Mafia and Public Spending: Evidence on the Fiscal Multiplier from a Quasi-Experiment[†]

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A law issued to combat political corruption and Mafia infiltration of city councils in Italy has resulted in episodes of large, unanticipated, temporary contractions in local public spending. Using these episodes as instruments, we estimate the output multiplier of spending cuts at provincial level—controlling for national monetary and fiscal policy, and holding the tax burden of local residents constant—to be 1.5. Assuming that lagged spending is exogenous to current output brings the estimate of the overall multiplier up to 1.9. These results suggest that local spending adjustment may be quite consequential for local activity. (JEL D72, E62, H71, K42)

The widespread resort to fiscal stimulus at the onset of the global crisis and, more recently, the emerging need to consolidate deficits in response to rising fiscal imbalances have revitalized the empirical debate on the transmission of fiscal policy—"the multiplier." While the literature has mostly focused on aggregate effects at a national level, several recent contributions (reviewed below) have called attention to the local dimension. This shift in focus is motivated by specific policy questions, combined with the opportunity to exploit institutional information to address econometric issues in identification.[1]

[1] Multipliers are typically estimated by tracing the effects of exogenous fiscal impulses on economic activity. Much of the debate has focused on identifying innovations in spending or taxation, 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, coupled with possible anticipation effects, may spuriously raise (or lower) estimated multipliers (see, e.g., Blanchard and Perotti 2002, and Ramey 2011).

A key question concerns the efficacy of fiscal policy in countering area-specific recessionary shocks, which would entail a redistribution of fiscal resources across regions. A related question concerns the geographical and distributional consequences of crises that may force local administrations to undertake budget cuts of different intensities. The body of evidence from aggregate studies gives limited or no guidance on these issues. Compared to national economies, regional and provincial economies are much more open and face a mix of monetary and budget policy that, being set at the national level, is largely unresponsive to their idiosyncratic conditions.

In this article, we provide evidence on output multiplier effects of government purchases at a local level, relying on a quasi-experiment. Focusing on public investment in Italian provinces, we instrument spending by exploiting an Italian law which, upon evidence of Mafia infiltration in a city council, mandates the dismissal of all elected officials, who are replaced by three external commissioners appointed by the central government. The instrument builds on the fact that (i) the police investigation and the emergence of the incriminating evidence leading to a city council dismissal are unrelated to fluctuations in local economic activity; and (ii) the compulsory administration by external commissioners, after the dismissal of elected officials, typically translates into an immediate, unanticipated, and temporary cut of public investment projects. The first year of compulsory administration, indeed, records a strong contraction in provincial public spending, with an average drop of 20 percentage points (corresponding to about one-half of a percentage point of provincial value added). In addition, due to the characteristics of fiscal federalism in Italy during our sample period, variations in public expenditure in a municipality cause little or no variation in the tax burden faced by residents, since virtually all local spending is financed by transfers from the central government. Hence, we are able to estimate multipliers of local spending controlling for the aggregate business cycle and the national monetary and fiscal policies (with fixed effects), and independent of the implied adjustment in taxes.

In our findings, the contemporaneous output multiplier of spending contractions—not compensated by monetary expansions, holding the tax burden constant—is as high as 1.5. Furthermore, under the maintained assumption that lagged spending is exogenous to current output, the combined effects of past and current spending bring our multiplier estimate up to 1.9. We also find no significant spillovers of provincial spending into adjacent areas, suggesting that local economies may actually be quite "insular" from each other.

A key concern in our analysis is the possibility that city council dismissals for Mafia infiltration may affect output via channels that are independent of spending cuts, in violation of the exclusion restriction. We specifically address two potential channels: (i) variations in the scale of Mafia activities in response to a dismissal, as some of these activities may directly or indirectly affect provincial value added; and/or (ii) a "shock to government" induced by the replacement of elected officials with external commissioners, as the change may result in a slowdown in the issuance of licences to build or permits to start new businesses, or in a hiring freeze. As regards the first channel, our regressions include controls for the variations in the size of Mafia activities during a compulsory administration, capturing the outcome of police investigation and legal action against mobsters (such as arrests of, and

charges against, mobsters for Mafia-related crimes) over time. We show that excluding these controls from our regressions tends to reduce the estimated multipliers, suggesting that the Mafia-activity channel and spending contractions have opposite effects on short-run economic activity. As regards the second channel, we collect data on the universe of city council dismissals in Italy, and show that those not motivated by Mafia infiltration are not associated with a contraction in spending and have no effects on output.

Together with the present study, a number of recent works have also delved into the analysis of output multiplier effects using subnational data. Looking at state-level relative to national-level military spending in the United States, Nakamura and Steinsson (2014) estimate multipliers in the range 1.4–1.9, based on biannual data. Serrato and Wingender (2011) use fund reallocation across US counties due to revisions in the estimates of local populations following changes in the estimation methodology, while Shoag (2010) exploits the idiosyncratic components in the returns on defined-benefit pension plans managed by the US states. In these two studies, multipliers are as high as 1.88 and 2.12, in the respective baseline specifications. Fishback and Kachanovskaya (2010) exploit a swing voting measure, which varies primarily across US states, to instrument government grants during the New Deal. In their results, the point estimate of the multiplier for public works grants is 1.67.[2] Similar to these studies, in our empirical model we control for national monetary policy and wealth effects from tax adjustment. Relative to the literature, however, in addition to using non-US data, our contribution has two novel and distinct features. First, our analysis disentangles impact and dynamic effects of the multiplier. Second, although our regression model does not explicitly allow for asymmetric effects of spending increases and cuts, our estimates of the multiplier mainly rely on sharp fiscal contractions. Finally, we should stress that our spending variable consists of government purchases, instead of transfers.

The rest of the article is organized as follows. Section I presents the empirical model. Section II is devoted to the analysis of our instrument, starting with some institutional details on the laws targeting Mafia connections. Section III discusses our main results. Section IV provides evidence that our measure of the multiplier is not contaminated by transmission channels unrelated to variations in spending. Section V discusses results for alternative specifications of the empirical model. Section VI concludes.

I. The Empirical Model

In our study, we aim to recover the short-run multiplicative effects of public spending on output at the provincial level in Italy. We present the regression model in this section and discuss our instrument in the following section.

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[2] An output multiplier of about two is also implied by the estimates of Chodorow-Reich et al. (2012), looking at the employment effects of state fiscal relief. Analogously, large employment local effects are found by Moretti (2010). Nonetheless, multipliers are found to be not significantly different from zero by Clemens and Miran (2012), who build on differences in the balanced-budget requirements at the state level. Cohen, Coval, and Malloy (2011), who instrument public spending with changes in congressional committee chairmanship, note that spending variations appear to significantly dampen corporate sector investment and employment activity. However, as suggested by the authors themselves, their results may reflect the high level of employment prevailing in their sample.

To carry out this study, we have assembled a dataset on output and public investment spending in each province of Italy over the ten-year span between 1990 and 1999, a period over which we could obtain comparable series of local expenditure on public works. An Italian province is a geographic entity similar to a US county and contains several municipalities. During this period, there were 95 provinces in Italy; hence, we have 950 annual observations.

For each province, let y_i denote the real per capita value added, and $Y_{i,t}$ its rate of growth, defined as $Y_{i,t} = (y_{i,t} - y_{i,t-1})/y_{i,t-1}$; similarly, let g_i denote the real per capita public investment in infrastructure, and $G_{i,t}$ its year-on-year change as a ratio of lagged value added, $G_{i,t} = (g_{i,t} - g_{i,t-1})/y_{i,t-1}$. In line with the recent literature (see, e.g., Barro and Redlick 2011), we estimate the spending multiplier relating the growth of per capita value added in a province ($Y_{i,t}$) to the year-on-year change in per capita spending on infrastructure in the same province ($G_{i,t}$). The empirical model is

$$(1) \qquad Y_{i,t} = \beta G_{i,t} + \alpha_i + \lambda_t + \gamma \mathbf{X}_{i,t} + v_{i,t},$$

where the coefficient β measures the contemporaneous one-year government spending multiplier; α_i is a province fixed effect; λ_t is a year fixed effect; and $\mathbf{X}$ denotes a vector of further control variables, to be discussed below.

The inclusion of a year fixed effect in equation (1) serves two main purposes. First, it controls for national components of public investment and GDP common to all provinces. As variations in aggregate spending and output are usually predictable and arguably endogenous to cyclical developments, they may lead to spurious estimates of the multiplier due to reverse causation.

Second, year fixed effects control for monetary and fiscal policy at the national level. As is well understood, the transmission of fiscal stimulus or contraction is bound to be crucially affected by the monetary stance, as well as by the anticipation of fiscal measures (spending cuts or tax hikes) dictated by the need to stabilize public debt in the medium and long term (see, e.g., Christiano, Eichenbaum, and Rebelo 2011; Corsetti, Meier, and Muller 2012b; and Woodford 2011). Failure to control for these factors means that the estimated multipliers conflate the effects of fiscal shocks with those of the monetary and budget policy that is anticipated to prevail over both short- and long-term horizons.[3]

Through province fixed effects we address potential endogeneity issues raised by the possibility that province-specific characteristics may be correlated with spending allocation criteria. By way of example, it may be possible that the central government systematically allocates relatively large projects in lower-growth provinces in an effort to spur local economic activity. Under this allocation criterion, the OLS estimates of the multiplier would tend to be spuriously low.

An advantage specific to our data relates to the type of fiscal federalism in Italy during our sample years, based on Law No. 281/1970 and Law No. 382/1975. On

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[3] The challenge of estimating aggregate multipliers while accounting more explicitly for budget and monetary policy has been taken on by a new generation of contributions (see Corsetti, Meier, and Muller 2012a; Canova and Pappa 2011; Ilzetzki, Mendoza, and Vegh 2013; Leeper, Walker, and Yang 2009, among others). Heterogeneity in consumption responses to fiscal stimulus is explored by Misra and Surico (2013).

the spending side, these laws gave the central government the power to budget the overall flow of resources accruing to local governments. The latter in turn retained full control of these funds, including the power to select public projects and the firms to carry them out. On the revenue side, however, local governments had very little power to set tax rates.[4] Therefore, throughout our sample years, the public resources channeled by the central government into local investment projects were not matched by variations in the tax burden of the local residents.[5] For this reason, we do not face potential issues arising from the omission of tax changes (or debt) from our set of controls.[6]

As regards the matrix of controls, **X**, we include five variables measuring the number of people reported to the judicial authority for (i) Mafia-type association; (ii) extortion; (iii) Mafia-related murders; (iv) corruption; and (v) the number of corruption crimes reported to the judicial authority.[7] All these variables are defined in per capita difference terms and entered in the regression model both contemporaneously and lagged up to two years. As argued below, to the extent that episodes of council dismissals coincide with intense police investigation, and/or changes in the scale of Mafia activities, crime deterrence implied by a council dismissal may affect economic activity in a province independently of cuts in public spending. The five variables defined above are included in order to control for this alternative transmission channel, under the maintained assumption that the scale of Mafia activities is correlated with the outcome of police investigation in terms of arrests and the number of people charged with Mafia-related crimes. We should note here that, from a cross-sectional perspective, areas where the Mafia presence is relatively high are likely to be characterized by a relatively high average number of mobsters arrested by the police. However, the inclusion of province fixed effects accounts for possible cross-province differences in these averages. By the same token, it is possible that the degree of law enforcement may vary over time, due to, for instance, waves of political or media pressure, changing priorities of the law enforcers, or the efforts of judges and prosecutors. In our analysis year fixed effects also control for variation in enforcement over time.

To control for local cyclical conditions, we include lagged changes of two proxies for unemployment, namely, the ($t - 1$ and $t - 2$ log difference of the) per capita

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[4] In our sample years, the Italian municipalities had the option to marginally adjust the rate of two taxes set at national level, to address local financial needs. The revenue from these adjustments (if any) nonetheless accounts for a very small share of their overall budget.

[5] Not surprisingly, local governments lobbied strongly for public funding from the central government (Cassese 1983). Their success was helped by the fact that, historically, public investment was used in Italy as a key policy instrument to foster growth and sustain social cohesion.

[6] While the magnitude of the tax multipliers—relating output to marginal income tax rates or tax revenues—is controversial, recent empirical literature provides evidence that tax changes have a nonnegligible negative effect on output—see, for instance, Barro and Redlick (2011) and Romer and Romer (2010). The latter contribution emphasizes that aggregate spending and tax changes may occasionally become strongly correlated, reflecting emerging political concerns with the ongoing government deficit. To the extent that tax changes can have a negative impact on output, theses authors argue that the omission of this variable induces a downward bias in the estimate of the spending multiplier.

[7] Corruption crimes include embezzlement, misappropriation of public funds, extortion, and bribery agreements. The categories (i) through (v) are used in the reporting of official statistics by the "Istituto Nazionale di Statistica" ISTAT, according to the classification of crimes in Italian law. The first three categories strictly refer to Mafia crimes (see "Codice di Procedura Penale art. 51, comma 3 bis."), while the last two are related to corruption of public officials. In particular, article 416-bis of the Italian penal code defines the crime of Mafia-type association, while murders related to Mafia activity are recorded by ISTAT according to information supplied by the police force.

employment, and hours of wage supplement provided by the unemployment insurance scheme available to employees of large private firms in Italy (*Cassa Integrazione* Guadagni).[8] Including these controls is especially important if employment changes are highly persistent (Chodorow-Reich et al. 2012; Shoag 2010).[9] We also include the $t - 2$ and $t - 3$ lags of the number of municipalities under compulsory administration in the province, weighted by the relative population, as well as two lags of our spending variable $G_{i,t}$.

The key identifying assumption in the SVAR literature after Blanchard and Perotti (2002) is that lags of $G_{i,t}$ are predetermined with respect to $Y_{i,t}$. Under the same assumption, the coefficients on the lags of $G_{i,t}$ in our regressions provide estimates of the dynamic multiplier, complementing our IV estimates of the contemporaneous multiplier.

The provinces in our panel have different sizes. To account for this heterogeneity, our regressions are weighted by province-level population.[10] In addition, as is well known, inference in panel estimation can be highly misleading if there is spatial correlation within groups of observations, or serial correlation, or both (see, e.g., Bertrand, Duflo, and Mullainathan 2004; Angrist and Pischke 2009). Regarding the serial correlation problem, we will use up to two lags of the dependent variable. Regarding the spatial correlation problem, following Guiso, Sapienza, and Zingales (2004), we posit that provinces belonging to the same region are correlated, as a result of an unobserved cluster effect due to common regional rules and policies. Our inference will therefore be based on standard errors robust to contemporaneous spatial correlation allowing for 190 clusters (i.e., 10 yearly observations for 19 regions: because of its small size, we aggregate Valle D'Aosta with its neighbor Piedmont), as well as robust to heteroskedasticity.

II. Instrumenting Changes in Public Spending

Despite the advantages of our empirical model described above, OLS estimation of equation (1) would expose our results to two criticisms. First, spending on infrastructure is usually planned some years before it is implemented. Failure to account for anticipation effects over the timespan between the announcement and the realization of projects can substantially bias the multiplier estimates downward. Second, in our sample, the government may have allocated funds in response to local developments, in ways that are not accounted for by province fixed effects. To address these problems, we need a good instrument for unexpected variations in public spending.

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[8] The Cassa Integrazione Guadagni (CIG) is an Italian institution introduced after World War II with the goal of supporting large firms in a temporary crisis. It provides temporary wage supplements to workers who either have lost their jobs or are forced to work for reduced hours.

[9] Dropping these variables from our preferred specification, however, leaves our main results unchanged.

[10] Apart from minor numerical differences in the point estimates, the conclusions from our analysis are the same if we do not weight our regressions (see Acconcia, Corsetti, and Simonelli 2011).

A. *The Institutional Setting: Mafia Infiltration and Compulsory Administration*

We introduce our instrument by providing background information on the way the Italian law deals with Mafia-related crimes. In view of the rising presence of organized crime in the Italian economy, two articles were added to the penal code in 1982, explicitly targeting Mafia-type organizations.[11] Articles 416-bis and 416-ter target the use of intimidation, associative ties, and *omertà* (code of silence) to acquire direct or indirect control over otherwise legal economic activities, especially in relation to public investment and the provision of public services.[12] As already mentioned, the distorted incentives created by the laws on fiscal federalism between the 1970s and the end of 1990s favored a strong growth of local public spending.[13] During our sample years, indeed, public works managed by local administrations in Italy became one of the most lucrative sources of business for the Mafias.[14]

The rise in Mafia infiltration of public administration throughout the 1980s was arguably a key motivation for introducing tougher anti-Mafia measures in the early 1990s. Among these measures, a law was passed allowing the central government to remove elected local officials on evidence that their decisions were determined or influenced by the Mafias (D.L. 31/05/1991 n. 164). According to this law, upon the removal of a city council, the central government appoints three nonelected, external commissioners, who govern the municipality for a period of 18 months.

This new law gave prosecutors a key new tool to combat the Mafia, sharply increasing the value of police investigation. Before its introduction in 1991, incriminating evidence against, say, the alderman of a city, would lead to the arrest of an individual. After 1991, the same evidence could lead to the dismissal of the entire city council, thus creating opportunities to fight the networks connecting Mafia-controlled firms and public administrations.

Not surprisingly, the new tool has been extensively (although not exclusively) used in regions where criminal infiltration in the territory and the institutions is long-established and common knowledge. As shown in Table 1, over the years dismissals have been mostly concentrated in the provinces of Naples, Palermo, Reggio Calabria, and Caserta. The geographical distribution of the Mafia varies both across and within regions. It is highly concentrated in the southern regions of Sicily, Campania, Calabria, and Puglia, but is also significant in northern regions like Piedmont and Lombardia. Within these regions, in turn, there are substantial

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[11] Historically, different Mafia-like organizations have been active in different Italian regions: the *Camorra* in Campania, the *'Ndrangheta* in Calabria, the *Sacra Corona Unita* in Puglia, and the *Mafia* in Sicily. Each organization in turn comprises different groups or clans, with the best known being the *Cosa Nostra* in Sicily and, recently, the *Casalesi* in Campania.

[12] See Acconcia et al. (forthcoming), and references within, on the 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.

[13] An important role was also played by the strong earthquake that hit the south of Italy at the end of 1980. With the need to reconstruct housing and infrastructure, a large inflow of government funds benefited areas of Italy traditionally under the control of the Mafias.

[14] According to official estimates (Ministro dell'Interno 2000), over our sample period the profits accrued to organized crime from controlling public works were comparable to those of extortion and drug dealing. Mafia infiltration has created a vast network connecting legal and illegal activities. For instance, the Commissione Parlamentare di Inchiesta (2005) emphasizes that many firms suspected of Mafia collusion operate with high standards of efficiency across the country.

Table 1—Council Dismissals because of Mafia Infiltration

Napoli	48	Reggio C.	37	Palermo	23	Bari	5
Caserta	31	Catanzaro	8	Catania	9	Lecce	2
Salerno	6	Vibo V.	12	Trapani	6		
Avellino	4	Crotone	3	Caltanisetta	6		
Benevento	1	Cosenza	2	Agrigento	7		
				Messina	3		
				Ragusa	1		
Campania	90	Calabria	62	Sicily	55	Puglia	7

Notes: The table reports the number of council dismissals because of Mafia infiltration during 1991–2012 (July), by province, within the regions of Calabria, Campania, Puglia, and Sicily. Only seven council dismissals occurred in the rest of Italy during the same period.

differences across provinces, mostly driven by historical accidents and/or Mafias' own strategies and pervasiveness (see, for instance, Dickie 2004).[15]

Our sample includes 110 cases of city councils put under compulsory administration for Mafia infiltration, but since we carry out our study using provincial data, aggregating these cases by province, we obtain 47 observations.

B. *An Instrument "One Can't Refuse"*

When the government of a municipality is dismissed on evidence of Mafia infiltration, the external commissioners appointed by the central government typically cut financial flows into local public works and investment projects. On average, indeed, the first year of compulsory administration in a municipality is associated with a sharp contraction in spending on public works at provincial level. This is shown in Table 2, in which we compare changes in public investment in provinces with and without municipalities under ongoing compulsory administration.

As the treatment group, we pool together all the provinces with at least one case of ongoing dismissal and compute the change in investment in the calendar year following the publication of the dismissal decree. As control groups, we pool together all the province-year observations not in the treatment group, using either the whole sample (columns 1 and 2 of the table), or the subsample of provinces with at least one dismissal (columns 3 and 4). The rationale for defining two alternative control groups is to show that the mean differences are not driven by systematic heterogeneity in average spending changes across provinces which did/did not experience cases of compulsory administration.

Columns 1 through 4 in Table 2 show that the mean difference in investment between the treatment and each control group is negative and statistically significant at the 5 percent significance level. The average contraction in spending in the

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[15] Statistics on convictions of the crime of Mafia association by regions and provinces provide an indicator of geographical differences in the presence of the Mafias. Namely, 90 percent of the 5,443 mobsters convicted up to 2001 were put on trial by courts in the Southern regions—mainly Sicily, Campania, Calabria, and Puglia. There were, however, significant differences within each region. In the Campania region, only 239 mobsters were convicted in the judicial district of Salerno (corresponding to 24 convictions per 100,000 inhabitants), against 1,483 in the district of Naples (32 convictions per 100,000 inhabitants). In the Calabria region, convictions in Catanzaro and Reggio amounted to, respectively, 204 and 343 (that is, 14 and 59 per 100,000 inhabitants); in the Puglia region, the corresponding numbers in Bari and Lecce were 142 and 534 (6 and 30 per 100,000 inhabitants). In the North, many convictions were handed down by courts in Piedmont and Lombardia.

TABLE 2—INVESTMENT SPENDING IN THE FIRST YEAR AFTER COUNCIL DISMISSAL

	(1)	(2)	(3)	(4)	(5)	(6)
Difference	−19.65***	−0.46**	−23.67***	−0.49*	−4.72	−0.04
	[5.36]	[0.19]	[7.12]	[0.26]	[5.29]	[0.18]
Observations	950	950	180	180	905	905

Notes: The table reports one-sided mean difference test results for investment changes between the treatment and control groups, columns 1–4, as well as changes between the different control groups, columns 5 and 6. Investment changes are in percentage of either lagged investment, columns 1, 3, and 5, or lagged value added, columns 2, 4, and 6. The treatment group consists of province-year observations in the first calendar year after a city council dismissal. The control group consists of the rest of the sample. In the third and fourth columns provinces which never experienced local government dismissals are dropped. Data are annual from 1990 to 1999 at provincial level. The standard errors are reported in brackets.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

treatment group amounts to about half a percentage point of provincial value added, comparable in size to the change in fiscal variables in leading empirical analyses of multipliers.[16] The last two columns in the table (columns 5 and 6) show that spending variations are not statistically different across the two control groups, consistent with the assumption that treatment and control groups are homogenous except for their treatment status.

To gain insight into how the dismissal of a city council for Mafia infiltration affects spending in practice, we collected extensive documentation on the case of Pompei (within the province of Naples).[17] The city council was dismissed on September 11, 2001, following the arrests of the speaker of the municipal council and the city alderman for street maintenance for Mafia association. The councilman was identified as the main link between the local administration and the boss of the Mafia clan operating in Pompei, who was also arrested in the course of the same investigation.

The extent and type of spending cuts associated with a dismissal are best analyzed via a detailed comparison between the (ex ante) annual budget, and the actual expenditure flows. In the case of Pompei, the 2001 budget prepared by the elected officials before the dismissal had allocated 4 million euros to public works. Upon taking over the city administration, the commissioners formally ratified the budget but, at the same time, cut spending on public works by more than 3 million euros.[18] During 2001, actual spending amounted to a mere 20 percent of that planned.

The spending cuts affected a variety of budget chapters: (i) extraordinary street maintenance; (ii) improvement and maintenance of the public lighting system;

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[16] As regards defense spending, changes in fiscal variables related to the Korean War amounted to 5.6 percentage points of GDP in 1951, 3.3 in 1952, and 0.5 in 1953, followed by a contraction of 2.1 percentage points of GDP in 1954. Changes that occurred during the Vietnam War amounted to −1.2 and 1.1 percentage points of GDP in 1966 and 1967, respectively (see Barro and Redlick 2011). On the revenue side, the effect of the 54 legislated exogenous tax changes identified by Romer and Romer (2010) amounts to −0.03 percentage points of GDP.

[17] While the Pompei dismissal was just after the end of our sample, it was the case for which we were able to obtain the richest and most accurate information from a variety of sources, including interviews with local administrators and commissioners.

[18] From an accounting point of view, this was accomplished by moving three million euros of investment to the item *economie*, that is, savings on expenditures.

(iii) purchase of mechanical equipment; (iv) demolitions; (v) extraordinary maintenance of the water system; (vi) maintenance of public parks and gardens; (vii) extraordinary maintenance of the sewage system; (viii) building restoration; (ix) municipal cemeteries. Not surprisingly, the list includes projects under police investigation, or under the control of the city councilman charged with Mafia association. However, the commissioners also decided to implement cuts across the board, arguably with the objective of acquiring more information before underwriting past spending decisions.

C. *Is the Instrument Variation Systematically Related to Local Economic Activity?*

Police investigations leading to the dismissal of city councils because of Mafia infiltration may be conducted for a variety of crimes, mostly unrelated to the control of local public works. Based on the reports by the *Commissione Parlamentare d'Inchiesta* to the Italian parliament, dismissals typically follow from (i) investigations of crimes by local administrators or politicians (not necessarily linked to their official functions); (ii) investigations of extortions, illegal trade in weapons and drugs, and Mafia wars for the control of local territory; (iii) investigations prompted by whistleblowers, providing information on crimes typically unrelated to Mafia infiltration in public administration; (iv) investigations prompted by the resignation of a city mayor or a city council member, suggesting Mafia pressure (Commissione Parlamentare d'Inchiesta 2005). The same document emphasizes that city council dismissals are not prompted by indicators of administrative inefficiency in the procurement procedure. On the contrary, it is often the case that the procurement procedures involving firms connected to the Mafia are completed quickly and at a low price, with no apparent waste of public resources.[19]

The account of the circumstances leading to city council dismissals by the *Commissione Parlamentare d'Inchiesta* suggests no systematic link between dismissals and local economic activity, as the incriminating evidence often emerges randomly in the course of ongoing police investigations. Nevertheless, to provide formal statistical evidence, we compare the growth rates of "treated provinces" prior to their first dismissal, with the growth rates of provinces that never experienced a dismissal, by running the following regression:

$$(2) \qquad Y_{i,t} = d_0 + d_1 D_{i,t} + d_2 t + d_3 (t \times D_{i,t}) + \psi_{i,t},$$

where t is a time trend and $D_{i,t}$ is a dummy variable equal to 1 for any province $\times$ year observation before the first episode of council dismissal and 0 otherwise. Based on the sample 1986–1999, OLS results yield a point estimate of the coefficient d_3

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[19] By way of example, city council dismissals followed the arrest of local administrators on charges of drug trafficking in Roghudi (province of Reggio Calabria), and Cesa (province of Caserta); and the arrest of the mayor and members of the city council on charges of theft, infringement of building laws, and bid rigging in Sant'Andrea Apostolo dello Ionio (province of Catanzaro). In a few cases (e.g., Gioia Tauro, province of Reggio Calabria), the mayor was explicitly charged with the crime of Mafia association. City council dismissals followed from investigations of deadly Mafia ambushes in Lametia Terme and Guardavalle (province of Catanzaro), and the investigation of threats against local administrators in Bordighera (Imperia). Direct and indirect links between local administrators and criminal organizations were uncovered after the resignation of city council members and/or the mayors of Taurianova and San Ferdinando (Reggio Calabria), Sant'Onofrio (Vibo Valentia), and Frattamaggiore (Napoli).

Table 3—Provincial Growth Rates Prior to Council Dismissals

	Above average	Below average	Switching
$t - 1$ and $t - 2$	1/3	1/6	1/2
$t - 1$ and $t - 2$ and $t - 3$	1/9	0	8/9

Notes: For any province that experienced cases of compulsory administration, we compute GDP growth rates in the two and three years before the first dismissal. The table reports the proportion of provinces for which these growth rates were always above, always below, or fluctuated around the national average.

equal to -0.07, with a standard error adjusted for clusters equal to 0.19. Hence, the null hypothesis $d_3 = 0$ cannot be rejected (the *p*-value is 0.74)—thus confirming the absence of a differential trend in growth rates before council dismissals. We should also note that we cannot reject the null hypothesis $d_1 = 0$, suggesting that the average growth rate of the treated provinces is no different from the rest of the sample.

In a related exercise, we verify whether the growth rate of the provincial value added in the years preceding a council dismissal is systematically above or below the national average. Results are shown in Table 3, for all provinces with at least one council dismissal.[20] As apparent from the table, no systematic pattern emerges from the data.

A key feature of our instrument is that the time span from the emergence of evidence of Mafia infiltration to the replacement of the city council by the external commissioners is quite short—in our sample, it is often the case that the whole process takes two months.[21] Hence, conditional on the news that the dismissal procedure has been set in motion, anticipations of government-mandated contractions in spending are unlikely to play a significant role in our sample with yearly observations.

D. *Implementation*

We implement our IV strategy accounting for two key facts. First, as we aggregate information at provincial level, the effects of council dismissal in one or more municipalities in the province need to be appropriately weighted. Second, the dismissal of a city council can occur at different times of the year. The yearly flow of investment spending, and in turn its possible effects on the year-to-year change in local value added, may crucially depend on how close the dismissal date is to the end of the calendar year.

We use two instruments. The first instrument, dubbed "Council-dismissal-S1" ($CDS1$), equals the number of municipalities put under compulsory administration, provided that the official decree is published in the first semester of the year, weighted by the share of the province population living in these municipalities. Including the number of municipalities proxies for the number of projects whose

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[20] For provinces with repeated cases of dismissals, we consider only the first one, in order to insulate the results from possible lagged effects of spending cuts implemented during previous compulsory administrations.

[21] According to the law, the dismissal of a city council should normally follow a formal decree by the President of the Republic. However, there are circumstances under which the local *Prefetto* (the highest nonelected representative of the central government in the territory) can process the dismissal immediately, without waiting for the formal decree. This speedy procedure has indeed been common practice in the years after the new law was introduced.

finance may be cut by the commissioners. Relative population weight accounts for the relative economic importance of the area under compulsory administration. The second instrument, "Council-dismissal-S2" ($CDS2$), is similarly defined, except that, for each case of compulsory administration, we first 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. Then, for every province $\times$ year observation for which this average is less than 180, "Council-dismissal-S2" equals the number of municipalities under compulsory administration.[22]

In particular, we instrument $G_{i,t}$ with "Council-dismissal-S1" and the one-period lag of "Council-dismissal-S2." Thus, the first-stage regression of our baseline specification is

$$(3) \qquad G_{i,t} \,=\, \delta_1\, CDS1_{i,t} \,+\, \delta_2\, CDS2_{i,t-1} \,+\, \alpha_i \,+\, \lambda_t \,+\, \gamma \mathbf{X}_{i,t} \,+\, e_{i,t}.$$

III. Results: Impact and Dynamic Multipliers

The results from our baseline model are shown in Table 4, which reports the OLS as well as the 2SLS estimates for two versions of our model: one excluding, the other including lagged values of the endogenous variable.

The first two columns of the table show the OLS estimates. The OLS coefficients on the contemporaneous and one-year lagged public spending are both statistically significant, but small in magnitude. In particular, the estimated impact multiplier is 0.2. As already mentioned, it is well understood that OLS estimates of the multiplier of spending on public projects are potentially subject to at least two types of bias. On the one hand, there are usually long lags between the announcement of the fund allocation and the implementation of local investment projects, which typically takes place over several years. Empirical models may fail to account for anticipation effects, which may bring forward in time variations in economic activity. On the other hand, public projects may be systematically implemented in phases of output expansion or contraction. As OLS estimates conflate the cyclical reaction of fiscal policy to income with the fiscal multiplier, the bias can have either sign: an upward (downward) bias is more likely and larger where fiscal policy is pro- (counter-) cyclical.

The results from our IV model are shown in the last four columns of Table 4. The first-stage regressions in columns 3 and 5 confirm the point suggested by the evidence in Table 2: on average, provinces under compulsory administration experience a sharp drop in public investment.[23] As is apparent from Table 4, the estimates of the coefficients on both instruments are negative, as expected, and highly statistically significant. The value of the F-statistic testing the power of the instruments is 12.58 and 11.83 for the model without and with lagged output growth, respectively.

In the second-stage regression, the coefficient on contemporaneous spending is statistically different from zero at the 1 percent significance level, with a point

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[22] Results are robust to instrumenting public spending without weighting the number of dismissals using the relative population size of the municipalities under compulsory administrations.

[23] We compute the 2SLS estimators using variables in deviation from province and year averages.

TABLE 4—PUBLIC SPENDING MULTPLIER

	OLS		2SLS		2SLS	
			First stage	Second stage	First stage	Second stage
$G(t)$	0.21** [0.07]	0.23** [0.07]		1.46** [0.49]		1.55*** [0.43]
$G(t-1)$	0.22** [0.08]	0.26** [0.08]	−0.41*** [0.07]	0.73*** [0.21]	−0.41*** [0.07]	0.79*** [0.19]
$G(t-2)$	0.00 [0.07]	0.04 [0.07]	−0.13* [0.06]	0.14 [0.11]	−0.13* [0.06]	0.19 [0.11]
$Y(t-1)$		−0.16* [0.06]			0.03 [0.02]	−0.20** [0.06]
$Y(t-2)$		−0.03 [0.05]			−0.02 [0.02]	−0.02 [0.05]
$CDS1(t)$			−2.07*** [0.54]		−1.97*** [0.56]	
$CDS2(t-1)$			−4.02*** [0.98]		−4.08*** [0.94]	
F-stat instruments			12.58		11.83	
Observations	950	950	950	950	950	950

Notes: Data are annual from 1990 to 1999 at the provincial level. The dependent variable in the OLS and the second-stage 2SLS regression 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 national 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 include year dummies, the first two lags of employment and the hours of "cassa integrazione" (both entered as per capita log-difference), the number of municipalities put under compulsory administration for a given province at $t-2$ and $t-3$, and a set of five crime-related variables—the number of people reported to the judicial authority because of (i) organized crime, (ii) extortion, and (iii) Mafia murders; (iv) corruption; and (v) the number of corruption crimes reported to the judicial authority (all specified in first-difference, in per capita terms, up to two lags). Estimation is by two-stage least-squares using Council-dismissal-S1 and lagged Council-dismissal-S2 as instruments. The standard errors clustered at the region × year level and robust to heteroskedasticity are reported in square brackets. Because our p-values begin at 0.001, our scheme for denoting significance is as follows:
*** Significant at the 0.1 percent level.
** Significant at the 1 percent level.
* Significant at the 5 percent level.

estimate of 1.46 or 1.55, depending on whether we include lagged output growth among the controls (compare columns 4 and 6). Remarkably, the point estimate of this coefficient remains quite stable across the two versions of the model: an exogenous cut in local public infrastructure by 1 percent of local value added determines a contemporaneous reduction in local output of about 1.5 percent.

Relative to the OLS estimates, our IV estimate of the contemporaneous effect of spending is about seven times larger, suggesting a downward bias in the OLS estimators. In our sample, a downward bias in the OLS estimates can be reasonably attributed to the long and complex administrative process governing the allocation and implementation of public projects. Because of this process, actual spending is unlikely to be related to cyclical movements in local output, and some of the effects materialize before projects are carried out. Notably, a large negative difference between OLS and IV estimates is also reported by Serrato and

Wingender (2011) and Nakamura and Steinsson (2014) in their studies based on US data. The differences between OLS and IV reported by these authors are even larger than in our study.[24]

An assessment of the dynamic effects of spending is provided by our baseline model including all controls, shown in the last column of Table 4. Since the first lag of output growth is significantly different from zero, the point estimate of the multiplier including lagged output effects is 1.29 (the ratio between the estimate of β and one minus the coefficient of $Y(t-1)$). In other words, an exogenous cut in local public infrastructure by 1 percent of local value added determines a cumulated reduction in local value added of 1.29 percent over two years.

Further dynamic multiplier effects can be derived under the maintained assumption that lagged spending is exogenous to current output. Under this assumption, we add up the coefficients on the contemporaneous spending growth and its one-year lagged value, which is significantly different from zero. After correcting for the impact of the first lag of the dependent variable, the point estimate of the overall multiplier is as high as 1.95. In this model specification, we are not able to reject the null hypothesis $\beta \leqslant 1$ in favor of $\beta > 1$ at the 5 percent significance level.[25]

In closing this section, two remarks are in order. First, the transmission of fiscal policy may differ across provinces, reflecting area-specific characteristics. If this were the case, and the probability of treatment is correlated with the relevant characteristics, IV regressions would deliver estimates of the multiplier of public spending for the treated areas, rather than average estimates—a well-known issue in quasi-experiments. In studies focused on public investment, it is reasonable to expect the Mafia to affect the productivity of local public spending differently across provinces, as Mafia involvement may cause misallocation of public capital, but could also "grease the wheels" of public investment. This dimension of heterogeneity, however, seems more relevant to assessing the long-run effects of spending on public capital than to estimating the short-run multiplier.

Second, relative to a multiple-equation framework, a potential issue with single-equation models like ours is that the estimated effects of government spending do not take into account the possible feedback from value added to spending. Thus, strictly speaking, our results cannot be compared with results from SVAR models—a point stressed by Sims (2010). However, in our sample, infrastructure investment does not react to value added changes. Namely, in the first-stage regression the coefficients of the two value added lags are not statistically different from zero (see Table 4). In view of these results, the Sims critique does not appear to be a concern in our study.

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[24] As pointed out by one of our referees, potential differences between the OLS and IV estimates between Italy and the United States could reflect the different degrees of pro- or countercyclicality in spending. This is because fiscal policy is arguably less countercyclical in Italy than in the United States at the aggregate as well as the local level. As already mentioned, insofar as the estimated multiplier is positive, the downward bias tends to be larger where fiscal policy is more countercyclical.

[25] In both specifications, the Anderson-Rubin test rejects the null hypothesis, $\beta = 0$, at the 5 percent level (with a p-value about 0.01). Moreover, the Hansen J-Statistic is characterized by a very high p-value, suggesting that the instruments are uncorrelated with the error term.

IV. Do City Council Dismissals Affect Output
Independently of Variation in Public Spending?

For the proposed IV estimation to be reliable, our instrument must not only have a clear effect on $G_{i,t}$, it must also be uncorrelated with the error term *conditional* on controls, i.e., it must satisfy the exclusion restriction. Hence, in our regression relating changes in value added to public spending, we should be confident that the dismissal of the city council matters for provincial output growth only to the extent that it causes a (temporary but sharp) reduction in public spending.

We observe upfront that the inclusion of the province fixed effect takes care of many plausible reasons why the exclusion restriction could fail, due to a systematic negative relationship between our instrument and the *average* output growth at the provincial level. By way of example, provinces in which public spending drops when the city council is dismissed may have a below-average growth rate, because of the Mafia.[26] Conversely, the incidence of Mafia activities may be relatively high in slow-growing provinces, given the lack of opportunities for lawful businesses. Finally, the risk of detection may be correlated with the intensity of Mafia activity.

Nonetheless, there are two other potential channels that could cause the exclusion restriction to fail despite the use of province fixed effects and, thus, deserve careful discussion. The first, already mentioned in Section II, could work through the direct impact on provincial value added of a contraction in mob activities occurring in conjunction with a council dismissal. The second channel could work through changes in the output of the local bureaucracy during periods of compulsory administration.

A. *Variation in Mafia Activities*

The exclusion restriction might not hold if the empirical model fails to control for the possibility that variations in Mafia activities during an episode of council dismissal affect output above and beyond variations in public spending. On theoretical grounds, a successful war on the Mafia can be expected to enhance economic activity in the *long run*, mainly through a rise in investment by local and foreign entrepreneurs. Since a key objective of city council dismissals is to permanently reduce the presence of the Mafia in the country, one could argue that failing to control for this channel may produce a downward bias in the estimate of the multiplier: the negative output effect of a contraction in public spending would be at least in part compensated by the positive effect of "less Mafia."

For our purpose, the root of the concern with a Mafia-activity channel is the way different forms of legal action and police work weighs on the interests and activities of the Mafia in the *short run*, affecting contemporaneous value added. The impact of strong legal action during the compulsory administration of a municipality cannot be signed unambiguously. On the one hand, the removal of elected officials connected to the Mafia, and a reduction in political corruption and crimes such as extortions, which act like a "tax" on firms and households, may stimulate the economy in

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[26] Country-level studies suggest a negative relationship between the diffusion of corruption and long-run economic growth. Via corruption, the historical presence of the Mafia in a province might have a negative effect on its long-run growth, which would be reflected in low output growth rates over our sample years.

the short run. On the other hand, the Mafia may downsize or close down activities that were previously generating value added, then causing output losses. Moreover, any relocation of Mafia activities across provinces may make our results sensitive to the level of aggregation we use in the analysis. These possibilities raise intriguing and difficult issues in identification. On empirical grounds, the challenge is to find controls that can successfully account for variations in Mafia activities.

As discussed in Section I, in our estimation we aim at controlling for the Mafia-activity channel using measures of the outcome of police investigation at local level. The idea underlying this strategy is that, as Mafia groups run many activities—some of which may in principle contribute to, while others damage, provincial value added—the arrest of *mafiosi* and intense police investigation are correlated to changes in either or both types.[27] In this section, we follow up on our idea, by checking whether our estimates of the spending multiplier rise or fall when we omit these controls from the regression model. The exercise aims to produce evidence on the direction of the net effect on output of a Mafia-activity channel. If, when the controls for police investigation are omitted, the estimated multipliers rise, this would suggest that, overall, changes in Mafia activities following a city council dismissal could translate into output losses. In this case, omitting the control would amount to misattributing the contraction of economic activity to spending cuts.

The first column of Table 5, however, shows that our estimate of the spending multiplier *falls* when we exclude the controls for Mafia activity from our model: the output effect of a spending contraction becomes less negative. Thus, the net direct effect of the Mafia-activity channel appears to push output in the opposite direction relative to cuts in public spending. We stress that our controls for Mafia activity are correlated with the instruments, as can be shown by studying the reduced-form regression of our model:

$$(4) \qquad Y_{i,t} \,=\, \delta_1 \, CDS1_{i,t} \,+\, \delta_2 \, CDS2_{i,t-1} \,+\, \alpha_i \,+\, \lambda_t \,+\, \varsigma \mathbf{X}_{i,t} \,+\, \xi_{i,t}.$$

When **X** includes all the controls, as in our baseline specification, the effect of the instruments on output change is statistically significant and negative. The F-statistic testing the joint hypothesis that $\delta_1 = \delta_2 = 0$ is 5.13 (p-value = 0.0067). When the controls for Mafia activity are omitted, the point estimates of δ_1 and δ_2 become less negative, i.e., they increase towards zero. Their statistical significance is affected, too. The F-statistic testing the hypothesis $\delta_1 = \delta_2 = 0$ drops to 2.77 (p-value = 0.0651).[28]

These results provide evidence that, as expected, our controls for the Mafia-activity channel are correlated with the instruments. They also provide evidence that the overall short-run output effect of police investigation against the Mafia, if any, is positive—the same sign as it is commonly argued in studies of its long-run effect.

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[27] For our measures to be a good proxy for the Mafia-activity channel, the scale of Mafia activities must be correlated with arrests and charges of *mafiosi* by the police. Arrests and charges may actually fall during a compulsory administration, if overall deterrence causes *mafiosi* to withdraw from the province. Moreover, a fall in the number of arrests may reflect the decision by the Mafia to downscale activities. We thank two referees for stressing these points.

[28] As a complementary exercise, we replace the left-hand side of the reduced form with $Y - \hat{\beta}G$, imposing $\hat{\beta} = 1.55$ as suggested by our estimate. In this case, the F-statistic becomes very close to zero (p-value = 0.99), consistent with the presumption that the orthogonality conditions are valid.

TABLE 5—DO CITY COUNCIL DISMISSALS AFFECT OUTPUT
INDEPENDENTLY OF VARIATION IN PUBLIC SPENDING?

	(1)	(2)	(3)	(4)	(5)
$G(t)$	1.30**	1.56***	1.53***	1.59***	1.64***
	[0.45]	[0.41]	[0.44]	[0.47]	[0.44]
$G(t-1)$	0.69***	0.79***	0.78***	0.81***	0.82***
	[0.19]	[0.19]	[0.19]	[0.20]	[0.20]
$G(t-2)$	0.16	0.19	0.19	0.20	0.20
	[0.09]	[0.11]	[0.11]	[0.11]	[0.11]
$Y(t-1)$	−0.19**	−0.20**	−0.20**	−0.20**	−0.20**
	[0.06]	[0.06]	[0.06]	[0.06]	[0.06]
$Y(t-2)$	−0.02	−0.01	−0.02	−0.02	−0.01
	[0.05]	[0.05]	[0.05]	[0.05]	[0.05]
Resignation(t)		−0.00			
		[0.05]			
Resignation$(t-1)$		0.04			
		[0.06]			
Election(t)			−0.03		
			[0.08]		
Election$(t-1)$			0.03		
			[0.13]		
Budget—no confidence vote(t)				0.03	
				[0.15]	
Budget—no confidence vote$(t-1)$				−0.16	
				[0.16]	
Others(t)					0.45
					[0.39]
Others$(t-1)$					0.29
					[0.39]
F-stat instruments	12.56	14.17	12.30	12.12	14.75
Observations	950	950	950	950	950

Notes: The table shows results from either dropping proxies for Mafia activity (column 1) or adding controls capturing council dismissals for reasons unrelated to Mafia infiltration (columns 2–5). Dismissals may occur because of resignation by elected officials and special cases of ineligibility of the mayor; failure to organize elections; failure to pass the annual budget; and political crisis in the ruling coalitions. All the other (few) circumstances have been bundled together in the last two regressors, accounting for all the dismissals unrelated to Mafia infiltration. The standard errors clustered at the region × year level and robust to the heteroskedasticity are reported in square brackets. Because our p-values begin at 0.001, our scheme for denoting significance is as follows:
  ***Significant at the 0.1 percent level.
  **Significant at the 1 percent level.
  *Significant at the 5 percent level.

Arguably, these results do alleviate concerns that a Mafia-activity channel—when not appropriately controlled for—would induce an upward bias in the estimated local multipliers.

B. *A Shock to Government*

Independently of their effects on public spending, it might be possible that city council dismissals *per se* are negative shocks to the productivity of local administration. Specifically, one concern is that the sudden replacement of elected officials with external commissioners may reduce administrative output, with negative

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effects on economic activity. By way of example, the number of business licenses issued in a municipality may drop during compulsory administration.

This concern turns out to be unfounded on both institutional and empirical grounds. On institutional grounds, city council dismissals are envisioned as a proactive initiative in the fight against the Mafia. The commissioners are given the mandate to act as efficiently as possible, with the specific goal of showing the population the social benefits of freeing local institutions from the Mafia. Below, we report two informative quotes from official documents. "The compulsory administration in itself must be an opportunity for improving the administration, the politics, and the relations between the government and the citizens" (De Rita 1995, p. 10); "the compulsory administration must not be a simple bridge towards new elections, but an opportunity for the development and growth of local institutions, as well as an opportunity for a new beginning for the local community" (Commissione Parlamentare d'Inchiesta 2005, p. 9).[29]

Based on an in-depth analysis of a sample of 19 municipalities (over the years covered by our study), the report by the Ministro dell'Interno (2000) concludes that the commissioners pursued their mandate scrupulously in this dimension. In the findings of the report, the external commissioners made sure that administrative acts (such as new hiring), which were de facto blocked or suspended because of distortions attributable to the Mafia, were taken to completion, in areas spanning health, education, police force, and social work.[30]

To shed light on the issue, we extend our empirical model exploiting the fact that city councils can also be dismissed for reasons other than Mafia infiltration, without necessarily implying a spending cut on public works. If council dismissals are *per se* shocks to government, they should have a negative effect on output even when they do not entail any contraction in spending.

For our model extension, we built a dataset including all the cases of city council dismissals in Italy not related to the 1991 anti-Mafia law. Reasons for dismissals include: (i) resignation by elected officials; (ii) failure to organize elections; (iii) special cases of ineligibility of the mayor; (iv) failure to pass the annual budget; and (v) political crisis in the ruling coalitions. To carry out our analysis, as (iii), (iv), and (v) were the least common cases, we merged (iii) with (i), and (iv) with (v).[31] We also aggregated the municipality-level information by province, consistent with our main dataset.

The key result from our extended model is that the cases of council dismissals not related to Mafia infiltration are uncorrelated with a drop in public spending: in the first-stage regression for our augmented model (not shown), none of the new covariates is statistically significant at the 5 percent level. The question is then

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[29] Our own translation.

[30] However, the official documents also recognize the limits of the commissioners' achievements, pointing out that in most municipalities there were few or no fundamental changes by the end of compulsory administrations. In other words, in the assessment by the *Commissione*, the main shortcoming of the law was not a reduction in the output by the local administration, but the absence of any significant improvement in its performance: the achievements by the commissioners were limited to ongoing administrative activities (Commissione Parlamentare d'Inchiesta 2005).

[31] During our sample period, the total number of city councils dismissed for reasons not related to Mafia infiltration was 2,031. The most common reason was the resignation by elected officials, which accounted for about half of all cases.

whether dismissals of city councils for reasons other than Mafia infiltration (hence not associated with a significant variation in public spending) have any significant effect on output. As shown in columns 2 through 5 of Table 5, this is not the case: the estimated coefficients are not significantly different from zero.

Summing up, neither administrative documents nor statistical analysis produce evidence of a "compulsory administration channel" which affects economic activity through a drop in the performance of local bureaucracy. The output effects appear to materialize only when dismissals are associated with a cut in public spending.

V. Further Results

In this section, we further investigate the properties of our empirical model. Specifically, we analyze the cross-border effects of local spending on the output of neighboring provinces, the influence of individual provinces on our estimates, and the implications of restricting our sample to the southern regions only.

A. *Cross-Border Effects*

Spending variations in one province may affect economic activity in neighboring provinces, through different channels. On the one hand, some of the contraction in demand in one municipality may "leak" into nearby areas, driving down economic activity simultaneously within and outside the province where spending is cut. Demand spillovers would induce a positive correlation in the response of value added in adjacent provinces. On the other hand, in response to a localized spending shock, it is possible that production factors relocate, moving across the borders of the province hit by the fiscal contraction. With this second type of spillovers, the fall in local economic activity in the province under compulsory administration would correspond to an increase in economic activity in the nearby areas, inducing a negative correlation in the response of provincial value added across borders. If either type of spillover were to be empirically relevant, our estimates would miss part of the output effects of spending innovation in a province, reflecting either demand leakages or relocation effects.

We carry out an analysis of the cross-border effects of local spending both by extending the set of regressors, and by aggregating small adjacent provinces. Results are shown in Table 6. Specifically, in the first column, the regression model also includes the variable $SG_{i,t} = \frac{Sg_{i,t} - Sg_{i,t-1}}{Sy_{i,t-1}}$ and its first lag, where for province i and year t, $Sg_{i,t}$ is the per capita investment in provinces which are in the same region excluding province i itself, and the variable $Sy_{i,t-1}$ is defined accordingly. The coefficients of the newly defined variable and its lag are low; that of $SG_{i,t}$ is not significantly different from zero.

In the second column of Table 6, we enter $SG_{i,t-1}$ interacted with $G_{i,t-1}$, both measured in terms of deviation from the respective mean value. We thus allow for the possibility that the effect of local spending reflects either complementarity (as a result of demand leakages), or substitutability (as a result of high spatial mobility of factors of production) between spending in adjacent areas. The coefficient on the interaction term is not significantly different from zero. Note that including the

TABLE 6—SPILLOVERS

	(1)	(2)	(3)
$G(t)$	1.44**	1.50***	1.24**
	[0.47]	[0.41]	[0.45]
$G(t-1)$	0.73***	0.76***	0.74**
	[0.20]	[0.17]	[0.23]
$G(t-2)$	0.17	0.20	0.17
	[0.11]	[0.10]	[0.16]
$Y(t-1)$	−0.20**	−0.20**	−0.21**
	[0.06]	[0.06]	[0.08]
$Y(t-2)$	−0.02	−0.02	−0.05
	[0.05]	[0.05]	[0.06]
$SG(t)$	0.20		
	[0.18]		
$SG(t-1)$	0.35*		
	[0.16]		
$G(t-1) \times SG(t-1)$		0.19	
		[0.12]	
F-stat instruments	10.61	12.00	24.20
Observations	950	950	410

Notes: For each province i, the variable SG denotes public spending variations in provinces that are in the same region as i, excluding province i itself. In the second column, we enter $SG_{i,t-1}$ interacted with $G_{i,t-1}$, both measured in deviation from the mean value. In the third column we show results where our original observations are replaced by new ones, which aggregate either two or three adjacent provinces in a single area. The standard errors clustered at the region × year level and robust to heteroskedasticity are reported in square brackets. Because our p-values begin at 0.001, our scheme for denoting significance is as follows:
 ***Significant at the 0.1 percent level.
 **Significant at the 1 percent level.
 *Significant at the 5 percent level.

spillover term in the set of regressors has only minor effects on the point estimates of the coefficients on the contemporaneous and lagged spending variables (see columns 1 and 2 of Table 6).

The third column of the table shows the results of replacing observations relative to small provinces with new ones, aggregating adjacent provinces into a single unit. The excluded instruments F-statistic is now about 24. The coefficient of $G_{i,t}$ falls somewhat; that of $G_{i,t-1}$ is more or less unaltered. Overall, from these exercises, the evidence on the spillovers effect of spending contractions is weak.

B. *Influence of Individual Provinces*

Some episodes in our sample may exert a disproportionate influence on our estimates, similar to how certain episodes of fiscal expansions—e.g., the US military build up during World War II—are recognized to be key in ascertaining aggregate multiplier effects. We address this issue by analyzing the extent to which our main evidence is sensitive to the exclusion of any particular province from the analysis.

In our check, no single province appears to be a crucial driver of our estimates. In the first seven columns of Table 7, we report results excluding the following provinces in turn: Napoli, Caserta, Palermo, Catania, Salerno, Bari, Reggio Calabria. As shown in Table 1, these are the provinces with the most episodes of city council

TABLE 7—DROPPING PROVINCES

	NA	CE	PA	CT	SA	BA	RC
$G(t)$	1.86***	1.47**	1.46**	1.35*	1.36**	1.53***	1.37**
	[0.44]	[0.47]	[0.46]	[0.54]	[0.47]	[0.41]	[0.43]
$G(t-1)$	0.93***	0.76***	0.76***	0.72**	0.72***	0.78***	0.73***
	[0.22]	[0.21]	[0.20]	[0.24]	[0.21]	[0.19]	[0.18]
$G(t-2)$	0.24	0.17	0.18	0.16	0.18	0.18	0.16
	[0.12]	[0.11]	[0.11]	[0.11]	[0.10]	[0.11]	[0.10]
$Y(t-1)$	−0.20**	−0.21**	−0.20**	−0.20**	−0.19**	−0.19**	−0.17**
	[0.07]	[0.06]	[0.06]	[0.06]	[0.06]	[0.06]	[0.06]
$Y(t-2)$	−0.00	−0.03	−0.02	−0.03	−0.02	−0.01	−0.04
	[0.05]	[0.05]	[0.05]	[0.05]	[0.05]	[0.05]	[0.05]
F-stat instruments	19.59	9.48	11.31	10.90	9.25	11.85	9.42
Observations	940	940	940	940	940	940	940

Notes: Each column reports estimates after dropping the headline province. NA: Naples; CE: Caserta; PA: Palermo; CT: Catania; SA: Salerno; BA: Bari; RC: Reggio Calabria. The standard errors clustered at the region × year level and robust to heteroskedasticity are reported in square brackets. Because our *p*-values begin at 0.001, our scheme for denoting significance is as follows:

  *** Significant at the 0.1 percent level.
  ** Significant at the 1 percent level.
  * Significant at the 5 percent level.

dismissals. The point estimates of β (all statistically significant at the 5 percent level) are in the range 1.35–1.86, in a roughly constant proportion to the coefficients on the first lag of public spending.

C. *Influence of Heterogeneity across Macro Areas, Time, and Province-Specific Dummies*

In Table 8, we show the effects of restricting the sample to southern provinces only, and dropping the year and province fixed effects. These exercises could in principle be consequential for our estimates. For instance, without year fixed effects, our estimates could be exposed to the influence of national monetary and budget policies, as well as aggregate cyclical fluctuations, as discussed in Section II. Similarly, without province fixed effects, multipliers could reflect the spurious cross-sectional effects discussed in Section V.

In the first column of Table 8 we show the effects of restricting the sample to southern provinces only, as a way to detect whether heterogeneity across macro areas could impinge on our results (see column "Drop north"). By excluding Northern provinces, the coefficients of contemporaneous and lagged spending rise somewhat (to 1.89 and 0.95, respectively). But so does, in absolute value, the coefficient of the lagged value of output growth (now equal to −0.34). Thus, our estimate of the overall multiplier remains unaffected. In the second column we consider the case of removing the year fixed effect (see column "Drop λ_t"). Also in this case the coefficient of contemporaneous spending rises somewhat, to 1.92. The point estimates of the coefficients on lagged spending and output are, however, unaffected. Similar results follow from dropping the province fixed effect (see the third column, "Drop α_i," of Table 8). In this case our point estimate of β is 1.62. In conclusion,

Table 8—Further Results

	Drop north	Drop λ_t	Drop α_i
$G(t)$	1.89***	1.92***	1.62***
	[0.42]	[0.52]	[0.37]
$G(t-1)$	0.95***	0.75**	0.74***
	[0.19]	[0.27]	[0.18]
$G(t-2)$	0.23	0.12	0.12
	[0.14]	[0.13]	[0.10]
$Y(t-1)$	−0.34***	−0.14*	−0.11
	[0.10]	[0.06]	[0.07]
$Y(t-2)$	−0.01	0.09	0.05
	[0.08]	[0.07]	[0.06]
F-stat instruments	10.54	23.89	13.16
Observations	340	950	950

Notes: In the first column we restrict the sample to the south of Italy. In the second and third columns we drop, respectively, year and province dummies. The standard errors clustered at the region × year level and robust to heteroskedasticity are reported in square brackets. Because our p-values begin at 0.001, our scheme for denoting significance is as follows:
   *** Significant at the 0.1 percent level.
    ** Significant at the 1 percent level.
     * Significant at the 5 percent level.

none of these experiments appears to produce results that are significantly different from those of our baseline estimation.

VI. Conclusions

In this article we have contributed evidence of a nonnegligible *short-run* output effect of public spending at the local level. By relying on episodes of sharp contractions in infrastructure expenditure in Italian provinces, we estimated the local multiplier to be 1.5 on impact, and 1.9 including dynamic effects. We also find no relevant spillovers of spending shocks in a province on the economic activity of nearby provinces. By the features of our empirical model and data, these estimates do not reflect budgetary and monetary policy interactions—interactions that arguably play a key role in determining the aggregate output effects of deficit-financed public spending at the national level.

The policy relevance of quantifying local multipliers is apparent. First, our estimates shed light on the extent to (and the conditions under) which fiscal tools, mainly through redistribution of resources, may provide effective instruments to address area-specific downturns. Second, in times of crisis, financial and fiscal stress may force local governments to implement deep, upfront cuts in spending, with large variation in their extent across areas. Our estimates suggest that differences in the intensity of the upfront retrenchment at local level can translate into significant geographical variation in economic activity.

The Italian provinces we focus on in our study are akin to very small and very open economies sharing a common currency. Our results may thus suggest that economies with these characteristics may actually be quite "insular" in their dynamic response to temporary variation in public spending. Analytical insights on the transmission

mechanism are provided by new-Keynesian models of regional fiscal policy in a currency union or in a credible system of fixed exchange rates, as developed by Corsetti, Kuester, and Muller (2013)—a point also stressed by Nakamura and Steinsson (2014). These analyses put forward a mechanism by which quantitative models of small, open economies without monetary autonomy tend to yield values of the output multipliers around or above unity, for a variety of alternative budget adjustment rules.[32]

Yet the analysis of local multipliers raises distinct issues, typically not addressed in open-economy studies. As discussed in Section VI of this article, for instance, cross-border spillovers at the local level may differ from those implied by the "demand leakages" emphasized by the open-economy literature. Their study requires a careful specification, at both the theoretical and the empirical levels, of spatial models accounting for cross-border mobility of both capital and labor. This new promising direction for research could help in bridging the local and aggregate dimensions of the multiplier, by providing a framework to assess the combined effects at national level of transferring resources across regions.

Data Appendix

Public spending: Our public spending variable is public investment, which 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 plants, swamps, land reclamation, other categories); Buildings (public buildings and schools; public spending devoted to private buildings). ISTAT provides a consistent data series on current-prices spending on infrastructure at the provincial level from 1987 to 1999. The source is: ISTAT, Annuario delle Opere Pubbliche (various issues). We deflate the provincial public investment using the national GDP deflator for Italy.

Value added: The value added at the provincial level is measured in millions of euros at current prices. We deflate the provincial value added using the national GDP deflator for Italy. Source of value added: Istituto Guglielmo Tagliacarne. Source of population: Italian Institute of Statistics, ISTAT (Statistiche Demografiche). Source of deflator: ISTAT (Contabilità Nazionale). In particular, we construct the value added growth rate by relying on two different series of provincial-level value added for the periods 1985–1991 and 1991–1999, respectively. Data for the first period are used to construct values of Y up to 1991, while for the period after 1991 we use the more recent series.

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

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[32] In a currency area, an unexpected contraction in public demand in a region tends to reduce local prices in the short run. Given nominal rates, this drives up the short-term real rates in the region. Because purchasing power parity holds in the medium to long run, however, local prices are expected to rise back to the level prevailing outside the region. Correspondingly, given nominal rates, future short-term real rates are expected to fall. These opposite movements in current and anticipated future short-term real rates imply that the impact response of the *long-term* real rates to a fiscal shock, arguably the relevant ones for private spending decisions, is actually quite small. As a result, in the region hit by the shock, private demand is not crowded-in appreciably, and economic activity tends to fall initially by the full extent of the unexpected fiscal contraction.

Employment: Sources: Istituto Guglielmo Tagliacarne and ISTAT.

Cassa Integrazione Guadagni: Hours of wage supplement provided by the "Cassa Integrazione Guadagni," the main unemployment benefit arrangement covering employees of large private firms in Italy. Source: Istituto Guglielmo Tagliacarne.

Council dismissal related to the 1991 anti-Mafia law: The number of 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 and politicians or through indirect influence. Source: *Commissione parlamentare d'inchiesta sul fenomeno della criminalità organizzata mafiosa o similare. Technical Report* (various issues).

Council dismissal not related to the 1991 anti-Mafia law: All cases of city council dismissals not related to Mafia infiltration. Dismissals may occur because of (i) resignation by elected officials; (ii) failure to organize elections; (iii) special cases of ineligibility of the mayor; (iv) failure to pass the annual budget; (v) political crisis in the ruling coalitions; and (vi) other reasons.

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

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

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

Corruption: Crimes and prosecutions for corruption, defined to include embezzlement, misappropriation of public funds, extortion, and bribery agreements. Source: ISTAT, *Statistiche giudiziarie* (various issues).

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