, the impact effect is twice as big as the one year lagged effect. The p-value for testing the null hypothesis that the overall multiplier is less than, or equal to 1 is 0.031, whereas the p-value for the same test restricted to the impact effect is 0.26.

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 and correlated with errors (see Bound, Jaeger, and Baker 1995; Angrist and Pischke 2009). In principle, this may be the case for our overidentified models due to the reduction in the first-stage F-statistic, which is nevertheless still significant at 1% confidence level. In this perspective, the results of the Hansen J-test and the stability of our estimates across specifications are thus noteworthy, suggesting the validity of the estimates from the model with 9 instruments.

In the final column we present OLS results: estimates are statistically significant but small in economic magnitude. The OLS coefficient of the contemporaneous multiplier is about seven times lower than the corresponding IV estimate — a comparable result is reported by Serrato and Wingender (2010). As already mentioned, this result may at least in part reflect a systematic policy of fund allocation towards the provinces with lower long-run growth by the central government. In addition, there are long lags between the announcement of the fund allocation, and the implementation of the projects, which tend to be multi-year. Anticipation effects clearly weigh on the low OLS estimates.

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 in many dimensions, including the fact that lower GDP growth rates are associated with higher government spending and mafia presence may be expected to be more pervasive. In Table 4, we address the issue of whether North-South variability is crucial for our results. We do so in three different ways. Specifically, starting from the 9-instrument specification, in the first column we add a dummy variable for regions located in the south of Italy. This amounts to overcontrolling, as a part of the differences between the South and the rest of Italy is already captured by the proxy of employment. Yet, by including this dummy we can ascertain whether the effect we find is completely due to the North-South cross-sectional variability. Introducing a southern region dummy, however, the sizes of the coefficients of investment spending are only slightly altered.

In the second column we exclude observations from the North, while in the third column we re-estimate the model allowing for fixed effects at regional level. Overall, the multipliers tend to be a bit smaller. Yet, the proportion between the lagged and the contemporaneous effect is still 1 to 2. For the sample restricted to southern regions, the coefficients remain statistically significant at the 5% level. A key conclusion here is that at least 70% of the total estimated effect of public spending is unrelated to North-South differences, clearly corroborating our main results. One difference worth stressing, however is that in the restricted sample, we can no longer reject the possibility of crowding out of private spending at standard confidence levels.

Spillovers across borders 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, we consider the variable $SG_{i,t} = \frac{Sg_{i,t} - Sg_{i,t-1}}{Sy_{i,t-1}}$, where, for each province i and year t, $Sg_{i,t}$ is the average investment across provinces which are part of the same region as i (excluding province i itself) and $Sy_{i,t-1}$ is defined accordingly. In particular, the first column labelled "Spillovers" only considers $SG_{i,t}$ while in the second one we also add two lags of this variable. As can be seen, no evidence of spillovers is, however, detected. This result suggests that most of the effects of government spending are within the boundaries of the province.

Spuriously high multipliers 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. To the extent that the model fails to capture the systematic part of procyclical spending, reverse causation translates into spuriously high estimates for the "multiplier". To assess this issue in our sample, we report estimates of the coefficients of interest obtained by dropping the calendar year dummies from our set of regressors, first only considering $G_{i,t}$, then also allowing for its lags (respectively in the last two columns of Table 4). As expected, the lack of control for the common business cycle movement raises the impact “multiplier” to 2.15 for the dynamic specification and up to 2.58 for the static one. The coefficient capturing the delayed effect of spending is affected, too.

5 Conclusions

We have provided an estimate of the government spending multiplier using cross-sectional variations in 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. Our point estimate of the multiplier, checked with extensive robustness analysis, is 1.4 on impact, and 2 overall.

How should these estimates be interpreted in the context of the long-standing debate on the “size of the multiplier”? In addition to the benefits of our instrument, we have emphasized that a cross-sectional study is less exposed than time series analysis to the risk of deriving estimates of large multipliers as a result of reverse causation — whereas public expenditure may indeed rise in periods of relatively high growth. Equally important is the fact that we derive estimates of the multipliers controlling for common components — including the national monetary stance — and in an institutional environment in which local public spending is allocated by the central government, with no consequences for the level of local taxation. In light of these observations, our empirical estimates should be read as approximating the macroeconomic transmission of variations 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.

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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 total sample of observations into two groups: those relative to year-province after a compulsory administration is appointed by the central government (treatment group) and those in the rest of the sample (control group). "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 of results we restrict to provinces characterized by at least one case of local government dismisal during the period analyzed. 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	R4	R5-OLS
G(t)	1.67***	1.53***	1.66***	1.42***	0.18***
	[2.80]	[2.78]	[3.17]	[3.35]	[2.70]
G(t-1)		0.72***	0.77***	0.68***	0.23***
		[3.06]	[3.24]	[3.59]	[3.61]
G(t-2)		0.13	0.14	0.12	0.01
		[1.30]	[1.40]	[1.29]	[0.19]
First-Stage F	12.73	14.42	5.84	2.64	
(p-value)	(0.00)	(0.00)	(0.00)	(0.01)	
Anderson-Rubin test	6.68	6.47	7.33	6.08	
(p-value)	(0.01)	(0.01)	(0.00)	(0.00)	

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 and the hours of 'cassa integrazione' divided by the provincial population. Moreover, all equations but the OLS-one also 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, Further Results

	North-South Differences			Spillovers		Spurious "Multiplier"	
G(t)	1.288***	1.061**	1.085*	1.431***	1.289**	2.585***	2.153***
	[2.78]	[2.33]	[1.91]	[2.98]	[2.44]	[3.71]	[3.06]
G(t-1)	0.626***	0.514**	0.575**	0.685***	0.613***		0.819***
	[3.00]	[2.52]	[2.33]	[3.26]	[2.70]		[2.64]
G(t-2)	0.103	0.058	0.111	0.124	0.102		0.092
	[1.11]	[0.68]	[1.15]	[1.33]	[1.13]		[0.69]
SG(t)				0.102	0.195		
				[0.53]	[0.92]		
SG(t-1)					0.253		
					[1.13]		
SG(t-2)					0.031		
					[0.19]		
N	950	540	950	950	950	950	950

Note: See the note to Table 3. For each province i and year t, SG(t) is the dated t year-in-year change in per capita real infrastructure investment - measured as the average investment across provinces which are part of the same region as i, excluding province i itself - divided by the previous year's average per capita real Value Added. For all estimated equations the instruments are the same as those used in Table 3, column R4. Standard errors clustered at the region level. The t-statistic is reported in brackets: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.