



Istituto Nazionale Previdenza Sociale



# WorkINPS Papers

Organized Crime and Economic Growth: Evidence from Municipalities Infiltrated by the Mafia

Alessandra Fenizia

Raffaele Saggio

Aggiornamento WorkINPS papers n.32 – Gennaio 2021

ISSN 2532 -8565

Lo scopo della serie WorkINPS papers è quello di promuovere la circolazione di documenti di lavoro prodotti da INPS o presentati da esperti indipendenti nel corso di seminari INPS, con l'obiettivo di stimolare commenti e suggerimenti.

Le opinioni espresse negli articoli sono quelle degli autori e non coinvolgono la responsabilità di INPS.

The purpose of the WorkINPS papers series is to promote the circulation of working papers prepared within INPS or presented in INPS seminars by outside experts with the aim of stimulating comments and suggestions.

The views expressed in the articles are those of the authors and do not involve the responsibility of INPS.

Responsabile Scientifico Maurizio Franzini

Comitato Scientifico Agar Brugiavini, Daniele Checchi, Maurizio Franzini

In copertina: uno storico "Punto cliente" a Tuscania INPS, Direzione generale, Archivio storico

#### I WORKINPS PAPER

Le basi dati amministrative dell'*INPS* rappresentano una fonte statistica unica per studiare scientificamente temi cruciali per l'economia italiana, la società e la politica economica: non solo il mercato del lavoro e i sistemi di protezione sociale, ma anche i nodi strutturali che impediscono all'Italia di crescere in modo adeguato. All'interno dell'Istituto, questi temi vengono studiati sia dai funzionari impiegati in attività di ricerca, sia dai *VisitInps Scholars*, ricercatori italiani e stranieri selezionati in base al loro curriculum vitae e al progetto di ricerca presentato.

I **WORKINPS** hanno lo scopo di diffondere i risultati delle ricerche svolte all'interno dell'Istituto a un più ampio numero possibile di ricercatori, studenti e policy markers.

Questi saggi di ricerca rappresentano un prodotto di avanzamento intermedio rispetto alla pubblicazione scientifica finale, un processo che nelle scienze sociali può chiedere anche diversi anni. Il processo di pubblicazione scientifica finale sarà gestito dai singoli autori.

Maurizio Franzini

## Organized Crime and Economic Growth: Evidence from Municipalities Infiltrated by the Mafia

Alessandra Fenizia (The George Washington University) Raffaele Saggio (University of British Columbia and NBER)

Aggiornamento WorkINPS papers n. 32\* - gennaio 2021

### Criminalità Organizzata e Crescita Economica: L'Impatto della Rimozione dei Consigli Comunali Infiltrati

#### Abstract

*Italiano*: Questo articolo studia l'impatto di lungo periodo della lotta alla criminalità organizzata. Utilizziamo dati amministrativi italiani per studiare una delle politiche più aggressive nella lotta alle organizzazioni di stampo mafioso: il commissariamento dei comuni per mafia. In base a questa politica, i consigli comunali in odore di infiltrazione mafiosa vengono commissariati per un periodo di circa due anni. Le stime basate su una strategia di differenze delle differenze rivelano che questa politica è in grado di stimolare la crescita economica delle aree interessate. In particolare, si rileva un incremento dell'occupazione, del numero di imprese e del prezzo degli immobili a destinazione industriale. Tali effetti si manifestano prevalentemente in settori ad alto rischio di infiltrazione mafiosa ed in comuni dove viene rieletto un minor numero di politici appartenenti consiglio comunale sciolto. Il commissariamento genera crescita economica indebolendo la criminialità organizzata e aumentando la fiducia nelle istituzioni. Le nostre analisi indicano che il commissariamento rappresenta uno strumento in grado di restituire legittimità alle istituzioni e generare attività economica in aree tradizionalmente controllate da organizzazioni di stampo mafioso.

Parole chiave: Mafia, Ciminalità Organizzata, Corruzione, Mercati del Lavoro, Crescita Economica.

### Organized Crime and Economic Growth: Evidence from Municipalities Infiltrated by the Mafia

Alessandra Fenizia Raffaele Saggio<sup>\*</sup>

Abstract

This paper studies the long-run economic impact of dismissing city councils infiltrated by organized crime. Applying a matched difference-in-differences design to the universe of Italian social security records, we find that city council dismissals (CCDs) increase employment, the number of firms, and industrial real estate prices. The effects are concentrated in Mafia-dominated sectors and in municipalities where fewer incumbents are re-elected. The dismissals generate large economic returns by weakening the Mafia and fostering trust in local institutions. The analysis suggests that CCDs represent an effective intervention for establishing legitimacy and spurring economic activity in areas dominated by organized crime.

Key words: mafia, organized crime, corruption, labor markets, economic growth.

<sup>\*</sup>Alessandra Fenizia, The George Washington University; email: afenizia@gwu.edu. Raffaele Saggio, University of British Columbia and NBER; email: rsaggio@mail.ubc.ca. We thank Nicolas Ajzenman, Christopher Blattman, Matilde Bombardini, David Card, Ernesto Dal Bó, Gianmarco Daniele, Patrick Francois, Francesco De Carolis, Claudio Ferraz, Fred Finan, Nicholas Li, Giovanna Marcolongo, Vincent Pons, Paolo Pinotti, Bryan Stuart, Francesco Trebbi, and Anthony Yezer for useful suggestions. We also thank Begum Akkas, Prerna Dokania, and Martino Kuntze for excellent research assistance and all the members of the administrative and technical staff of the VisitINPS program who provided invaluable support and help. The realization of this article was possible thanks to the sponsorship and financial support of the "VisitINPS Scholars" program. We also thank IIEP for research support. The findings and conclusions expressed are solely those of the authors and do not represent the views of INPS. An earlier version of this draft was circulated under the title "Can the Mafia's Tentacles Be Severed? The Economic Effects of Removing Corrupt City Councils."

#### 1 Introduction

Organized crime has large economic and social costs. Hundreds of millions of people live in areas controlled by criminal organizations (Blattman et al., 2021). Thousands more are regularly displaced by the violence that accompanies these organizations (Daniele et al., 2020). Organized crime thrives on illegal activities (Sviatschi, 2022), preys on healthy businesses (Mirenda et al., 2022), weakens competition and innovation among firms (Slutzky and Zeume, 2019), and ultimately hinders economic growth (Pinotti, 2015a,b; UNICRI, 2016). While studies have documented its origin (Acemoglu et al., 2019; Bandiera, 2003) and spread (Alesina et al., 2018; Sviatschi, 2020; Mirenda et al., 2022), much less is known on how the State can regain control and reassert legitimacy in areas where criminal organizations have been active for years, if not centuries. Even less is known about whether attempts to remove organized crime would ultimately manifest in long-run economic development.

This paper evaluates the long-run economic impact of one of the most aggressive policies aimed at combating organized crime in Italy: the city council dismissal (henceforth CCD). Following allegations of Mafia infiltration in the local government, the central government dismisses the entire political apparatus of the municipality—including the mayor and the city council. It then appoints a team of commissioners who administer the municipality for about two years with full legislative and executive powers until new elections occur. CCDs represent a unique policy used by the central government to regain control and legitimacy in areas where corruption was so pervasive that the Mafia de facto ran the local government.

We study the economic impact of 245 CCDs between 1991 and 2016 using a matched difference-in-differences design applied to rich administrative data on workers, firms, real estate prices, and public finances. We compare treated municipalities subject to CCDs to observationally similar untreated municipalities. Because of a strict procedure designed to limit its potential for abuse, CCDs are not triggered by poor economic performance. Consistent with that, there is no evidence of differential pre-trends between treated and control units over a variety of outcomes, lending credibility to the research design.

We find that CCDs spur economic activity. Relative to their matched counterfactual, treated municipalities experience an average increase in employment of 16.9% nine years after the intervention. CCDs also increase the stock of firms by 9.4% after nine years, reflecting an increase in firm entry that outpaces an increase in firm exit. The increase in both firm entries and exits reflects increased economic "dynamism" caused by the CCD intervention. Detailed administrative data on real estate transactions show that CCDs' benefits are capitalized into a 15% increase in industrial real estate prices. Real estate transactions are not subject to informal sector underreporting. Thus, the surge in the prices of business properties is consistent with

the employment effects of CCDs reflecting increases in real economic activities as opposed to a reallocation from the informal to the formal sector. Finally, CCDs have positive spillovers on neighboring towns. The increase in economic activity in treated municipalities does not come with a cost of employment losses in surrounding municipalities.

There are two alternative explanations for the economic effects of CCDs. The first explanation is that CCDs lead to economic growth without, however, weakening the Mafia's presence. For instance, CCDs may generate economic effects simply because the central government increases transfers to local governments following a dismissal. These transfers might then be reinvested in policies that generate employment gains (e.g. job training programs). More broadly, the re-centralization of power (i.e., the substitution of local politicians with experienced public servants appointed by the central government) might independently generate positive economic effects (Bardhan and Mookherjee, 2000). The second interpretation is that CCDs spur economic development *because* they erode the power of the Mafia.

We do not find evidence in favor of the first explanation. First, there is little evidence that CCDs concretely change the operations of local governments. They do not lead to an increase in transfers from the central government to treated municipalities. Moreover, there is no increased spending on job training programs and only modest effects on investment in infrastructure and sanitation. Second, we use CCDs that arise from factors unrelated to Mafia infiltration. As for Mafia-related CCDs, the central government appoints experienced bureaucrats who administer the municipality until new elections. We find that these "alternative" CCDs generate much smaller economic effects. It thus appears that the re-centralization of power is not the main driver of our results.

Instead, we find ample evidence consistent with the second explanation. First, CCDs increase the number of assets that are confiscated from the Mafia and are repurposed for public use. Second, the economic effects are concentrated in sectors that are traditionally associated with a strong Mafia presence (e.g., construction and waste disposal), suggesting that CCDs weaken the Mafia's ability to enforce monopolies (Gambetta, 2000). Third, the employment effects are particularly pronounced for younger individuals, who are disproportionately more likely to be recruited by criminal organizations (Sviatschi, 2022). Finally, the public officials elected after the CCD are more likely to be young, college-educated, and first-time politicians (Daniele and Geys, 2015b). They are also much more likely to be female: there is a 67% increase in the probability of electing women following a CCD. Given the patriarchal view of society that is perpetuated by the Mafia (Fiandaca, 2007), and recent evidence that points to women being less likely to engage in corruption (Decarolis et al., 2020), this finding suggests that CCDs weaken the Mafia's ability to influence electoral results. Complementary evidence from dismissals unrelated to Mafia infiltration shows that the radical political transformation

induced by CCDs does not simply reflect the elimination of the incumbency advantage. Instead, the change in the political landscape originates from shifts in voters' preferences.

Finally, we show that the economic effects of CCDs are concentrated in municipalities where the dismissal severs the ties between the Mafia and the local politicians. Conversely, in municipalities where the Mafia retains influence over the local government, CCDs do not generate economic growth. We proxy for whether CCDs weakened the connections between the Mafia and local politicians using the change in the share of non-incumbent politicians following a CCD. We interpret the re-election of the dismissed politicians (who were found to be connected to organized crime) as an indication that the CCD does not get rid of Mafia infiltration. On the other hand, if municipalities elect different politicians after the CCD, then it is likely that this intervention was successful in severing (some of) the ties between the local government and organized crime. We find that CCDs do not lead to any significant economic effect in municipalities that do not change their politicians. The economic effects materialize only in municipalities that experience a change in the composition of the elected officials. We conclude that CCDs weaken the Mafia's influence and that this is the primary channel through which CCDs generate long-run economic development.

This paper contributes to three strands of literature. First, it contributes to a recent literature studying efforts to re-exert control over areas governed by criminal organizations. All previous studies document large unintended consequences. For example, combating money laundering reduces deposits (Slutzky et al., 2019), and cracking down on drug trafficking increases homicides (Dell, 2015). Deportations expand criminal networks and increase violence (Sviatschi, 2020), and increased policing increases gang rule (Blattman et al., 2021). The paucity of success speaks to the infiltration of organized crime in these areas.

We contribute to this literature in three ways. First, we study economic growth in contrast to previous studies that focus on bank deposits, violence, and crime. We provide novel empirical evidence of long-run increases in economic activity and formal employment using detailed administrative data covering the universe of social security records. Notably, the data capture impacts on smaller businesses, which constitute the bulk of firms operating in poor areas. Small firms have been overlooked in past empirical research due to data limitations. Second, our study examines an aggressive policy that *directly* targets local institutions as opposed to illegal activities. With the aggressiveness comes more economic upside potential but also more risk of backlash and unintended consequences. Ultimately, we find that CCDs are highly effective and do not generate backlash. Thus, targeting corrupt institutions may be more effective than simply targeting illegal activities. Third, our data and setting allow us to investigate mechanisms. The evidence suggests that CCDs' success is not due to improved efficiency of the local government via the appointment of trustworthy public servants (Bardhan and Mookherjee, 2000; Acemoglu, 2006). Rather, CCDs succeed because they weaken Mafia's ability to infiltrate local institutions and this, in turn, generates large, long-run economic returns.

Second, our paper fits into the broader literature that studies the economic effects of organized crime and corruption. Most studies find that criminal organizations generate violence (Daniele et al., 2020), negatively affect firm performance (Calamunci and Drago, 2020; Mirenda et al., 2022), stifle competition and investment (Slutzky and Zeume, 2019), and ultimately hinder economic growth (Melnikov et al., 2020; Pinotti, 2015a,b). Along the same lines, Colonnelli et al. (2020) document that corruption reduces economic activity. One notable exception is recent work by Le Moglie and Sorrenti (2020) which shows that the Mafia can mitigate the negative impact of recessions when it invests in legitimate businesses. We find that the Mafia does hinder competition and economic activity, and find no evidence that organized crime "greases the wheels" of cumbersome bureaucracies or generates economic growth (Leff, 1964).

Finally, our paper relates to the literature that examines the effects of CCDs. Previous studies find that CCDs reduce petty crimes and violence against politicians (Baraldi et al., 2022; Cingano and Tonello, 2020); do not affect the pool of candidates running for local offices (Baraldi and Immordino, 2021); and increase the quality of newly elected politicians (Daniele and Geys, 2015a). They also appear to have short-term effects on spending in treated municipalities (Acconcia et al., 2014) and have spillover effects on spending and public procurement in neighboring towns (Galletta, 2016; Tulli, 2019). Our paper contributes to this literature by studying the impact of CCDs on workers, firms, and economic growth. Our detailed administrative data are more disaggregate, permitting a credible empirical analysis of both the short-and long-run economic impacts of CCDs.

#### 2 Institutional Background

In response to the Mafia's growing influence on local governments in the 1980s, the Italian parliament introduced a policy to dismiss city councils in 1991 (D.L. 31/05/1991 n. 164). If local governments appear to be under the influence of the Mafia, the law permits the central government to replace the mayor, executive committee, and city council with external commissioners (*Commissari Straordinari*) composed of experienced career civil servants from other areas. With full executive and legislative powers, these commissioners run the municipality for 24 to 36 months until new elections occur.<sup>1</sup> This law is arguably the government's most aggressive policy tools to fight organized crime (CNE, 1995), and it aims to prevent future corruption by severing the ties between criminal organizations and the local government.

<sup>&</sup>lt;sup>1</sup>See Online Appendix A for a description of the political institutions of Italian municipalities and additional institutional details on CCDs.

CCDs are typically initiated due to unrelated police investigations. However, the evidentiary bar is lower than for prosecution. Rather than looking for incontrovertible evidence of illegal activity, the Ministry of the Interior looks for connections between local politicians and organized crime, many of which occur during routine police investigations.<sup>2</sup> Other times, the CCD is triggered by actual crimes such as extortion, drug and arms trafficking, money laundering, vote buying, and collusion in public procurement. It is not triggered by poor municipality financial performance or by inefficiencies and delays in public procurement.

To limit the possibility of arbitrariness or delays, the law establishes a very rigid procedure that governs the dismissal of the local government from the emergence of evidence to the final decision. Evidence of connections between elected public officials and the Mafia is first reported to the *prefetto*, the provincial representative of the Ministry of the Interior. The *prefetto* then forms a commission (*Commissione d'Accesso*) that investigates the allegations and issues a report within three months. In consultation with the cabinet, the interior minister uses the report to make a final decision on the dismissal, which is publicly decreed by the president in the *Gazzetta Ufficiale*, the government's official journal.<sup>3</sup> Although the central government might in principle use this procedure to take over municipalities run by political opponents, Mete (2009) 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.

Reviewing official reports of the interior minister to Parliament, external commissioners typically implement four types of interventions. First, they freeze all investments in new projects while reviewing the municipality's financial situation and scrutinizing procurement contracts, permits, and business licenses. Second, they 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, they change the municipal government's personnel practices. The official reports show that municipality bureaucrats are often poorly qualified and occasionally uncooperative. To professionalize the local bureaucracy, the commissioners often mandate training for employees. They also hire temporary workers for understaffed sites. Finally, they try to gain the trust and support of local communities. For example, they provide services such as free job training and local infrastructure investment.

Since its introduction in 1991, 245 different municipalities have been subject to the CCD; 151

 $<sup>^{2}</sup>$ For example, one of the elements that contributed to the dismissal of the Bovalino city council 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 to by the groom himself.

<sup>&</sup>lt;sup>3</sup>Anecdotally, most investigations result in a dismissal. The Ministry of the Interior has published the results of these investigations since 2009. Out of the 97 investigations initiated between 2009 and 2016, 65 resulted in a dismissal. We cannot use the sample of municipalities that were investigated but not dismissed as a control group because there are too few of them.

municipalities experienced one dismissal, 35 experienced two, and 8 experienced three. Multiple dismissals are indicative of the challenge of severing the very deep infiltration of organized crime into local government. Figure 1 plots the annual frequency of CCDs from 1991 to 2016. The spike in 1993 reflects the reaction to the terrorist attacks of Cosa Nostra in the early '90s, and the spike in 2012 coincides with Monti succeeding Berlusconi as prime minister. The government dismissed 23 municipal governments as part of the Monti government's agenda to implement structural changes to Italian institutions. Figure 2 illustrates the geographic variation of affected municipalities. CCDs are concentrated in Southern Italy, where the Mafia emerged at the end of the 19th century (Acemoglu et al., 2019). However, northern regions such as in Piedmont, Lombardy, and Liguria are not immune to Mafia infiltration (Dipoppa, 2021).

#### 3 Data

We estimate CCDs' effects on three different categories of outcomes: economic, local government, and political. Each category of outcomes draws from a number of different data sources, laid out below.

#### **3.a** Economic Outcomes

**Social Security Data.** Our main source of data is the confidential matched employeremployee dataset (1983–2017) collected by the Italian social security agency (*Istituto Nazionale di Previdenza Sociale*—INPS hereafter). This longitudinal dataset contains the universe of non-agricultural firms with at least one employee. These data include unique firm and worker identifiers that allow us to track them over time. Each firm is identified by a tax identification number, and workers are identified by their social security number. These administrative data contain wages, annual days worked at each job in a year, contract type, occupation, detailed industry codes, part- versus full-time status, gender, age, firm location, and workers' residence. However, the social security records do not include information about workers who are unemployed, self-employed, or employed in the informal or public sectors. INPS also collects data on the opening and closing of businesses as well as firms' juridical status.

These data are uniquely well suited for studying the impact of CCDs because they capture small businesses and sole proprietorships, which constitute a large share of local establishments in municipalities with a Mafia presence (Section 4.b). Small firms and sole proprietorships have often been overlooked in empirical research, partly due to their absence from common firm-level datasets (e.g., Cerved, AIDA, and Amadeus). Our sample consists of all firms and workers operating in any of the nine regions that have experienced at least one CCD from 1991 to  $2016.^4$ 

**Real Estate Prices.** We use complete administrative data on real estate prices and rents (2002–2015) collected and harmonized by the Italian Treasury.<sup>5</sup> This dataset has two notable features. First, unlike most real estate price datasets, it includes information on both residential and non-residential units. Second, it contains information on sale prices and rents.

#### 3.b Local Government Outcomes

Local Government Expenditures, Revenues, and Population. We use data on municipality finances and population (1998–2015) collected by the Ministry of the Interior. Our analysis separately investigates expenditures and revenues. Municipal expenditures are divided into 12 separate functions: administration, justice, local police, public education, culture (libraries, museums, theaters), sports, tourism, road safety and public transportation, territory and sanitation (urban planning, sanitation, parks, and garbage collection), social assistance, economic development, and other services. For each function, we analyze CCDs' effects on total spending, the sum of short-term current expenditures and longer-term investments. We combine the smaller categories of social assistance, economic development, and other services into an "other social policies" residual category. We also analyze each of the four main categories of revenue: taxes, transfers from the central government, loans, and other revenue.

Seized Mafia Assets. Mafia assets are regularly subject to civil forfeiture by the courts following investigations of illegal activities. The Italian Authority for Assets Seized from Criminal Organizations (*L'Agenzia Nazionale per l'amministrazione e la destinazione dei beni sequestrati e confiscati alla criminalità organizzata*) re-purposes seized assets for public benefit (1983–2019). Assets are most often real estate but can occasionally include entire firms.

#### **3.c** Political Outcomes

We use data on local politicians (1986–2020) collected by the Ministry of the Interior. These data contain the name, surname, highest educational attainment, age, mandate length, and office (e.g., mayor, city council member, alderman) of all local politicians.

<sup>&</sup>lt;sup>4</sup>These regions are Liguria, Piedmont, Lombardy, Lazio, Campania, Calabria, Basilicata, Apulia, and Sicily. <sup>5</sup>These data are collected by a Transpury department (Accepting dalla Entreta, Transitionia, Occeptuationia, dalla

<sup>&</sup>lt;sup>5</sup>These data are collected by a Treasury department (Agenzia delle Entrate - Territorio - Osservatorio del Mercato Immobiliare) tasked with monitoring the housing market.

#### 4 Research Design

In this section, we discuss the matched difference-in-differences design we use to examine the effects of the CCD.

#### 4.a Matching Algorithm

We use nearest-neighbor propensity score matching to match each of the 245 CCDs that occurred between 1991 and 2016 to a control municipality. To do so, we first group municipalities by their region, r, and the year they were subject to a CCD,  $t^*$ . For each group, we select the set of potential control municipalities to be the never-treated municipalities in one of the nine regions that experienced a CCD other than r. We require the control group to be in a region other than r to avoid contamination from spillover effects. This choice is corroborated by the analysis presented in Section 5.b that documents the presence of large spillovers from CCDs.

For each group, we then estimate a separate probit model on a cross-sectional sample of municipalities consisting of the treated group and the potential control group. The probit regressions relate the CCD in the year of treatment to one-year-lagged average log earnings, one- and two-year-lagged log employment, 1991 population, and one-year-lagged local industry shares. Using the estimated predicted values as the treatment propensity, we match each treated municipality to the untreated municipality with the closest propensity score. Altogether, we match 87% (211) of the events.

#### 4.b Summary Statistics

Table 1 reports the summary statistics in the year before the CCD 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 15,264 inhabitants (in 1991) and 251 firms.<sup>6</sup> However, the level of firms masks substantial churn: 14% and 10% of firms are born and die in municipalities before the CCD. Fifty-three percent of firms are sole proprietorships, which are often omitted from many firm datasets due to lower reporting requirements.

The average municipality in our sample employs 2,349 private sector workers, implying an average firm size of 9.4 (2,349/251). The ratio of employment to 1991 population is only 15.4%, reflecting the high rate of unemployment, high rate of informality, and high share of public sector employment characteristic of municipalities in Southern Italy, which are overrepresented in our sample. Of the workers formally employed in the year before the CCD, 26% were not formally

 $<sup>^{6}</sup>$ The average municipality has 261 establishments, and, correspondingly, most firms have only one establishment. Because there is little distinction between firms and establishments, we focus on firms throughout the analysis.

employed the year before, and 14% had never been formally employed. Work is predominantly full time and blue collar, with an average daily wage of 72.74 euros. Workers who were not employed two years before the CCD earn substantially lower daily wages (63.56 euros) than the workers who were (74.10 euros).

Differences in the number of employees notwithstanding, the covariates are relatively well balanced as a whole between the treated and control groups. Balance on economic variables is an expected result of the matching algorithm. However, treatment and control municipalities are also balanced when looking at electoral turnout and local politician characteristics (Table D.1), which are not included in the matching procedure.<sup>7</sup> Nevertheless, as we discuss in the next section, imbalances in outcome levels between treatment and control municipalities are not a threat to our empirical strategy.

#### 4.c Econometric Specification

To estimate CCDs' impact on municipal outcomes, we estimate the following model on the matched sample of treated and control municipalities:

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

where  $y_{mt}$  is an outcome variable (such as log employment) for municipality m in year t.<sup>8</sup>  $CCD_m$ is an indicator equal to 1 if municipality m experienced the CCD event,  $\mathbf{1}\{t = t^* + k\}$  are the event time dummies, and  $t_m^*$  is the year of the CCD event for municipality m.<sup>9</sup> We control for municipality fixed effects,  $\alpha_m$ , and region-by-time fixed effects,  $\lambda_{r(m),t}$ , where r(m) denotes the region associated with municipality m.<sup>10</sup> We omit the dummy for the year before the CCD event in the above specification so that  $\theta^k$  identifies the changes in outcome  $y_{mt}$  between treated and counterfactual municipalities relative to the same difference at k = -1.  $u_{mt}$  is the error term. The regression results are weighted by the logarithm of the number of firms in the year

<sup>&</sup>lt;sup>7</sup>Figures D.1a and D.1b in Online Appendix D show that the overall *distribution* of both employment and earnings are also well balanced between treated and counterfactual municipalities.

<sup>&</sup>lt;sup>8</sup>All labor market outcome variables such as log employment or average wages are calculated based on the geography of the *employers* in municipality m. For instance, if a worker lives in municipality m' but is employed by a firm in municipality m, they will count as employed for municipality m.

<sup>&</sup>lt;sup>9</sup>We assign the event time of each treated municipality to its matched control. Therefore, the event time dummies are defined both for treated and control municipalities.

<sup>&</sup>lt;sup>10</sup>Each municipality-event is included separately. Thus, a municipality that is treated multiple times will have multiple observations, each event with its own set of fixed effects. For instance, if municipality  $m^*$  was subjected to a CCD event in 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.

before the CCD. Online Appendix C.2 shows that our results are unaffected by the weights. Standard errors are clustered at the municipality level.

#### 4.d Validity of the Design

This empirical specification builds on the dynamic matched difference-in-differences design used in recent papers (Jäger, 2019; Goldschmidt and Schmieder, 2017). The effect of the CCD thus comes from comparing treated municipalities to matched counterfactual municipalities that are never treated. Using a matched control group circumvents challenges scrutinized in recent research (Goodman-Bacon, 2018 and Borusyak et al., 2021) that arise in event-study models that rely solely on the variation in the timing of treatment. The key identifying assumption is that the outcomes in treated and control municipalities would have followed parallel trends in absence of the CCD. Although we cannot directly test this identifying assumption, we look for violations of parallel pre-trends in the years leading up to the event by evaluating the eventstudy coefficients for k < 0. Lending credibility to the design's validity, placebo tests show no evidence of differential pre-trends between treated and control units over a variety of outcomes. This is consistent with the strict procedures described in Section 2, that ensure that CCDs cannot be triggered by poor economic performance.

However, even in the presence of parallel pre-trends, one might still worry that the control municipalities do not represent an adequate counterfactual. We discuss some of these concerns below.

**Differential Trends in Mafia Presence.** One concern is that there may be differential trends in Mafia behavior between treated and control municipalities. For example, the Mafia's growing presence in treated municipalities might have triggered the CCD and while also having an independent effect on the economic outcomes. Several facts push against this interpretation. First, if this was the case, the dynamic differences in Mafia presence between the treatment and the control group would have also impacted the economic outcomes *before* CCDs and thus would have been reflected in non-parallel pre-trends. We do not find evidence consistent with this explanation. Second, although we cannot directly test for differential trends in Mafia behavior, our results are virtually unchanged when we include measures that proxy for the degree of Mafia presence in the matching algorithm (Online Appendix C.1).<sup>11</sup>

**Differential Trends in Law Enforcement Capacity.** Another potential concern is that an increase in the media coverage of the Mafia or changes in the sentiment toward organized

<sup>&</sup>lt;sup>11</sup>These include a municipality-level indicator for high-Mafia prevalence, turnout at the previous election, and a measure of the political orientation of the dismissed government.

crime may induce treated municipalities to increase law enforcement efforts and this may in turn trigger the CCD. If changes in law enforcement capacity also have an independent effect on economic outcomes, this would represent a threat to our empirical strategy. However, we find no evidence consistent with this potential confounder. Lending credibility to our research design, Figure D.3 shows no systematic difference between treated and control municipalities in the expenditure on the justice system (panel a) or police (panel b) in the years leading up to the CCD. All differences are economically very small and not statistically significant.

**Other Unobserved, Sudden Shocks.** Difference-in-differences research designs are threatened if treated groups are affected by an unrelated shock at the same time as treatment. Our research design ameliorates some of those concerns. First, we have variation in event timing, so a single regional shock would have a minimal effect on our results. Second, even if unrelated regional shocks were to coincidentally co-vary with our events, our design absorbs region-time fixed effects. Third, the timing of the economic effects is not consistent with shocks triggering the CCD and affecting economic outcomes. As shown in the next section, the economic effects of CCDs do not materialize until the third year after the CCD. It is highly unlikely that there was a large enough shock in year  $t_m^*$  to trigger the CCD but had no economic effects until  $t_m^* + 3$ . Conversely, the third year after a CCD is very important for this intervention as it typically represents the year when new elections occur following the dismissal of the city council, a point that we come back to in Section 6.

**Spillovers from CCDs in Other Regions.** As discussed above, we match treated municipalities "out of region" so that the control municipalities are not indirectly affected by the CCD. However, one potential concern is that the control units may still suffer from spillovers from CCDs in other regions. To evaluate this, we drop all municipalities within a 20 km radius from *any* treated units from the set of potential controls. Online Appendix C.5 shows that our main results are robust to dropping all the units that may be potentially affected by the spillovers from the donor pool.

### 5 Economic Effects of CCDs

This section examines how CCDs affect the local economy. The first part presents their effects on workers, firms, and wages and on real estate demand. The second part examines their spillover effects. The last part lays out robustness tests.

#### 5.a Main Results

Effects on Workers, Firms, and Wages. Figure 3 reports the event-study coefficients  $\theta_k$  from equation (1) on log employment, log number of firms, log wage bill, and log average wage. Table 2 summarizes the immediate (k = 0), short-run (k = 3), and long-run (k = 9) effects of the CCD on these outcomes.

Figure 3a shows that log employment in treated municipalities closely tracks control municipalities in the years leading up to the CCD, corroborating the validity of our research design. In the first two years following the CCD, municipal employment grows modestly, and the average difference with the matched pairs is not statistically significant. However, employment starts increasing sharply three years after the intervention, coinciding with the end of the commissioners' mandate and the convening of the new city council. Employment is 16.9% higher in the long run. Figure 3b shows that the logarithm of the number of firms follows a similar pattern. There are approximately 9.4% more firms in the long run.

Table 3 reports the CCD's effects on flows of workers and firms. Rather than decreased firm exit, the increase in the number firms is driven by increased entry (column 5) overwhelming increased exit (column 6). We interpret the growth in both firm entry and exit as evidence that CCDs increase economic dynamism. This manifests in an increase of 6 percentage points in the share of new firms that did not exist before the CCD. The effect is economically large, representing an almost 40% increase relative to the mean of the control group at baseline.

Our long-run effects might seem particularly large. However, they represent a large percentage of changes from a very low baseline (see Table 1). It is also useful to benchmark our results to those in Colonnelli and Prem (2022) who study the economic consequences of a large Brazilian anti-corruption program. Compared to the 2% increase in the number of firms found in Colonnelli and Prem (2022), our 9% estimated effect is somewhat larger. Differences in context notwithstanding, our estimates may be larger for two reasons. First, the dismissal of entire local governments is likely a more dramatic change in local institutions than the Brazilian anti-corruption audits. Second, the CCD explicitly targets allegedly corrupt municipalities. In contrast, the Brazilian audits are randomly assigned to all municipalities, including those that may not have any corruption. Brazilian municipalities with less corruption face less legal action (Avis et al., 2018). Thus, we estimate a "treatment-on-the-treated" effect of changing a corrupt government while Colonnelli and Prem (2022) capture "intention-to-treat" effects.

CCDs' increases in the number of workers and firms do not translate into increases in the wage bill (Figure 3c), the sum of wages paid to all individuals employed in a given municipality. Instead, employment increases are offset by wage decreases (Figure 3d). After no immediate effect in the short run, wages decline and are, on average, 4.6% lower in the long run.

The negative effects on average wages are driven primarily by the entry of new workers

employed in low-paying jobs. The new entrant share of pre-CCD employment is 4.5 percentage points higher in treated municipalities than control municipalities in the long run (Figure 4a, blue squares), a 35% increase relative to the control group mean at baseline.<sup>12</sup> Similarly, the CCD increases the previously not-employed share of pre-CCD employment by 10.2 percentage points (Figure 4b, blue squares), a 40% increase over the baseline control group mean.<sup>13</sup> Because they tend to be employed in lower-paying jobs (Table 1), new workers drive down the average wage. This interpretation is corroborated by D.2, which shows that CCDs do not systematically change incumbent workers' wages.

Treated municipalities are primarily in Southern Italy, where informal employment is prevalent (Di Porto et al., 2016). Thus, the CCD's employment effect might be partially driven by transitions from the informal to the formal sector. The fact that the new individuals entering the formal labor market after the CCD are mostly young, however, suggest that is not the case (orange triangles in Figures 4a and 4b). If the effects on entry in the formal labor market were driven by older workers, that would suggest that the employment effects are reallocative because older workers are unlikely to have spent their entire adult lives without being employed at least once in the formal sector. The fact that CCDs can draw young workers in the labor market is important for two additional reasons. First, employment rates for the youth are extremely low in Southern Italy (Dolado, 2015) and our results suggest that CCDs effectively decrease youth unemployment rates. Second, the young are disproportionately likely to be recruited by criminal organizations, and breaking this pattern has proven to be quite difficult (Sviatschi, 2022).

Effects on Real Estate Demand. CCDs' effects on real estate demand provide further evidence that the firm and labor market effects reflect real increases rather than reallocation from the informal sector to the formal sector. Real estate prices are much less subject to underreporting than administrative employment data.<sup>14</sup> If CCDs increase economic activity, increases in input demand—both labor and land—should follow. Figure 5 reports CCDs' effects on industrial real estate prices, office real estate prices, residential real estate prices, and

 $<sup>^{12}</sup>$ The share of new entrants is constructed as the number of workers who appear for the first time in 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.

<sup>&</sup>lt;sup>13</sup>The share of "previously not-employed individuals" is defined as the fraction of workers who are employed in municipality m at time t but who do not appear in social security records at t-1 over baseline employment.

<sup>&</sup>lt;sup>14</sup>An alternative strategy is to measure increased economic activity via night-light data. Although several researchers have used the Defense Meteorological Satellite Program Operational Linescan System (DMSP) night-light data to proxy for GDP in developing countries, recent work by Gibson et al. (2021) shows that these data are a poor proxy for economic activity.

population.<sup>15</sup> CCDs' effects on industrial real estate prices are initially modest and not statistically significant and increase sharply three years after the CCD (Figure 5a), mirroring the employment effects (Figure 3a). Nine years after the intervention, industrial real estate prices grow by 15%. CCDs also increase office prices, but the effects are smaller and less precisely estimated and fade away (Figure 5b). Finally, CCDs do not impact residential real estate prices (Figure 5c) or population (Figure 5d), perhaps as a result of the typical low levels of mobility of Italians (Sánchez and Andrews, 2011).

Given the large increases in industrial real estate prices, we conclude that the increase in formal employment and the number of firms depicted in Figure 3 represents primarily an increase in overall economic activity as opposed to a reallocation from the informal to the formal sector.

#### 5.b Spillover Effects

We assess whether CCDs displace organized crime, negatively impacting the labor markets of neighboring municipalities. For each CCD, we select all the never-treated municipalities in a 20 km radius and match them with observationally similar control units using the matching algorithm described in Section 4.a. Figure 6 reports the results on log employment, number of firms, municipality wage bill, and average wages. Figures 6a and 6b show that the CCD generates a statistically significant increase in employment and the number of firms in surrounding municipalities in the short run and that the magnitude of these effects becomes larger over time. Like Figure 3, Figure 6d displays a negative effect on the average wages of workers employed in a small radius of treated units.

Because panels a and d present some evidence of non-parallel pre-trends, the magnitude of these estimates should be interpreted cautiously. However, the pattern of the results presented in Figure 6 is consistent with sizable and statistically significant long-run spillovers on nearby cities. This implies that the increase in economic growth in treated municipalities does not come does at the expense of losses in neighboring cities. These findings are in line with previous studies showing that CCDs have spillover effects on the spending and procurement of neighboring municipalities (Galletta, 2016; Tulli, 2019) and are likely to be driven by an increase in scrutiny in surrounding municipalities after the intervention (Marcolongo, 2020).

<sup>&</sup>lt;sup>15</sup>Industrial real estate includes factories, industrial buildings, and craft workshops.

#### 5.c Robustness

Online Appendix C shows that the results are not sensitive to (i) including socio-political variables in the matching algorithm,<sup>16</sup> (ii) not using weights, (iii) restricting the sample to the subset of municipalities that experience only one CCD, (iv) restricting the sample to the balanced panel, and (v) dropping all potential control municipalities within 20 km from any treated municipality.

As an additional robustness check, we also test robustness to matching treated units with potential control units in the *same* region. With this procedure, we match only 163 events. Table D.2 shows that the estimates are noisier and smaller in magnitude, as one would expect with a smaller sample and the presence of positive spillovers documented in Section 5.b. Nevertheless, the qualitative results are largely unchanged.

To summarize, CCDs increase employment, the number of firms operating in treated municipalities, and industrial real estate prices. Overall, they generate economic growth in highly depressed areas. The next section investigates potential mechanisms.

#### 6 Mechanisms

There are two alternative explanations for the economic effects of CCDs. First, they may spur economic growth *without* necessarily reducing the Mafia's entrenchment. For instance, they could increase transfers and resources from the central government. Similarly, the municipalities may implement policies with high economic returns like job training programs (Katz et al., 2022) or investments in public infrastructure (Donaldson, 2018). Moreover, substituting elected officials with experienced and presumably more competent public servants appointed by the central government (i.e., the re-centralization of power associated with a CCD) might independently generate positive economic effects (Bardhan and Mookherjee, 2000). Section 6.a presents evidence against this first explanation. First, we find that CCDs do not lead to substantial increases in government transfers and expenditure on job training programs and public investment. Second, using CCDs unrelated to Mafia infiltration, we show that the re-centralization of power has small economic effects.

The second explanation is that CCDs may generate economic effects *because* they weaken the Mafia. By eroding the Mafia's power, CCDs may be able to generate large and persistent economic effects. This interpretation is consistent with the Mafia operating as a tax on the local economy (Romer, 1994; Colonnelli et al., 2020). Section 6.b provides evidence consistent with

<sup>&</sup>lt;sup>16</sup>The socio-political variables we include are turnout at the previous election, a municipality-level indicator for high-Mafia prevalence, a coarse left-right measure of the local government political orientation, and the average age and educational level of local politicians.

CCDs weakening the Mafia. We find that CCDs increase the number of assets seized from the Mafia that are repurposed for social benefit, and the economic effects of CCDs are concentrated in Mafia-dominated sectors. We also find that CCDs lead to large political transformations, and the entirety of our economic effects is driven by municipalities that experience these large political swings.

#### 6.a Mechanisms Unrelated to Mafia Infiltration

**Government Transfers.** In principle, the central government may allocate additional financial resources to treated municipalities following a CCD, and increased spending may translate into increased economic activity. Importantly, this effect does not necessarily imply that CCDs also weaken the Mafia's local presence. In fact, Mafia-related businesses might be those that benefit from these additional resources (Daniele and Dipoppa, 2022). To test whether CCDs increase transfers from the central government, we estimate equation (1) using data on the revenues of Italian municipalities (Table D.4). We find no evidence that CCDs increase transfers as a share of local revenues in either the medium or long run (Column 3). While there is a positive and marginally significant immediate effect, the magnitude is too small to generate the large, long-run employment gains documented in Figure 3a.

**Expenditure on Other Policies.** Following a CCD, the external commissioners and the newly elected politicians may implement policies that generate large employment effects such as job training programs (Katz et al., 2022) or infrastructure investments (Donaldson, 2018). The implementation of these policies does not necessarily require a lesser presence of the Mafia. Table D.5 reports the results obtained by estimating equation (1) on municipality expenditures. There is little systematic impact of CCDs on expenditures in both the short and long run. Notably, we find no effect on "other social policies," an item that includes job training programs (Column 11). The effects on roads and infrastructure (Column 9) and educational policies (Column 5) are economically small and generally statistically insignificant. The newly elected politicians increase expenditure on sanitation, parks, and garbage collection in the short run (Column 10); however, the magnitude of the effect is economically small (see Figure D.3c) and is thus unlikely to explain the bulk of the economic effects of CCDs. More generally, Tables D.4 and D.5 show little evidence that CCDs concretely change government operations.

**Re-Centralization of Power.** The external commissioners appointed by the central government are selected among a pool of expert bureaucrats and may be more competent than the dismissed politicians. The increased competency may lead to higher long-run economic growth.

Importantly, these positive effects due to re-centralization caused by CCDs do not necessarily imply a lower infiltration of the Mafia.

To isolate the impact of substituting elected officials with experienced bureaucrats (i.e., re-centralization), we study the effect of CCDs that are caused by instances other than Mafia infiltration. These instances include (i) mayoral death, resignation, or impeachment; (ii) resignation of more than 50% of the city council; (iii) failure to pass a timely budget; (iv) serious violation of the law or constitution; and (v) lack of public order. Similar to Mafia-related CCDs, when the city council is dismissed, the central government appoints an external commissioner.

We use the same matched event-study research design to estimate the effects of Mafiaunrelated CCDs. Namely, we select municipalities that had a Mafia-unrelated CCD between 1991 and 2015 in one of the nine regions that constitute our main analysis sample and match them using our baseline matching algorithm. Figure 7 compares the estimated impact of CCDs due to Mafia infiltration (blue squares) with the impact of CCDs unrelated to Mafia infiltration (orange triangles), respectively. CCDs unrelated to Mafia infiltration have modest positive effects on employment and the number of firms (panels a and b), but the effects are significantly smaller than those for Mafia-related CCDs. We find no appreciable effects on wage bills and average wages (panels c and d). We conclude that CCDs can generate large economic effects only when they target infiltration caused by the Mafia.

#### 6.b Mechanisms Related to Mafia Infiltration

The previous sections rule out several channels through which CCDs may generate economic growth *without* weakening criminal organizations. This section provides evidence that CCDs weaken the Mafia and that this is the primary channel through which CCDs generate long-run economic development.

Assets Confiscated to the Mafia. Measuring changes in Mafia's influence across municipalities is challenging. Existing composite proxy measures of the Mafia's presence are either too coarse or time invariant (Calderoni, 2011; Dugato et al., 2020). Reporting issues notwithstanding, proxies constructed from news reporting of violent crimes (Dipoppa, 2021) have become less informative in recent years as Mafia tactics have become less violent.<sup>17</sup> To overcome these challenges, we leverage data on assets that are seized from the Mafia and are repurposed for public use, a direct measure of the intensity of the State's action against the Mafia. Specifically, we estimate equation (1) using as the outcome the cumulative number of assets confiscated from

<sup>&</sup>lt;sup>17</sup>Several commentators, for instance, argue that the Sicilian Mafia ("Cosa Nostra"—Our Thing) should now be called "Cosa Grigia" (Gray Thing), stressing its decreased visibility. Despite rarely resorting to violence, it remains strongly embedded in various aspects of society. For instance, see Di Girolamo (2012).

the Mafia and redistributed to the local population. In the long run, the cumulative increase in seized and repurposed assets is statistically significant, suggesting that the State erodes the Mafia's power (Figure 8). This corroborates other studies that find that CCDs reduce petty crimes (Cingano and Tonello, 2020) and violence against politicians (Baraldi et al., 2022).

Comparing Effects for Mafia-Related and Mafia-Unrelated Sectors. Gambetta (2000) argues that the Mafia's most important activity is the enforcement of monopolies over the largest possible number of resources in any given territory. If CCDs weaken the Mafia's ability to enforce monopolies, the increase in the number of businesses would be concentrated in sectors where the Mafia has traditionally held monopolies.

We test this prediction by analyzing how CCDs impacted the number of firms in sectors defined by the Anti-Mafia Dictorate as being at "high risk of Mafia infiltration."<sup>18</sup> Figure 9a displays event-study coefficients from equation (1) where the outcome is log number of firms in Mafia and non-Mafia sectors. The evidence suggests that the positive effects of CCDs on the number of firms are entirely coming from Mafia sectors (blue squares). The number of firms in the remaining sectors does not appear to grow post-CCD (orange triangles). Similarly, employment growth is much more pronounced in Mafia sectors (blue squares) while being statistically insignificant in non-Mafia sectors (orange triangles). It thus appears that the totality of our economic effects is driven by sectors that used to represent Mafia strongholds.

Political Effects of CCDs. Criminal organizations use their power to influence electoral results (Alesina et al., 2018). If CCDs weaken the Mafia, voters may elect different politicians in the aftermath of the policy. To test for this, we build on Daniele and Geys (2015b) and estimate equation (1) using observable characteristics of local politicians as outcome variables.<sup>19</sup> The blue squares in Figure 10 report the results. Politicians in treated and control municipalities have similar characteristics before the CCD event (Table D.1). After the event, politicians differ significantly (Figure 10). For instance, the share of first-time politicians elected after the CCD increases by 13 percentage points (almost 24% increase relative to its mean). Newly elected politicians are also about two years younger and have higher educational attainments than their predecessors; however, the estimates for the latter are somewhat noisy. Notably, treated municipalities are 6 percentage points more likely to elect a female representative. Considering the low levels of female political representation in our setting—the female share is around

<sup>&</sup>lt;sup>18</sup>Namely, the sectors are construction, waste disposal, gambling, extraction, supply and transportation of inert materials, concrete production, dry lease of machines, third-party transportation, and supply of manufactured iron. See article 5-bis of law n. 122/2012.

<sup>&</sup>lt;sup>19</sup>The coefficients at event time 0 and 1 are missing because in those years, treated municipalities are administrated by external commissioners.

10%—this represents a very large effect (a 67% increase relative to the mean at baseline).

The election of female representatives contrasts starkly with the patriarchal view of society that is perpetuated by the Mafia (Fiandaca, 2007). More broadly, CCDs cause voters to elect first-time politicians who are younger, more educated, and more likely to be female, all factors generally associated with less corruption (e.g., Decarolis et al., 2020). These results are thus consistent with a weakened political influence of the Mafia.

We argue that CCD-driven changes in the type of elected officials are indicative of shifts in voter preferences. One concern with this interpretation is that CCDs may induce young, female, and college-educated candidates to run for local offices for the first time. Rather than reflecting voter preferences, this may reflect the changes in the supply of candidates. However, Baraldi and Immordino (2021) show that CCDs do not change the characteristics of political candidates *running* for local elections.

Another concern with our interpretation is that CCDs may, almost mechanically, eliminate the incumbency advantage without necessarily reflecting systematic changes in the underlying political climate. To evaluate this claim, we exploit Mafia-unrelated CCDs. If the change in the composition of elected officials reflected the elimination of the incumbency advantage, then the estimated impact of Mafia-related and Mafia-unrelated CCDs should be similar. The blue squares and the orange triangles in Figure 10 report the results for Mafia-related and Mafia-unrelated CCDs, respectively. Mafia-unrelated CCDs do not generate changes in the characteristics of elected officials, while Mafia-related CCDs do.<sup>20</sup>

We therefore conclude that the changes in the type of elected officials are reflective of shifts in voter preferences as opposed to changes in the supply of politicians and the elimination of the incumbency advantage.

**Economic Effects When CCDs Change Local Politicians.** We conclude by showing that the economic effects of CCDs are concentrated in municipalities where the dismissal severs the ties between the Mafia and the local politicians. Conversely, in municipalities where the Mafia retains influence over the local government, CCDs do not generate economic growth.

We proxy for whether CCDs weakened connections between the Mafia and local politicians using the *change* in the share of non-incumbent politicians following a CCD. Given that the dismissed politicians were found to be connected to organize crime, CCDs are unlikely to have broken the connections between the Mafia and the local government if municipalities re-elect a large share of dismissed officials after the CCD. On the other hand, CCDs are likely to have

 $<sup>^{20}</sup>$ Mafia-unrelated CCDs generate a small short-lived increase in the share of first-time politicians. This effect is driven by the fact that more than 80% of Mafia-unrelated CCDs are triggered by the resignation of the mayor or a large fraction of the city council members. Officials who resign typically do not run in the following elections.

severed (some of) the ties between the local government and organized crime if the municipalities elect different politicians after the CCD.

We estimate the effects of Mafia-related CCDs separately in locations that experienced an above- versus below-median change. Figure 11a shows that the employment gains are driven by municipalities that experienced a large change in their political class. In fact, municipalities that had a below-median change in the composition of politicians did not experience any significant employment effects after the intervention. Figure 11b shows that the number of firms display the same pattern.

Moreover, recall that Mafia-unrelated CCDs see neither a change in local politicians (Figure 10) nor a change in local economic activity (Figure 7). We thus conclude that the results presented in this section provide evidence that CCDs generate economic growth primarily by weakening the Mafia.

Summary. Mafias operate via violence and fear. Increased risk of victimization and rent extraction suppresses economic activity (Pinotti, 2015a). By dismissing the city council, the central government sends a strong signal that Mafia infiltration in the local government should not be tolerated. This intervention erodes the Mafia's power and thus makes citizens update their beliefs on whether the State can fight organized crime. CCDs therefore help municipalities transitioning from a climate of risk to a climate of trust. This can be appreciated by several facts: i) the increase in the number of firms in sectors historically dominated by the Mafia (Figure 9a), (ii) the increase in business-related real estate prices (Figure 5a), and (iii) the increase in the employment of young individuals who are more likely to be recruited by organized criminal organizations (Sviatschi, 2022) (Figure 4a).

The shift in climate caused by CCDs is also evident from the political swings shown in Figure 10, with municipalities now electing different types of politicians who are more likely to be running for the first time, younger, women, and with higher levels of education. In those instances where CCDs do not weaken the Mafia—either because the CCD is unrelated to Mafia infiltration or because it ultimately does not lead to changes in newly elected politicians—we do not find sizable economic effects. We thus conclude that the economic effects of CCDs are driven by the erosion of the Mafia's power. The resulting renewed sense in trust in both institutions and local economy leads to persistent economic growth.

#### 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 the long-term economic consequences of these actions. This paper attempts to fill this 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.

Our results suggest a few important insights. First, CCDs allow the central government to reassert its legitimacy in areas where criminal organizations have been active for centuries and also spur economic growth. We find that the CCDs increase employment and the number of firms. Treated municipalities also display higher economic dynamism and a surge in industrial real estate prices after the intervention. Moreover, the policy has positive spillover effects on neighboring cities. The increase in economic activity in treated municipalities does not come at the expense of employment losses in the surrounding cities.

Second, the short-run impact of policies aimed at reasserting the State's legitimacy may underestimate the long-run impact. Our results suggest that CCDs generate economic growth by weakening the Mafia and fostering trust in local institutions. However, the impact of the policy materializes only a few year after the dismissal, suggesting that it takes time to eradicate criminal organizations and build trust in local authorities.

Third, the attitudes of the residents of treated municipalities toward criminal organizations may determine the policy's effectiveness. In our setting, support from the central government lent support to the policy. However, in other contexts where the central government is viewed as very unfavorably by the local population, a policy like a CCD might actually generate a strong backlash (Blattman et al., 2021; Sviatschi, 2020; Dell, 2015).

Fourth, directly targeting local institutions infiltrated by criminal organizations may have larger returns than only targeting illegal activities (e.g., drug trafficking, money laundering, and homicides).

We conclude by noting an interesting question that emerges from our analysis: why did the Mafia not fight back after the CCD either by trying to re-establish its position in affected municipalities or by expanding to nearby cities? Baraldi et al. (2022), for instance, shows that there is a *decrease* in Mafia violence following a CCD both in treated municipalities and in neighboring municipalities. A possible explanation is that the Mafia has radically changed its modus operandi in the last 30 years. In particular, many commentators argue that the Mafia now believes that violent confrontation with the central government is bad for business (Di Girolamo, 2012). Examining how different types of organized crime—from the more recent organizations in South America to more mature ones, such as the Italian Mafia—respond to policies aimed at increasing the State's legitimacy represents an interesting avenue for future research.

#### References

- Acconcia, Antonio, Giancarlo Corsetti, and Saverio Simonelli, "Mafia and Public Spending: Evidence on the Fiscal Multiplier from a Quasi-experiment," *American Economic Review*, 2014, 104 (7), 2185–2209.
- Acemoglu, Daron, "A Simple Model of Inefficient Institutions," The Scandinavian Journal of Economics, 2006, 108 (4), 515–546.
- \_, Giuseppe De Feo, and Giacomo Davide De Luca, "Weak States: Causes and Consequences of the Sicilian Mafia," *The Review of Economic Studies*, 02 2019. rdz009.
- Alesina, Alberto, Salvatore Piccolo, and Paolo Pinotti, "Organized Crime, Violence, and Politics," The Review of Economic Studies, 07 2018, 86 (2), 457–499.
- Avis, Eric, Claudio Ferraz, and Frederico Finan, "Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians," *Journal of Political Economy*, 2018, 126 (5), 1912–1964.
- Bandiera, Oriana, "Land Reform, the Market for Protection, and the Origins of the Sicilian Mafia: Theory and Evidence," *Journal of Law, Economics and Organization*, April 2003, 19 (1), 218–244.
- **Baraldi, Anna and Giovanni Immordino**, "Self-Selecting Candidates or Compelling Voters: How Organized Crime Affects Political Selection," 2021. Working Paper.
- Baraldi, Anna Laura, Erasmo Papagni, and Marco Stimolo, "Neutralizing the Tentacles of Organized Crime. Assessment of an Anti-Crime Measure in Fighting Mafia Violence," 2022.
- Bardhan, Pranab K and Dilip Mookherjee, "Capture and governance at local and national levels," *American economic review*, 2000, *90* (2), 135–139.
- Blattman, Christopher, Gustavo Duncan, Benjamin Lessing, and Santiago Tobón, "Gang rule: Understanding and Countering Criminal Governance," NBER Working Papers 28458, National Bureau of Economic Research, Inc February 2021.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event study designs: Robust and efficient estimation," *arXiv preprint arXiv:2108.12419*, 2021.
- Calamunci, Francesca and Francesco Drago, "The economic impact of organized crime infiltration in the legal economy: evidence from the judicial administration of organized crime firms," *Italian Economic Journal*, 2020, pp. 1–23.

- Calderoni, Francesco, "Where is the mafia in Italy? Measuring the presence of the mafia across Italian provinces," *Global Crime*, 2011, 12 (1), 41–69.
- Cingano, Federico and Marco Tonello, "Law enforcement, social control and organized crime: Evidence from local government dismissals in Italy," *Italian Economic Journal*, 2020, 6 (2), 221–254.
- CNEL, I Consigli Comunali sciolti per Infiltrazioni Mafiose 1995.
- Colonnelli, Emanuele and Mounu Prem, "Corruption and firms," The Review of Economic Studies, 2022, 89 (2), 695–732.
- \_ , \_ , and Edoardo Teso, "Patronage and Selection in Public Sector Organizations," American Economic Review, October 2020, 110 (10), 3071–99.
- Daniele, Gianmarco and Benny Geys, "Exposing politicians? Ties to criminal organizations: the effects of local government dissolutions on electoral outcomes in southern Italian municipalities," Working Papers 2015/41, Institut d'Economia de Barcelona (IEB) 2015.
- and \_ , "Organised Crime, Institutions and Political Quality: Empirical Evidence from Italian Municipalities," The Economic Journal, 2015, 125 (586), F233–F255.
- and Gemma Dipoppa, "Fighting Organized Crime by Targeting their Revenue: Screening, Mafias, and Public Funds," The Journal of Law, Economics, and Organization, 2022.
- \_ , Marco Le Moglie, and Federico Masera, "Pains, Guns and Moves: The Effect of the US Opioid Epidemic on Mexican Migration," BAFFI CAREFIN Working Papers 20141, BAFFI CAREFIN, Centre for Applied Research on International Markets Banking Finance and Regulation, Universita' Bocconi, Milano, Italy 2020.
- Decarolis, Fransco, Raymond Fisman, Paolo Pinotti, and Silvia Vannutelli, "Rules, Discretion, and Corruption in Procurement: Evidence from Italian Government Contracting," Technical Report, Mimeo 2020.
- Dell, Melissa, "Trafficking Networks and the Mexican Drug War," American Economic Review, June 2015, 105 (6), 1738–79.
- **Dipoppa, Gemma**, "How Criminal Organizations Expand to Strong States: Migrant Exploitation and Political Brokerage in Northern Italy," 2021.
- **Dolado, J**, "No country for young people," Youth labour market problems in Europe. London, 2015.

- **Donaldson, Dave**, "Railroads of the Raj: Estimating the impact of transportation infrastructure," *American Economic Review*, 2018, *108* (4-5), 899–934.
- Dugato, Marco, Francesco Calderoni, and Gian Maria Campedelli, "Measuring organised crime presence at the municipal level," *Social Indicators Research*, 2020, 147 (1), 237–261.
- Fiandaca, Giovanni, Women and the mafia: Female roles in organized crime structures, Vol. 5, Springer Science & Business Media, 2007.
- Galletta, Sergio, "Law enforcement, municipal budgets and spillover effects: Evidence from a quasi-experiment in Italy," IdEP Economic Papers 1601, USI Università della Svizzera italiana January 2016.
- Gambetta, Diego, "Mafia: the price of distrust," *Trust: Making and breaking cooperative relations*, 2000, 10, 158–175.
- Gibson, John, Susan Olivia, Geua Boe-Gibson, and Chao Li, "Which night lights data should we use in economics, and where?," *Journal of Development Economics*, 2021, 149, 102602.
- Girolamo, Giacomo Di, Cosa Grigia, Il saggiatore, 2012.
- Goldschmidt, Deborah and Johannes F Schmieder, "The rise of domestic outsourcing and the evolution of the German wage structure," *The Quarterly Journal of Economics*, 2017, 132 (3), 1165–1217.
- Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," Technical Report, National Bureau of Economic Research 2018.
- Jäger, Simon, "How substitutable are workers? evidence from worker deaths," Evidence from Worker Deaths, 2019.
- Katz, Lawrence F, Jonathan Roth, Richard Hendra, and Kelsey Schaberg, "Why do sectoral employment programs work? Lessons from WorkAdvance," *Journal of Labor Economics*, 2022, 40 (S1), S249–S291.
- Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach, "School finance reform and the distribution of student achievement," *American Economic Journal: Applied Economics*, 2018, 10 (2), 1–26.

- Le Moglie, Marco and Giuseppe Sorrenti, "Revealing "Mafia Inc."? Financial Crisis, Organized Crime, and the Birth of New Enterprises," April 2020. Working Paper.
- Leff, Nathaniel H, "Economic development through bureaucratic corruption," American behavioral scientist, 1964, 8 (3), 8–14.
- Marcolongo, Giovanna, "Organized Crime, Earthquakes and Local Public Procurement: Evidence from Italy," March 2020. Working Paper.
- Melnikov, Nikita, Carlos Schmidt-Padilla, and Maria Micaela Sviatschi, "Gangs, Labor Mobility and Development," Working Paper 27832, National Bureau of Economic Research September 2020.
- Mete, Vittorio, Fuori dal Comune: Lo scioglimento delle amministrazioni locali per infiltrazioni mafiose, Buonanno, 2009.
- Mirenda, Litterio, Sauro Mocetti, and Lucia Rizzica, "The economic effects of mafia: Firm level evidence," *American Economic Review*, 2022, *112* (8), 2748–73.
- Pinotti, Paolo, "The Economic Costs of Organised Crime: Evidence from Southern Italy," The Economic Journal, 2015, 125 (586), F203–F232.
- \_, "The Causes and Consequences of Organised Crime: Preliminary Evidence Across Countries," The Economic Journal, 08 2015, 125 (586), F158–F174.
- Porto, Edoardo Di, Leandro Elia, and Cristina Tealdi, "Informal work in a flexible labour market," Oxford Economic Papers, 2016, 69 (1), 143–164.
- Romer, Paul, "New goods, old theory, and the welfare costs of trade restrictions," *Journal of development Economics*, 1994, 43 (1), 5–38.
- Sánchez, Aida Caldera and Dan Andrews, "Residential mobility and public policy in OECD countries," OECD Journal: Economic Studies, 2011, 2011 (1), 1–22.
- Slutzky, Pablo and Stefan Zeume, "Organized Crime and Firms: Evidence from Italy," 2019. Working Paper.
- \_ , Mauricio Villamizar-Villegas, and Thomas Williams, "Drug Money and Bank Lending: The Unintended Consequences of Anti-Money Laundering Policies," 2019. Working Paper.
- Sviatschi, Maria Micaela, "Making a narco: Childhood exposure to illegal labor markets and criminal life paths," *Econometrica*, 2022, *90* (4), 1835–1878.

- Sviatschi, Micaela, "Spreading Gangs: Exporting US Criminal Capital to El Salvador," September 2020. Working Paper.
- Tulli, Andrea, "Sweeping the Dirt Under the Rug: Measuring Spillovers from an Anti-Corruption Measure," 2019. Working Paper.
- UNICRI, United Nations Interregional Crime and Justice Research Institute, "Organized Crime and the Legal Economy," Technical Report, Torino 2016.

### 8 Figures



Figure 1: Number of CCDs

Notes: This figure summarizes the time variation in the number of CCDs due to Mafia infiltration between 1991 and 2016.



Figure 2: Spatial Variation in CCDs

Notes: This map shows the counts of CCDs for each of the 110 Italian provinces between 1991 and 2016.



Figure 3: Effects of CCDs on Employment, Number of Firms, and Wages

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 CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. Quantitative results are summarized in Table 2.

Figure 4: Effects of CCDs on New Entrants and Previously Not Employed Workers as a Share of Baseline Employment



Notes: Matched municipality sample, INPS data (1983–2017). 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 CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. The share of new entrants is defined as the number of workers who appear for the first time in social security records in year t and municipality m over the employment level in the same municipality in the year before the CCD. The share of previously not-employed individuals is constructed as the number of workers who are employed in municipality m at time t but who do not appear in social security records at t - 1 over the employment level in the same municipality in the year before the CCD. "All" refers to all workers in the economy (blue squares). "Young" is defined as 30 years old or younger (orange triangles). Quantitative results are summarized in Table 3.



Figure 5: Effects of CCDs on Municipality Population and Real Estate Prices

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 report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. The outcome variables are industrial real estate prices (panel a), office real estate prices (panel b), residential real estate prices (panel c), and municipality-level population (panel d), all expressed in logarithms. The x-axis indexes event time. Quantitative results are summarized in Table 4.



Figure 6: Spillover Effects of CCDs on Employment, Firms, and Wages (20-km Radius)

Notes: Matched spillover municipality sample in a 20 km radius, INPS data (1983–2017). Panels a-d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time.



Figure 7: Effects of CCDs Unrelated to Mafia Infiltration

Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. The baseline estimates from Figure 3 are reported for comparability and are denoted by the blue squares in all panels. Each panel compares the estimates of CCDs due to Mafia infiltration (blue squares) with those of CCDs unrelated to Mafia infiltration (orange triangles).



Figure 8: Effects of CCDs on Repurposed Assets

Notes: Matched municipality sample, ABNSC data (1983–2019). This figure displays the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. The outcome variable is the cumulative number of assets seized from the Mafia that are repurposed for social benefit. Assets include both firms and real estate.

Figure 9: Effects of CCDs in Mafia and Non-Mafia Sectors



Notes: Matched municipality sample, INPS data (1983–2017). This figure displays the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. The outcome variables are the log number of firms (panel a) and the log number of employees (panel b) in sectors at risk of Mafia infiltration (blue squares) and the log number of firms in sectors not at risk of Mafia infiltration (orange triangles).



Figure 10: Effects of CCDs on the Characteristics of Elected Politicians

Notes: Matched municipality sample, Ministry of the Interior data (1986–2020). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. Coefficients at 0 and 1 are missing because in those years treated municipalities are administrated by the external commissioners. The outcome variables are the municipality-level characteristics of elected politicians, namely the share of first-time politicians (panel a), the share of male politicians (panel b), the average highest educational attainment (panel c), and the average age (panel d). We define the highest educational attainment as in Daniele and Geys (2015b). The x-axis indexes event time. Each panel compares the estimates of CCDs due to Mafia infiltration (blue squares) with those of CCDs unrelated to Mafia infiltration (orange triangles).

Figure 11: Heterogeneous Effects of CCDs on Employment and Number of Firms



Below-Median Share of Non-Incumbent Politicians A- Above-Median Share of Non-Incumbent Politicians

Notes: Matched municipality sample, INPS data (1983–2017). Panels a and b report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. The x-axis indexes event time. Model (1) is estimated separately for municipalities that experienced above-/below-median changes in the share of non-incumbent politicians (blue triangles/orange squares). The outcome variables are log employment (panel a) and log number of firms (panel b).

### 9 Tables

	(1)	(2)	(3)
	Matched Sample	Т	Ċ
Population in 1991	15263.83	15522.71	15004.95
N Establishments	260.97	229.60	292.34
N Firms	250.80	220.93	280.67
N Sole Proprietorship	132.51	113.11	151.91
N of Employees	2348.95	1572.30	3125.61
Av. Daily Wage	72.74	73.21	72.28
Av. Daily Wage: Prev. Not Employed	63.56	64.07	63.04
Av. Daily Wage: Prev. Employed	74.10	74.06	74.14
Municipal Wage Bill (Millions of $$	41.21	20.16	62.26
Share New Entrants	0.14	0.15	0.13
Share New Entrants under 30 y.o.	0.10	0.11	0.10
Share Prev. Not Employed	0.26	0.28	0.25
Share Prev. Not Employed under 30 y.o.	0.15	0.16	0.14
Share Firm Entries	0.14	0.14	0.13
Share Firm Exists	0.10	0.10	0.10
Turnout	0.78	0.77	0.79
Observations	422	211	211

Table 1: Municipality Characteristics in the Year before the CCD

*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 in the year before the CCD. Column 1 reports statistics on the full matched sample, and columns 2 and 3 limit the sample to treated and control municipalities, respectively.

	(1)	(2)	(3)	(4)
	Log(Empl)	Log(N Firms)	Log(Wage Bill)	Log(Wages)
On Impact	0.019	0.013	-0.012	-0.004
	(0.013)	(0.008)	(0.012)	(0.009)
Short Run	0.070	0.014	-0.004	-0.014
	(0.030)	(0.017)	(0.030)	(0.011)
Long Run	0.169	0.094	0.025	-0.046
	(0.056)	(0.035)	(0.057)	(0.016)
Mean	6.196	4.379	15.073	4.236
Ν	$14,\!654$	$14,\!654$	$14,\!654$	$14,\!654$
Muni FE	Yes	Yes	Yes	Yes
Reg-Year FE	Yes	Yes	Yes	Yes

Table 2: Effects of CCDs on Municipality Employment, Wages, and Firms

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated  $\theta_k$  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 results in graph format are reported in Figure 3.

	(1)	(2)	(3)	(4)	(5)	(6)
	Share New	Share New	Share Prev.	Share Prev.	Share	Share
	Entrants	Entrants	Not Empl.	Not Empl.	Firm	Firm
		< 30 y.o.		< 30 y.o.	Entries	Exits
On Impact	0.001	-0.001	0.013	0.006	0.006	0.006
	(0.008)	(0.006)	(0.016)	(0.009)	(0.007)	(0.006)
Short Run	0.010	0.008	0.034	0.0181	0.011	0.006
	(0.009)	(0.007)	(0.019)	(0.010)	(0.010)	(0.008)
Long Run	0.045	0.035	0.102	0.053	0.061	0.043
	(0.014)	(0.010)	(0.025)	(0.013)	(0.016)	(0.014)
Mean	0.176	0.117	0.303	0.163	0.15	0.121
Ν	$14,\!654$	$14,\!654$	$14,\!654$	$14,\!654$	$14,\!654$	$14,\!654$
Muni FE	Yes	Yes	Yes	Yes	Yes	Yes
Reg-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3: Effects of CCDs on Entries and Exits

Notes: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated  $\theta_k$  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 in the matched sample. 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 results in graph format are reported in Figure 4.

	(1)	(2)	(3)	(4)
	Log Industrial Real	Log Office Real	Log House Real	Log Population
	Estate Prices	Estate Prices	Prices	
On Impact	0.0102	0.0229	-0.0100	0.0297
	(0.0132)	(0.0187)	(0.0147)	(0.0304)
Short Run	0.0658	0.0725	0.0327	0.0196
	(0.0332)	(0.0343)	(0.0298)	(0.0301)
Long Run	0.1435	0.0567	0.0072	-0.0120
	(0.0437)	(0.0535)	(0.0483)	(0.0360)
Mean	6.01	6.71	6.606	8.903
Ν	$2,\!474$	$2,\!453$	2,860	7,462
Muni FE	Yes	Yes	Yes	Yes
Reg-Year FE	Yes	Yes	Yes	Yes

Table 4: Effects of CCDs on Municipality Population and Real Estate Prices

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  $\theta_k$  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 results in graph format are reported in Figure 5.

### For Online Publication

Online Appendix for "Organized Crime and Economic Growth: Evidence from Mafia-Infiltrated Municipalities"

Alessandra Fenizia Raffaele Saggio

### 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 oversees 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 on the city council and on the executive committee.

#### Additional Details on CCDs

As we discussed in Section 2, the CCD aims at severing 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 Ministry of the Interior's representative in the province *(prefetto)* is required to inform law enforcement authorities and can suspend the allegedly corrupt bureaucrat or move them to another office during the police investigation.

Regarding mandate length, the external commissioners inherit the powers of the dismissed administrative and executive bodies and run the municipality for 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.

Finally, the Ministry of the Interior's decision 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*).

#### 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 daily wages (municipality level): the average daily wages paid to formal private sector workers employed in municipality m in year t.

**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** (municipality level):

- Administration: expenditures on the local government's day-to-day administration.
- Justice system: 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) 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 handle 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.
- Sports: 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.
- Sanitation: expenditure on garbage collection, sanitation, local landscape maintenance, and pollution monitoring and reduction.
- Other expenditures: other expenditures of the municipality. These include, for example, expenditures on social assistance and local economic development.

Loans (municipality level): revenue generated from loans contracted by the municipality.

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

**Other revenues** (municipality level): other revenue of the municipality. These include, for example, revenue from fines, administrative penalties, and insurance compensations as well as revenue 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.

Share of first worker appearances (municipality level): the number of workers who appear for the first time in 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 INPS in municipality m in year t over the number of businesses operating in municipality m in the year before the CCD.

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

Taxes (municipality level): local taxes collected by the municipality.

**Transfers** (municipality level): 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 : Robustness Checks

Our main results are robust to a variety of alternative specifications. Specifically, we show that our main results are not sensitive to (i) including socio-political variables in the matching algorithm, (ii) not using weights, (iii) restricting the sample to the subset of municipalities that experience only one CCD, (iv) restricting the sample to the balanced panel, and (v) dropping all potential control municipalities in a 20 km radius of any treated unit.

#### C.1 Alternative Matching Algorithms

The matching algorithm presented in Section 4.a matches treated and control units on baseline economic characteristics. If treatment municipalities are characterized by a very different sociopolitical environment, one concern is that the control units may not represent an adequate counterfactual. To address this concern, we include several socio-political variables in the matching algorithm and evaluate whether our results are sensitive to the set of variables we add. We proceed in two steps. We start by including a basic set of socio-political variables, namely turnout at the previous election, a municipality-level indicator for high-Mafia prevalence, and a coarse left-right measure of the local government political orientation at t-1 (where t is the year in which the CCD event occurred).<sup>21</sup> Next, we add the baseline average age and educational level of local politicians at t-1.

Figure C.1 compares the baseline estimates from Figure 3 (blue squares) with those obtained from augmenting the matching algorithm with a basic set of socio-political variables (green circles) and with a larger set of socio-political variables (orange triangles), respectively. Our results on employment, number of firms, and average wages are not sensitive to the set of variables we include in the matching algorithm. When we include socio-political variables in the matching procedure, the long-run estimates of the CCDs' impact on the wage bill are larger in magnitude although not statistically significant. Given the size of the confidence intervals, we prefer to be conservative and use the baseline coefficients as our preferred estimates.

#### C.2 Weights

Another concern is that our results may be driven by the weights we use. As a robustness check, Figure C.2 compares the baseline estimates from Figure 3 (blue squares) with those obtained from estimating model (1) without weights (orange triangles). As our results are unchanged, we conclude that our main findings are not sensitive to the weights we use.

<sup>&</sup>lt;sup>21</sup>We define as high-Mafia presence all the municipalities that exhibit an above-mean Mafia index (Dugato et al., 2020). Our measure of political orientation ranges from -1 (left wing) to 1 (right wing).

#### C.3 Municipalities with Only One CCD

As discussed in Section 4.c, our baseline specification includes municipalities that experience multiple CCDs during the period of study. Following Jäger (2019), we duplicate the lines for these municipalities and allow for different fixed effects. Although this is a fairly standard approach, one may be concerned that municipalities that are treated multiple times may be somewhat different from the average treated unit and may be disproportionately driving our main findings. To address this concern, we estimate model (1) on the subset of municipalities that experience only one CCD. Figure C.3 compares the baseline estimates from Figure 3 (blue squares) with those obtained from estimating model (1) on the subsample of municipalities that experience only one CCD (orange triangles). The pattern of results is unchanged, although the standard errors are marginally larger due to the smaller sample size. We conclude that our results are robust to excluding municipalities that are treated multiple times.

#### C.4 Balanced Panel

Because INPS data end in 2017, we cannot track the outcomes of municipalities dismissed after 2008 for nine full years after the CCD. To address the concerns relative to the unbalanced nature of our data, we estimate model (1) on the subset of municipalities treated before 2009 (balanced sample). Figure C.4 compares the baseline estimates from Figure 3 (blue squares) with those obtained on the balanced sample (orange triangles). Our results are virtually unchanged, suggesting that the unbalanced nature of our data is not driving our main findings. If anything, the impacts estimated on the balanced panel appear larger in size than our baseline impacts, although they are not statistically different.

#### C.5 Dropping Potential Controls within 20 km

One additional concern is that the control municipalities may be indirectly affected by spillovers from other treated municipalities. To address this concern, we drop all municipalities within a 20 km radius from any treated unit from the set of potential control municipalities and reestimate the matching algorithm. Figure C.5 compares the baseline estimates from Figure 3 (blue squares) with those obtained from estimating model (1) on the matched sample obtained from discarding all potential controls in a 20 km radius of any treated municipality (orange triangles). As our results on employment and number of firms are virtually unchanged, we conclude that our main results are robust to dropping potential controls that may be affected by the spillovers. When we use this alternative matched sample, the coefficients on the wage bill are larger in magnitude (albeit not statistically significant) and the impacts on wages are more muted than in the baseline specification. Given the size of the confidence intervals, we prefer to be conservative and use the baseline coefficients as our preferred estimates.



Figure C.1: Robustness: Alternative Matching Algorithms

Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. The baseline estimates from Figure 3 are reported for comparability and are denoted by the blue squares in all panels. Each compares the baseline estimates (blue squares) with those obtained from augmenting the matching algorithm with a basic set of sociopolitical variables (green circles) and with a large set of socio-political variables (orange triangles), respectively. The small set of political variables includes turnout at the previous local elections, a municipality-level indicator for high-Mafia presence, and political orientation. The large set of political variables also includes the average age and education of local politicians.



Figure C.2: Robustness: No Weights

Notes: Matched municipality sample, INPS data (1983–2017). Panels a-d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. The baseline estimates from Figure 3 are reported for comparability and are denoted by the blue squares in all panels. Each panel compares the baseline estimates (blue squares) with those obtained from estimating model (1) without weights (orange triangles).



Figure C.3: Robustness: Municipalities with Only One CCD

Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. The baseline estimates from Figure 3 are reported for comparability and are denoted by the blue squares in all panels. Each panel compares the baseline estimates (blue squares) with those obtained from estimating model (1) on the subsample of municipalities that experience only one CCD over the study period (orange triangles).



Figure C.4: Robustness: Balanced Sample

Notes: Matched municipality sample, INPS data (1983–2017). Panels a-d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. The baseline estimates from Figure 3 are reported for comparability and are denoted by the blue squares in all panels. Each panel compares the baseline estimates (blue squares) with those obtained from estimating model (1) on the balanced sample (orange triangles).



Figure C.5: Robustness: Dropping Potential Controls within 20 km

Notes: Matched municipality sample, INPS data (1983–2017). Panels a-d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, 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 (panel a), log number of firms (panel b), log wage bill (panel c), and log average wages (panel d). The x-axis indexes event time. The baseline estimates from Figure 3 are reported for comparability and are denoted by the blue squares in all panels. Each panel compares the baseline estimates (blue squares) with those obtained from estimating model (1) on the matched sample obtained from discarding all potential controls in a 20 km radius from any treated municipality (orange triangles).

### Appendix D : Additional Figures and Tables



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

(b)



*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.





Notes: Matched municipality sample, INPS data (1983–2017). This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. The outcome variable is log average wages for incumbent workers attached to the labor market. The x-axis indexes event time.



Figure D.3: Effects of CCDs on Expenditures

Notes: Matched municipality sample, Ministry of the Interior data (1998–2015). This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the  $\hat{\theta}^k$  from equation (1). The coefficients at k = -1 are normalized to zero. Panels a and b represent the share of municipality expenditure devoted to expenses in the administration of the justice system and policing relative to the overall budget, respectively. Panel c and d show expenditures on sanitation/garbage collection and roads and infrastructure. See Appendix B for details. The x-axis indexes event time. The results in table format are reported in Table D.5.

	(1)	(2)	(3)	(4)
	Matched Sample	Т	$\mathbf{C}$	Ν
Panel A: Real Estate Prices and Rent	ts			
Sale Price: Housing	886.59	799.63	967.92	194
Sale Price: Commercial Real Estate	832.50	746.57	911.64	191
Sale Price: Office Real Estate	934.24	884.18	983.06	170
Sale Price: Industrial Real Estate	463.98	461.62	466.20	168
Sale Price: Parking	549.72	513.49	580.59	164
Panel B: Population and Public Fina	nces			
Population	20273.68	19323.31	21276.53	239
Revenue (in Thousands)	$25,\!043$	$23,\!820$	$26,\!333$	239
Taxes/Revenue	0.32	0.29	0.34	239
Expenditure/Revenue	0.79	0.78	0.80	239
Panel C: Characteristics of Public Ele	ected Officials			
Share of First-Time Politicians	0.53	0.53	0.54	403
Share of Male Politicians	0.91	0.93	0.88	403
Education	13.21	13.35	13.08	403
Age	44.46	44.23	44.67	403

#### Table D.1: Additional Municipality Characteristics

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 the register of local politicians (1986–2020). Treated municipalities are matched to out-of-region potential control municipalities. All statistics are calculated across municipality-year observations at k = -1. Column 1 reports statistics on the full matched sample, and columns 2 and 3 limit the sample to treated and control municipalities, respectively.

	(1)	(2)	(3)	(4)
	Log(Empl)	Log(N Firms)	Log(Wage Bill)	Log(Wages)
On Impact	-0.006	0.006	-0.021	-0.012
	(0.013)	(0.008)	(0.016)	(0.012)
Short Run	0.043	0.024	-0.003	-0.004
	(0.030)	(0.018)	(0.03323)	(0.014)
Long Run	0.073	0.063	0.061	-0.032
	(0.055)	(0.035)	(0.058)	(0.020)
Mean	6.076	4.317	15.29	4.604
Ν	$11,\!400$	11,400	11,400	$11,\!400$
Muni FE	Yes	Yes	Yes	Yes
Reg-Year FE	Yes	Yes	Yes	Yes

Table D.2: Effects of CCDs on Municipality Employment, Wages, and Firms (Matching within Region)

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  $\theta_k$  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.

	(1)	(2)	(3)
	Housing	Industrial Real	Office Real
	Rents	Estate Rents	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	0.934	0.670	1.336
Ν	2,833	2,429	$2,\!427$
Muni FE	Yes	Yes	Yes
Reg-Year FE	Yes	Yes	Yes

Table D.3: Effects of CCDs on Municipality Rents

Notes: Matched municipality sample, Treasury data (2002–2015). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated  $\theta_k$  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.

	(1)	(2)	(3)	(4)	(5)
	Log Total	Taxes/	Transfers/	Loans/	Other Rev./
	Revenue	Tot. Rev.	Tot. Rev.	Tot. 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	0.277	0.261	0.093	0.371
Ν	$4,\!457$	$4,\!457$	$4,\!457$	$4,\!457$	$4,\!457$
Muni FE	Yes	Yes	Yes	Yes	Yes
Reg-Year FE	Yes	Yes	Yes	Yes	Yes

Table D.4: Effects of CCDs on Municipality Revenue

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  $\theta_k$  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.

	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)
											Other Social
	Tot. $Exp./$	${ m Admin}/{ m }$	Justice Sys./	$\operatorname{Police}/$	Educ./	Culture/	Sport/	Tourism/	$\mathrm{Roads}/$	Sanitat./	$\operatorname{Policies}/$
	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	Tot. Rev.	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	0.827	0.264	0.002	0.033	0.071	0.018	0.016	0.009	0.098	0.228	0.089
N	4,456	4,457	$4,\!456$	4,456	$4,\!457$	4,456	4,457	4,457	$4,\!457$	$4,\!457$	4,456
Muni FE	${ m Yes}$	${ m Yes}$	$\mathbf{Y}_{\mathbf{es}}$	$\mathbf{Yes}$	${ m Yes}$	$\mathbf{Yes}$	$\mathbf{Yes}$	$\mathbf{Y}_{\mathbf{es}}$	$\mathbf{Y}_{\mathbf{es}}$	$\mathbf{Y}_{\mathbf{es}}$	${ m Yes}$
Reg-Year FE	$\mathbf{Yes}$	$\mathbf{Yes}$	$\mathbf{Yes}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Notes: Matched mun	icipality sample, M	inistry of the Int	erior data (1998–2015	). Treated munic	cipalities are mat	sched to out-of-re	gion potential co	ntrol municipalit	ies. This table re	ports the estimat	ed $\theta_k$
soefficients from $(1)$ .	We define "on imp	act" as $k = 0$ , "s	short run" as $k = 3$ , and	nd "long run" as	k = 9. "Mean" i	s the mean of the	e dependent varia	ble. Standard er	rors are reported	in parentheses ar	d are
slustered at the mun	icipality level. Reg	ression results a	re weighted by the log	sarithm of the nu	mber of firms in	the year before t	the CCD. Each o	utcome is normal	lized relative to t	the municipality's	total
budget. The first col	$ umn \ reports \ total \ \epsilon$	expenditures of a	a municipality relative	e to its total reve	nue. The remain	ting columns rep	resent the differen	it items on which	1 the municipalit	y can spend its n	oney,
again normalized rel.	ative to the overall	budget. Colum	n 9 reports expenditu	res on roads and	other infrastruc	tures. Column 1	0 reports expend	itures on the env	/ironment, which	is mainly allocat	ed to
garbage collection.											

Table D.5: Effects of CCDs on Municipality Expenditures