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Can the Mafia's Tentacles Be Severed? The Economic Effects of Removing Corrupt City Councils
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Can the Mafia's Tentacles Be Severed?
The Economic Effects of Removing Corrupt City Councils

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Abstract

This paper evaluates the long-run economic impact of the fight against organized crime. It uses rich administrative data from Italy and studies one of the most aggressive policies aimed at combating criminal organizations: the city council dismissal. Under this policy, local administrations believed to be infiltrated by the Mafia are dismissed by the central government and the municipality is then administrated by a team of public servants appointed by the central government for approximately two years. Using a matched difference-in-differences design, we find that this policy fosters economic growth. Specifically, the city council dismissal increases formal employment by 16.9% nine years after the dismissal and this effect appears to be partially driven by the entry of new workers in the formal sector. Treated municipalities also display higher economic dynamism and a surge in industrial real estate prices in the aftermath of the intervention. These effects appear to be mediated by an increase in the quality of local politicians elected after the city council dismissal. We show that these newly elected politicians raise local tax compliance and were able to increase expenditures on roads and infrastructures. Overall, our results imply that there are significant long-run economic benefits associated with targeted law enforcement actions against criminal organizations.

JEL codes: D73, G38, K42.

1   Introduction

The fight against organized crime is central to the political agenda of both developed and developing countries. Key to the survival of criminal organizations is their ability to infiltrate local institutions (Acemoglu et al., 2013). The fertile environment generated by corrupt institutions allows organized crime to move from predatory crimes to much more profitable business crimes (e.g., construction, bid rigging, and money laundering) (Pinotti, 2015b). The ties between criminal organizations and local political institutions (i.e., the Mafia's "tentacles") are viewed by many commentators as a major obstacle to economic growth (UNICRI, 2016).[1]

Several studies have documented how criminal organizations originated (Acemoglu et al., 2019; Bandiera, 2003; Buonanno et al., 2012; Marcolongo, 2020; Mastrobuoni and Patacchini, 2012). Much less is known about the types of policies that are effective, *today*, at combating the widespread corruption generated by these criminal organizations. Even more scarce is rigorous evidence on whether anti-corruption policies can ultimately generate long-run economic development, especially in areas where criminal organizations have been active for decades, if not centuries.

This paper attempts to fill the gap by estimating the long-run economic impact of one of the most aggressive policies aimed at combating organized crime in Italy: the city council dismissal (CCD henceforth). When a municipality is (allegedly) infiltrated by the Mafia, the entire political apparatus of the municipality — including the mayor and the city council — is dismissed and replaced by a team of external commissioners appointed by the central government who retain full legislative and executive powers for about two years. Their primary purpose is to break the ties between the Mafia and the local government by, among other things, strengthening state capacity and gaining the trust of the local communities.

This policy has two remarkable features: first, it generates sharp variation in the "quality" of local institutions in a given municipality (Acemoglu et al., 2001). Second, CCDs represent a unique type of place-based policy (Kline and Moretti, 2014). While most place-based policies provide tax subsidies or public investments to disadvantaged areas, CCDs directly target local institutions by removing allegedly corrupt politicians and replacing them with trusted public servants. Despite these unique features, whether CCDs are successful at generating economic growth is ultimately an empirical question. Many commentators argue that CCDs are unlikely to be effective due to their short time frame, especially in areas that represent longstanding Mafia strongholds (Mete, 2009). More generally, whether anti-corruption programs can generate positive effects on economic growth has long been an open question in the crime literature and

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[1]We use the terms "Mafia," criminal organizations, and organized crime interchangeably throughout the paper.

only very recently have some empirical studies started tackling this question (Colonnelli and Prem, 2020).

Our evaluation of the economic impact of CCDs exploits rich administrative data, including the universe of Italian social security records and detailed information on public finances, public procurement, and real estate prices. We match these data with 243 CCD events that occurred between 1991 and 2016. The procedural requirements of the CCD lend credibility to the identification strategy. The CCD has a strict procedure designed to limit its potential for abuse. As a result, the central government does not use the CCD to expel political opponents (Mete, 2009). The rigidity of the procedure requires the intervention to be mafia-related, which has the additional effect of also limiting the government from intervening for economic reasons, a fact that we corroborate in our econometric analysis.

The CCD policy varies over both time and place allowing us to study its effects via a matched difference-in-differences analysis. Specifically, each municipality subject to a CCD event (or each firm operating in a municipality subject to a CCD) is matched to an observationally similar municipality (or firm) that has never been exposed to the treatment. Placebo tests provide evidence in favor our identification assumption by showing no evidence of differential pre-trends between treated and control units over a variety of outcomes both at the municipality and at the firm level.

We find that a CCD spurs economic activity. Difference-in-differences estimates show that treated municipalities experience, relative to their matched counterfactual, an increase in formal employment of 16.9% in the long run, which we define as 9 years after the intervention. We document that this effect is partially driven by a shift from informal to formal employment, as the share of individuals entering the social security records for the first time increases by 4.4 percentage points (about 25% of the baseline share observed in the estimation sample). Similarly, the share of individuals that were not employed in a given year and obtained a formal job in the following year increases by 10 percentage points in treated municipalities (approximately 33% of the aggregate share).

The overall increase in formal employment maps into a *decrease* in average earnings at the municipality level. The increase in formal employment and the coincident decrease in average earnings is consistent with some reallocation of lower-paid workers from informal to formal jobs. The increase in the share of new formally employed workers following a CCD is accompanied by a significant increase in the number of firms operating in treated municipalities (9.4 percentage points 9 years after the intervention). Moreover, firm exits increase by 4.3 percentage points in the long run. We interpret the increase in both firm exits and entries as an increase in economic "dynamism" caused by the CCD intervention.

Next, we evaluate the impact of the policy on incumbent firms. We compare within-firm

changes in key outcomes of firms operating in treated municipalities relative to matched control firms. Difference-in-differences estimates show that incumbent firms in treated municipalities experience a 3.9% increase in their size. This expansion results in an increase of 3.2% in the wage bill. In contrast to what we find at the municipal level, average earnings do not appear to be affected at the firm level. This suggests that the negative impact found at the municipal level is mainly driven by either the entry of new firms that are paying less than the average firm or the exit of high-paying firms.

A key fact that emerges from the analysis is that CCD events are able to significantly raise the number of both formal employees and firms. Given the prevalence of the informal sector in the municipalities under study, one might wonder whether the increase in formal employment represents real economic growth or a mere reallocation of workers and firms from the informal to the formal sector (Ulyssea, 2018).

Answering this question requires some proxy for economic activity for both the formal and the informal sector. To this end, we use detailed administrative data on real-estate transactions. As the supply of real estate tends to be relatively fixed over time, especially in a place like southern Italy, any increase in economic activity or more generally in the attractiveness of a given municipality is expected to result in an increase in real-estate prices. Crucially for our purposes, variation in real-estate prices is induced both by the informal and the formal sector. Therefore, if the CCD intervention results in no more than a mere reallocation of workers and firms from the informal to the formal sector with no "real" economic effects, we would expect no impact on real estate prices.

On the contrary, we find that CCDs cause a 10% increase in the price of industrial real estate (i.e., small factories and craft workshops). We interpret the surge in the prices of business properties as further evidence that CCDs spur local economic development and that our reduced-form evidence on formal employment and number of firms is not entirely driven by a shift from the informal to the formal sector.

In the second part of the paper, we try to shed some light on the mechanisms that generate the positive economic impact of CCDs. We start by investigating the changes induced by this policy on the fiscal budget of treated municipalities. Using unique and detailed administrative data on both revenues and expenditures of Italian municipalities, we show that the external commissioners affect the local budget on a key margin: tax enforcement. The share of revenues generated via the collection of municipal taxes increases by around 3 percentage points, roughly a 10% increase. Crucially, this effect materializes coincidently with the arrival of the external commissioners and persists even after the elections that take place in the aftermath of the dismissal, suggesting that the commissioners induce a persistent change in this key margin for the local administration. The dynamic evolution of the treatment effects as well as the fact

that external commissioners are unable to change tax rates jointly suggest that effects of the policy on tax-revenues are not due to an increase in local economic activity nor changes in the tax rate but should instead be primarily attributed to better tax enforcement.

We then examine the expenditure margin, defined as the fraction of municipal revenues that are spent by the local administration on different expenditure items (e.g., garbage collection, administrative costs, etc.). We find that CCDs affect two key margins. First, local administrative expenditure increases coincidently with the arrival of the external commissioners. We suspect that this may be due to the effort of the external commissioners to increase local tax enforcement, which may be achieved by hiring more staff. Second, expenditures on roads and other transportation infrastructures also appear to increase after the CCD intervention and in particular right after a new city council is elected, although effects on this expenditure margin appear somewhat imprecise.

Finally, we investigate whether a CCD affects public procurement. We find a small and not statistically significant long-run impact on the number of tenders, the reserve price, and the number of winners. We conclude that a CCD does not appear to change public procurement in an economically meaningful way.

A stylized fact that emerges from our analysis is that most of our estimated treatment effects display a similar pattern: they are essentially flat until year 3 post-intervention and then start growing significantly thereafter.[2] We believe that this is not mere coincidence as year 3 represents a turning point in the life of the municipality: it coincides with the end of the commissioners's mandate and the formation of a new local government.

This suggests that a mechanism that is likely to mediate the positive economic impact of CCDs is the election of higher-quality politicians. Daniele and Geys (2015b) present evidence of this mechanism by showing that after the intervention municipalities tend to elect politicians with higher levels of education. We complement the findings in Daniele and Geys (2015b) in two important ways. First, we provide evidence on how local budgets are affected by higher-quality politicians (better tax enforcement and higher expenditure on roads and infrastructure). Second, we show how the CCD maps into higher and persistent economic growth. Our analysis suggest that CCDs represent a viable tool to sever the ties between the Mafia and local administrations and foster local economic development in areas characterized by a pervasive presence of criminal organizations.

This paper contributes to three strands of literature. First, it relates to the literature on the impact of the fight against organized crime and more broadly corruption. Slutzky and Zeume (2019) and Calamunci and Drago (2020) study the judicial administration of Mafia firms and

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[2]This can be seen in the effect of CCDs on formal employment, number of firms, expenditure margin, share of new entrants, industrial real prices, and expenditure on roads and infrastructure.

find that the influence of organized crime reduces competition, innovation, and investment. Along the same lines, Colonnelli and Prem (2020) evaluate the impact of an anti-corruption program in Brazil and document that audited municipalities experience an increase in economic activity concentrated in those sectors most dependent on government relationships. In contrast to these previous studies, Slutzky, Villamizar-Villegas and Williams (2019) illustrate the perverse and unintended consequences of anti-money laundering policies in Colombia. They find that the crackdown on money laundering severely reduces deposits and negatively affects local banks.

We contribute to this literature in three ways: first, we provide novel empirical evidence that CCDs foster long-run economic activity and significantly increase formal employment. Second, compared to previous studies, we examine a much more aggressive policy and leverage a unique source of variation in the quality of local institutions. Third, we document the impact of the fight against the Mafia not only on medium and large firms but also on small businesses, which constitute the bulk of firms operating in poor areas characterized by high levels of informality. Small businesses have been often overlooked in empirical research due to data limitations; however, the universe of social security records is uniquely suited for studying them.

Second, our paper fits into the broader literature that studies the economic effects of organized crime. In his seminal paper Pinotti (2015a) documents that southern Italian regions under the control of the Mafia experience a 16% reduction in GDP over thirty years together with a significant increase in homicide rates. Mirenda et al. (2019) find that, although revenues of infiltrated firms increase after criminal organizations take over, Mafia infiltration has long-run negative effects on local economic growth. Le Moglie and Sorrenti (2020) examine how the Mafia invests in the legal economy and show that provinces with higher criminal infiltration were less severely impacted by the 2008 financial crisis, a finding that they interpret as consistent with the presence of investment by the Mafia in legitimate businesses. We add to this literature by evaluating the economic consequences of a policy aimed at severing the ties between local government and organized crime. Our results are in line with Pinotti (2015a) and Mirenda et al. (2019) as they suggest that the Mafia hinders competition and economic activity. Importantly, we do not find support for the hypothesis that organized crime "greases the wheels" of cumbersome bureaucracies and generates economic growth (Colonnelli and Prem, 2020).

Finally, our work also speaks to the literature that examines the effects of city council dismissals. Acconcia et al. (2014) argue that CCDs have a negative effect on province-level GDP, while other studies document that they negatively impact the electoral performance of incumbent politicians, reduce turnout in local elections (Daniele and Geys, 2015a), and increase the quality of newly elected politicians in the aftermath of the intervention (Daniele

and Geys, 2015b). CCDs also appear to have short-term spillovers effects on spending and public procurement in neighboring municipalities (Galletta, 2016; Tulli, 2019). Our paper expands this literature by studying the impact of CCD policy on workers and firms. We improve on previous studies by using detailed administrative data at a much more granular level of disaggregation and providing credible empirical evidence of both short- and long-run impacts.

2 The City Council Dismissal due to Mafia Infiltration

The city council dismissal due to Mafia infiltration was introduced in 1991 as a response to the growing influence of the Mafia on local governments in the 1980s (D.L. 31/05/1991 n. 164). According to the law, if a local government appears to be infiltrated by the Mafia, the mayor, the executive committee, and the city council are dismissed and replaced by a team of external commissioners appointed by the central government (*commissari straordinari*). The external commissioners have full executive and legislative powers and they typically run the municipality for 24 to 36 months until new elections take place.[3] These officers are distinguished public servants who have had an impeccable career in public administration and are often selected from a different geographical area to minimize potential connections with the dismissed municipality.

By removing allegedly corrupt local politicians, the law aims at severing the ties between criminal organizations and the local government. This is arguably the most aggressive policy the Italian government uses in the fight against organized crime (CNE, 1995).

This policy has a preventive purpose: replacing local politicians with external commissioners aims to prevent future crimes rather than prosecute past ones. For this reason, the government does not require evidence of crimes or illegal activities to initiate a CCD but deems sufficient any evidence that suggests a *connection* between organized crime and local governments. Official reports document that CCDs typically get initiated during independent ongoing police investigations and that the most common type of evidence that triggers these interventions relates to the personal life of local politicians and municipality bureaucrats (CPI, 2005).[4] Other types of investigations that can trigger a CCD focus on actual crimes such as extortion, drug and arms trafficking, money laundering, vote buying, collusion in public procurement, and so forth. Interestingly, these reports suggest that the CCD is not triggered by poor municipality financial performance or by inefficiencies and delays in public procurement.

The law establishes a very rigid timeline from the moment in which the evidence emerges to

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[3]See Online Appendix A for a description of the political institutions of Italian municipalities and additional institutional details on CCDs.

[4]For example, one of the elements that contributed to the dismissal of the city council of Bovalino in 2014 was the fact that a local Mafia boss attended the wedding of a politician's close relative. On that occasion, the mafioso was treated as a guest of honor and was attended by the groom himself.

the final decision on whether to dismiss the local government. This strict regulation is meant to limit arbitrariness and avoid unnecessary delays. When evidence of connections between elected public officials and the Mafia emerges, it is reported to the *prefetto*, who is the representative of the Ministry of the Interior in the province. The *prefetto* forms a commission that is in charge of conducting an investigation (*commissione d'accesso*) and sends it to the allegedly corrupt municipality. At this time, it becomes public knowledge that the CCD procedure is underway.

The investigation lasts for a maximum of three months, at the end of which the investigators draft a report. The *prefetto* has 45 days to send the report to the Ministry of the Interior, who makes the final decision after consulting with the cabinet. If the Minister decides to dismiss the local government, the decision is published in the *Gazzetta Ufficiale* by a decree of the President of the Republic.[5]

One concern is that the central government might use this procedure to take over municipalities run by political opponents. Mete (2009) documents that this is not the case and shows that the central government is not more likely to dismiss a city council when the mayor is affiliated with the opposition compared to when she is affiliated with the coalition in power.

Figure 1 summarizes the time variation in the use of CCDs between 1991 and 2016. This policy was introduced in 1991 and has been used repeatedly since then: 245 municipalities went through this procedure (151 municipalities did once, 35 twice, and 8 three times). The fact that some cities experience multiple CCDs suggests that it is sometimes challenging to sever the connection between criminal organizations and local politicians. Figure 1 exhibits a spike in 1993 and another one in 2012. The extremely high number of municipalities that were put under the control of the external commissioners in 1993 is a reaction to the terroristic attacks of Cosa Nostra in the early '90s. In 2012, a few months after Monti replaced Berlusconi as the Prime Minister, the government approved city council dismissals in 23 municipalities. This decision is in line with the broader agenda promoted by the Monti government to structurally change Italian institutions. Figure 2 illustrates the geographic variation in the use of this procedure. As one might expect, most of the CCDs take place in southern Italy, where the Mafia originated at the end of the nineteenth century (Acemoglu et al., 2019). However, there are several instances of dismissals happening in northern regions such as in Piedmont, Lombardy, and Liguria.

Next, we review some anecdotal and empirical evidence on the activities undertaken by the external commissioners during their mandate. We gather the anecdotal evidence from the

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[5]Anecdotally, the vast majority of investigations result in a dismissal. Since 2009 the Ministry of the Interior has published the results of these investigations. Out of the 97 investigations initiated between 2009 and 2016, only 32 of them did not result in a city council dismissal. Although it would be natural to use this subset of municipalities as a control group, we do not pursue this strategy because the number of municipalities that were investigated but did not go through a dismissal is too small.

detailed official reports that the Ministry of the Interior periodically presents to the Parliament.[6]

The commissioners typically implement five types of interventions. First, they freeze all investments in new projects in order to conduct an initial review. During the review period, they carefully examine the municipality's financial situation and scrutinize awarded procurement contracts, permits, and business licenses. As the external commissioners have direct control over the municipality's expenditure and public procurement, this initial review process slows down public investments and new procurement contracts.

Second, the external commissioners focus on the restoration of legality and revoke public procurement contracts, permits, and business licenses if they appear to have been obtained illegally or by means of connections to organized crime.

Third, the external commissioners strengthen state capacity by changing personnel practices within the municipality. The official reports document that municipality bureaucrats are often poorly qualified and at times uncooperative whereas the commissioners take actions to move toward a functional and professional bureaucracy. To this end, they often reorganize municipality offices and restructure the work of the employees by reassigning tasks within and across offices. As part of the restructuring process, the commissioners often mandate municipality employees to take part in training courses. The commissioners also (temporarily) hire additional workers to be employed at understaffed sites.

Fourth, the external commissioners focus on urban planning and set the basis for future investments in infrastructure. Finally, they try to gain the trust and support of local communities. They may do so by providing services such as free access to training courses for young residents, improving the sewage system, repairing the local infrastructure, and so forth.

3 Data

For our main analysis, we use the universe of social security records combined with the list of CCDs obtained from the decrees of the President of the Republic. To shed light on the mechanisms that generate the positive economic impact of CCDs, we combine data from multiple sources including public procurement contracts, municipality finances, the register of local politicians, real estate prices, and information from the official dismissal reports.

Social Security Data: Our main source of data is the confidential matched employer-employee dataset collected by the Italian Social Security Agency (*Istituto Nazionale di Previdenza Sociale* — INPS hereafter). This administrative dataset contains the universe of non-agricultural firms with at least one employee from 1983 to 2017. These data include unique

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[6]We describe these official reports in detail in Section 3.

firm and worker identifiers that allow us to track them over time. Each firm is identified by a tax identification number (TIN) and workers are identified by their social security number (*codice fiscale*). These data contain earnings, the number of days worked at each job in a year, contract type, occupation, detailed industry codes, part-time vs. full-time status, gender, age, firm location, and workers' residence. Importantly, the social security records do not include self-employed or unemployed workers, informal sector employees, and public sector employees. The Social Security Administration also collects data on the opening and closing of businesses as well as firms' juridical status.

These data are uniquely suited for studying the impact of CCDs as the local economy of dismissed municipalities is characterized by the presence of a myriad of small businesses (Section 4.d). Small firms and sole proprietorships have often been overlooked in empirical research as they do not have to report their income statements to the Chamber of Commerce and, as a result, do not appear in most firm-level datasets (e.g., Cerved, AIDA, and Amadeus). The social security records represent the most comprehensive available data on small firms and sole proprietorships. Our sample consists of all firms and workers operating in any of the nine regions that have experienced at least one city council dismissal from 1991 to 2016.[7]

City Council Dismissals due to Mafia Infiltration: We collected the list and the dates of city council dismissals due to Mafia infiltration from the decrees of the President of the Republic published in the *Gazzetta Ufficiale*. We also gathered anecdotal evidence on the activities undertaken by the external commissioners from the reports that the Ministry of the Interior periodically presents to the Parliament.[8] These reports are drafted every six months to a year and contain detailed information on the interventions pursued by the external commissioners in each municipality.

Public Procurement Data: We also have access to detailed data on public procurement contracts collected by the Italian Authority for Public Contracts (*Associazione Nazionale Anticorruzione* — ANAC hereafter). These data include the universe of public procurement contracts for public works with a reservation price above 150,000 euros between 2000 and 2007, and the universe of public procurement contracts for public works, services, and supplies with a reservation price above 40,000 euros for the period between 2008 and 2016. Any public administration that intends to award a public procurement contract with a reservation price greater

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[7]These regions are Liguria, Piedmont, Lombardy, Lazio, Campania, Calabria, Basilicata, Apulia, and Sicily.

[8]*Relazioni parlamentari sull'–attivitá svolta dalla gestione straordinaria dei comuni i cui consigli comunali sono stati sciolti per condizionamenti di tipo mafioso.*

than the abovementioned thresholds is required to report it to ANAC.[9] Public procurement data include the type of contract (i.e., public works, supplies, and services), the public entity that issued it, the identity of the winner(s), the reservation price, the actual price paid by the municipality (starting from 2008), and the type of auction or procedure used to award the contract.

Public Expenditure and Local Politicians Data: We complement the data sources described above with information on municipality finances and population (1998–2015) and the register of local politicians (1991–2016) collected by the Ministry of the Interior. Municipality expenditure is classified into two main categories: current expenditure and investment. The former is used to cover the day-to-day expenses, while the latter is directed toward long-term investment. Each of these two broad categories is disaggregated into twelve categories that reflect different municipality functions: administration, justice, local police, public education, culture (libraries, museums, theaters), sports, tourism, road safety, and public transportation, territory and environment (urban planning, sanitation, parks), social assistance, economic development, and other services. In the analysis, we sum the day-to-today expenses and long-term investments for each category. We also combine categories that collect expenditures on social assistance, economic development, and other services into one residual category (called "other expenditures").

House Prices: To shed some light on the mechanisms that drive our main findings, we use municipality data on real estate prices and rents collected and harmonized by Italian Treasury.[10] This dataset spans the period between 2002 and 2015 and has two unique features. First, unlike most real estate price datasets, it includes information on both residential and non-residential units. This is important in our context as it allows us to evaluate whether these two margins are differentially affected by the CCD. Second, it contains information both on sell prices and rents.

4 Research Design

In this section, we introduce the matched difference-in-differences research design used to conduct the policy evaluation of the effects of the CCD. First, we describe the matching algorithm

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[9]As we are exploiting city council dismissal as a source of plausibly exogenous variation, we will focus on public procurement contracts issued by municipalities only. In principle, regions, provinces, and other administrative units can issue public procurement contracts as well, but these contracts are not the focus of this study.

[10]These data are collected by a Treasury department (*Osservatorio del Mercato Immobiliare* – OMI) tasked with monitoring the housing market.

we use to construct the municipality-matched sample and the firm-matched sample that form the basis of our analysis. Second, we develop our econometric model. Third, we discuss threats to our identification strategy. Fourth, we provide summary statistics of our estimation samples.

4.a   Matching Algorithm

Municipality-Matched Sample

Using official records published in the *Gazzetta Ufficiale*, we identify the set of municipalities subjected to a city council dismissal due to Mafia infiltration. Our sample consists of 243 CCD events over the period 1991–2016.

We then match each treated municipality to a counterfactual municipality using nearest-neighbor propensity score matching. As the Mafia operates at a sub-regional level, one concern is that the CCD might have spillover effects on neighboring municipalities. In order to mitigate this concern, we select counterfactual municipalities that are not located in the same region as the treated units. The pool of potential counterfactual municipalities for town i in region r is defined as the set of municipalities that were never subjected to a CCD but are located in a region, other than r, that experienced at least one CCD. There are a total of 4,446 potential counterfactual municipalities.

The propensity score is estimated by predicting the treatment status using a probit model that includes as covariates municipal averages of log earnings at $t-1$ (where t is the year in which the CCD event occurred), log number of employed individuals at $t-1$ and $t-2$, number of residents at $t-1$, population in 1991, and sectoral composition computed using 1-digit ATECO codes. We match 211 events (87%).

Firm-Matched Sample

To construct the firm-level matched sample, we start by identifying incumbent firms operating in a municipality subjected to a CCD. We define a firm as incumbent if it was active in the year prior to the CCD event. Our initial sample includes 50,399 treated firms. We match each of these treated firms to a counterfactual firm using nearest-neighbor propensity score matching. The potential control firms are selected among those located in the same region as the treated firm, with the same juridical status, and that existed in the year prior to the event.[11] The propensity score is obtained by fitting a probit model of treatment status using as covariates: average log earnings at t-1, average log firm size at t-1 and t-2, age of the firm at t-1, and dummies for 2-digit ATECO sectoral indicators. We were able to find a match for

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[11]Firms can have two juridical statuses in our dataset: they can be either a sole proprietorship (*persona fisica*) or a legal person (*persona giuridica*).

42,665 treated firms (85%). This matching strategy allows us to select control firms that are more likely to share a common trend with the treated firms. However, the disadvantage of this approach is that it attenuates our estimates toward zero in the presence of within-region spillover effects.

4.b   Econometric Specification

To develop our empirical specification we build on a number of recent papers that have exploited a dynamic difference-in-differences research design (Jäger, 2019; Goldschmidt and Schmieder, 2017). We first focus on the municipality-level analysis as it allows us to evaluate the aggregate effects of a CCD. Then we shift our attention to the impacts of this policy on incumbent firms.

To estimate the impact of the dismissal of a city council infiltrated by the Mafia on municipal-level outcomes, we estimate the following difference-in-differences model on the matched sample of municipalities described above:

$$y_{mt} = \alpha_m + \lambda_{r(m),t} + \sum_{k=-5}^{9} \tilde{\theta}_k \mathbf{1}\{t = t_m^* + k\} + \sum_{k=-5}^{9} \theta_k \mathbf{1}\{t = t_m^* + k\} \times CCD_m + r_{mt}, \quad (1)$$

where y_{mt} is an outcome variable (such as the logarithm of the number of individuals employed in a firm located in municipality m), CCD_m is an indicator equal to 1 if municipality m experienced the city council dismissal event, $\mathbf{1}\{t = t^* + k\}$ are the event–time dummies,[12] and t_m^* is the year of the CCD event for municipality m. We control for municipality fixed effects, α_m, and region-by-time fixed effects, $\lambda_{r(m),t}$, where $r(m)$ denotes the region associated with municipality m.[13] We omit the dummy for the year prior to the CCD event in the above specification so that θ^k identifies the changes in outcome y_{mt} between treated and counterfactual municipalities relative to the same difference at $k = -1$. Regression results are weighted by the logarithm of the number of firms in the year before the CCD. Standard errors are clustered at the municipality level.

To estimate the impact of the dismissal of a city council infiltrated by the Mafia on firm-level outcomes we estimate the following empirical specification on the matched sample of incumbent firms described in Section 4.a:

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[12]We assign the event time of each treated municipality to its matched control. Therefore, the event–time dummies are defined both for treated and for control municipalities.

[13]More formally, in the case of a municipality being treated multiple times we duplicate the lines and allow for different fixed effects. For instance, if municipality m^* was subjected to a CCD event in YEARS 1995 and 2007, the model includes different fixed effects $\alpha_{m^*,1995}$ and $\alpha_{m^*,2007}$. See also Jäger (2019) for a similar approach and Lafortune et al. (2018) for a general discussion on event studies with multiple event times and associated weighting strategies. We proceed in a similar way when focusing on the firm–level design.

$$Y_{jt} = \psi_j + \lambda_{r(j),t} + \sum_{k=-5}^{10} \tilde{\beta}_k \mathbf{1}\{t = t^*_{m(j)} + k\} + \sum_{k=-5}^{10} \beta_k \mathbf{1}\{t = t^*_{m(j)} + k\} \times CCD_{m(j)} + \epsilon_{jt} \quad (2)$$

where Y_{jt} is an outcome variable for the incumbent firm j in period t, ψ_j and $\lambda_{r(j),t}$ represent firm and region-by-year fixed effects, respectively, and $r(j)$ and $m(j)$ are functions that report the region and the municipality of firm j. All the remaining variables are defined as in the previous equation.

We stress that identification in our research design comes from the comparison over time between treated municipalities (or firms) and counterfactual matched municipalities (or firms) that are never treated. The usage of a matched control group allows us to bypass some of the identification and interpretability issues that arise when relying solely on the timing of treatment as the source of identification in event-study or difference-in-differences research designs; see the discussion in Goodman-Bacon (2018) and Borusyak and Jaravel (2017).

4.c   Threats to Identification

The identifying assumption is that, in the absence of the removal of the allegedly corrupt city council, treatment and control units would have followed parallel trends. This policy is implemented by the national government and it is unlikely to correlate with local trends in the economic performance of treated municipalities and firms. Yet, a threat to our empirical strategy is that the timing of the dismissal is correlated with a time-varying factor that is unobservable to the econometrician.

To address this challenge, we rely on a matching procedure where we match observationally similar municipalities both in levels and *in trends* of employment before the intervention. Although we can not directly test our identifying assumption, the generalized difference-in-difference framework nests a set of placebo test that can be used to assess its validity. We test for violations of the parallel trend assumption in the years leading up to the event by evaluating the event–study coefficients for $k < 0$.

Another concern is that if criminal activity increases at the municipality level, it might be easier to find evidence that connects the local politicians with organized crime and, as a result, the municipality is more likely to go through a CCD. This concern is akin to the one discussed above and challenges the notion that the matched control units represent the appropriate counterfactuals and it can be evaluated by testing for non-parallel pre-trends as discussed above.

We also note that the threat to identification described above is likely to be less of a concern

in the firm-level design. There, the empirical exercise compares outcomes within a particular firm before and after the arrival of the external commissioners triggered by the CCD event. Importantly, a treated firm in our sample is rather small and therefore unlikely to influence the CCD intervention made by the national government.

As in all difference-in-differences designs, unbalances in the outcome levels (or logs) between treatment and control units do not represent a threat to our empirical strategy as they are absorbed by the municipality (or firm) FE in our empirical specification.

4.d   Summary Statistics

Table 1 reports the summary statistics for the matched municipality sample in column (1); columns (2) and (3) display the statistics for treated and control municipalities, respectively. The average municipality in our sample has 15,000 inhabitants, 248 firms, and 258 establishments. As in most small towns, the vast majority of firms have only one plant. Therefore in this context, it is not relevant to distinguish between firms and establishments and we focus on firms throughout the analysis. The average firm in our sample has 6.7 (1657/248) employees. The Italian legal system distinguishes between limited liability companies and sole proprietorships. The latter are associated with less stringent reporting requirements and as such, they typically do not appear in most firm datasets. Importantly, INPS data contain both sole proprietorships and limited liability companies and 52% (128/248) of these small businesses are sole proprietorships in our sample. These rather small economies display a considerable level of dynamism as on average 29 new firms are established every year and 22 firms exit the market. The average municipality in our sample employs 1,657 workers (10.8% of its 1991 population). While this figure is fairly low, it is consistent with southern Italy being characterized by a high prevalence of public sector employment, a large informal economy, and high unemployment rates. Consistent with high informality rates, in each year 24.8% (412/1657) of workers employed in the formal sector did not appear in the social security records in the previous year and 15.4% (255/1656) of employees appear for the very first time in their career in the social security archives. The typical worker in these small economies has a blue-collar job, works full-time, and earns about 8000 euros per year.

Table D.2 presents summary statistics for the matched sample of municipalities in the year before the CCD event. Overall, we find that, although control municipalities are on average larger in the year before the intervention, covariates are relatively well balanced between the treated and control groups. This is the case for both covariates we directly used in the propensity score matching and variables not directly used in the matching procedure. As we discussed in Section 4.c, unbalances in the outcome levels between treatment and control municipalities do

not represent a threat to our empirical strategy.[14] Tables D.3 and D.4 report the analogous statistics for the sample of matched incumbent firms and depict the same stylized facts we just discussed.

5   Results

We begin our empirical analysis by documenting the impact of the removal of the (allegedly) Mafia-infiltrated city councils on municipal–level outcomes. We then study how this policy affects incumbent firms operating in treated municipalities.

5.a   Effect of City Council Dismissal on Municipality-Level Outcomes

Figure 3 reports the estimated impact of the CCD on municipality employment, number of firms, wage bill, and average earnings (namely, the estimated $\hat{\theta}_k$ coefficients from (1)).[15] A subset of these coefficients are also reported in Table 2. Reassuringly, Figure 3a shows that there are no differential changes in log employment between treated and control municipalities in the years leading up to the CCD (placebo test), which corroborates the validity of our research design. Municipality employment displays a modest and statistically insignificant increase in the first two years after the intervention. This variable exhibits a sharp increase three years after the intervention (short-run effect, $k = 3$) and peaks at 16.9% nine years after the CCD (long-run effect, $k = 9$). The third year after the intervention represents a turning point in the life of the municipality as it coincides with the end of the commissioners' mandate and with the appointment of a newly elected city council. We postpone the discussion on the timing of the effects to Section 6.b. Figure 3b shows that the logarithm of the number of firms follows a similar pattern and the number of firms increases by approximately 9% in the long run (although the effects become statistically significant starting from year 6).

Next, we investigate how the CCD impacts workers' earnings. We start by examining the municipality-level wage bill, which we define as the sum of wages paid to formally employed individuals in a given municipality. Interestingly, Figure 3c shows that the increase in overall employment does *not* map into an increase in the wage bill. This result is driven by the sharp reduction in average earnings documented in Figure 3d. Average earnings decrease by

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[14]Figures C.1a and C.1b in the Appendix show that the overall distribution of both employment and earnings are also well balanced between treated and counterfactual municipalities.

[15]Municipality employment is defined as the number of workers employed in all the establishments located in municipality m at time t.

approximately 3.7% in the first two years after the intervention, start plummeting in year 3, and decrease by 14.5% in the long run.

The evidence presented in Figure 3 shows that the removal of a city council infiltrated by the Mafia leads to a rise in formal employment, a smaller and more delayed increase in the number of firms, and a reduction in average earnings in treated municipalities.[16] These results should be interpreted in light of the fact that a large fraction of employment is informal in the south of Italy, where the vast majority of CCD events are concentrated (Di Porto et al., 2016). The increase in formal employment is likely to be at least partially driven by workers who were previously hired under the table obtaining a formal job. Moreover, the new formally employed workers may drive down observed average earnings if they are disproportionately employed in low-paying jobs.

To understand whether the CCD induces a shift from informal to formal employment, we examine how the dismissal of an allegedly corrupt local administration impacts the share of workers who enter social security records for the first time. Figures 4 reports the results (a subset of these coefficients are also reported in Table 3). Figure 4a shows that the share of "first worker appearances" displays a similar trend in treated and control municipalities in the years before the CCD and starts diverging in the third year after the event (albeit the divergence is not statistically significant until year five).[17] The timing of these effects aligns with that of municipality employment as depicted in Figure 3a. Overall, we document a long-run increase in the share of first worker appearances of 4.4 percentage points, which amounts to a 25% increase relative to its mean. We also study the evolution of the share of "previously non-employed individuals," defined as the fraction of workers who are employed in municipality m at time t but do not appear in the social security records at $t-1$.[18] Figure 4b shows that the share of previously not-employed workers displays no evidence of pre-trends in the years leading up to the event and starts diverging, once more, three years after the CCD. Nine years after the CCD,

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[16]These results are at odds with the work of Acconcia et al. (2014). These authors document that the CCD has a negative effect on province-level public investment and that this leads to a large negative impact on province-level GDP. This result seems implausible to us because treated municipalities are on average quite small and represent less than 3% of the population in treated provinces. We believe that the discrepancy between their results and our findings is primarily driven by differences in the design and data quality. We exploit a difference-in-difference design at the municipality- and firm-levels, while they implement an instrumental variable strategy at the province-level. Moreover, we have access to detailed administrative data on workers and firms and comprehensive data on municipality finances, while they rely on estimates of province-level GDP and only one category of public spending (i.e., investment in infrastructure).

[17]The share of first worker appearances is constructed as the number of workers who appear for the first time in the social security records in year t and municipality m over the employment level in the same municipality in the year before the CCD. Workers appear in social security records whenever they are formally employed in the private sector.

[18]This share is calculated relative to the employment level observed in a given municipality m in the year before the CCD intervention.

there is an increase of roughly 10 percentage points in the share of previously not-employed workers ($\approx 33\%$ of the average share in the sample).

Overall, Figure 4 documents that new workers enter the formal labor market in treated municipalities in the aftermath of the CCD. Next, we conduct a similar analysis focusing on firms rather than individuals. Column (3) of Table 3 shows that the share of newly–established firms increases in the long run.[19] Coincidentally with the increase in firm entries we also find evidence of an increase in the share of firm exits. In the long run, treated municipalities experience an increase in the share of firms exiting the labor market of around 4.5 percentage points, a 45% increase. As entry rates are higher than exit rates, this results in an increase in the overall number of firms operating in treated municipalities, as documented in Figure 3b.

Based on the evidence shown in Table 3, we draw two conclusions. First, although we are unable to observe individuals (or firms) transitioning from the informal to the formal sector, the large effects found on the share of new entrants, along with the fact that our analysis focuses on areas with a large prevalence of the informal sector, suggest that CCD events are to some extent successful at decreasing the level of economic informality and raising total formal employment. Second, CCD events are associated with a significant increase in the level of economic dynamism, which we measure as the share of firms entering and exiting local labor markets.

The positive effect of CCDs on economic formalism might be due to the specific activities carried out by external commissioners whose "mission" is being to able to restore a sense of trust running between the citizens and the local authorities. We postpone the discussion on whether the CCDs have any "real" economic effect or induce a mere reallocation of workers and firms from the informal to the formal sector to Section 6.a.

The magnitude of the estimated impacts of the CCD presented in this section might seem prima facie large; we argue that there are two main reasons for it. First, these effects represent a large percentage of changes from a very low baseline (Table 1). Second, the program we study is extremely aggressive and mostly takes place in economically stagnant areas characterized by the pervasive presence of organized crime.

We benchmark the size of our estimates with the work of Colonnelli and Prem (2020) who study the economic impact of a large anti-corruption program targeting local governments in Brazil. We focus on the number of firms because the authors choose this outcome as their main proxy for economic activity. As documented in Table 2, we find that the removal of an allegedly corrupt city council leads to a 9% increase in the number of firms 9 years after the intervention. Colonnelli and Prem (2020) show that the Brazilian program increases the number

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[19]We use the opening and closing dates collected by the Social Security Agency to construct the number of firms that enter and exit the market in each municipality in each period.

of firms operating in audited municipalities by approximately 2% over the same time horizon. Although these results are qualitatively similar, our point estimate is significantly larger and we argue that this is driven by the different nature of the two programs. Specifically, we study the economic impact of an aggressive anti-corruption program that removes city councils believed to be infiltrated by the Mafia. Colonnelli and Prem (2020) focus on a less intrusive program that randomly audits Brazilian municipalities; as a consequence, their treated municipalities are representative of the average Brazilian municipality and might or might not exhibit high levels of corruption.[20]

5.b   Effect of City Council Dismissal on Incumbent Firms

We now turn to the firm-level analysis. Our matched sample of firms is composed of incumbent treated and control firms. We define incumbent firms as those businesses that existed in the year prior to the CCD.

Figure 5 examines how employment and earnings of treated firms change relative to matched control firms after the removal of the city government (a subset of these coefficients are also reported in Table 4). Figure 5a documents that the CCD increases firm size, although the coefficient is smaller and less precisely estimated than the municipality-level effect (4%, 9 years post-intervention). The dynamic pattern mirrors the municipality-level analysis: the effects are statistically insignificant in the first few years after the intervention and start becoming larger in magnitude and statistically significant in the fourth year.

Next, we examine how the CCD affects the wage bill and the average earnings of workers employed at incumbent firms. Figure 5b shows that the CCD significantly increases the wage bill and that the effect peaks 8 years after the intervention (4%). This increase may be driven by a rise either in employment or in average worker earnings. Figure 5c shows that the main driver appears to be the former. Unlike what we find in the municipality-level analysis, the average worker earnings at incumbent firms are not affected by the CCD.

Finally, we examine whether CCD impacts firm composition; see Table 5. Remarkably, we do not find any detectable change in worker composition. This suggests that the increase in firm size documented in Figure 5a does not translate into changes in the type of workers incumbent firms employ as measured by the share of full time vs. part-time employees, the share of permanent vs. temporary contracts and occupation categories (i.e., managers, apprenticeships, white-collar, and blue-collar).

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[20]In other words, Colonnelli and Prem (2020) estimate an "intention-to-treat" effect, while we estimate a treatment on the treated. Relatedly, Avis et al. (2018) study the same program as Colonnelli and Prem (2020) and show that being audited increases the probability of legal action against the mayor (and hence her removal) by 1 percentage point (35.4 percent) in municipalities with a judiciary district.

The evidence presented in this section complements the municipality-level analysis and allows us to study the extent to which the changes documented in Section 5.a are driven by newly created businesses vs. incumbent firms. Employment at incumbent firms grows significantly (Figure 3a). Yet, the firm-level estimates account for less than one–fourth of the municipality-level effects (Figure 5a). This suggests that the aggregate effect is mainly due to a change in firm composition in treated municipalities.

Our firm- and municipality level estimated impacts of CCDs on average earnings also diverge. The firm-level analysis shows no effect of the CCD on average earnings (Figure 5a), while the municipality-level analysis shows a *negative* effect (Figure 3d). This implies that the decrease in average earnings at the municipality-level is driven by a change in firm composition. As Figure 3b documents a large and significant positive effect of the CCD on the number of firms, we conclude that the lion's share of the negative effect on municipality earnings should be attributed to the entry of new legal firms paying lower earnings.

Due to the high prevalence of informal labor markets, we suspect that some fraction of this negative effect may be driven by changes in "the extensive margin of informality" (Ulyssea, 2018). Specifically, it seems likely that the lower average municipality-level earnings may be due to previously informal firms becoming formal and appearing for the first time in the social security records.

6   Discussion

6.a   Are CCDs Leading to Higher Economic Growth?

The reduced form evidence presented in Section 5 points to large and significant effects of the CCD intervention: after the dismissal of the allegedly corrupt city council, treated municipalities experience an increase in formal employment and number of firms. Given the high prevalence of informality in southern Italy (where the events are disproportionately concentrated), one may wonder whether the CCDs have any "real" economic effect on the local economy or only induce a reallocation of both workers and firms from the informal to the formal sector.

Addressing this concern is hard as we lack a reliable municipality-level proxy for economic activity that incorporates both the formal and the informal sector. We try to circumvent this problem by using detailed data on real estate transactions collected by the Italian Treasury. Specifically, we interpret an increase in the prices of, say, industrial real estate, as a proxy for an increase in the level of real economic activity. Such an interpretation is motivated by the assumption that the supply of real estate, especially that related to industrial activities, is fixed and hence an increase in demand spurred by higher economic activity should result in

an increase in price. Crucially for our purposes, real estate prices incorporate demand coming from both the formal and the informal sector.

Figure 6 reports the estimated impact of the CCD on real estate prices. Results in table format are presented in Table 6. Figure 6a shows that industrial real estate prices increase after the CCD.[21] Importantly, the dynamic pattern of the event–study coefficients matches the one in formal employment (Figure 3a): the estimated impact of the policy becomes statistically significant three years after the CCD. Nine years after the intervention, there is an increase of roughly 15% in real estate prices reflecting an increase in the ability of treated municipalities to attract new business and firms. The removal of the city council is also associated with a surge in real estate prices of offices, although this effect fades away over time (Figure 6b).[22]

Italians are known for exhibiting low mobility and often living most of their life within a small radius from where they were born. Unsurprisingly, Figures 6c and 6d show that the CCD policy has no effect on house prices and municipality population.[23] We conclude that CCDs increase formal employment without attracting new residents. If treated municipalities are unable to attract new residents, where are the newly employed workers coming from? Figure C.2 in Online Appendix C shows that less than one–third of the increase in formal employment presented in Figure 3a is explained by an increase in the number of workers living in treated municipalities. These results are consistent with treated municipalities drawing workers from surrounding towns in the aftermath of the CCD (spillover effects).

We also examine the impact of CCDs on real estate rents and we find qualitatively similar results, although the point estimates tend to be larger and somewhat noisier (Table D.8 in Online Appendix D).

In conclusion, we document a large and significant increase in the price of industrial real estate that is consistent with higher levels of economic activity in treated municipalities. We conclude that the removal of the allegedly corrupt local city council generates some "real" economic growth and the reduced–form evidence on formal employment and number of firms depicted in Section 5.a does not represent a mere reallocation from the informal to the formal sector.

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[21]Industrial real estate includes factories, industrial buildings, and craft workshops.

[22]These patterns are consistent with the fact that it is harder to increase the stock of industrial real estate compared to offices.

[23]Population is collected from the Italian registry (*anagrafe*) and it is not subject to measurement error associated with informal labor markets. All citizens are enrolled in the registry (*anagrafe*) at birth and remain registered until death. Immigrants are also registered as long as they live in the country. The Ministry of the Interior collects the data from the registry and distributes them in the form of municipality-year aggregates.

6.b   Potential Mechanisms

Next, we try to shed some light on *how* the dismissal of the city council is able to generate positive long-run effects on formal employment and overall economic activity. We investigate three potential channels: (i) changes in how the local administration handles and invests local public finances, (ii) changes in public procurement contracts, and (iii) changes in the quality of newly elected politicians.

6.b.1   Local Public Finances: Revenues

We start the analysis of public finances by examining the impact of the CCD on municipality revenues. Results are presented in Figure 7 and Table 7. Figure 7a displays the effect of the CCD intervention on the logarithm of total revenues. We detect an immediate drop of roughly 7% on impact and thereafter the point estimates remain negative albeit somewhat imprecise.

Next, we analyze the components of total revenues and investigate whether the CCD induces a shift in their relative importance. Figure 7b focuses on the "tax share" (which we define as the revenues generated from local taxes over total municipality revenues). This figure shows a clear and significant *positive* on-impact effect of the CCD on the tax share of 3–4 percentage points (which amounts to roughly a 11–15% increase).[24] The fact that these effects are visible from the onset of the intervention is consistent with the external commissioners focusing on tax collection and law enforcement. Interestingly, our effect does not fade away after years 2 and 3 when the external commissioners leave and newly elected politicians take their place. This suggests that the newly elected city council provides continuity to the actions of the external commissioners with respect to tax collection, a point that we discuss further in Section 6.b.4. The effects persist in later years although the statistical precision somewhat deteriorates, making the resulting estimates less precise.

Figure 7 shows that, while the tax share is increasing following the city council dismissal, the overall budget appears to decrease, especially in the first years following the intervention. This suggests that CCDs may induce a shift in the relative importance of different sources of revenues. We examine this phenomenon in Table 7. We find that the increase in the tax share is offset by a decrease in the share of the budget coming from loans (column (3)). This effect materializes with the arrival of the external commissioners, persists after the election of the new city council, but fades away in the long run.

The remaining two balance sheet items display a less systematic pattern and are more imprecisely estimated. There is some evidence that proceeds from other revenues (which typically come from selling municipal real estate and properties) were frozen upon the arrival of the

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[24]Taxes account for around 27% of the total budget for an average municipality in our data.

external commissioners, perhaps out of fear that those proceeds might favor local crime organizations (column (4)). This drop in total revenues appears relatively large on impact and explains the lion's share of the decrease in the total budget observed in the first year after the intervention. However, this effect appears to be only temporary and the coefficients change sign in the long run.

A longstanding question is whether the increases in the tax share shown in Figure 7b is primarily due to a decrease in total revenues (i.e., a decrease in the denominator) or an increase in the amount of money that the municipality collects through local taxes (i.e., an increase in the numerator). Appendix Figure C.3a shows the effects of the CCD on total taxes collected normalized relative to the budget in the year before the event. This exercise "fixes the denominator" and isolates the changes in the tax share coming from the numerator. We detect a positive effect of CCDs on the rescaled version of total taxes collected. As the dynamic pattern of the coefficient appears similar to the one in Figure 7b, we believe that the effects shown in Figure 7b are mostly driven by changes in the numerator.

In conclusion, the evidence presented here suggest that, after the city council dismissal, municipalities raise revenues from local taxes while they experience a reduction in revenues coming from loans. The structural changes on the tax front are initiated by the external commissioners and continued by the newly elected city council. The fact that these effects are visible from the onset of the intervention suggests that these effects are not driven by higher local economic activities as the latter effects tend to materialize only 3-4 years post-intervention. Moreover, current legislation typically prevents external commissioners to raise local taxes.

Based on these two facts, we conclude that the positive effects on local taxes are driven by better tax enforcement strategies initiated by the external commissioners. Our results are consistent with the work of Di Cataldo and Mastrorocco (2020) who show that corrupt local governments impact the allocation of public resources and local tax collection.

6.b.2 Local Public Finances: Expenditures

Having established how the different sources of revenues changed after the CCD, we now turn to municipality expenditure. Specifically, for each euro of revenues generated by a municipality, we investigate what share is spent by the administration and on which items (police, education, roads and infrastructure, garbage collection, etc.).

We start by looking at total expenditures relative to total revenues.[25] Figure C.4, Panel (a) shows that the "expenditure share" (i.e., total expenditures per revenues) starts increasing right after the arrival of the external commissioners, although the estimates are quite imprecise.

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[25]On average, municipalities in our data spend 83 cents for every euro collected.

Once the new city council is elected, which typically coincides with year 3, there is a statistically significant increase in public spending. Specifically, it increases by approximately 5 percentage points, accounting for an increase of roughly 7%. These effects persist in the first two years after the newly elected city council takes office, remain positive, but become increasingly less precise over time.

What explains these changes in total expenditure? Figure C.4, Panel (b)–(d) as well as Table D.9 address this question by examining changes in expenditures on several budget items. Each dependent variable is defined as expenditure for item x over total revenues. We find that administrative expenses increase substantially during the commissioners' mandate; see Figure C.4, Panel (b). This is consistent with the qualitative evidence discussed in Section 2 suggesting that the commissioners reorganize municipality offices, train bureaucrats, and hire temporary workers. We speculate that part of these expenditures may be related to the positive effects on the tax share shown in Figure 7b and that the external commissioners may invest in local tax enforcement.

The increase in the overall expenditure share observed with the arrival of the new city council (year 3 post-event) appears to be driven by three key items: expenditures on roads and infrastructure, sanitation (primarily garbage collection), and local administration. Interestingly, while increases in the share spent on these last two items tend to decrease over time both in economical and statistical significance, the share spent on roads and infrastructure remains relatively high in the long run, see Figure C.4, Panel (c), suggesting a *permanent* change in the amount of money devoted to infrastructure, a key margin of intervention for the local government to foster economic growth (Freeman, 2007, Donaldson, 2018). An important limitation of this analysis is that our estimates on each individual budget item are quite noisy due to the narrower time window and instrinsic year-to-year variability of budget items.

The dynamic effects on the remaining budget items are fairly noisy and do not exhibit a systematic pattern, see the remaining columns of Table D.9. Expenditures on the police and the justice system do not appear to change in response to the city council dismissal. This is not surprising as the police and the justice system are virtually entirely funded by general taxation and not by municipality revenues.[26]

6.b.3   Public Procurement

Next, we examine whether changes in spending on public procurement contribute to the economic growth we documented in Section 5. We focus on four dimensions of public procurement:

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[26]Municipalities are responsible for funding the traffic police (*polizia municipale*), who mostly regulate traffic and issue parking tickets, and for paying the utility bills of local courts.

number of tenders, reserve price, number of winners, and procurement costs.[27] We standardize these outcomes by total municipality revenues as we did in Section 6.b.1. We estimate the difference-in-differences model specified in equation (1) and report the results in Table D.10. On impact, there is a negative but not statistically significant decrease in the number of tenders, reserve price, number of winners, and public procurement costs (columns (1), (2), (3), and (4)). These results are consistent with the anecdotal evidence suggesting that at the beginning of their mandate the external commissioners inspect current projects and municipality finances and that this initial review slows down public investment and new procurement contracts. The estimates for the standardized number of tenders and number of winners turn positive in the short term and increase in magnitude in the long term, while the reserve price remains negative. All these effects remain economically small and not statistically significant. Taking the estimates at face value, we find that the CCD appears to have a large (but extremely imprecisely estimated) positive impact on the cost of public procurement. Given the large standard errors and the fact that the cost variable is available only after 2008, we prefer to interpret these effects conservatively and conclude that the CCDs do not have detectable impacts on the long-term resources allocated to public procurement.

6.b.4   Quality of Local Politicians

The vast majority of our estimated treatment effects (on formal employment, number of firms, expenditure margin, share of new entrants, industrial real prices) follow a similar pattern: they are essentially flat until year 3 post-intervention and then start growing significantly. Given the importance of year 3 in our institutional context, we believe that this is not mere coincidence and that these findings suggest that the first election of a city council after the removal of an allegedly corrupt one is able to induce fundamental and persistent changes in the local economy.

But how can these effects materialize? A candidate explanation is that changes in the quality of local politicians may be driving the positive long-run effects we document in Section 5. This is motivated by prior work by Bó et al. (2006) who argue that the pervasive presence of "bribe-punishment" organizations (such as the Italian Mafia) tend to go hand in hand with low-quality local politicians. A (dramatic) event such as the removal of the entire city council due to allegations of infiltration by the Mafia and the consequent appointment of external commissioners from the central government can "open the eyes" of the local population to the issue of corruption and ultimately result in an increase in the demand of higher-quality local politicians. Empirical evidence of this mechanism is found in Daniele and Geys (2015b)

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[27]When municipalities announce a tender, they specify the maximal reserve price above which they are unwilling to pay for the contract. Firms bid below the reserve price and the municipality ends up paying the cost associated with the winning bid. Firms can create a consortium and submit a joint bid. See Decarolis et al. (2020) for a thorough discussion of public procurement in the context of Italian municipalities.

who report that after being exposed to a CCD the voting response of the local population is characterized by: (i) a significant reduction in the probability of electing incumbents and purely local parties and (ii) an increase in the human capital of the newly elected politicians.

Interestingly, the arrival of these higher–quality politicians leads to significant changes in the handling of local public finances, as documented in Sections 6.b.1 and 6.b.2. Hence, we complement the key findings of Daniele and Geys (2015b) in two important ways. First, we provide evidence on how local budgets tend to change with the appointment of higher–quality politicians (better tax enforcement, higher expenditure on roads and infrastructure). Second, we show how the chain of events induced by the removal of an allegedly corrupted city council (arrival of external public servants, election of higher–quality politicians, change in the local budget) eventually maps into higher and persistent economic growth.

All in all, we believe that these findings help paint a more complete picture of why anti-corruption programs, such as the one analyzed in this paper, are able to generate potentially very large economic effects.

7 Conclusion

Despite the prominent role that the fight against organized crime has in the political agenda of both developed and developing countries, little is known about how to effectively fight criminal organizations and what the long-term consequences of these actions are.

This paper attempts to fill the gap by estimating the long-run economic impact of one of the most aggressive policies aimed at combating organized crime in Italy: the city council dismissal. This policy represents a unique type of place-based policy where the central government replaces the elected public officials of Mafia-infiltrated municipalities with a team of experts who run the city for about two years. This policy generates sharp variation in the "quality" of local institutions in a given municipality and has the potential to sever the connection between the city government and local organized crime.

We find that this policy fosters economic growth. Specifically, the city council dismissal increases formal employment by 16.9% nine years after the dismissal and this effect appears to be partially driven by a decrease in informality in the labor market. Treated municipalities also display higher economic dynamism and a surge in industrial real estate prices in the aftermath of the intervention. We interpret the surge in the prices of business properties as further evidence that CCDs spur local economic development and that our reduced-form evidence on formal employment and number of firms is not entirely driven by a mere reallocation of workers and firms from the informal to the formal sector. These effects appear to be mediated by an increase in the quality of local politicians elected after the city council dismissal, an increase in

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the share of public investment dedicated to infrastructure, and an increase in tax compliance.

These findings document that a simple yet effective policy has the potential to increase formality and generate long-run economic growth even in places that are traditionally considered Mafia strongholds. Our results imply that there are large long-run economic benefits associated with targeted law enforcement actions against criminal organizations.

Policymakers who may be considering whether to implement this policy in other settings should be aware that substituting elected public officials with trusted civil servants is unlikely to generate large economic growth on its own. Key to the success of this policy is the commitment of the politicians elected in the aftermath of the intervention to carry on the work initiated by the commissioners and invest in the territory. Another important aspect that we believe contributes to our effects is the length of the commissioners' mandate. Although our design does not allow us to evaluate how effective this policy would have been if their mandate had been shorter, many commentators argue that it is extremely challenging to fundamentally change local institutions over a short period (Mete, 2009). Anecdotally, whenever the government initially appointed the commissioners for only one year, this time horizon was not deemed sufficient and the government extended the commissioners' mandate for an additional year.

Despite these caveats, our analysis provides a first very important step toward understanding what policies are effective at combating organized crime and evaluating their long-term economic consequences.

We intended to perform additional analyses to dig deeper into informalities and corroborate our proposed mechanism. Due to coronavirus, however, the Social Security Archives (where one of us has access to the social security confidential data) were shut down in March and have subsequently opened at a limited capacity to comply with COVID-19 safety protocols. We hope that as international travel resumes in the next few months so that we will be able to go back to the Social Security Archives in Italy and conduct these additional analyses that we believe will strengthen our paper.

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8 Figures

Figure 1: Time Variation in CCDs

City Council Dismissals due to Mafia Infiltration
1991−2016
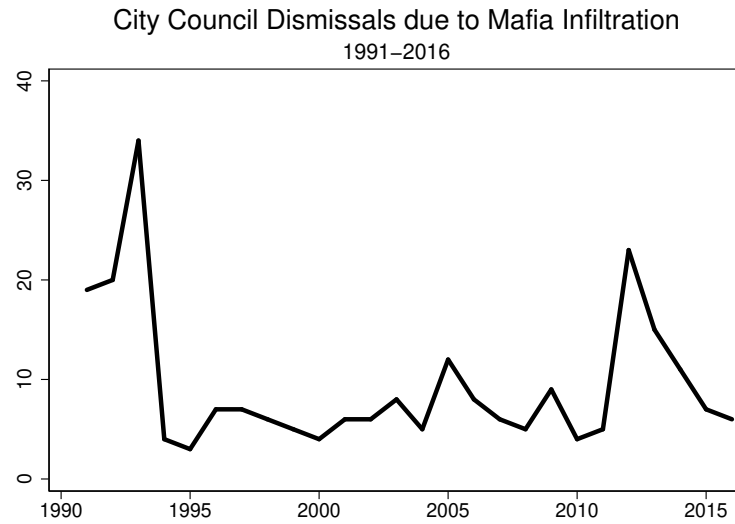
Notes: This figure summarizes the time variation in the use of city council dismissals due to Mafia infiltration between 1991 and 2016.

Figure 2: Spatial Variation in CCDs
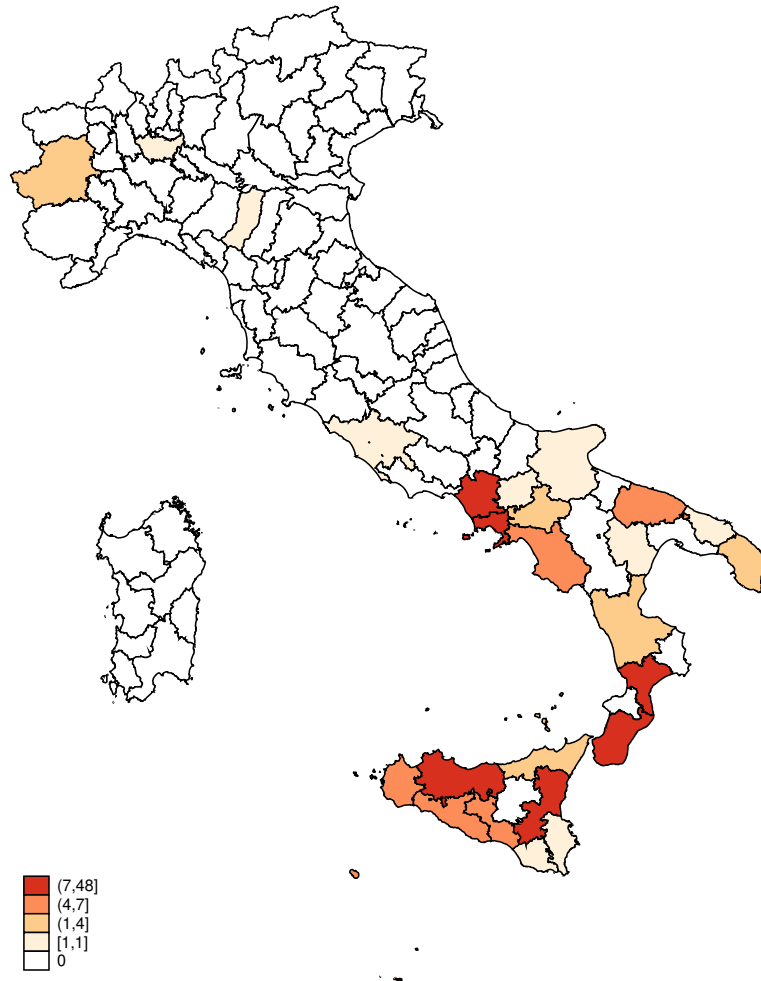
Notes: This figure summarizes the spatial variation in the use of city council dismissals due to Mafia infiltration between 1991 and 2016. The map shows the counts of CCDs for each of the 110 Italian provinces. Twenty-five provinces experienced at least one CCD. Darker shaded provinces experienced more CCDs than lighter shaded ones.

Figure 3: Estimated Effects of CCD on Employment, Firms, and Earnings

(a)

Log(N Empl)

(b)

Log(N Firms)

(c)

Log(Wage Bill)

(d)

Log(Av. Earnings)
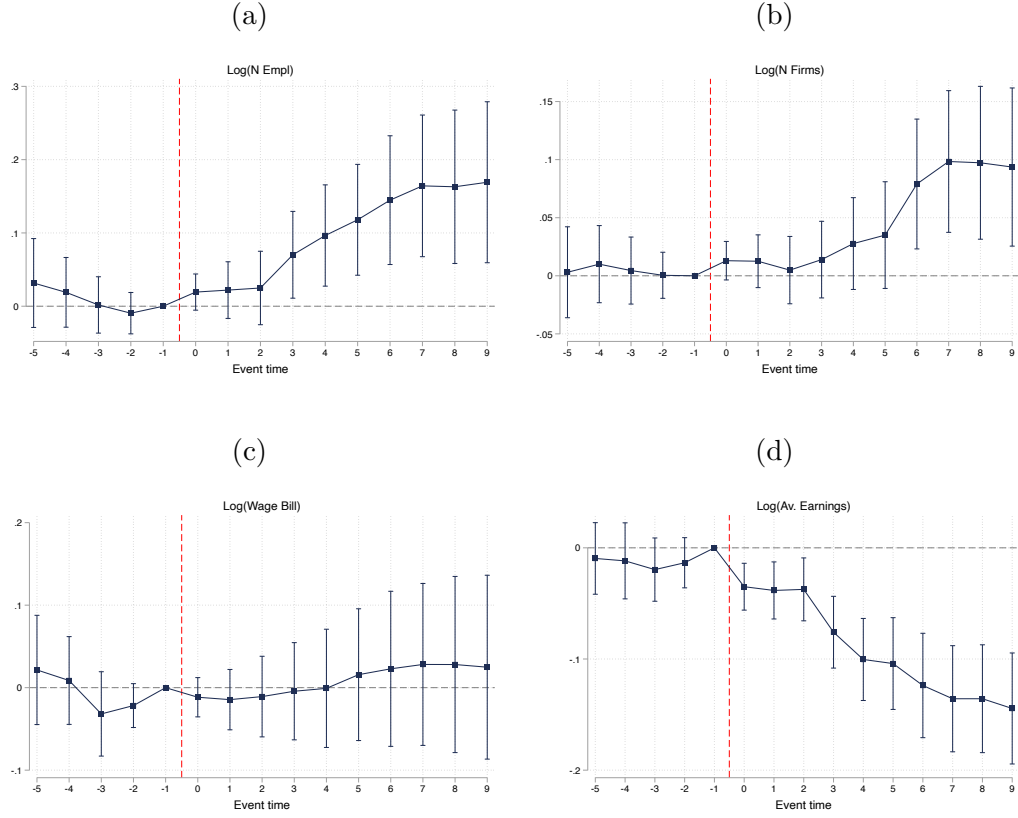
Notes: Matched municipality sample, INPS data (1983–2017). Panels (a)–(d) display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level log employment, log number of firms, log wage bill, and log average earnings. The x-axis indexes event time. Results in Table format are reported in Table 2.

Figure 4: Estimated Effects of CCD on the Share of New Entrants and Share of Previously not Employed

(a)

(b)
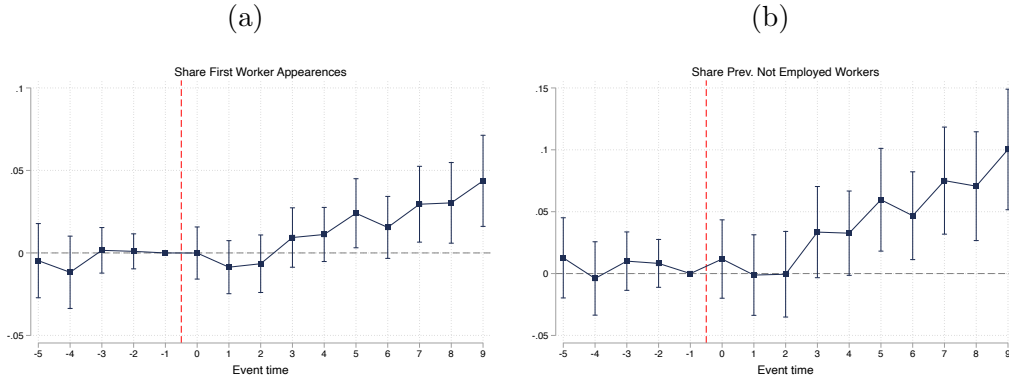
Notes: Matched municipality sample, INPS data (1983–2017). Panels (a)–(b) display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variable in Panel (a) is defined as the number of workers who appear for the first time in the social security records in year t and municipality m over the employment level in the same municipality in the year before the CCD. The outcome variable in Panel (b) is constructed as the number of workers who are employed in municipality m at time t but do not appear in the social security records at $t-1$ over the employment level in the same municipality in the year before the CCD. Results in table format are reported in Table 3.

Figure 5: Estimated Effects of CCD on Firm Size, Wage Bill, and Average Earnings

(a)

(b)

(c)

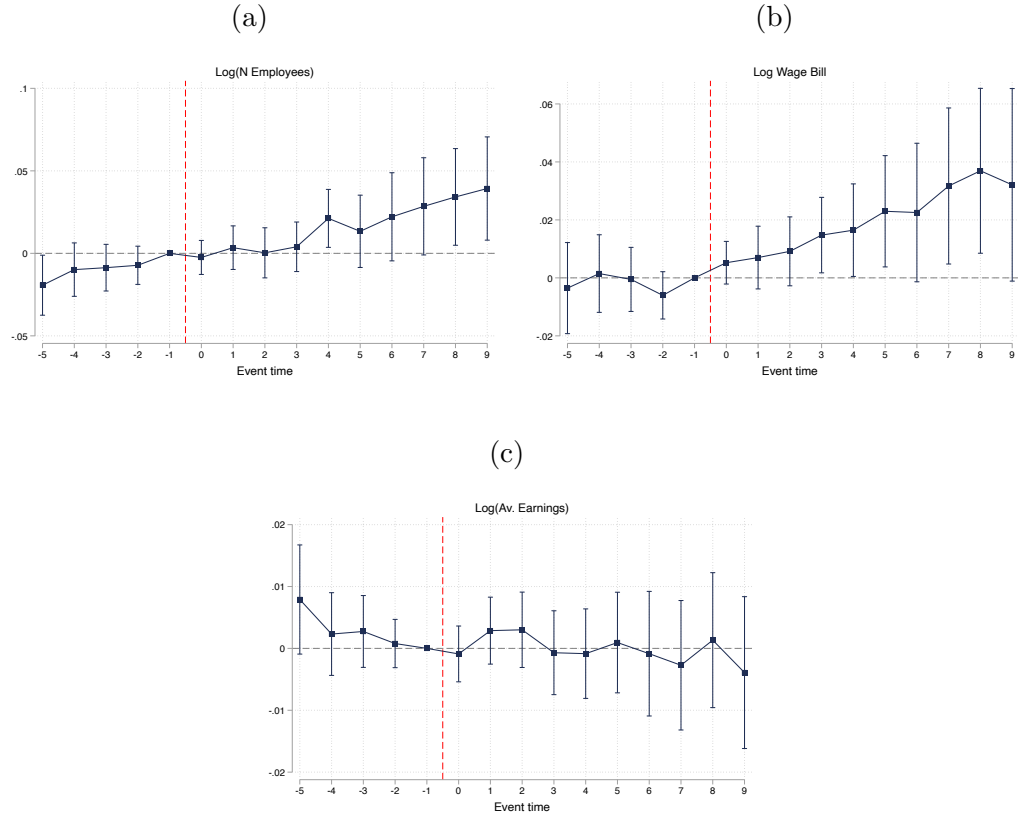*Notes*: Matched firm sample, INPS data (1983–2017). Panels (a)–(c) display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\beta}^k$ from equation (2). The coefficients at $k = -1$ are normalized to zero. The outcome variables are firm-level log employment, log wage bill, and log average earnings of the employees within a given firm. The x-axis indexes event time. Results in table format are reported in Table 4.

Figure 6: Estimated Effects of CCD on Municipality Population and Real Estate Prices

(a)

(b)

(c)

(d)
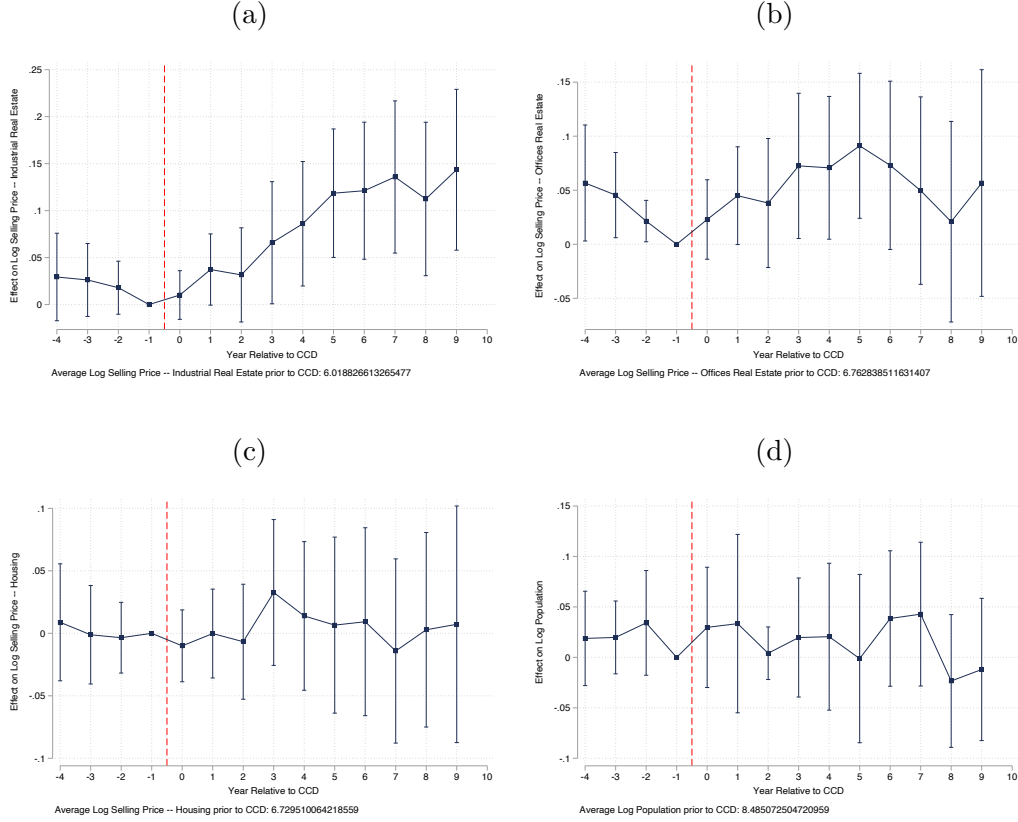
Notes: Matched municipality sample, Treasury data (2002–2015) in Panels (a)–(c) and Ministry of the Interior data (1989–2015) in Panel (d). Panels (a)–(d) display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level population, house prices, industrial real estate prices, and office real estate prices. The x-axis indexes event time. Results in table format are reported in Table 6.

Figure 7: Estimated Effects of CCD on Local Public Finances

(a) Log Total Revenues

(b) Taxes Collected / Total Revenues
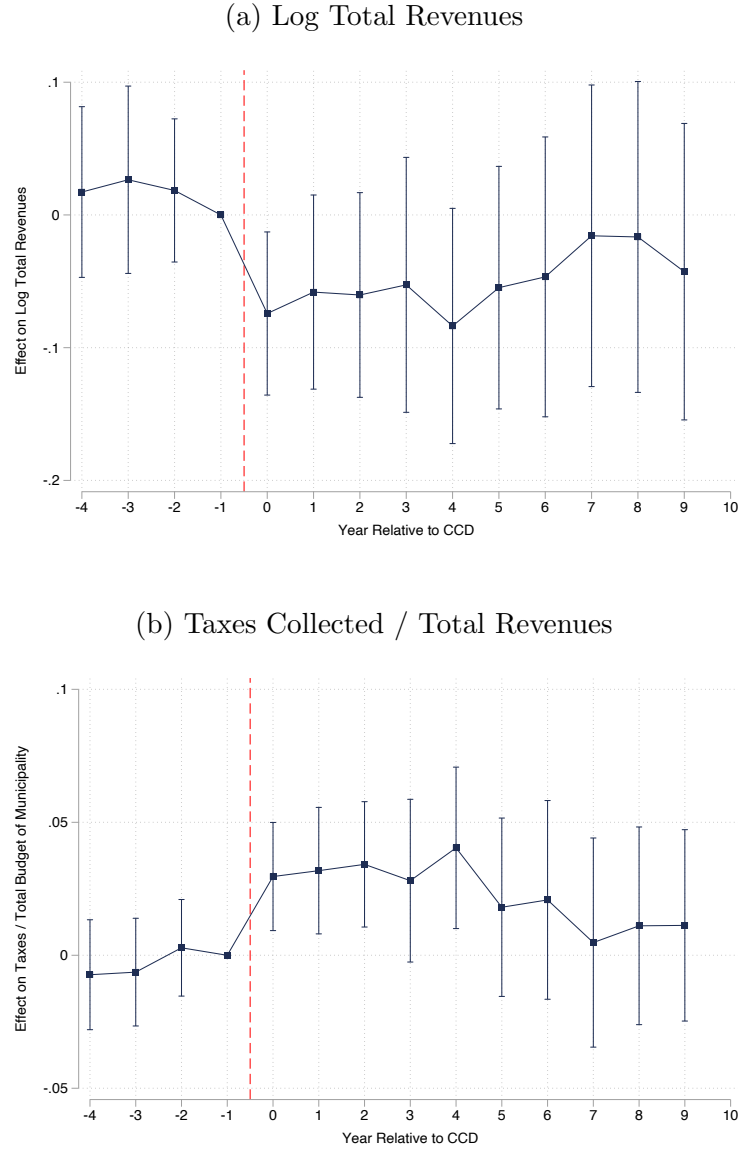
Notes: Panels (a) and (b) display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variables are the logarithm of total revenues collected by the municipality (panel a) and the tax-margin, which is defined as total taxes collected by the municipality over total revenues (panel b). See Appendix B for details. The x-axis indexes event time. Results in table format are reported in Table 7.

9 Tables

Table 1: Municipality Characteristics

	(1)	(2)	(3)
	Matched Sample	T	C
1991 Population	15281.20	15558.17	15004.95
N of Firms	247.82	228.29	267.30
N of Establishments	257.94	237.79	278.04
N of Employees	1656.96	1135.49	2177.09
N of Sole Proprietorships	128.25	115.58	140.89
N of Firm Entries	29.21	29.44	28.97
N of Firm Exits	22.61	22.51	22.71
N of First Worker Appearances	255.00	209.00	301.02
N of Prev. Not Employed Workers	412.61	351.67	473.59
N of Managers	11.82	3.82	19.80
N of White Collar Workers	596.48	311.26	880.97
N of Blue Collar Workers	1029.17	825.33	1232.48
N of Apprentices	57.74	34.58	80.85
N of Other Workers.	1.87	1.21	2.52
N of Part-Time Workers	430.07	351.04	508.90
N of Full-Time Workers	1878.65	1342.65	2413.27
Wage Bill (Thousands)	30400	17600	43200
Av. Earnings	8057.41	7628.04	8485.69
N	14751	7366	7385

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. All statistics are calculated across municipality-year observations. Column (1) reports statistics on the full matched sample, and columns (2) and (3) limit the sample to treated and control municipalities, respectively. The number of observations for the number of first worker appearances and for the number of previously not employed workers are 14728, 7366, and 7362, respectively.

Table 2: Estimated Effects of CCD on Municipality Employment, Earnings, and Firms

	(1) Log(Empl)	(2) Log(N Firms)	(3) Log(Wage Bill)	(4) Log(Av. Earnings)
On Impact	0.019	0.013	-0.012	-0.035
	(0.013)	(0.008)	(0.012)	(0.011)
Short-Run	0.070	0.014	-0.004	-0.076
	(0.030)	(0.017)	(0.030)	(0.016)
Long-Run	0.169	0.094	0.025	-0.145
	(0.056)	(0.035)	(0.057)	(0.025)
Mean	6.196	4.379	15.073	8.873
R-squared	0.963	0.982	0.970	0.892
Adj. R-squared	0.961	0.981	0.968	0.887
N	14,654	14,654	14,654	14,654
Muni FE	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD. Results in graph format are reported in Figure 3.

Table 3: Estimated Effects of CCD on Entries and Exits

	(1) Share First Appearances	(2) Share Prev. Not Employed	(3) Share Firm Entries	(4) Share Firm Exits
On Impact	0.000	0.012	0.006	0.006
	(0.008)	(0.016)	(0.007)	(0.006)
Short-Run	0.009	0.033	0.011	0.006
	(0.009)	(0.019)	(0.010)	(0.008)
Long-Run	0.044	0.100	0.061	0.043
	(0.014)	(0.025)	(0.016)	(0.014)
Mean	0.176	0.303	0.150	0.121
R-squared	0.596	0.620	0.687	0.673
Adj. R-squared	0.575	0.600	0.670	0.656
N	14,631	14,631	14,654	14,654
Muni FE	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Table 4: Estimated Effects of CCD on Firm Employment, Wage Bill, and Worker Earnings

	(1) Log(Empl)	(2) Log(Wage Bill)	(3) Log(Av. Earnings)
On Impact	-0.002	0.005	-0.001
	(0.005)	(0.004)	(0.002)
Short-Run	0.004	0.015	-0.001
	(0.008)	(0.007)	(0.003)
Long-Run	0.039	0.032	-0.004
	(0.016)	(0.017)	(0.006)
Mean	0.849	7.753	6.832
N	1,373,545	1,372,986	1,373,545
Firm FE	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes

Notes: Matched firm sample, INPS data (1983–2017). Treated firms are matched to potential control firms in the same region and with the same juridical status. This table reports the estimated β_k coefficients from (2). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Results in graph format are reported in Figure 5.

Table 5: Estimated Effects of CCD on Firm Composition

	(1) Share Part-Time	(2) Share Full-Time	(3) Share Managers	(4) Share White Collar	(5) Share Blue Collar	(6) Share Apprentices
On Impact	-0.0010	0.0008	-0.0001	0.0009	-0.0008	0.0021
	(0.0020)	(0.0017)	(0.0001)	(0.0013)	(0.0019)	(0.0010)
Short-Run	-0.0005	-0.0002	0.0002	0.0033	-0.0022	0.0017
	(0.0040)	(0.0040)	(0.0001)	(0.0017)	(0.0031)	(0.0019)
Long-Run	-0.0007	0.0007	0.0001	0.0031	-0.0034	-0.0018
	(0.0041)	(0.0040)	(0.0002)	(0.0032)	(0.0056)	(0.0027)
Mean	0.082	0.523	0.0000	0.225	0.516	0.045
N	1,373,545	1,373,545	1,373,545	1,373,545	1,373,545	1,373,545
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Region-by-Time	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Matched firm sample, INPS data (1983–2017). Treated firms are matched to potential control firms in the same region and with the same juridical status. This table reports the estimated β_k coefficients from (2). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level.

Table 6: Estimated Effects of CCD on Municipality Population and Real Estate Prices

	(1) Population	(2) Housing Prices	(3) Industrial Real Estate Prices	(4) Office Real Estate Prices
On Impact	0.0297	-0.0100	0.0102	0.0229
	(0.0304)	(0.0147)	(0.0132)	(0.0187)
Short-Run	0.0196	0.0327	0.0658	0.0725
	(0.0301)	(0.0298)	(0.0332)	(0.0343)
Long-Run	-0.0120	0.0072	0.1435	0.0567
	(0.0360)	(0.0483)	(0.0437)	(0.0535)
Mean	8.903	6.606	6.010	6.71
N	7462	2860	2474	2453
Muni FE	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, Ministry of the Interior data (1989–2015) in column (1) and Treasury data (2002–2015) in columns (2)–(4). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD. Results in graph format are reported in Figure 6.

Table 7: Estimated Effects of CCD on Municipality Revenues

	(1) Log Total Revenues	(2) Taxes / Tot. Rev.	(3) Transfers / Tot. Rev.	(4) Loans / Tot. Rev.	(5) Other Rev. / Tot. Rev.
On Impact	-0.0743	0.0296	0.0193	-0.0180	-0.0306
	(0.0314)	(0.0104)	(0.0077)	(0.0110)	(0.0136)
Short-Run	-0.0527	0.0281	-0.0018	-0.0497	0.0306
	(0.0490)	(0.0156)	(0.0112)	(0.0163)	(0.0213)
Long-Run	-0.0428	0.0112	-0.0235	-0.0064	0.0269
	(0.0570)	(0.0184)	(0.0192)	(0.0257)	(0.0264)
Mean	15.906	.277	.261	.093	.371
N	4457	4457	4457	4457	4457
Muni FE	Yes	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, Minister of the Interior data (1998–2015). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Online Appendix

Appendix A : Institutional Background

In this section, we provide a brief overview of the political institutions of Italian municipalities and further institutional details about the CCD and other policies aimed at fighting organized crime.

Local Politicians in Italian Municipalities

Italian cities are administered by the mayor (*sindaco*), the city council (*consiglio comunale*), and the executive committee (*giunta comunale*). The city council and the mayor are elected for five years and the latter can serve for at most two consecutive terms. The city council is the legislative body and is in charge of the municipality's financial statements, expenditure allocation, urban planning, and investment in infrastructure. The number of city council members (*consiglieri comunali*) is a function of population size and ranges from a minimum of 6 to a maximum of 64. The executive committee is appointed by the mayor and it is made up of 2 to 12 executive officers (*assessori comunali*). The executive committee is the body that together with the mayor effectively manages the city. The mayor sits both on the city council and on the executive committee.

## More Details on City Council Dismissals

As we discussed in Section 2, the CCD aims at severing the ties between the local government and organized crime by removing the allegedly corrupt politicians. This policy does not typically affect municipality bureaucrats. However, if a municipality bureaucrat appears to be connected to the Mafia, the representative of the Ministry of the Interior in the province *(prefetto)* is required to inform law enforcement authorities and can suspend the allegedly corrupt bureaucrat or move her to another office during the police investigation.

Next, we discuss mandate length. The external commissioners inherit the powers of the dismissed administrative and executive bodies and run the municipality for a period of two to three years. In a few cases, the commissioners were initially appointed for 12 months but in all these instances their powers were extended to two years. Figure A.1 displays the distribution of the commissioners' mandate length constructed using the register of local politicians. The effective mandate length ranges from 9 months to approximately three years. The reason why this figure displays a nonnegligible number of mandates shorter than two years is that it is not uncommon for commissioners to leave their office before the end of their mandate (Mete, 2009).

1

When this happens, the central government appoints a substitute.

Finally, the decision of the Ministry of the Interior to dismiss a city council can be challenged in court. We exclude from our sample the 19 municipalities for which the decision to dismiss the city council was later overruled (*decisioni annullate*).

Figure A.1: External Commissioners' Mandate Length
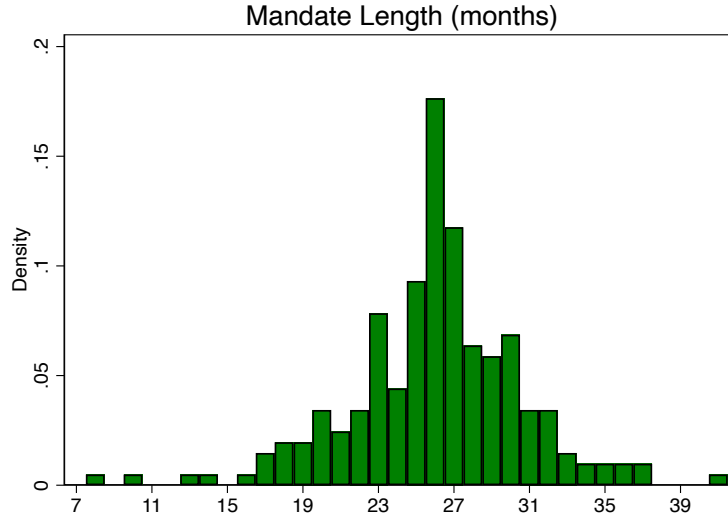
Notes: Register of local politicians, Ministry of the Interior (1991–2016).

Other Policies Aimed at Fighting Organized Crime

D.L. 31/05/1991 n. 164 is not the only law that targets Mafia-like organizations. Other measures include Law 09/13/1982 n. 646, which makes the creation of a Mafia organization a crime, and Article 41 of the Prison Administration Act, which establishes a stricter prison regime for mafiosi in an attempt to deter Mafia affiliates from associating with their former criminal associates.

Appendix B : Variable Definition

In this section, we define the variables we use in the analysis and provide further details about the institutional background related to these variables.

Average earnings (municipality-level): the average earnings paid to formal private sector workers employed in municipality m in year t. The average earnings of workers employed at

incumbent firms (firm-level average earnings) is constructed analogously.

Cost of public procurement (municipality-level): the total cost associated with public procurement contracts awarded in municipality m in year t. This variable is available starting from 2008.

Employment (municipality-level): the number of workers employed in the private sector in municipality m in year t. Our employment variable does not include informal workers and public sector employees. The number of workers employed at incumbent firms (firm-level employment) is constructed analogously.

Expenditure items

– Administration: expenditures on the day-to-day administration of the local goverment.

– Justice: expenditure related to the justice system. The justice system is funded by the central government. Municipalities are responsible only for the utilities (e.g., electricity, heating, etc.) of local courts and the offices associated with them.

– Police: expenditure related to local law enforcement and public order services. Law enforcement is funded by the central government. Municipalities are responsible for the traffic police (*polizia municipale*), tasked with regulating traffic and giving parking tickets.

– Education: expenditure related to education (of all grades) and school construction. Education is financed by the central government and municipalities are responsible only for a relatively small subset of services.

– Culture: expenditure related to cultural initiatives and the enhancement of cultural assets.

– Sport: expenditure related to local sports facilities and initiatives.

– Tourism: expenditure related to the promotion of tourism and the enhancement of the territory .

– Roads and Infrastructure: expenditure on local public transportation and other infrastructures.

– Environment: expenditure on garbage collection, local landscape maintenance, and pollution monitoring and reduction.

– Other Expenditures: Other expenditures of the municipality. These include, for example, expenditures on social assistance and on local economic development.

Loans: revenues generated from loans contracted by the municipality.

Number of firms (municipality-level): number of firms operating in municipality m in year t. Our data allows us to distinguish between firms and establishments but as the vast majority of firms have only one establishment we focus on firms in our empirical analysis.

Number of public procurement winners (municipality-level): number of firms winning a public tender in municipality m in year t. Firms can create a consortium and submit a joint bid. If the consortium wins the tender, all the consortium members are counted as winners. See Decarolis et al. (2020) for a thorough discussion of public procurement in the context of Italian municipalities.

Number of public tenders (municipality-level): number of public tenders in municipality m in year t. See Decarolis et al. (2020) for a thorough discussion of public procurement in the context of Italian municipalities.

Other revenues: other revenues of the municipality. These include, for example, revenues from fines, from administrative penalties, and from insurance compensations, as well as revenues obtained from selling municipal real estate and properties or from providing local services.

Population (municipality-level): number of residents of municipality m in year t. This information is collected from the Italian registry (*anagrafe*) and is not subject to measurement error associated with informal labor markets. All citizens are enrolled in the registry at birth and remain registered until death. Immigrants are also registered as long as they live in the country.

Real estate prices/rents (municipality-level): average real estate selling price/rents in municipality m in year t. The Treasury collects these averages separately for three types of properties: residential housing, industrial real estate, and offices. Industrial real estate includes factories, industrial buildings, and craft workshops.

Reserve price (municipality-level): sum of the reserve prices of all tenders in municipality m in year t. When municipalities announce a tender, they specify the maximal reserve price above which they are unwilling to pay for the contract. Firms bid below the reserve price and the municipality ends up paying the cost associated with the winning bid (Decarolis et al., 2020).

Share of first worker appearances (municipality-level): the number of workers who appear for the first time in the social security records in year t and municipality m over the employment level in the same municipality in the year before the CCD. Workers appear in social security records whenever they are formally employed in the private sector.

Share of closed businesses (municipality-level): number of businesses that shut down in municipality m in year t over the number of businesses operating in municipality m in the year before the CCD.

Share of newly established businesses (municipality-level): number of businesses that register at the Social Security Agency in municipality m in year t over the number of businesses operating in municipality m in the year before the CCD.

Share of previously non-employed individuals (municipality-level): the fraction of workers who are employed in municipality m at time t but do not appear in the social security records at $t-1$ relative to the employment level in the year before the CCD.

Taxes: local taxes collected by the municipality.

Transfers: transfers from the central government, the region where the municipality is located, and other public agencies (e.g., INPS).

Wage bill (municipality-level): the sum of all wages paid to formal private sector workers employed in municipality m in year t. The wage bill of workers employed at incumbent firms is constructed analogously.

Appendix C   : Additional Figures

Figure C.1: Distribution of Log Wages and Log Size at *t-1*

(a)

Trimmed at the 99th percentile.

(b)
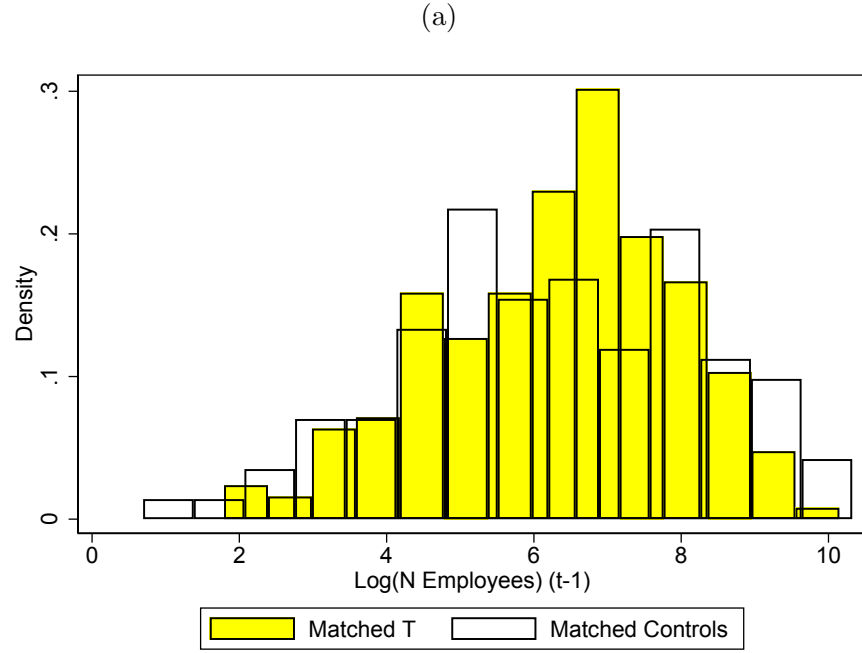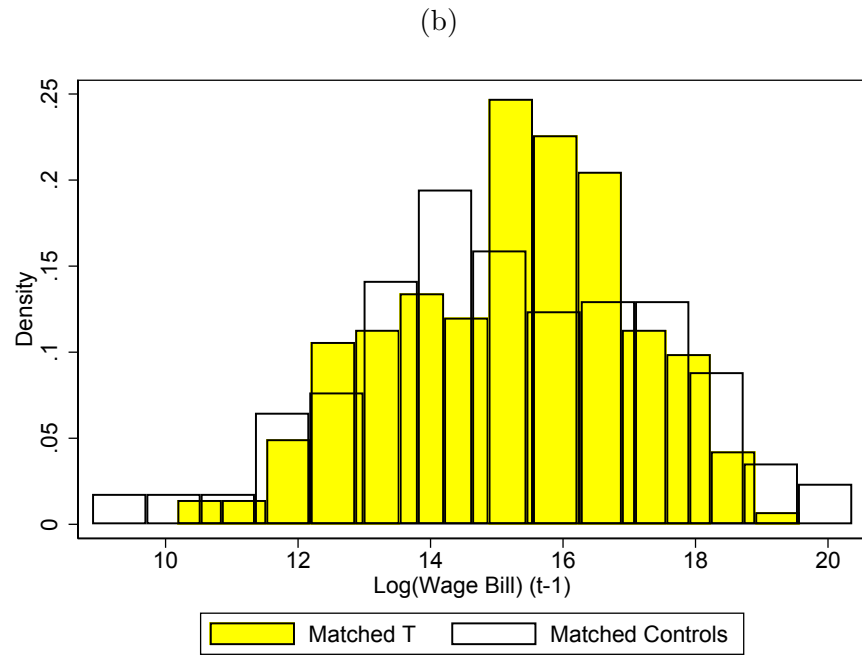
Trimmed at the 99th percentile.

Notes: Matched firm sample, INPS data (1983–2017). Panels (a) and (b) display the distribution of log average earnings and log size for treated and matched control firms in the year before the CCD.

Figure C.2: Estimated Effects of CCD on Resident Workers
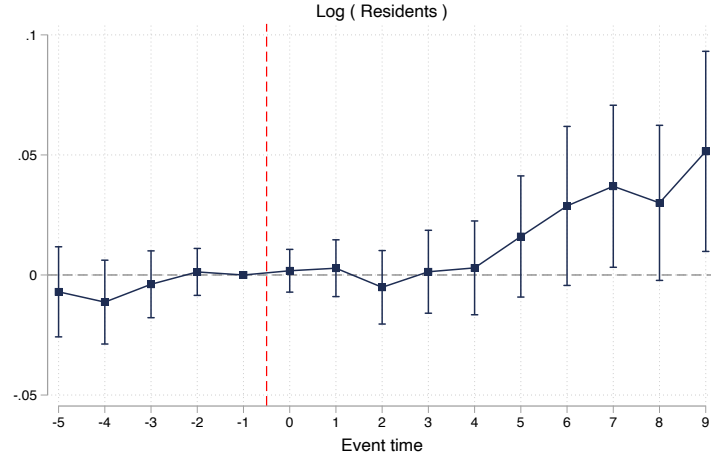
Notes: Matched municipality sample, INPS data (1983–2017). This figures displays the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variable is the municipality-level log number of workers who work and live in municipality m. The x-axis indexes event time.

Figure C.3: Estimated Effects of CCD on Total Taxes Collected / Total Revenues at $t_m^* - 1$

(a)
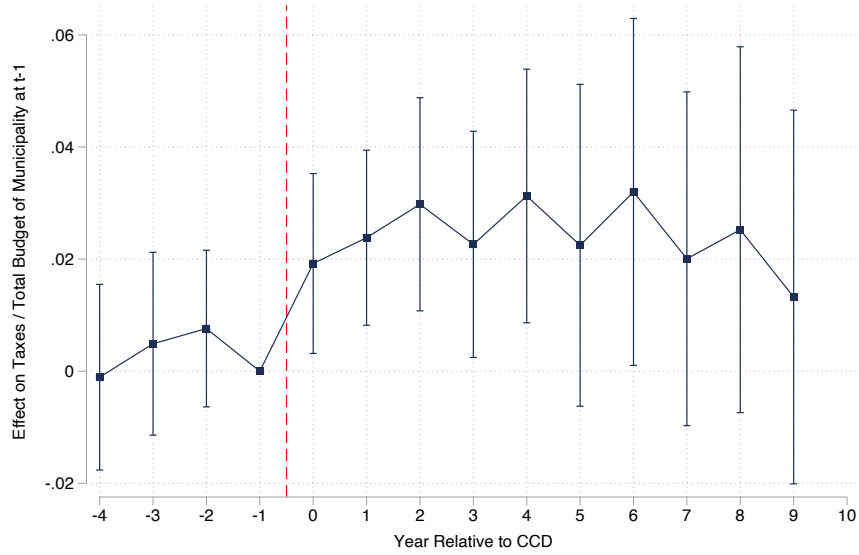
Notes: Matched municipality sample, Minister of the Interior data (1998–2015). This figure displays the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the year of the CCD, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variable is total taxes collected by the municipality over total revenues at $t_m^* - 1$, which corresponds to the year prior to the intervention. See Appendix B for details. The x-axis indexes event time.

Figure C.4: Estimated Effects of CCD on Expenditures

(a) Total Expenditures

(b) Administration

(c) Roads and Infrastructure

(d) Environment
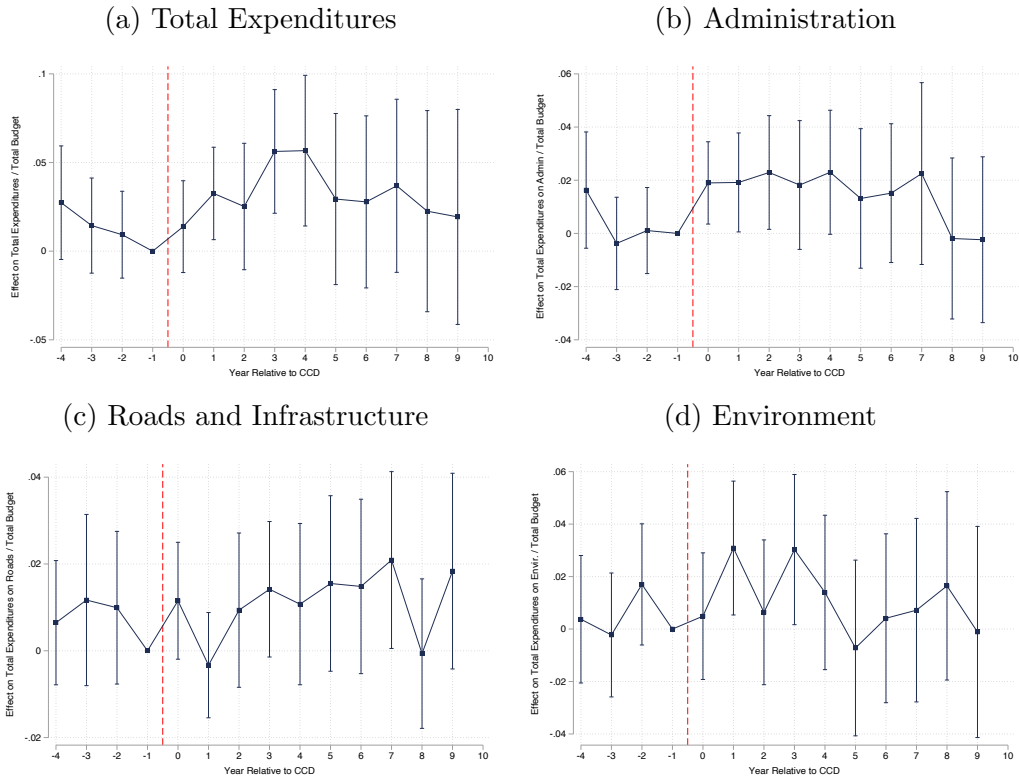
Notes: Matched municipality sample, Minister of the Interior data (1998–2015). Each outcome is normalized relative to the total budget of the municipality. The first panel represents total expenditures of a municipality relative to its total revenues. The remaining panels represent specific items on which municipality can spend its money, again normalized relative to the overall budget. Panel (c) shows expenditures on roads and other public transportation. Panel (d) shows expenditures on the environment, which are mainly allocated to garbage collection. See Appendix B for details. The x-axis indexes event time. Results in table format are reported in Table D.9.

Appendix D Additional Tables

Table D.1: Additional Municipality Characteristics

	(1) Matched Sample	(2) T	(3) C	(4) N
Panel A: Real Estate Prices and Rents				
Housing Prices	813.66	733.33	885.64	2861
Office Real Estate Prices	880.22	848.46	911.75	2457
Industrial Real Estate Prices	436.71	441.43	432.49	2479
Housing Rents	2.81	2.50	3.09	2840
Office Real Estate Rents	4.02	3.94	4.11	2437
Industrial Real Estate Rents	2.08	2.01	2.15	2444
Panel B: Population and Public Finances				
Population	14507.00	14590.28	14424.14	4451
Total Revenues (millions)	18.53	18.84	18.21	4457
Total Expenditures (millions)	14.78	14.50	15.06	4456
Taxes Collected (millions)	5.74	5.15	6.32	4457
Exp. on Administration (millions)	4.22	4.44	4.01	4457
Exp. on Roads and Infrastructure (millions)	1.64	1.41	1.86	4457
Exp. on Environment (millions)	4.31	4.56	4.05	4457
Panel C: Public Procurement				
N Tenders	1.874	1.572	2.176	3,978
Reserve Price (millions)	1.239	0.880	1.599	3,978
N of Winners	2.029	1.679	2.379	3,978
Cost (millions)	64.479	3.576	128.202	1,989

Note: Matched municipality sample. Panel A uses data from the Treasury (2002–2015), Panel B uses data from the Ministry of the Interior (1998–2015), and Panel C uses ANAC data (2000–2016). All statistics are calculated across municipality-year observations. Column (1)reports statistics on the full matched sample, columns (2) and (3)limit the sample to treated and control municipalities, respectively. Column (4) reports the number of observations in the full matched sample. The public finance variables in Panel B and the reserve price and the public procurement cost in Panel C are expressed in millions of euros; the latter is available starting from 2008.

Table D.2: Municipality Characteristics at *t-1*

	(1) T	(2) C	(3) p-value
Covariates used in the matching algorithm			
Log(N Employees) *(t-1)*	6.31	6.34	0.88
Log(N Employees) *(t-2)*	6.26	6.29	0.88
Log(Wage Bill) *(t-1)*	15.25	15.25	0.99
Population 1991 Census (Residents)	15522.71	15004.95	0.84
1d-Ateco 1	0.14	0.14	0.54
1d-Ateco 2	0.07	0.07	0.70
1d-Ateco 3	0.03	0.03	0.78
1d-Ateco 4	0.54	0.54	0.83
1d-Ateco 5	0.08	0.08	0.72
1d-Ateco 6	0.01	0.01	0.94
1d-Ateco 7	0.02	0.02	0.66
1d-Ateco 8	0.07	0.08	0.54
1d-Ateco 9	0.04	0.03	0.60
1d-Ateco 10	0.00	0.00	0.32
Other covariates			
N of Firms *(t-1)*	226.18	311.64	0.16
N of Establishments *(t-1)*	235.13	325.57	0.15
N of Firm Entries *(t-1)*	29.65	34.93	0.42
N of Firm Exits *(t-1)*	21.75	27.52	0.37
N			422

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. All statistics are calculated across municipality observations in the year before the CCD. Columns (1) and (2) limit the sample to treated and control municipalities respectively. The reported p-value represents p-value associated with the test for the difference in means between treated and control units. The 1-digit Ateco codes are constructed as follows. 1: agriculture, silviculture, fishing, food, drinks, tobacco, textile, garment, leather, wood and paper; 2: chemical, pharmaceutical, plastic, metal, vehicles, machines, appliances; 3: other manufacturing, maintenance, repair, installation, electricity, gas, vapor, waste disposal; 4: construction, car dealing, wholesale, retail, transportation, warehouse; 5: tourism, information and communication; 6: finance, insurance, real estate; 7: scientific and professional services, rental; 8: support to firms, education, health; 9: other services, sports and entertainment, associations, repair home appliances and computers; 10: other.

Table D.3: Characteristics of Incumbent Firms

	(1)	(2)	(3)
	Matched Sample	T	C
N of Employees	4.78	4.92	4.64
Wage Bill	5632.39	5840.87	5424.55
Av. Earnings	1041.88	1045.04	1038.72
Share FT	0.52	0.52	0.52
Share PT	0.08	0.08	0.08
Share Temp.	0.06	0.06	0.06
Share Manager	0.00	0.00	0.00
Share White C.	0.22	0.22	0.23
Share Blue C.	0.52	0.52	0.51
Share Apprent.	0.05	0.04	0.05
Survival	0.46	0.46	0.46
N	1373545	685724	687821

Notes: Matched firm sample, INPS data (1983–2017). All statistics are calculated across firm-year observations. Column (1) reports statistics on the full matched sample, columns (2) and (3) limit the sample to treated and control firms respectively. The number of observations for survival are 2986550, 1493275, and 1493275 for the matched sample, treated units, and control units, respectively.

Table D.4: Characteristics of Incumbent Firm at *t-1*

	(1)	(2)	(3)
	T	C	p-value
Covariates used in the matching algorithm			
N Employees *(t-1)*	4.44	4.25	0.24
N Employees *(t-2)*	4.22	4.05	0.23
Wage Bill *(t-1)*	5004.83	4742.01	0.32
Age *(t-1)*	9.76	9.70	0.80
Sole Proprietorship	0.48	0.48	1.00
Other Covariates			
Av. Earnings *(t-1)*	1044.05	1044.12	1.00
Share FT *(t-1)*	0.48	0.48	0.98
Share PT *(t-1)*	0.05	0.06	0.82
Share Manager *(t-1)*	0.00	0.00	0.31
Share White Collar *(t-1)*	0.20	0.22	0.40
Share Blue Collar *(t-1)*	0.51	0.50	0.20
Share Apprent. *(t-1)*	0.05	0.05	0.36
N			85330

Notes: Matched firm sample, INPS data (1983–2017). All statistics are calculated across municipality observations in the year before the CCD. Columns (1) and (2) limit the sample to treated and control municipalities, respectively. The reported p-value is the p-value associated with the test for the difference in means between treated and control units. Standard errors are clustered at the municipality level.

Table D.5: Municipality Characteristics

	(1) Matched Sample	(2) T	(3) C
1991 Population	14071.83	14761.79	13383.08
N of Firms	202.08	211.22	192.95
N of Establishments	210.90	220.37	201.45
N of Employees	1480.39	1479.40	1481.38
N Sole Proprietorships	108.17	107.60	108.74
N of Firm Entries	25.01	27.67	22.36
N of Firm Exits	19.44	21.38	17.50
N of First Worker Appearances	200.31	191.38	209.21
N of Prev. Not Employed Workers	334.94	321.18	348.68
N of Managers	3.77	3.30	4.24
N of White C. Workers	277.76	275.34	280.19
N of Blue C. Workers	720.29	726.01	714.59
N of Apprentices	32.32	32.78	31.87
N of Other Workers.	1.11	0.99	1.23
N of Part-Time Workers	291.96	307.13	276.82
N of Full-Time Workers	1208.42	1191.16	1225.65
Wage Bill (Thousands)	15400	15100	15800
Av. Earnings	7819.03	7582.79	8054.86
N	11400	5695	5705

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to potential control municipalities within the same region. All statistics are calculated across municipality-year observations. Column (1) reports statistics on the full matched sample, and columns (2) and (3) limit the sample to treated and control municipalities, respectively. The number of observations for the number of first worker appearances and for the number of previously not employed workers are 11399, 5695, and 5704, respectively.

Table D.6: Estimated Effects of CCD on Municipality Employment, Earnings, and Firms

	(1) Log(Empl)	(2) Log(Wage Bill)	(3) Log(Av. Earnings)	(4) Log(N Firms)
On Impact	-0.006	-0.021	-0.015	0.006
	(0.013)	(0.016)	(0.013)	(0.008)
Short-Run	0.043	-0.003	-0.046	0.024
	(0.030)	(0.033)	(0.018)	(0.018)
Long-Run	0.073	0.061	-0.020	0.063
	(0.055)	(0.058)	(0.023)	(0.035)
Mean	6.076	14.927	8.851	4.317
R-squared	0.961	0.967	0.881	0.980
Adj. R-squared	0.959	0.966	0.875	0.979
N	11,400	11,400	11,400	11,400
Muni FE	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to potential control municipalities in the same region. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Table D.7: Estimated Effects of CCD on Entries and Exits

	(1) Share First Appearances	(2) Share Prev. Not Employed	(3) Share New Firm Entries	(4) Share Firm Exits
On Impact	-0.010	-0.020	0.015	0.001
	(0.007)	(0.014)	(0.007)	(0.005)
Short-Run	-0.001	0.005	0.015	0.000
	(0.009)	(0.015)	(0.011)	(0.009)
Long-Run	-0.008	0.006	0.053	0.037
	(0.020)	(0.032)	(0.021)	(0.015)
Mean	0.229	0.393	0.170	0.136
R-squared	0.290	0.347	0.670	0.666
Adj. R-squared	0.255	0.315	0.654	0.650
N	11,399	11,399	11,400	11,400
Muni FE	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to potential control municipalities in the same region. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Table D.8: Estimated Effects of CCD on Municipality Rents

	(1) Housing Rents	(2) Industrial Real Estate Rents	(3) Office Real Estate Rents
On Impact	-0.0154	0.0070	0.0281
	(0.0177)	(0.0202)	(0.0185)
Short-Run	-0.0002	0.0811	0.0437
	(0.0388)	(0.0446)	(0.0334)
Long-Run	0.0726	0.2190	0.0017
	(0.0755)	(0.1190)	(0.0685)
Mean	.934	.670	1.336
N	2833	2429	2427
Muni FE	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes

Notes: Matched municipality sample, Treasury data (2002–2015). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Table D.9: Estimated Effects of CCD on Municipality Expenditures

	(1) Tot. Exp./ Tot. Rev.	(2) Admin/ Tot. Rev.	(3) Justice/ Tot. Rev.	(4) Police/ Tot. Rev.	(5) Education/ Tot. Rev.	(6) Culture/ Tot. Rev.	(7) Sport/ Tot. Rev.	(8) Tourism/ Tot. Rev.	(9) Roads/ Tot. Rev.	(10) Enviro./ Tot. Rev.	(11) Other/ Tot. Rev.
On Impact	0.0138	0.0190	0.0004	0.0005	-0.0068	-0.0020	-0.0023	-0.0033	0.0115	0.0049	-0.0080
	(0.0132)	(0.0079)	(0.0005)	(0.0012)	(0.0047)	(0.0022)	(0.0031)	(0.0021)	(0.0069)	(0.0123)	(0.0089)
Short-Run	0.0562	0.0182	0.0008	0.0012	-0.0060	0.0023	0.0052	-0.0004	0.0142	0.0303	-0.0094
	(0.0178)	(0.0123)	(0.0015)	(0.0017)	(0.0055)	(0.0031)	(0.0051)	(0.0028)	(0.0080)	(0.0146)	(0.0091)
Long-Run	0.0193	-0.0024	0.0002	0.0054	-0.0052	0.0006	0.0047	-0.0052	0.0184	-0.0011	0.0039
	(0.0309)	(0.0159)	(0.0010)	(0.0031)	(0.0074)	(0.0031)	(0.0045)	(0.0037)	(0.0115)	(0.0205)	(0.0131)
Mean	.827	.264	.002	.033	.071	.018	.016	.009	.098	.228	.089
N	4456	4457	4456	4456	4457	4456	4457	4457	4457	4457	4456

Notes: Matched municipality sample, Ministry of the Interior data (1998–2015). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD. Each outcome is normalized relative to the total budget of the municipality. The first column reports total expenditures of a municipality relative to its total revenues. The remaining columns represent the different items on which the municipality can spend its money, again normalized relative to the overall budget. Column (9) reports expenditures on public transportation and other infrastructures. Column (10) eports expenditures on the environment which is mainly allocated to garbage collection.

Table D.10: Estimated Effects of CCD on Public Procurement

	(1) N Tenders/ Tot. Rev	(2) Reserve Price/ Tot. Rev	(3) N Winners/ Tot. Rev	(4) Cost/ Tot. Rev
On Impact	-0.052	-0.029	-0.061	-1.843
	(0.036)	(0.032)	(0.041)	(7.879)
Short-Run	0.002	-0.008	0.009	41.286
	(0.043)	(0.030)	(0.049)	(42.297)
Long-Run	0.016	-0.017	0.001	84.916
	(0.072)	(0.052)	(0.080)	(83.705)
Mean	3712	3712	3712	1736
R-squared	.30	.14	.30	.18
N	1.94	1.28	2.10	73.78
Muni FE	Yes	Yes	Yes	Yes
Region-by-Time FE	Yes	Yes	Yes	Yes

Notes: Matched municipality sample, ANAC data (2000–2016). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated θ_k coefficients from (1). We define "on impact" as $k = 0$, "short-run" as $k = 3$, and "long-run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD. The public procurement cost is available starting from 2008. The revenues, the reserve prices, and the procurement costs are expressed in millions of euros.