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# ORGANIZED CRIME AND ECONOMIC GROWTH: EVIDENCE FROM MUNICIPALITIES INFILTRATED BY THE MAFIA

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**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.

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# 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. 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 treated municipalities 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 associated with a CCD (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 dismisses the local government and 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. Several results guide our interpretation that CCDs' economic effects come from the weakening of the Mafia. First, CCDs change who runs the municipality after the dismissal. Post-CCD elected politicians are younger, more educated, more likely to be first-time politicians and women; all factors generally associated with lower levels of corruption (e.g., Decarolis et al., 2020). Importantly, Baraldi and Immordino (2021) show that CCDs do not affect the characteristics of political candidates running for local elections. In other words, CCDs do not affect who runs for office. They change who wins. This is indicative of a shift in voter preferences.

Second, CCDs affect the level of connivance between politics and economics. Newly elected politicians are significantly less likely to hold positions on corporate boards while in office. This reduction in political connections is consistent with a weakened role of the Mafia because criminal organizations often rely on these connections to gain market power and enforce monopolies (Akcigit et al., 2023). To further corroborate the argument that CCDs sever the illicit political connections established by the Mafia, we show that the Mayor or Vice-Mayor who held power at the time of the CCD—individuals who were allegedly connected with the Mafia—are significantly less likely to hold positions of power within firms after the CCD.

Third, the diminished influence of the Mafia is also corroborated by an analysis of where the economic effects of CCDs are concentrated. For instance, the employment effects of CCDs are particularly large among young workers (<30 years of age) who are disproportionately more likely to be recruited by criminal organizations (Sviatschi, 2022). The positive effects of CCDs on the entry of new firms 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). Moreover, "connected firms"–firms that received a public procurement contract *before* the CCD and thus may have obtained such contract as a result of the corruption present in the municipality–tend to experience losses in both employment and value-added per worker. This result implies that the economic effects of CCDs are driven by non-connected firms, i.e. companies that were not in business with the allegedly corrupt local municipality in the pre-CCD era.

Finally, we show that when CCDs fail to reduce the Mafia's presence, they are also unable to generate significant economic outcomes. We argue that municipalities that re-elect politicians associated with the CCD are cities where the Mafia maintains a strong influence. These municipalities do not experience any significant economic growth post-CCD. As a result, the economic effects of CCDs materialize only in cities that experience a significant change in the composition of the elected officials following the dismissal of the city council. We therefore conclude that CCDs weaken the Mafia's influence and that this is the primary channel through which these interventions 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; Colonnelli and Prem, 2022; 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. Acconcia et al. (2014) pioneered the study of CCDs by exploiting these episodes as instruments to estimate a fiscal multiplier using province-level data. Their work spurred a new branch of literature on the direct impact of CCDs. Recent 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); increase the quality of newly elected politicians (Daniele and Geys, 2015a); and have spillover effects on spending and public procurement in neighboring towns (Galletta, 2016; Tulli, 2019). We believe that our detailed administrative data and matched difference in difference design offer a substantial improvement over Acconcia et al. (2014). More broadly, our paper contributes to this literature by studying the direct impact of CCDs on workers, firms, and economic growth. Our rich micro data permit a credible empirical analysis of both the short- and long-run economic impacts of CCDs and an evaluation of the potential economic mechanisms.

# 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.[1] 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.

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.[2] 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.[3] 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.

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

[2] 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.

[3] 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 2010 and 2016, 69 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.

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 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 1a 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 1b 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

Our analysis draws from a number of different data sources, which we describe below.

**Social Security Data.** Our main source of data is the confidential matched employer-employee 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.

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 (2002–2015) collected and harmonized by the Italian Treasury.[5] Unlike most real estate price datasets, our data include information on both residential and non-residential units.

**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.

**Ownership.** We also use data on the ownership structure of companies collected by the Chamber of Commerce. These data are available for virtually the universe of limited liability companies in Italy and contain the social security numbers for the members of companies' boards of directors and their top executives from 2003 to 2017. We match these individuals' social security numbers with those in the data on politicians.

**Public Procurement.** We use data on public procurement contracts (2000–2016) collected by the Italian Authority for Public Contracts (*Associazione Nazionale Anticorruzione*—ANAC hereafter). The data include all public works contracts with a reservation price above 150,000 euros. Between 2008–2016, the data also include contracts for public works, services, and supplies with a reservation price above 40,000 euros. We match the firm identifiers to those in the social security records and balance sheet data collected by Cerved.

**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

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

[5]These data are collected by a Treasury department (*Agenzia delle Entrate - Territorio - Osservatorio del Mercato Immobiliare*) tasked with monitoring the housing market.

into 12 separate functions (e.g. administration, tourism, etc.). We analyze CCDs' effects on total spending for each function, the sum of short-term current expenditures and longer-term investments. We also analyze each of the four main categories of revenue: taxes, transfers from the central government, loans, and other revenue.

# 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 Appendix C 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.[6] However, the level of firms masks substantial churn: 14% and 10% of firms are born

[6] 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.

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 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 G.1), which are not included in the matching procedure.[7] 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 equation on the matched sample of treated and control municipalities:

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

where $y_{mt}$ is an outcome variable (such as log employment) for municipality $m$ in year $t$.[8] $CCD_m$ 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 $m$.[9] We control for municipality fixed effects, $\alpha_m$, and region-by-time fixed effects, $\lambda_{r(m),t}$, where $r(m)$

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

[8] 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 $m$.

[9] 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.

denotes the region associated with municipality $m$.[10] 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. Some of our treated municipalities are very small, and we do not want these tiny municipalities to drive our estimates. To avoid using a set of weights that could be changing as a result of the CCD, we weigh the regression results by the logarithm of the number of firms observed in the year before the CCD. Online Appendix E.3 shows that we obtain similar results when weighting by the 1991 log population or when we do not use any 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 helps circumvent 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 the 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 event-study 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

[10] 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 specification includes different fixed effects $\alpha_{m^*,1995}$ and $\alpha_{m^*,2007}$.

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 E.1 and E.2).

**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 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 G.2 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, our results are robust to excluding the CCDs that occurred either in 1993 or 2012–two years with a large number of events (Online Appendix E.4). Fourth, 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 E.7 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, wages, and real estate demand. The second part lays out robustness tests.

## 5.a Main Results

**Effects on Workers, Firms, and Wages.** Figure 2 reports the event-study coefficients $\hat{\theta}_k$ 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 2a 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 2b 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 of 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 50% increase relative to the mean of the control group in the pre-period (Table G.2).

CCDs' increases in the number of workers and firms do not translate into increases in the wage bill (Figure 2c), the sum of wages paid to all individuals employed in a given municipality. Instead, employment increases are offset by wage decreases (Figure 2d). 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 3a, blue squares), a 32% increase relative to the control group mean in the pre-period (Table G.2).[11] Similarly, the CCD increases the previously not-employed share of pre-CCD employment by 10.2 percentage points (Figure 3b, blue squares), a 40% increase over the pre-CCD control group mean (Table G.2).[12] Because they tend to be employed in lower-paying jobs (Table G.2), new workers drive down the average wage. This interpretation is corroborated by Figure G.3, which shows that CCDs do not systematically change incumbent workers' wages. One concern with this analysis is that CCDs may benefit treated municipalities but displace organized crime to neighboring towns. We test this hypothesis and do not find evidence of negative spillovers (see Appendix C).

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, suggests that is not the case (orange triangles in Figures 3a and 3b). 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. If CCDs increase economic activity, increases in input demand—both labor and land—should follow. Figure 4 reports CCDs' effects on industrial real estate prices, office real estate prices, residential real estate prices, and population.[13] CCDs' effects on industrial real estate prices are initially modest and not statistically significant and increase sharply three years after the CCD (Figure 4a), mirroring the employment

[11] 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 when formally employed in the private sector.

[12] 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.

[13] These results are also reported in Table 4. Industrial real estate includes factories, industrial buildings, and craft workshops.

effects (Figure 2a). 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 4b).[14] Finally, CCDs do not impact residential real estate prices (Figure 4c) or population (Figure 4d), perhaps as a result of the typically 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 2 represents primarily an increase in overall economic activity as opposed to a reallocation from the informal to the formal sector.

## 5.b Robustness

Online Appendix E shows that the results are not sensitive to (i) including socio-political variables in the matching algorithm,[15] (ii) using alternative measures of mafia presence, (iii) not using weights, (iv) using population weights, (v) excluding the CCDs that occurred in 1993 or 2012, (vi) restricting the sample to the subset of municipalities that experience only one CCD, (vii) restricting the sample to the balanced panel, (viii) dropping all potential control municipalities within 20 km from any treated municipality, and (ix) relaxing the out-of-region restriction.

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 G.4 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 Appendix C. 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 weakening the Mafia. This can occur if, for instance,

[14] In Figure F.1, we address potential violations to the parallel trends assumption that might arise when studying the impact of CCDs on office real estate prices.

[15] 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.

the dismissal coincides with larger central government transfers, new investments in specific policies, or the appointment of more competent administrators. All of these channels may have a direct positive effect on economic growth and do not necessarily require a diminished influence of the Mafia. Second, CCDs may generate economic effects *because* they weaken the Mafia. Section 6.a presents evidence against this first explanation. Section 6.b provides evidence in favor of the second explanation.

## 6.a Mechanisms Unrelated to Mafia Infiltration

This section shows that CCDs do not increase government transfers or expenditures, and Mafia-unrelated CCDs do not manifest in economic growth like Mafia-related CCDs.

**Government Revenues and Expenditures** CCDs may induce growth if increased government transfers induce a stimulus effect via increased spending.[16] We estimate the effect of CCDs on local government finances. We find little evidence of this channel. Table G.5 shows no increase in transfers as a share of local revenues.

Even if CCDs have no effect on total expenditures, external commissioners or newly elected politicians may direct funds to programs that generate employment effects such as job training programs (Katz et al., 2022) or infrastructure (Donaldson, 2018). We find no evidence of this channel. Table G.6 shows no increased expenditure in "other social programs" (column 11, which would include job training), infrastructure (column 9), or educational policies (column 5). We only find small but significant effects in sanitation, parks, and garbage collection in the short run (column 10). Altogether, there is little evidence that CCD-driven changes to fiscal policy generate economic growth.

**Re-Centralization of Power.** Despite the absence of scale or compositional effects in spending, better-managed local government by experienced bureaucrats may itself generate economic growth independent of the effects on the local Mafia.

To isolate the impact of substituting elected officials with experienced bureaucrats (i.e., the re-centralization of power via the appointment of the external commissioners), we study the effect of CCDs that are caused by instances other than Mafia infiltration (see Appendix D for details). Figure 5 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

[16] Such a stimulus effect may in fact benefit local Mafia businesses (Daniele and Dipoppa, 2022).

Mafia-related CCDs. We find no appreciable effects on wage bills and average wages (panels c and d). A simple back-of-the-envelope calculation suggests that the "re-centralization" channel only explains 20% of the economic effects generated by CCDs due to Mafia infiltration. 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 section rules 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.[17] First, CCDs lead to the election of very different types of politicians (more likely to be first-runner, younger, more educated, and female) and reduce connivance between business and politics. Second, the effects of CCDs on the number of firms are concentrated in Mafia-dominated sectors, suggesting a lower ability of the Mafia to control the local economy post-CCD. Relatedly, we find that firms connected to the dismissed administration tend to lose employment and value added after the CCD. Finally, the economic effects are driven by municipalities that experience large political swings. The totality of the evidence thus suggests that CCDs generate growth by diminishing the economic drag generated by the Mafia.

**Political Turnover** Criminal organizations use their power to influence electoral results (Alesina et al., 2018). If CCDs weaken the Mafia, then criminal organizations have less power to influence electoral results, resulting in different politicians. We follow Daniele and Geys (2015b) and estimate CCDs' effects on the characteristics of local politicians using equation (1). Politicians in treated and control municipalities are initially similar (Table G.1). However, Figure 6 shows that CCDs cause elected officials to be 13 p.p. more likely to be first-time politicians (an almost 24% increase relative to the pre-CCD control mean, see Table G.3), two years younger, 6 p.p. more likely to be female (a 55% increase), and more educated.[18]

The large positive effect on the probability of electing women contrasts starkly with the patriarchal view of society perpetuated by the Mafia (Fiandaca, 2007). More broadly, CCDs

[17] Estimating the CCDs' direct effect on the Mafia's presence and strength is hampered by the inherent difficulty of measuring Mafia activity. Proxy measures tend to be coarse or time-invariant (Calderoni, 2011; Dugato et al., 2020). Furthermore, proxies constructed from news of Mafia violence (Dipoppa, 2021) have become less informative as the Mafia has become less overtly violent.

[18] In principle, a city council dismissal can erode the incumbency advantage, and the political effects we find can therefore be mechanical. However, we find no political effects of Mafia-unrelated CCDs (Figure 6), which would experience a similar effect on the incumbency advantage.

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). Importantly, Baraldi and Immordino (2021) show that CCDs do not change the characteristics of political candidates running for local elections. In other words, CCDs do not affect who runs for office. They change who wins. This is indicative of a shift in voter preferences.

**Political Connections** The economic success of the Mafia depends crucially on its ability to create and maintain monopolies (Gambetta, 2000). Recent studies show that the enforcement of such monopolies often relies on political connections (Akcigit et al., 2023). If the CCDs weaken the Mafia, we would then expect a reduction in the degree of connivance between politics and business and, in particular, in the illicit political connections established through the Mafia.

We measure political connection as the share of local politicians who contemporaneously serve in executive positions or on the boards of directors of private firms.[19] Figure 7a shows that political connections drop by around 5 p.p. (an effect of $\approx 33\%$) following the CCD.[20] Next, we analyze whether the CCD reduces specifically illicit connections established by the Mafia. We proxy for these illicit connections with instances where the Mayor or Vice-Mayor who held power at the time of the CCD—thus individuals who were allegedly connected with the Mafia—still hold positions of power within companies after the CCD. Figure 7b shows that there is an effect of 10-15 percentage points, which corresponds to an 80% reduction in the likelihood that the dismissed Mayor (or Vice-Mayor) serves on the board of firms in the aftermath of the CCD. [21] This section shows that 1) CCDs reduce the connivance between business and politics; 2) individuals who are presumably influenced by the Mafia are less likely to be in control of economic activities following the CCD. Both these results are indicative of CCDs weakening the Mafia.

**Mafia Firms and Sectors.** If the CCD weakens the Mafia's ability to enforce monopolies, as suggested by the reduction in political connections discussed in the previous section, we expect

[19]This is a measure of political connections akin to the one used in (Akcigit et al., 2023). In Akcigit et al. (2023) a political connection is established if *any* employees of the firm are local politicians, while in our definition, a connection is established if members of the board or top executives also serve on the city council; see Mirenda et al. (2022) for a similar approach.

[20]Figure G.4, panel (a), shows that this effect is large for Mafia-related CCDs. For mafia-unrelated CCDs the effects are very modest and, on impact, the magnitude of the effect is about a third of the magnitude we get for Mafia-related CCDs. When scaling these effects by the pre-CCD average of observed political connections, we see that the effect for Mafia-related CCDs is again about 3x larger than the one for non-Mafia-related CCDs (approximately 33% vs. 11%).

[21]Similarly to what happens when analyzing the impact of CCDs on the characteristics of elected politicians, the effect in Figure 7b may be the mechanical consequence of negating the incumbency advantage. However, we do not see evidence of such an effect when using Mafia-unrelated CCDs (Figure G.4, panel b), thus mirroring the findings on political turnover shown in Figure 6.

firm entry to be concentrated in traditionally Mafia-dominated sectors.

To test this hypothesis, we look at the effects of CCDs on the number of firms in "Mafia Sectors" which are defined as sectors at "high risk of Mafia infiltration" according to the Italian Anti-Mafia Directorate.[22] Figure 8a shows that CCDs increase the number of firms in Mafia sectors by almost 20% but have no apparent effect in non-Mafia sectors. CCDs also increase the number of employees in the Mafia sectors by a similar magnitude. The long-run employment effects in the Mafia sectors are larger than the corresponding estimates in the non-Mafia sector, although the difference is not statistically significant. The finding that the competitive effects are not broad-based and instead concentrated in Mafia sectors is consistent with a weaker Mafia driving the CCDs' economic effects.

Another testable implication that follows from a weakened Mafia's power is that firms under its influence should not directly benefit from the CCD. Paralleling the analyses on allegedly corrupt politicians, we thus analyze CCDs' effects on "connected firms", which we define as local firms that won a public procurement contract before the CCD. The idea is that these firms might have been awarded the procurement contract as a result of the political corruption present in the municipality prior to its dismissal. Figure 9 shows that connected firms in treated municipalities tend to experience *losses* in employment and value-added per worker relative to past winners in control municipalities, albeit these results are somewhat imprecise possibly because of the limited number of connected firms per municipality.[23] Overall, the evidence in Figure 9 implies that non-connected firms are the ones driving the aggregate, positive effects of CCDs on economic growth.

**Economic effects when there is no political change.** If the economic effects of CCDs are primarily a consequence of the Mafia's diminished power, then we should see no economic effects in instances where CCDs fail to reduce the influence of the Mafia.[24] Should this not hold, it would suggest that CCDs may exert economic effects through alternative mechanisms, distinct from merely weakening the influence of the Mafia.

Because Mafia's presence is unobservable, we proxy for it using the probability of re-electing corrupt politicians who were dismissed by the CCD. The idea is that municipalities that are

[22]The specific 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 (article 5-bis of law n. 122/2012).

[23]The public procurement data collected by the National Authority for Public Contracts includes information on all public works contracts awarded between 2000 and 2016 with a reservation price above 150,000 euros. Starting from 2008, the data also include contracts for public works, services, and supplies with a reservation price above 40,000 euros. Since most of our municipalities are relatively small, not many public contracts are above these thresholds.

[24]This is akin to the principle "when there is no first stage, there should not be an intention to treat" which is used to test the validity of the exclusion restriction in IV models (Kitagawa, 2015; Huber and Mellace, 2015).

disproportionally more likely to re-elect politicians associated with the CCD are cities where the Mafia still maintains a strong influence. We, therefore, sort municipalities according to the change in the share of non-incumbents that won in the first election following the CCD. Figure 10 shows that in places where the Mafia retains its political influence, CCDs have no significant effect on employment or number of firms. As a result, the economic effects of CCDs are entirely concentrated in cities that experience large political swings. This suggests that the primary channel through which CCDs generate an economic impact is by diminishing the influence of the local Mafia. This is consistent with the results on politicians, political connections, mafia firms, and mafia sectors, which all suggest a weakened role of 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 makes citizens update their beliefs on whether the State can fight organized crime. CCDs, therefore, help municipalities transition from a climate of risk to a climate of trust. This is reflected in several pieces of evidence: i) the increase in the number of firms in sectors historically dominated by the Mafia (Figure 8a), (ii) the increase in business-related real estate prices (Figure 4a), and (iii) the increase in the employment of young individuals who are more likely to be recruited by organized criminal organizations (Sviatschi, 2022) (Figure 3a) (iv) the decrease in the likelihood to observe the Mayor and Vice-Mayor from the dismissed municipality to be in charge of firms (Figure 7). These results are also consistent with other studies that found that CCDs reduce petty crimes (Cingano and Tonello, 2020) and violence against politicians (Baraldi et al., 2022).

The shift in climate caused by CCDs is also evident from the political swings shown in Figure 6 and 7, with municipalities now electing different types of politicians who are more likely to be running for the first time, younger, women, with higher levels of education and who are less likely to be sitting on the board of companies. The first election post-CCD marks a shift in the attitudes of the municipality residents towards the mafia and might, therefore, explain why the large economic effects of CCDs accrue right after these elections occur. When 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 (Figures 5 and 10). We thus conclude that the economic effects of CCDs are driven by the erosion of the Mafia's power. The resulting renewed sense of 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 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 local population views the central government unfavorably, a policy like a CCD might generate a strong backlash (Blattman et al., 2021).

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), as the latter is often accompanied by large unintended consequences (Dell, 2015; Slutzky et al., 2019; Sviatschi, 2020; Blattman et al., 2021).

We conclude by noting an interesting question that emerges from our analysis. Baraldi et al. (2022) shows that there is a *decrease* in Mafia violence following a CCD both in treated municipalities and in neighboring municipalities. Why did the Mafia not fight back after the CCD either by trying to re-establish its position in treated municipalities or by expanding to nearby cities? 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.

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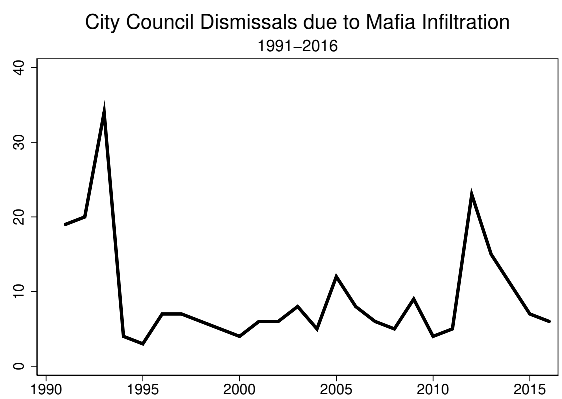
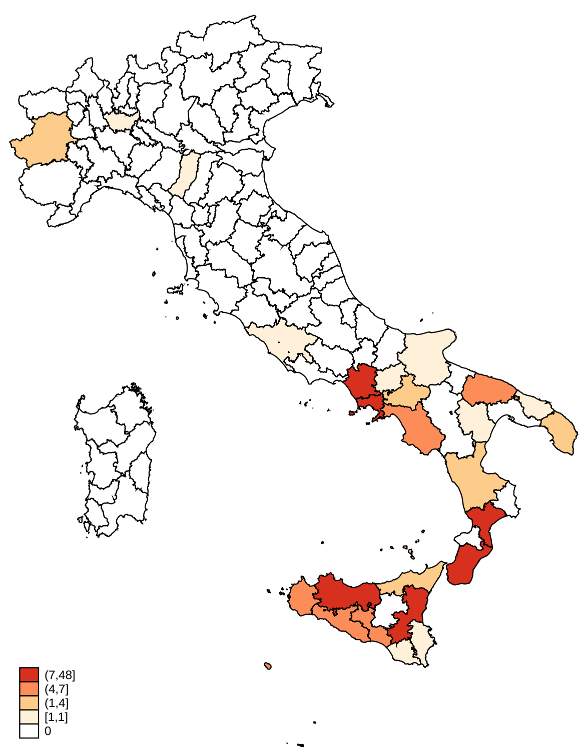
# 8 Figures

Figure 1: Temporal and Spatial Variation in CCDs

(a)




(b)

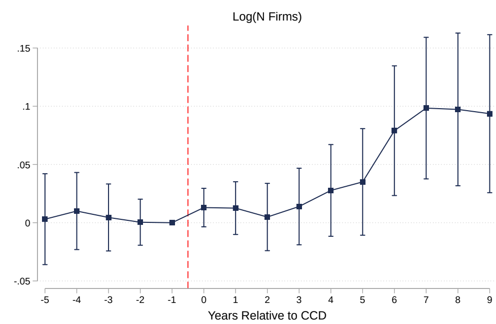



*Notes*: Panel a summarizes the time variation in the number of CCDs due to Mafia infiltration between 1991 and 2016. Panel b displays a map reporting the counts of CCDs for each of the 110 Italian provinces between 1991 and 2016.

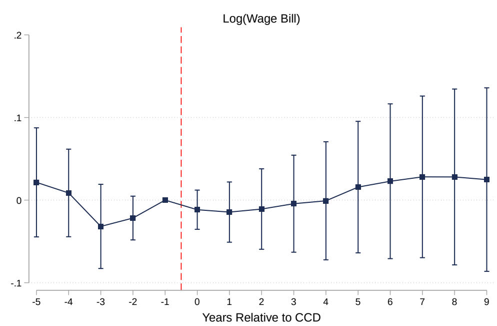
Figure 2: Effects of CCDs on Employment, Number of Firms, and Wages

(a)

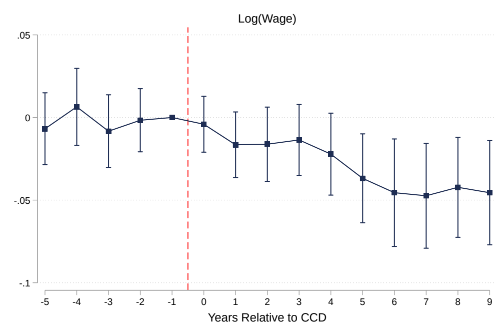

(b)




(c)



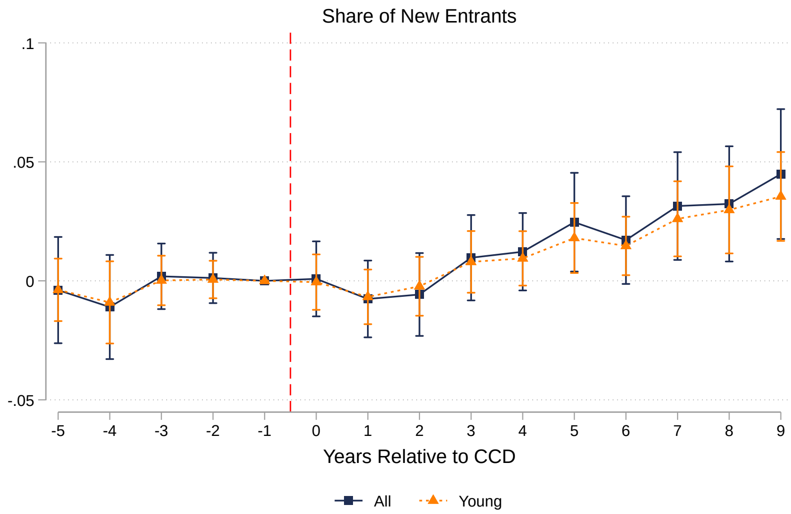

(d)



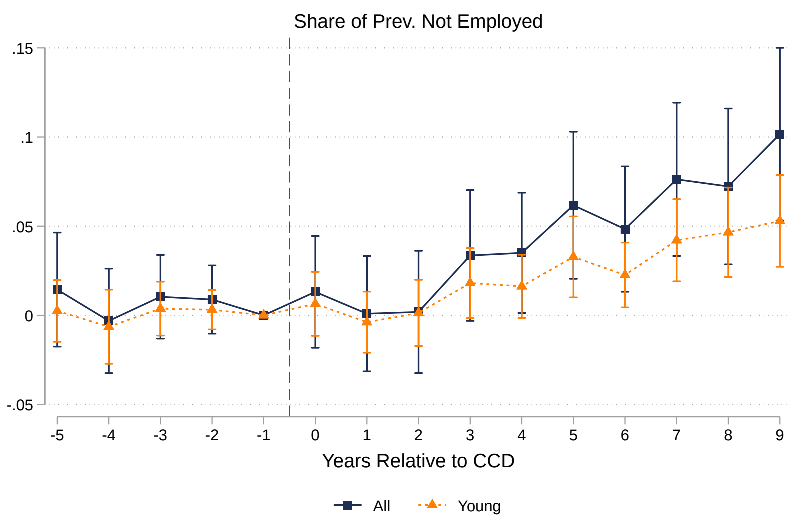

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 3: Effects of CCDs on New Entrants and Previously Not Employed Workers as a Share of Baseline Employment

(a)



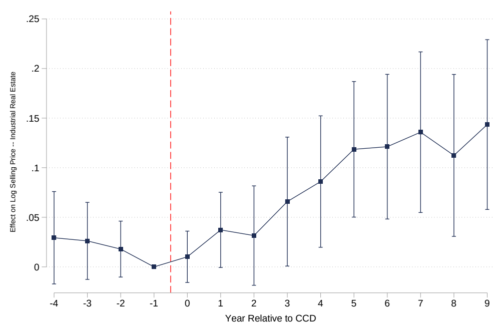

(b)



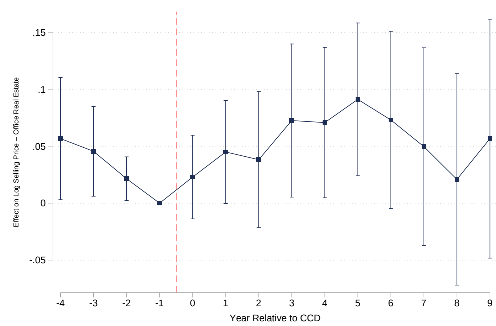

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a and b display the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 4: Effects of CCDs on Municipality Population and Real Estate Prices

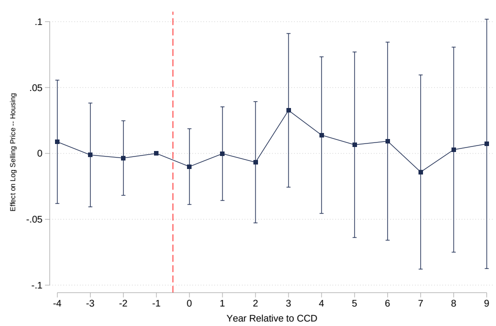
(a) Industrial Real Estate



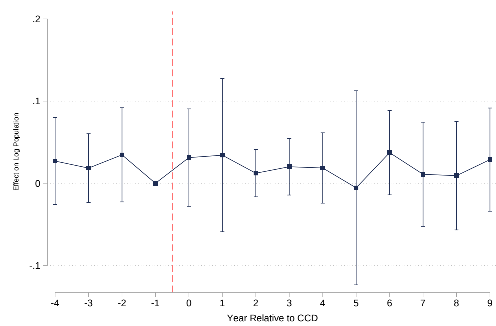

(b) Office Real Estate




(c) Residential Real Estate



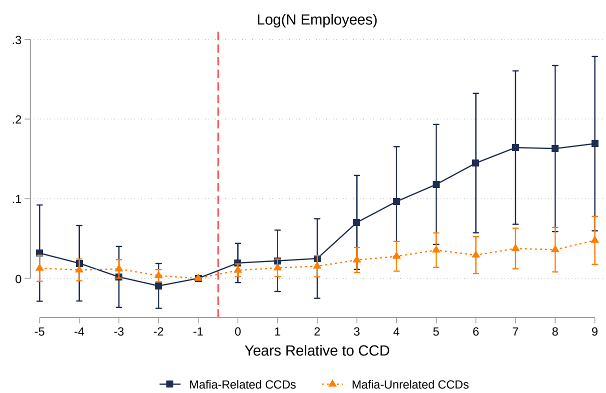

(d) Population



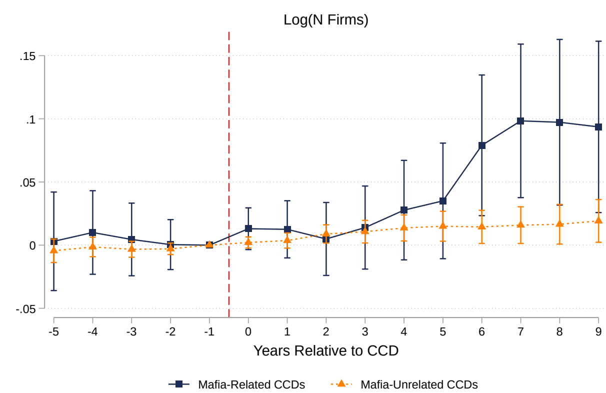

*Notes*: Matched municipality sample, Treasury data (2002–2015) in panels a–c and Ministry of the Interior data (1989–2015) in panel d. Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the 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 5: Effects of CCDs Unrelated to Mafia Infiltration

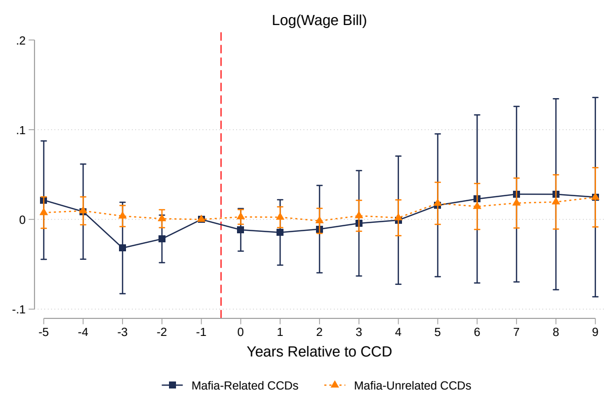
(a)



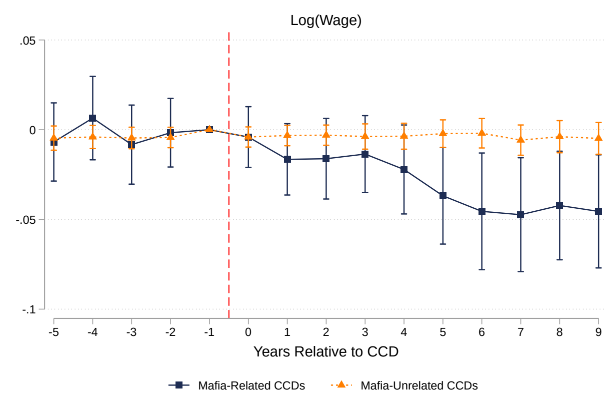

(b)




(c)



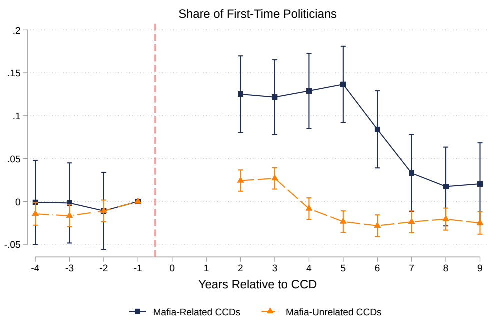

(d)



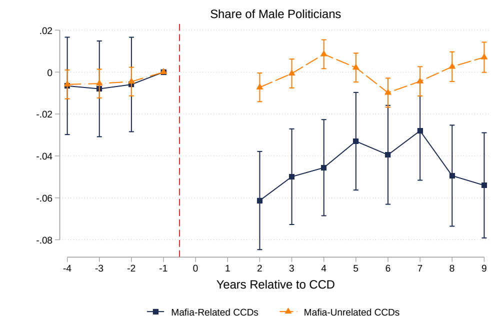

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 6: Effects of CCDs on the Characteristics of Elected Politicians

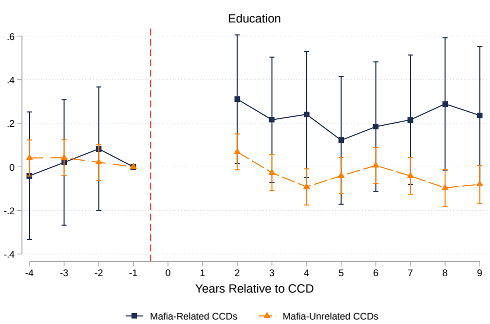
(a)



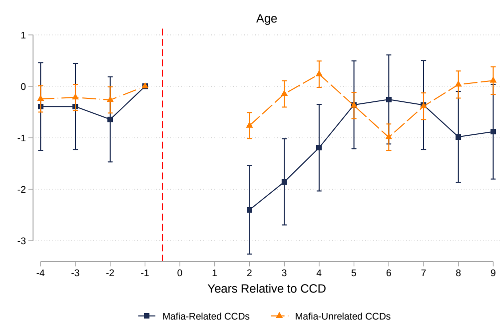

(b)




(c)



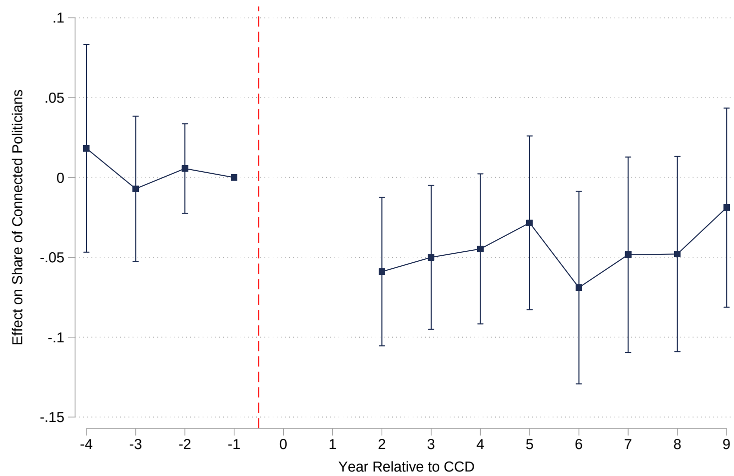

(d)



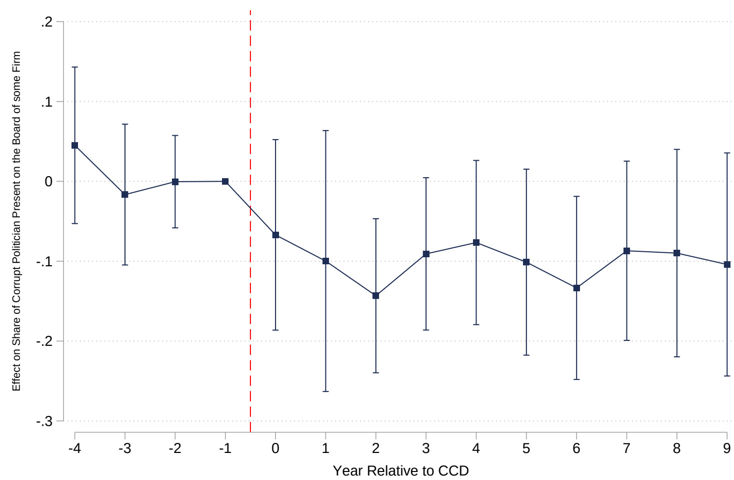

*Notes*: Matched municipality sample, Ministry of the Interior data (1986–2020). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). 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 7: Effect of CCDs on Political Connections

(a) Contemporaneous Connections



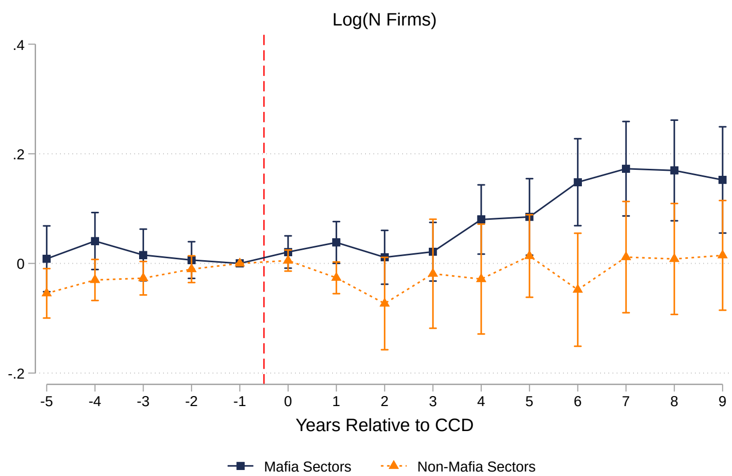

(b) Corrupt Politicians on the Board of Firms



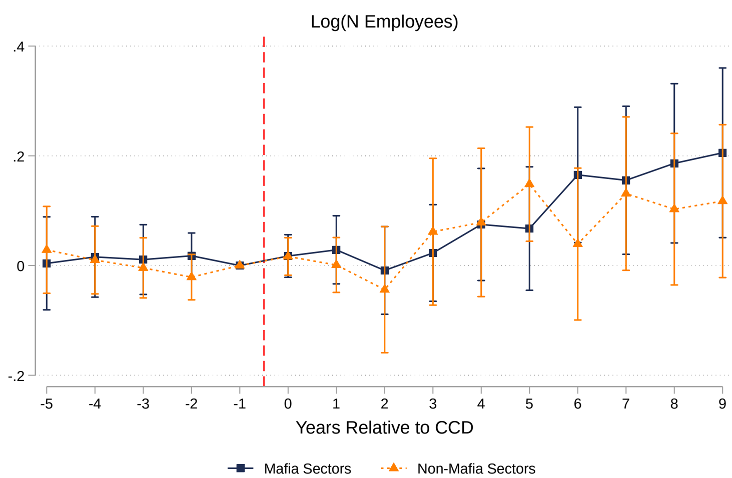

*Notes*: Matched municipality sample, Ministry of the Interior matched with data on ownership structure (2003-–2017). The figure displays the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. The outcome variable in panel(a) is the fraction of elected politicians of municipality $m$ in year $t$ who, in the same year, also sit on the board of some firm. Coefficients at 0 and 1 are missing because in those years treated municipalities are administrated by the external commissioners. The outcome variable in panel(b) is constructed as follows. We take the identities of the major and vice-major of treated and control municipalities in the year before the city council dismissal and label these as the "corrupt" politicians of municipality $m$. We then compute the fraction of these corrupt politicians who serve on the board of firms at time $t$ and use this as the dependent variable in the event-study.

Figure 8: Effects of CCDs in Mafia and Non-Mafia Sectors

(a)



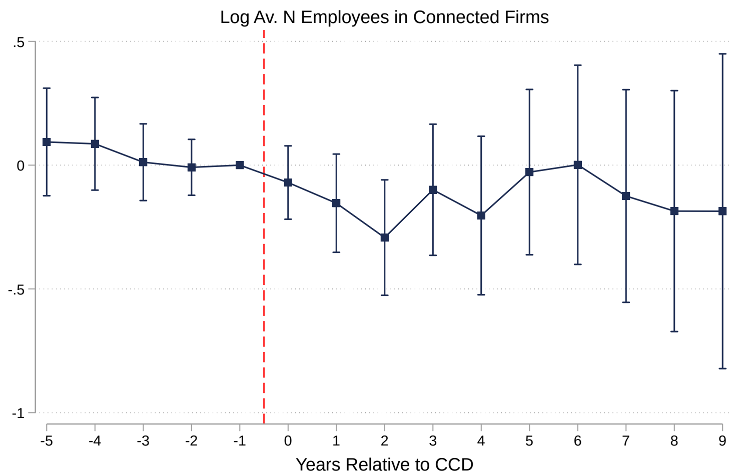

(b)



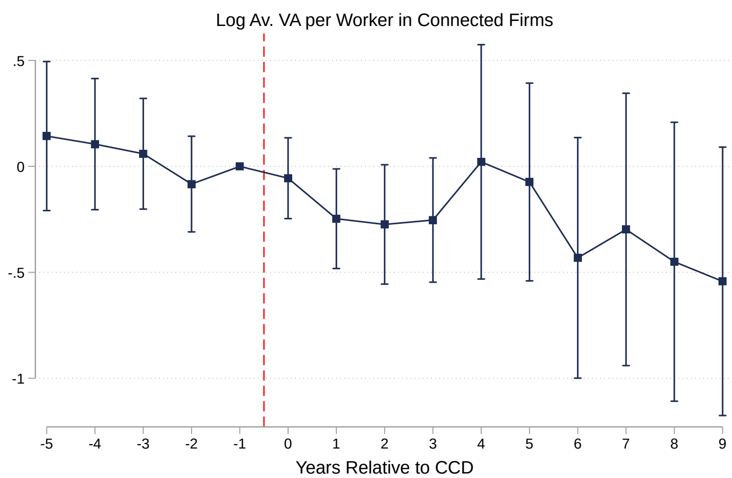

*Notes*: Matched municipality sample, INPS data (1983–2017). This figure displays the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 9: Effect of CCDs on Connected Firms

(a)



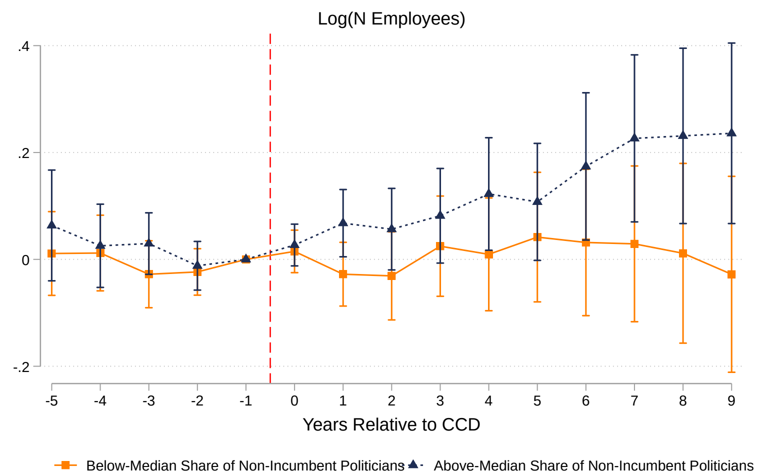

(b)



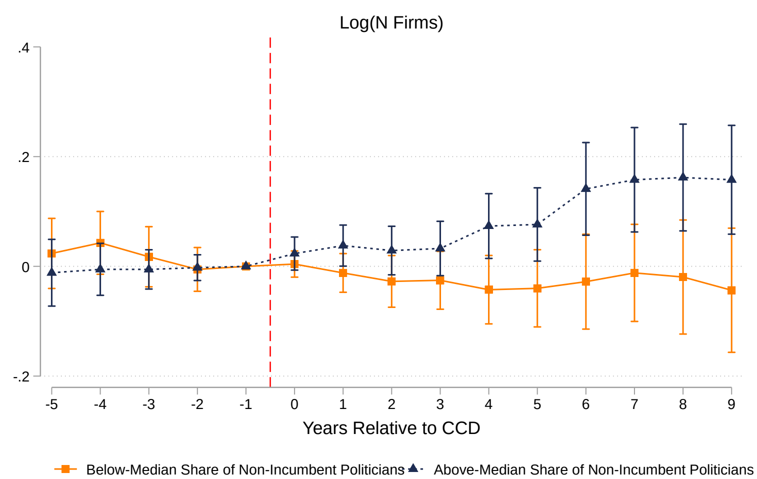

*Notes*: Matched municipality sample, INPS data (1983–2017) matched with balance sheet data from CERVED and public procurement data. Panels a–b report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at $k = -1$ are normalized to zero. A firm is defined as connected to municipality $m$ if the firm is in the same province of municipality $m$ and obtained a public procurement contract from city $m$ *before* the CCD. We then compute the logarithm of employment and value added (where the latter is rescaled by the number of employees in the year before the CCDs) for these connected firms and use them as the dependent variables in the event-study specification highlighted in equation (1).

Figure 10: Heterogeneous Effects of CCDs on Employment and Number of Firms

(a)




(b)




*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. The outcome variables are log employment (panel a) and log number of firms (panel b). Equation (1) is estimated separately for municipalities that experienced above-/below-median changes in the share of non-incumbent politicians (blue triangles/orange squares). The value of the median change in the share of non-incumbent politicians in our sample is 0.197.

# 9 Tables

Table 1: Municipality Characteristics in the Year before the CCD

| | (1)<br>Matched Sample | (2)<br>T | (3)<br>C | (4)<br>T-C | (5)<br>p |
|---|---|---|---|---|---|
| Population in 1991 | 15263.83 | 15522.71 | 15004.95 | 517.76 | 0.84 |
| N Establishments | 260.97 | 229.60 | 292.34 | -62.74 | 0.15 |
| N Firms | 250.80 | 220.93 | 280.67 | -59.73 | 0.15 |
| N Sole Proprietorship | 132.51 | 113.11 | 151.91 | -38.80 | 0.16 |
| N of Employees | 2348.95 | 1572.30 | 3125.61 | -1553.30 | 0.00 |
| Av. Daily Wage | 72.74 | 73.21 | 72.28 | 0.93 | 0.51 |
| Av. Daily Wage: Prev. Not Empl. | 63.56 | 64.07 | 63.04 | 1.03 | 0.47 |
| Av. Daily Wage: Prev. Empl. | 74.10 | 74.06 | 74.14 | -0.08 | 0.95 |
| Municipal Wage Bill (M of €) | 41.21 | 20.16 | 62.26 | -42.10 | 0.00 |
| Share New Entrants | 0.14 | 0.15 | 0.13 | 0.02 | 0.21 |
| Share Prev. Not Empl. | 0.26 | 0.28 | 0.25 | 0.03 | 0.39 |
| Share Prev. Not Empl. $<$ 30 y.o. | 0.15 | 0.16 | 0.14 | 0.02 | 0.29 |
| Share Firm Entries | 0.14 | 0.14 | 0.13 | 0.02 | 0.08 |
| Share Firm Exists | 0.10 | 0.10 | 0.10 | 0.00 | 0.64 |
| Turnout | 0.78 | 0.77 | 0.79 | -0.02 | 0.08 |
| Observations | 422 | 211 | 211 | | |

*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. The statistics in column 4 are calculated as (2)-(3), and column 5 reports the p-value associated with the null hypothesis that the difference in means is equal to zero.

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

| | (1) Log(Empl) | (2) Log(N Firms) | (3) Log(Wage Bill) | (4) 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 |
| N | 14,654 | 14,654 | 14,654 | 14,654 |
| Muni FE | Yes | Yes | Yes | Yes |
| Reg-Year FE | Yes | Yes | Yes | Yes |

*Notes*: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated $\theta_k$ 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 2.

Table 3: Effects of CCDs on Entries and Exits

| | (1) Share New Entrants | (2) Share New Entrants < 30 y.o. | (3) Share Prev. Not Empl. | (4) Share Prev. Not Empl. < 30 y.o. | (5) Share Firm Entries | (6) Share Firm 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 |
| N | 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 |

*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 3.

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

| | (1) Log Industrial Real Estate Prices | (2) Log Office Real Estate Prices | (3) Log House Real Prices | (4) Log Population |
|---|---|---|---|---|
| On Impact | 0.0102 | 0.0229 | -0.0100 | 0.0312 |
| | (0.0132) | (0.0187) | (0.0147) | (0.0302) |
| Short Run | 0.0658 | 0.0725 | 0.0327 | 0.0201 |
| | (0.0332) | (0.0176) | (0.0298) | (0.0301) |
| Long Run | 0.1435 | 0.0567 | 0.0072 | 0.0288 |
| | (0.0437) | (0.0535) | (0.0483) | (0.0320) |
| Mean | 6.01 | 6.71 | 6.606 | 8.903 |
| N | 2,474 | 2,453 | 2,860 | 7,462 |
| Muni FE | Yes | Yes | Yes | Yes |
| Reg-Year FE | Yes | Yes | Yes | Yes |

*Notes*: Matched municipality sample, Ministry of the Interior data (1989–2015) in column 1 and Treasury data (2002–2015) in columns 2–4. Treated municipalities are matched to out-of-region potential control municipalities. This table reports the estimated $\theta_k$ 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 4.

# 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** (municipality level): average real estate selling price 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).

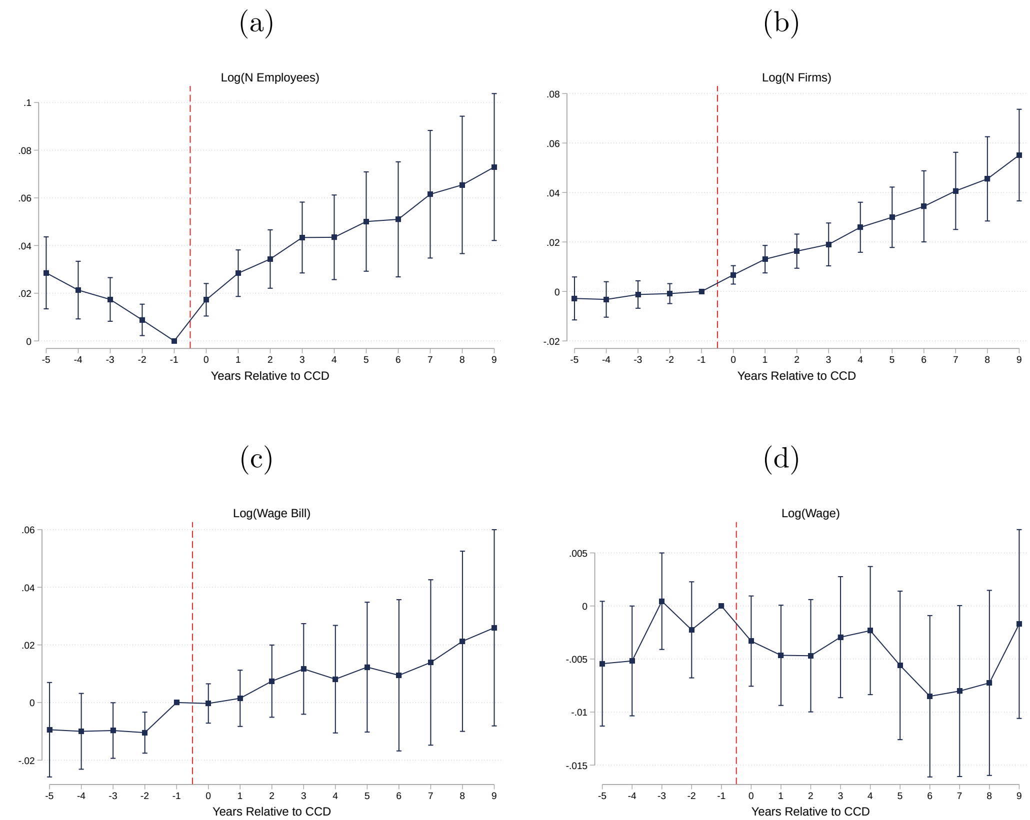
**Wage bill** (municipality level): the sum of all wages paid to formal private sector workers employed in municipality $m$ in year $t$. The wage bill of workers employed at incumbent firms is constructed analogously.

# Appendix C 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 C.1 reports the results on log employment, number of firms, municipality wage bill, and average wages. Figures C.1a and C.1b 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 2, Figure C.1d displays a negative effect on the average wages of workers employed in a small radius of treated units.

Panels a and d present some evidence of non-parallel pre-trends. In Figure F.1, we extrapolate the estimated linear trend found in pre-CCD era to post-intervention periods—and assess the validity of such approach using the honest pre-trend approach proposed by Rambachan and Roth (2023), see Appendix F for details. This analysis confirms the presence of sizable and statistically significant long-run spillovers on nearby cities, even after allowing for significant deviations from the linear extrapolation depicted in the left panel of Figure F.1. 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).

Figure C.1: 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.

# Appendix D Mafia-Unrelated CCDs

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. Like Mafia-related CCDs, the central government appoints an external commissioner when the city council is dismissed. The external commissioners appointed after a Mafia-unrelated CCD have the same powers as those appointed after Mafia-related CCDs. With full executive and legislative powers, their main task consists of managing the municipality from the dismissal to new elections.

We use the same matched event-study research design to estimate the effects of Mafia-unrelated 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.

Table D.1 reports the summary statistics in the year before the CCD for this matched sample in column 1. Columns 2 and 3 display the statistics for treated and control municipalities, respectively. There are about 2,300 municipalities that experienced this type of dismissal in our matched sample. The average municipality has 13,025 inhabitants (in 1991) and 235 firms. Similarly to our main sample, the ratio of employment to 1991 population is only 16.8%, reflecting a high rate of unemployment, high rate of informality, and high share of public sector employment.

Importantly, these municipalities tend to be broadly similar to municipalities that experienced Mafia-related CCDs in terms of size (measured as population, number of employees, or number of firms), employment to 1991 population ratio, wages, economic dynamism (i.e., the share of firm entries and exits), and turnout at the previous elections.

Table D.1: Municipality Characteristics in the Year before the Mafia-Unrelated CCD

| | (1) Matched Sample | (2) T | (3) C | (4) T-C | (5) p |
|---|---|---|---|---|---|
| Population in 1991 | 13025.26 | 12368.56 | 13681.96 | 1313.4 | 0.22 |
| N Establishments | 245.25 | 249.68 | 240.81 | 8.87 | 0.53 |
| N Firms | 235.45 | 240.38 | 230.51 | 9.87 | 0.46 |
| N Sole Proprietorship | 123.21 | 112.22 | 134.19 | -21.97 | 0.02 |
| N of Employees | 2188.94 | 2008.88 | 2369.00 | -360.12 | 0.02 |
| Av. Daily Wage | 74.05 | 73.83 | 74.27 | -0.44 | 0.26 |
| Av. Daily Wage: Prev. Not Empl. | 60.90 | 61.16 | 60.65 | 0.51 | 0.27 |
| Av. Daily Wage: Prev. Empl. | 76.06 | 75.69 | 76.42 | -0.73 | 0.06 |
| Municipal Wage Bill (M of €) | 44.03 | 35.11 | 52.96 | -17.85 | 0.00 |
| Share New Entrants | 0.10 | 0.10 | 0.10 | 0.00 | 0.07 |
| Share Prev. Not Empl. | 0.18 | 0.18 | 0.18 | 0.00 | 0.34 |
| Share Prev. Not Empl. $<$ 30 y.o. | 0.11 | 0.11 | 0.11 | 0.00 | 0.10 |
| Share Firm Entries | 0.11 | 0.11 | 0.11 | 0.00 | 0.62 |
| Share Firm Exists | 0.08 | 0.08 | 0.09 | 0.01 | 0.01 |
| Turnout | 0.80 | 0.80 | 0.80 | 0.00 | 0.55 |
| Observations | 4608 | 2304 | 2304 | | |

*Notes*: Matched municipality sample, INPS data (1983–2017). Treated municipalities that experience a Mafia-unrelated CCD 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. The statistics in column 4 are calculated as (2)-(3), and column 5 reports the pvalue on the null hypothesis that the difference in means is equal to zero.

# Appendix E  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) using alternative measures of mafia presence in the matching algorithm, (iii) not using weights, (iv) using population weights, (v) excluding CCDs that occurred either in 1993 or 2012, (vi) restricting the sample to the subset of municipalities that experience only one CCD, (vii) restricting the sample to the balanced panel, (viii) dropping all potential control municipalities in a 20 km radius of any treated unit, and (ix) relaxing the out-of-region restriction.

## E.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 socio-political 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).[25] Next, we add the baseline average age and educational level of local politicians at $t-1$.

Figure E.1 compares the baseline estimates from Figure 2 (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.

[25]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).

## E.2 Robustness to Alternative Measures of Mafia Presence

In this Section, we show that our results are robust to adding four different measures of mafia presence when estimating the propensity score matching. We list these measures below. First, our preferred measure for Mafia-presence is the composite index constructed by Dugato et al. (2020) who aggregate several different dimensions of mafia presence, namely the presence and activities of mafia groups, mafia violence, and infiltration in politics and the economy. The key advantages of this measure are i) its richness (it aggregates several distinct phenomena related to Mafia presence), ii) its granularity (municipality-level), and iii) its coverage (this measure is defined for all Italian municipalities). Second, the Mafia index constructed by Calderoni (2011) is also an aggregate of several dimensions, including information on mafia-type associations, mafia murders, mafia infiltration in politics, and assets confiscated from organized crime. While this measure covers the whole country, it is much coarser in nature (province-level). Third, the news-based measure of Mafia presence constructed by the University of Messina (*Uni ME*) is an indicator that identifies the municipalities that have been reported to have a Mafia presence prior to 1994–this variable is described in detail in De Feo and De Luca (2017). Finally, our last Mafia measure is an indicator for the municipalities mentioned in a 1987 report for a parliamentary committee compiled by the Italian military police (*Carabinieri*)–this variable is also described in detail in De Feo and De Luca (2017). Both the *Uni ME* and the *Carabinieri* encompass only the three Southern regions of Campania, Calabria, and Sicily – the traditional strongholds of the Mafia.

Table E.1 shows that in our baseline sample, there are significant differences in our preferred measure of mafia prevalence (Dugato et al., 2020) across treated and control municipalities. Even if differences in levels between treatment and control municipalities do not necessarily imply a violation of the parallel trend assumption, it is important to evaluate whether our results are sensitive to permutations of the matched control group based on augmenting the propensity score matching algorithm to include these measures of Mafia prevalence. We thus tested the robustness of our baseline results to the inclusion of each of the four above-mentioned measures of mafia presence in our matching algorithm. Figure E.2 reports the results. This figure compares our baseline (blue squares) with those obtained from 4 alternative matching algorithms that include a basic set of socio-political variables and a measure of Mafia presence. The four measures of mafia presence we use are the index constructed by Calderoni (2011) (orange triangles), the indicator for mafia presence from Dugato et al. (2020) (light-blue diamonds), the news-based measure constructed by the University of Messina–*Uni ME* (green circles), and the measure based on a report by the Italian military police–*Carabinieri* (red Xs), respectively. Our baseline results are robust to the inclusion of any of these measures of Mafia presence in the matching algorithm. Focusing on the results based on the measure of Dugato et al. (2020), we find virtually identical effects compared to our baseline estimates. If anything, using the measure of Dugato et al. (2020), leads to slightly larger effects of CCDs. This suggests that violations of the parallel trend assumption induced by the omission of Mafia prevalence are unlikely to be a first-order concern for our matched difference-in-differences research design. Given this and the fact that including these measures in the propensity score leads to slightly less precise estimates, we chose our baseline matching algorithm as our preferred specification. This permits us to have more power when investigating the mechanisms through which CCDs generate economic growth.

## E.3 Weights

Another concern is that our results may be driven by the weights we use. As a robustness check, Figure E.3 compares the baseline estimates from Figure 2 (blue squares) with those obtained from estimating equation (1) without weights (orange triangles). Similarly, Figure E.4 compares the baseline estimates from Figure 2 (blue squares) with those obtained from estimating equation (1) using log 1991 population as weights (orange triangles). As our results are unchanged, we conclude that our main findings are not sensitive to the weights we use.

## E.4 Excluding CCDs that Occurred in Either 1993 or 2012

As discussed in Section 4.d, difference-in-differences research designs are threatened if treated groups are affected by an unrelated shock at the same time as treatment. This concern is alleviated by the fact that CCDs take place between 1991 and 2016. Yet, because a significant share of CCDs occurred in 1993 and 2012, one may be concerned that some unobserved shocks to treated municipalities in one of these two years may be driving our results. As a robustness check, Figure E.5 compares the baseline estimates from Figure 2 (blue squares) with those obtained from estimating equation (1) excluding the CCDs that took place in either 1993 (green circles) or 2012 (orange triangles). Our point estimates are unchanged, although the confidence intervals are wider, as expected, given the smaller sample size and the fact that we cluster the standard errors at the municipality level. This exercise corroborates the argument that our baseline estimates are not driven by unobserved concurrent events that affected treated municipalities.

## E.5 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 equation (1) on the subset of municipalities that experience only one CCD. Figure E.6 compares the baseline estimates from Figure 2 (blue squares) with those obtained from estimating equation (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.

## E.6 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 equation (1) on the subset of municipalities treated before 2009 (balanced sample). Figure E.7 compares the baseline estimates from Figure 2 (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.

## E.7 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 of any treated unit from the set of potential control municipalities and re-estimate the matching algorithm. Figure E.8 compares the baseline estimates from Figure 2 (blue squares) with those obtained from estimating equation (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 the 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.

## E.8 Relaxing the Out-Of-Region Restriction

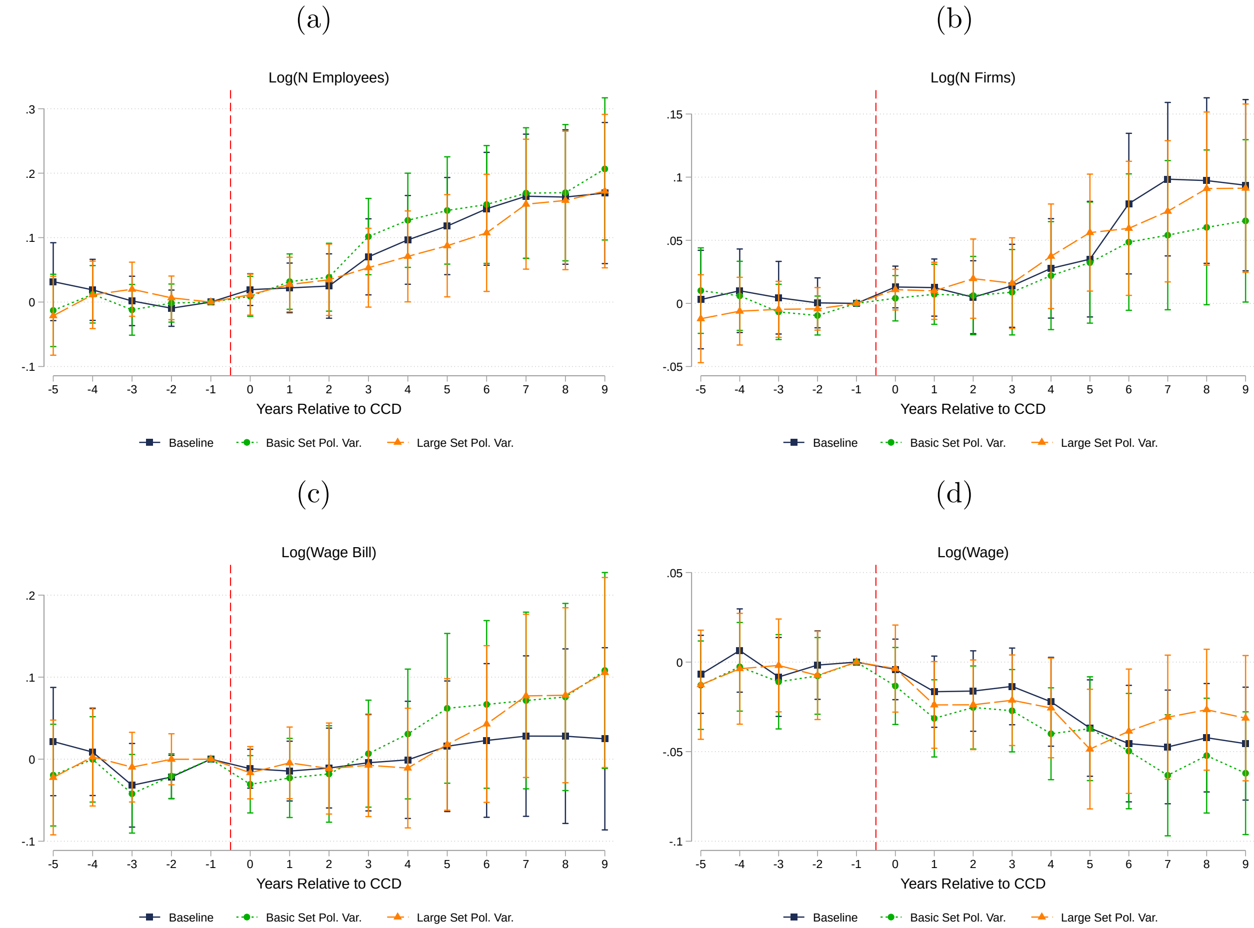
Because we document evidence of spillover effects in a radius of 20 km around treated municipalities, one may argue that matching out-of-region may be too restrictive. One may prefer instead to relax the out-of-region restriction and match treated municipalities with potential control units outside a 20-km radius of treated municipalities. We test the robustness of our results to this alternative matching strategy and report the results in Figure E.9. This Figure compares our baseline estimates (blue squares) with the estimates obtained using this alternative matching algorithm (orange triangles) and shows that these two sets of estimates are very similar to one another. We conclude that our results are robust to relaxing the out-of-region restriction.

Table E.1: Municipality Characteristics in the Year before the CCD

| | (1) Matched Sample | (2) T | (3) C | (4) T-C | (5) p |
|---|---|---|---|---|---|
| *Panel A: Baseline Sample* | | | | | |
| High Mafia Prevalence (Dugato et al., 2020) | 0.81 | 0.95 | 0.66 | 0.29 | 0.00 |
| Observations | 411 | 211 | 211 | | |
| *Panel B: Sample Matching on Basic Set of Socio-Political Variables* | | | | | |
| High Mafia Prevalence (Dugato et al., 2020) | 0.94 | 0.95 | 0.93 | 0.02 | 0.52 |
| Observations | 364 | 182 | 182 | | |

*Notes*: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to out-of-region potential control municipalities using our baseline matching algorithm and the algorithm augmented with basic socio-political variables in Panels A and B, respectively. 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. The statistics in column 4 are calculated as (2)-(3), and column 5 reports the p-value on the null hypothesis that the difference in means is equal to zero.

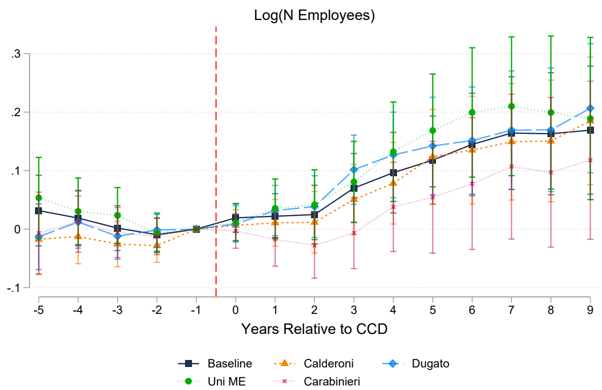
Figure E.1: Robustness: Alternative Matching Algorithms



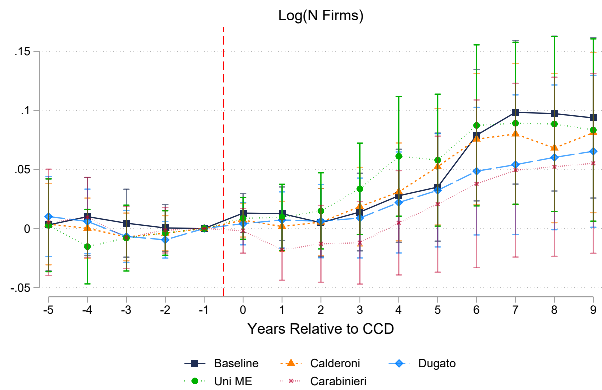

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 socio-political 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 E.2: Robustness: Alternative Mafia Measures

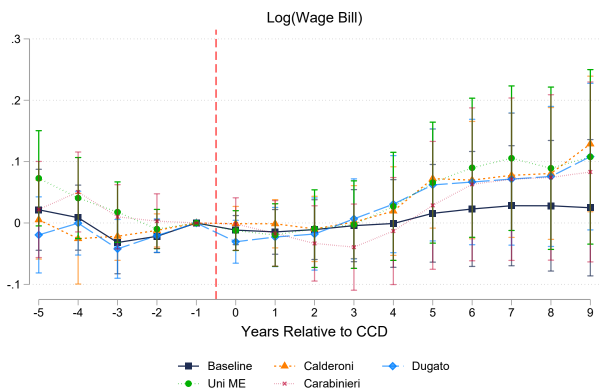
(a)



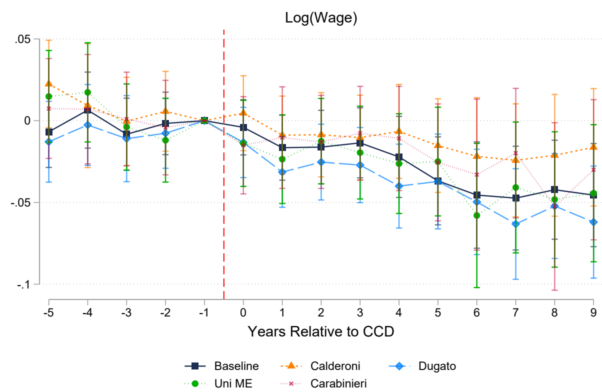

(b)




(c)




(d)

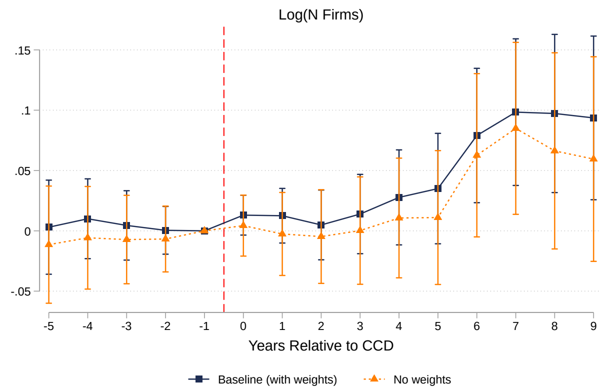



*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 4 alternative matching algorithms that include a basic set of socio-political variables and a measure of Mafia presence. The four measures of mafia presence we use are the index constructed by Calderoni (2011) (orange triangles), an indicator for mafia presence from Dugato et al. (2020) (light-blue diamonds), a news-based measure constructed by the University of Messina–Uni ME (green circles), and a measure based on a report by the Italian military police–"Carabinieri (red Xs), respectively. The small set of political variables includes turnout at the previous local elections and political orientation.

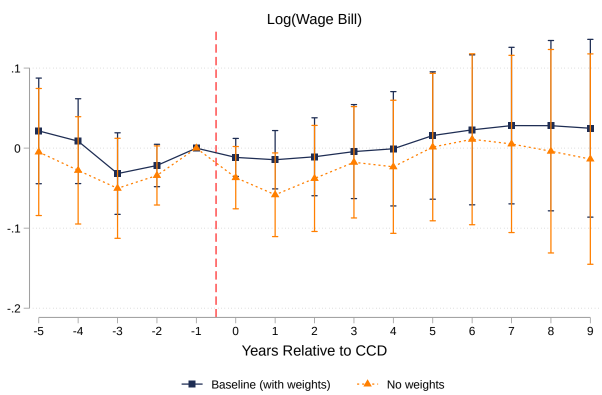
Figure E.3: Robustness: No Weights

(a)

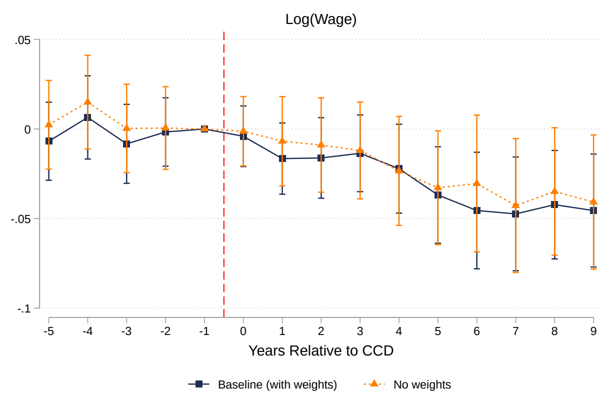

(b)




(c)

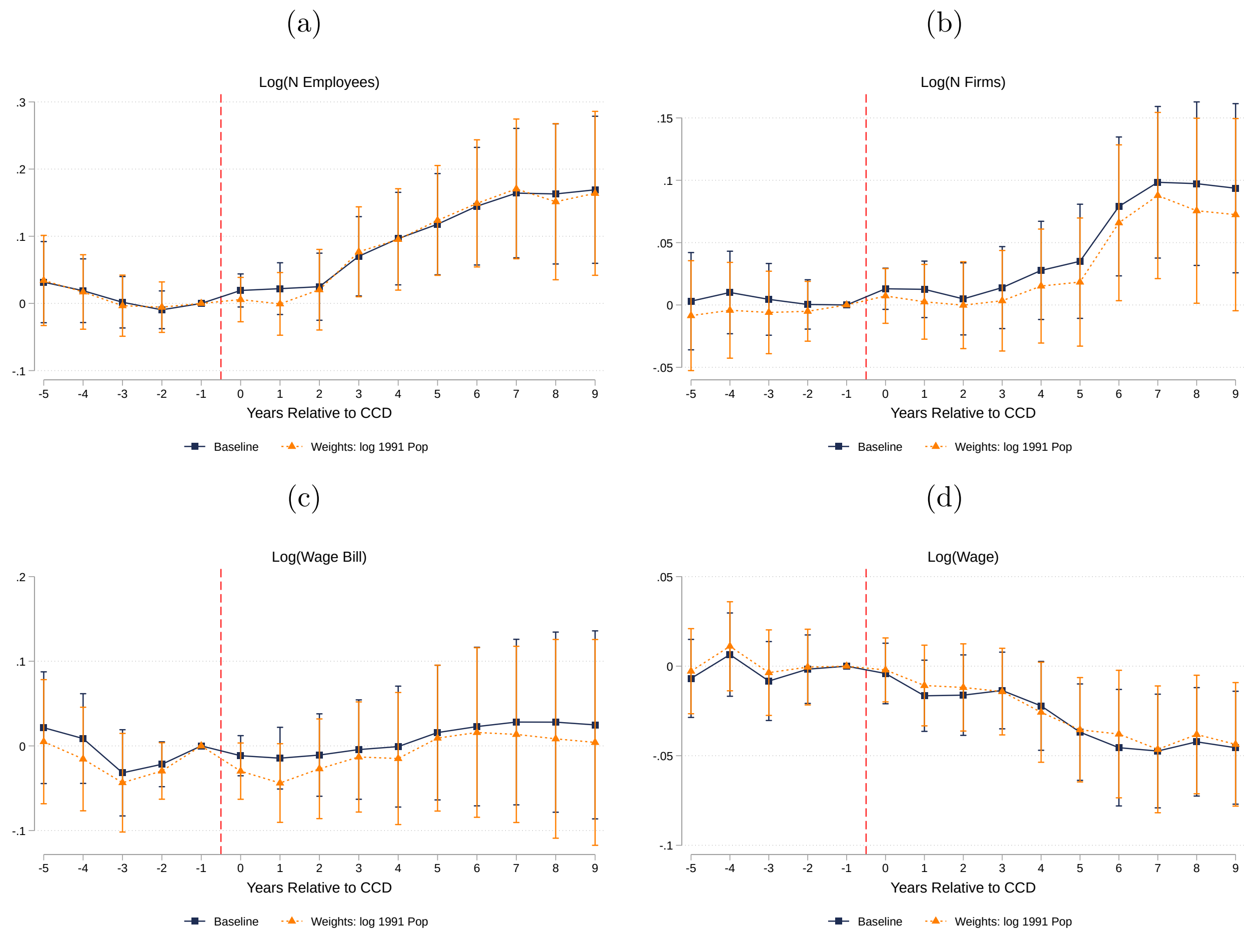



(d)




*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 2 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 estimating equation (1) without weights (orange triangles).

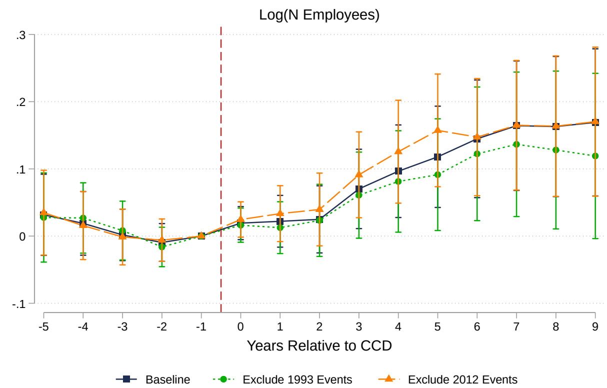
Figure E.4: Robustness: Population Weights



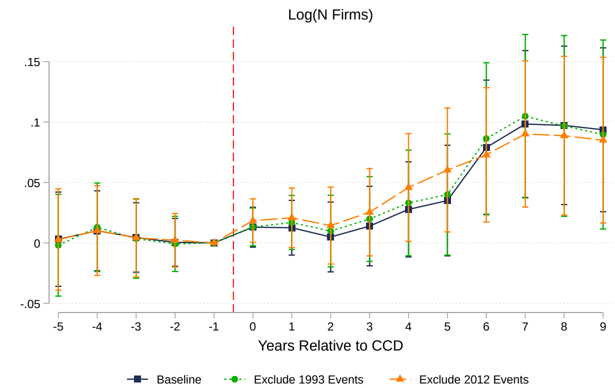

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 estimating equation (1) using as weights log 1991 population (orange triangles).

Figure E.5: Robustness: Exclude either 1993 or 2012 CCDs

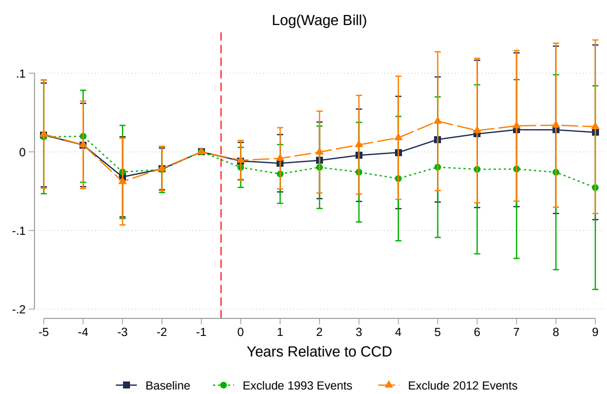
(a)



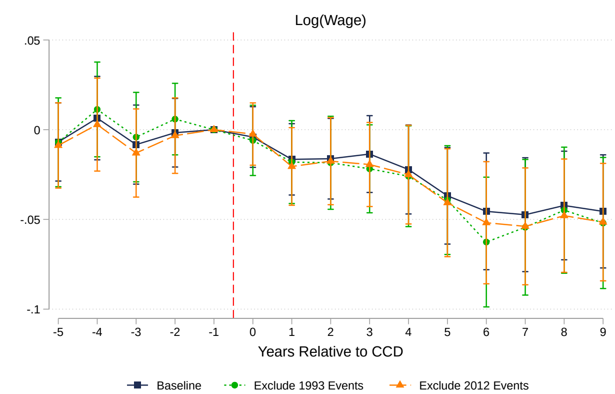

(b)




(c)



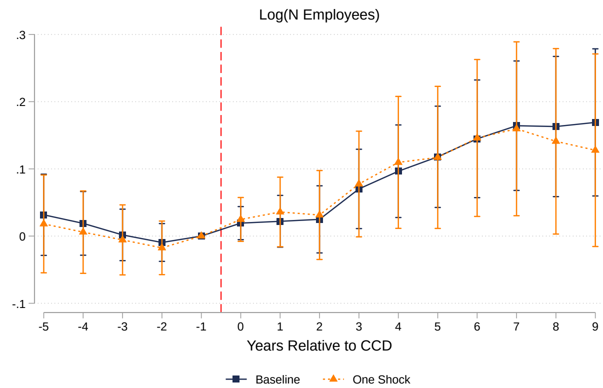

(d)



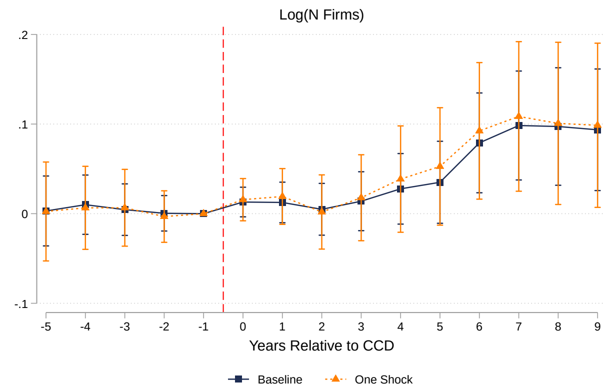

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 a regression excluding either the 1993 events (green circles) or the 2012 events (orange triangles), respectively.

Figure E.6: Robustness: Municipalities with Only One CCD

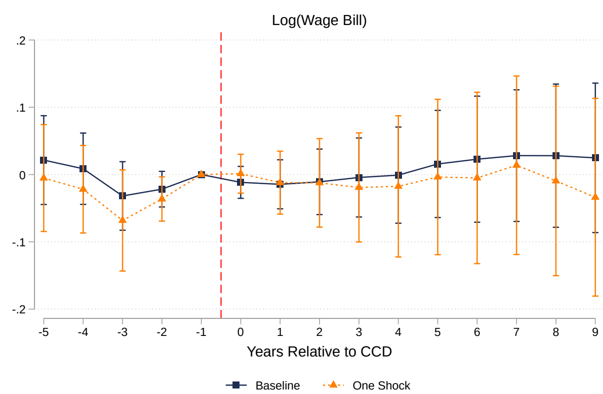
(a)



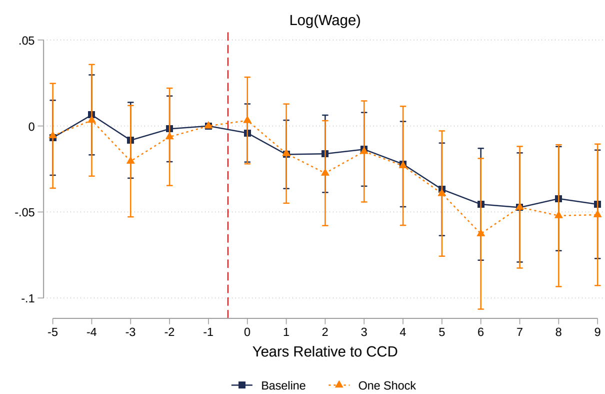

(b)




(c)

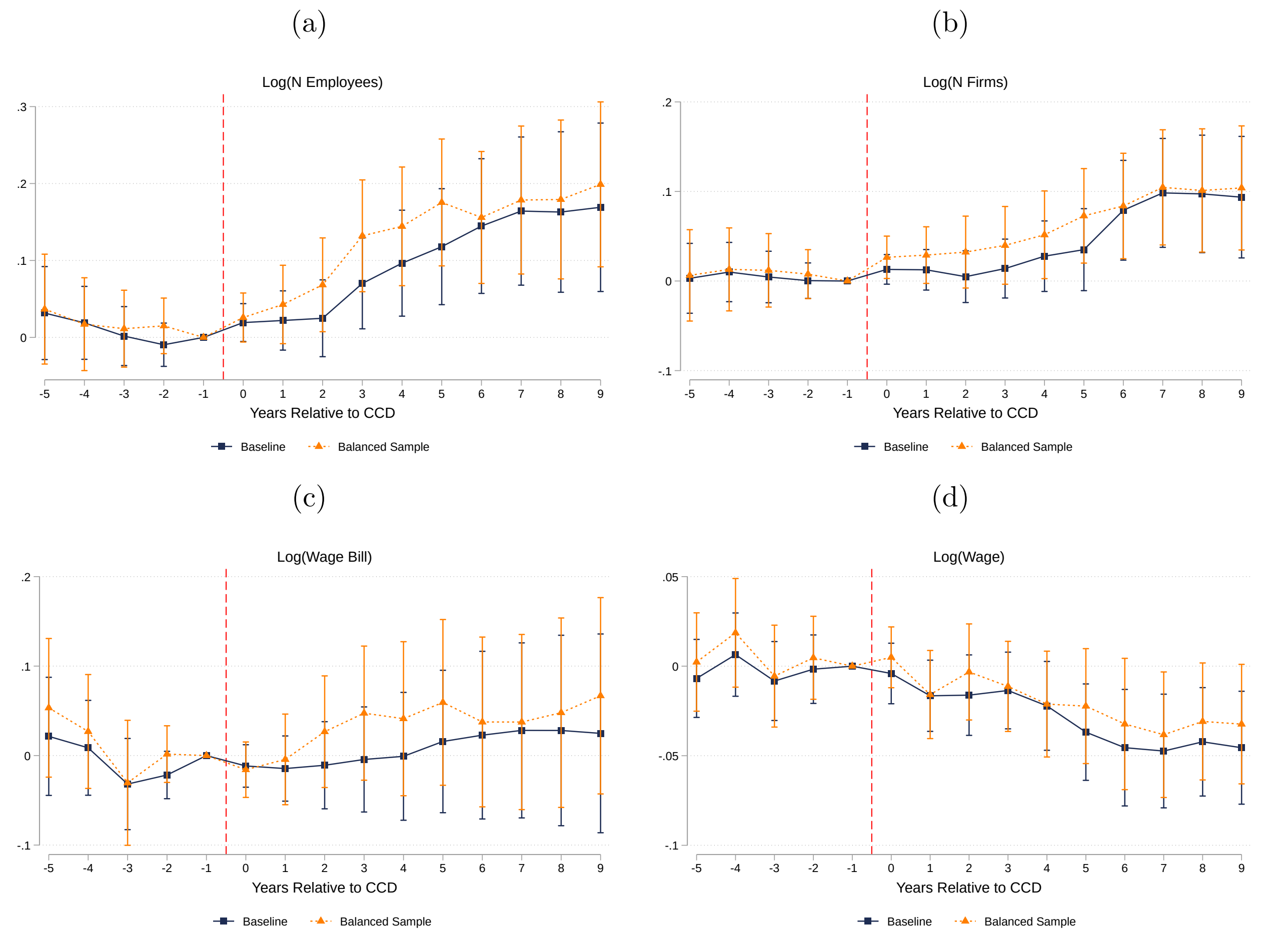



(d)



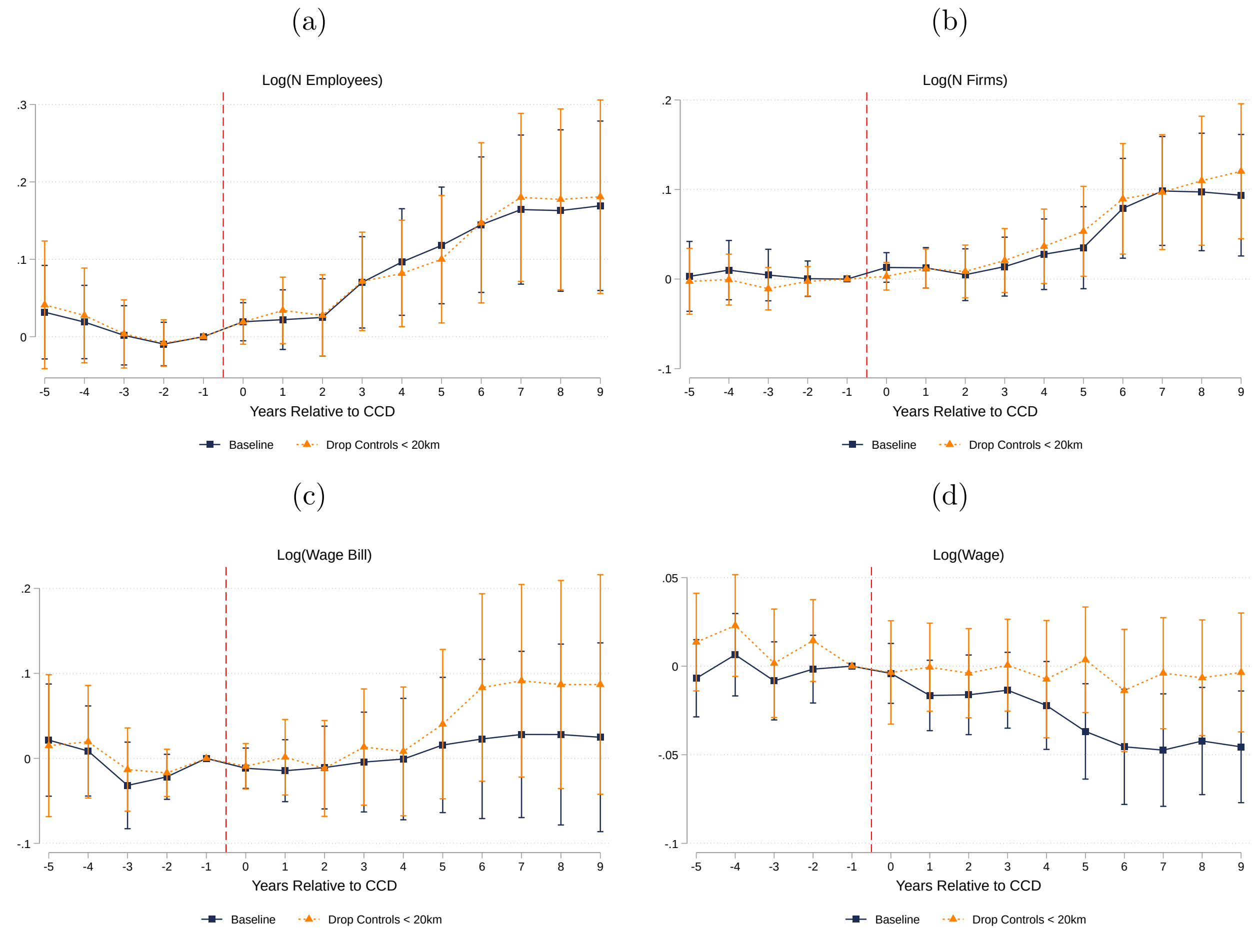

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 equation (1) on the subsample of municipalities that experience only one CCD over the study period (orange triangles).

Figure E.7: Robustness: Balanced Sample



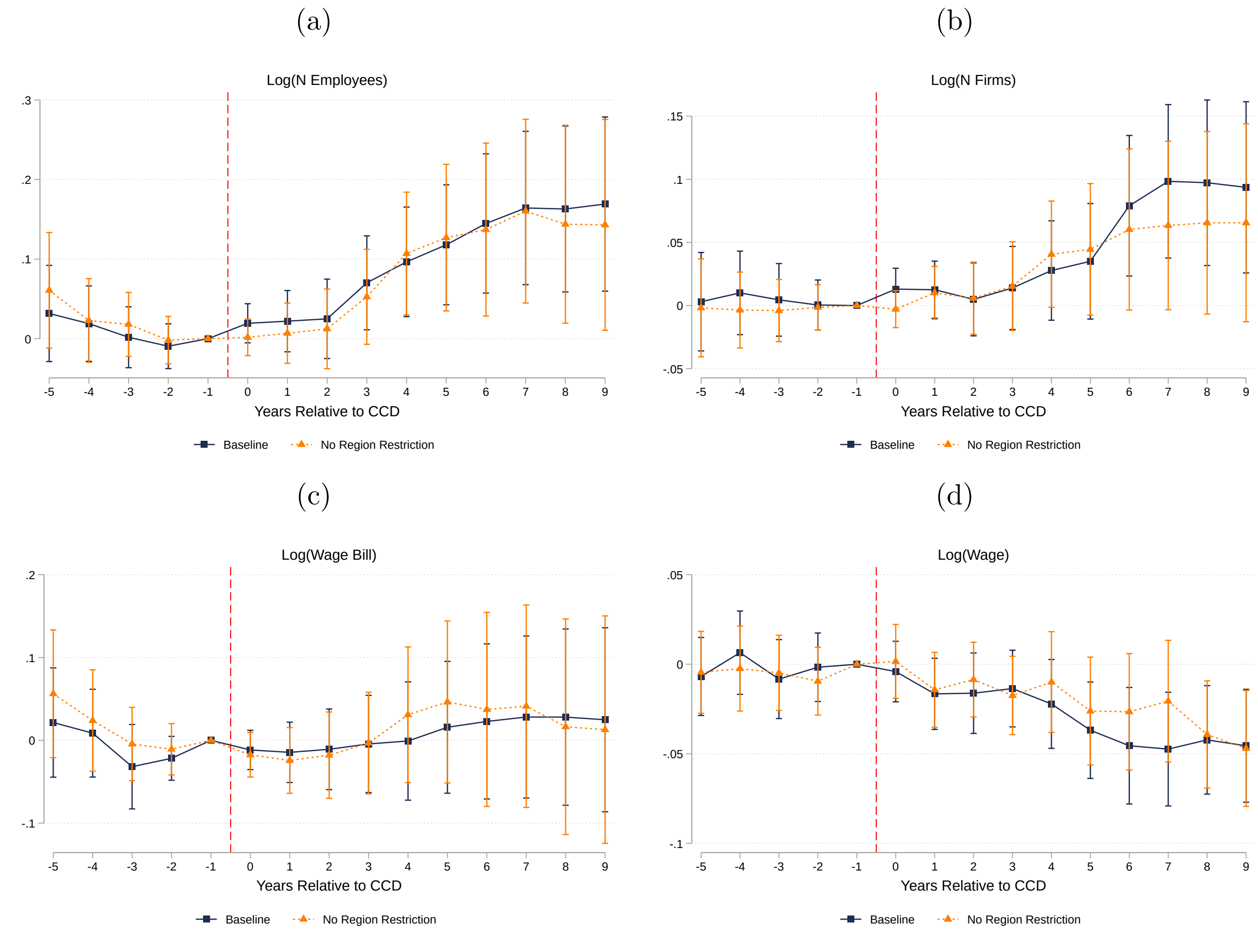

*Notes*: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 2 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 equation (1) on the balanced sample (orange triangles).

Figure E.8: 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 2 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 equation (1) on the matched sample obtained from discarding all potential controls in other regions in a 20 km radius from any treated municipality (orange triangles).

Figure E.9: Robustness: Relaxing the Out-of-Region Restriction (out of 20-km radius)




*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 2 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 estimating equation (1) on the matched sample obtained by relaxing the out-of-region restriction and discarding all potential controls in a 20 km radius from treated municipalities (orange triangles).

# Appendix F Addressing Potential Violations of Parallel Trends

This section follows Dustmann et al. (2022) and implements the honest approach to parallel trends proposed by Rambachan and Roth (2023) to address the potential violation of the parallel trend assumption in Figures 4b, C.1a, and C.1d.

Given the roughly linear shape of the pre-trends in these figures, we first estimate a linear trend based on pre-CCD event-study coefficients only (see left panel of Figure F.1). We then plot the deviations between the event-study coefficients and this linear trend (middle panel of Figure F.1). As the linear trend tends to go in the opposite direction of the post-event coefficients, this rotation returns positive and highly statistically significant coefficients in most cases (see for instance Figure F.1(b) or Figure F.1(e)).

We then assess the validity of this approach by reporting the results from the "honest approach" to parallel trends proposed by Rambachan and Roth (2023) (right panel of Figure F.1). Specifically, we bound the change in the slope of the differential trend between treated and control municipalities between two event-time periods using the following formula

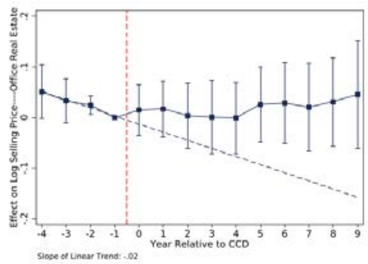
$$\Delta^{SD} \equiv \{\theta : |(\theta_{k+1} - \theta_k) - (\theta_k - \theta_{k-1})| \leq M\}. \tag{2}$$

Note that $M$ governs the maximum possible error of the linear extrapolation, i.e. by how much the slope of the pre-trend is allowed to change in post-intervention periods (assuming $M = 0$ thus implies that the counterfactual difference in trends between treated and control municipality in the outcome analyzed is exactly linear). The analysis reveals that, for the outcomes analyzed, the deviation from the estimated linear time trend needs to be economically large to have a null effect of the average impact of CCD—defined as the average of the post-CCD event-study coefficients. For instance, when looking at the effects on the price of office real estate —arguably the most important outcome among the figures considered—-we can reject a null effect unless we are willing to allow for the linear extrapolation across consecutive periods to be off in each event-year by more than ±15% from the linear trend estimated in the pre-period.

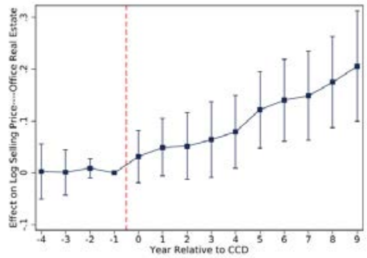
In conclusion, we assess the importance of differential pre-trends when analyzing the impact of CCDs on the price of office real estate and employment/wages spillover effects, outcomes for which the parallel trend assumption seems most likely to be violated. By extrapolating the estimated linear trend to post-intervention periods—and assessing the validity of such an approach using the recent methodology of Rambachan and Roth (2023)—we show that our results are robust even when allowing for significant deviations from this linear extrapolation.

Figure F.1: Rotation of Event-Study Coefficients and application of Rambachan and Roth (2023) approach to parallel trends

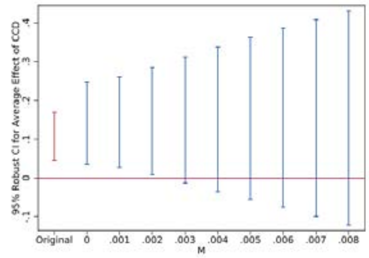
(a) Log Selling Price of Office Real Estate



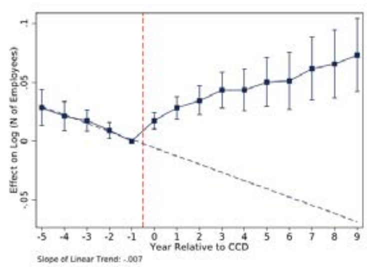

(b) Rotated Event-Study



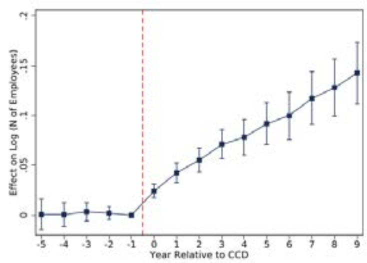

(c) Sensitivity



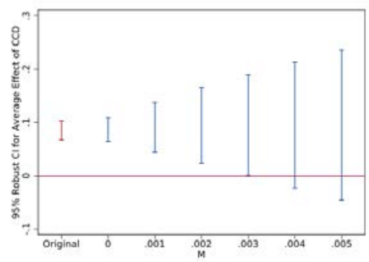

(d) Log Employment in Spillover Analysis



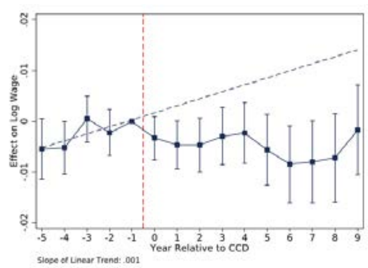

(e) Rotated Event-Study



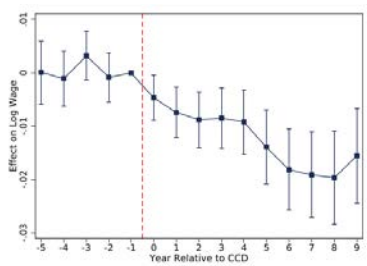

(f) Sensitivity



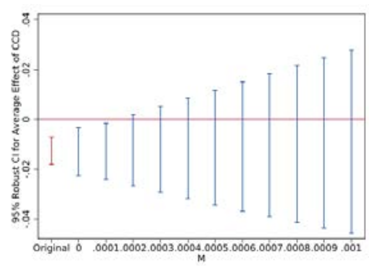

(g) Log Wages in Spillover Analysis




(h) Rotated Event-Study




(i) Sensitivity

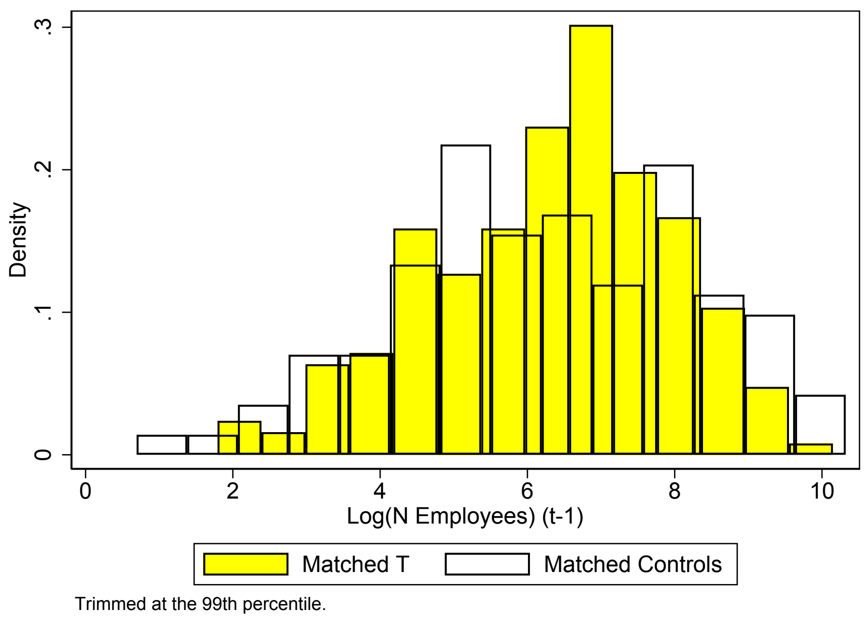



*Notes*: This figure analyzes potential violations of the parallel trend assumption in Figures 4b, C.1a, and C.1d. In the left panel, we overlay to the event-study coefficients a linear trend estimated using pre-CCD data and extrapolate it to the post-CCD era. The middle panel then reports the deviations from the event-study coefficients on the left panel and this linear time trend. Finally, the right panel reports the sensitivity of these results to the linear extrapolation of the pre-event coefficients using the honest approach to parallel trends of Rambachan and Roth (2023). In the right panel, we report the confidence sets described in Rambachan and Roth (2023) for the average of all post-CCD coefficients when we allow the slope of the pre-trend coefficients to change by no more than $M$ across consecutive periods.

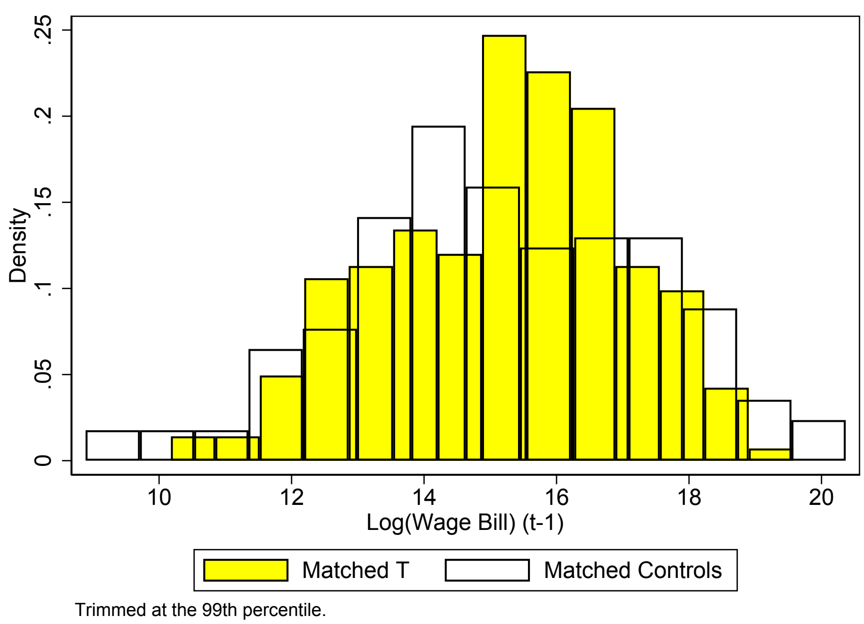
# Appendix G Additional Figures and Tables

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

(a)



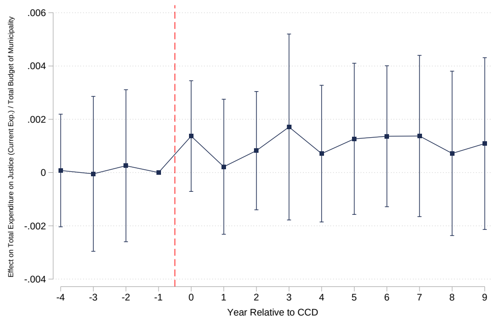

(b)



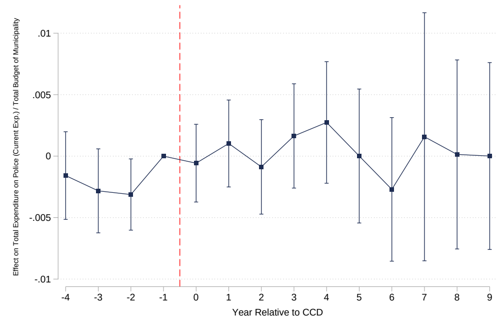

*Notes*: Matched firm sample, INPS data (1983–2017). Panels a and b display the distribution of log average earnings and log size for treated and matched control firms in the year before the CCD.

Figure G.2: Effects of CCDs on Expenditures

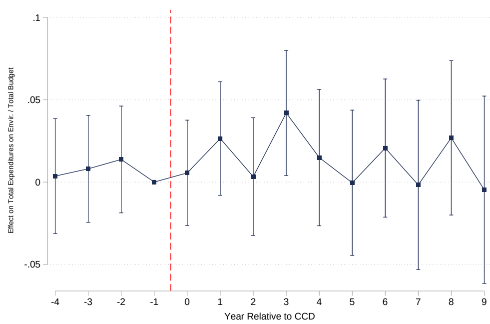
(a) Justice System



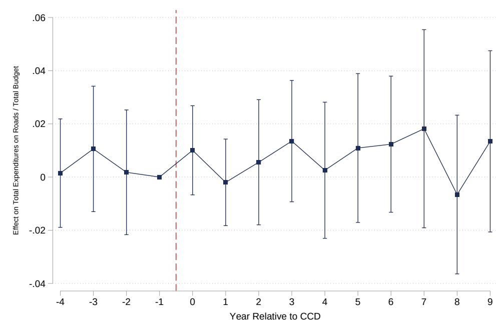

(b) Police




(c) Sanitation/Garbage Collection

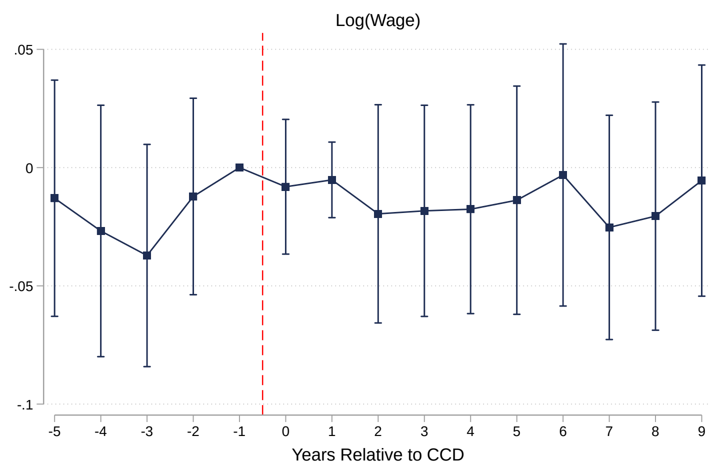



(d) Roads and Infrastructure




*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 G.6.

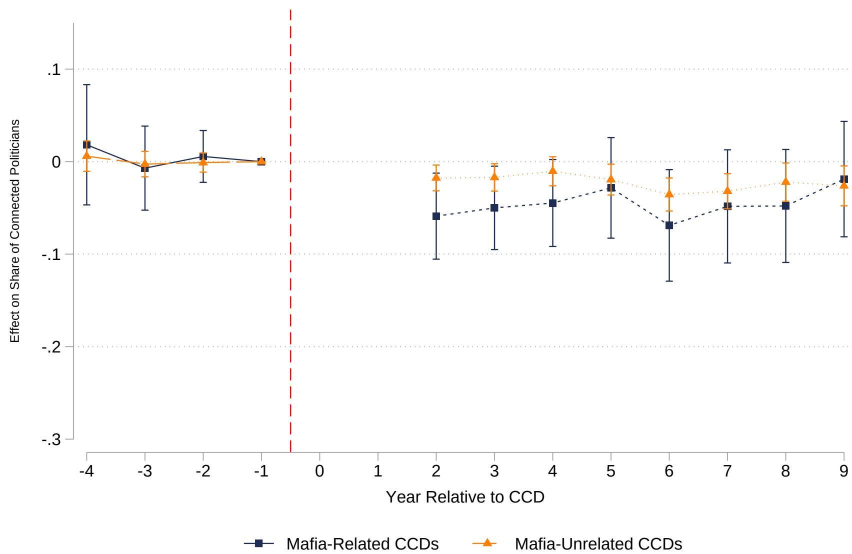
Figure G.3: Effects of CCDs on Incumbent Workers



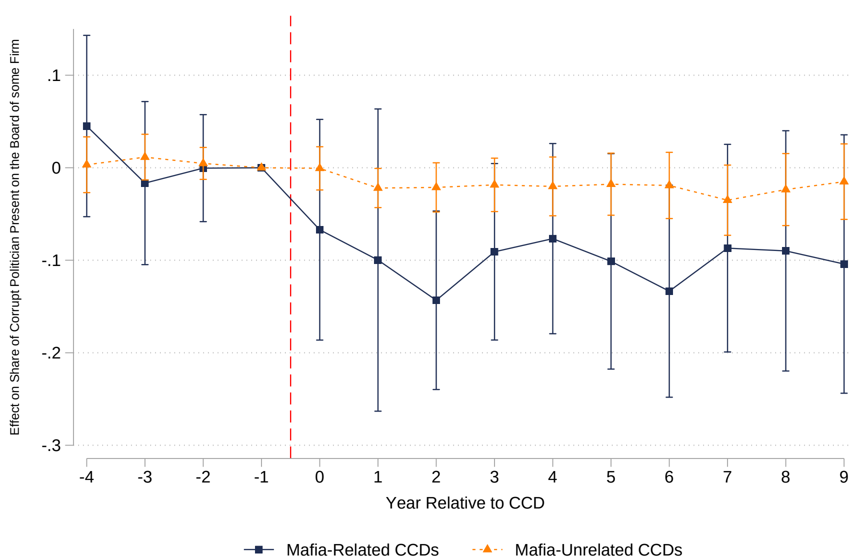

*Notes*: Matched municipality sample, INPS data (1983–2017). This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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 G.4: Political Connections and Corrupt Politicians on the Board of Firms Before and After the CCD for Mafia-Related CCDs and Mafia-Unrelated CCDs

(a) Contemporaneous Political Connections




(b) Corrupt Politicians




*Notes*: Matched municipality sample, Ministry of the Interior matched with data on ownership structure (2003–2017). The 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 in panel(a) is the fraction of elected politicians of municipality $m$ in year $t$ who, in the same year, also sit on the board of some firm. Coefficients at 0 and 1 are missing because in those years treated municipalities are administrated by the external commissioners. In Panel (b), the outcome variable is the fraction of "corrupt" politicians in municipality $m$ who serve on the board of firms at time $t$. We label "corrupt" those politicians who held power on the eve of the CCD. The blue squares and the orange triangles denote the Mafia-related and Mafia-unrelated CCDs, respectively.

Table G.1: Additional Municipality Characteristics

| | (1) Matched Sample | (2) T | (3 ) C | (4) T-C | (5) p | (6) N |
|---|---|---|---|---|---|---|
| *Panel A: Real Estate Prices* | | | | | | |
| Sale Price – Housing | 826.99 | 734.34 | 910.55 | -176.21 | 0.00 | 194 |
| Sale Price – Commercial Real Estate | 768.33 | 699.01 | 830.1 | -131.09 | 0.01 | 191 |
| Sale Price – Office Real Estate | 876.68 | 829.53 | 921.66 | -92.12 | 0.07 | 170 |
| Sale Price – Industrial Real Estate | 439.94 | 440.89 | 439.09 | 1.8 | 0.94 | 168 |
| Sale Price – Parking | 511.51 | 470.62 | 544.32 | -73.71 | 0.08 | 164 |
| *Panel B: Population and Public Finances* | | | | | | |
| Population | 14546.71 | 14913.13 | 14183.35 | 729.77 | 0.85 | 239 |
| Total Revenues | 18.04 | 18.37 | 17.72 | .64 | 0.9 | 239 |
| Taxes/Revenue | 0.29 | 0.28 | 0.31 | -0.03 | 0.13 | 239 |
| Expenditure/Revenue | 0.8 | 0.78 | 0.81 | -.03 | .16 | 239 |
| *Panel C: Characteristics of Public Elected Officials* | | | | | | |
| Share of First-Time Politicians | 0.53 | 0.53 | 0.54 | -0.01 | 0.54 | 403 |
| Share of Male Politicians | 0.91 | 0.93 | 0.88 | 0.05 | 0.00 | 403 |
| Education | 13.21 | 13.35 | 13.08 | 0.27 | 0.11 | 403 |
| Age | 44.46 | 44.23 | 44.67 | -0.44 | 0.26 | 403 |

*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. The statistics in column 4 are calculated as (2)-(3), and column 5 reports the p-value associated with the null hypothesis that the difference in means is equal to zero. Column 6 reports the number of observations.

Table G.2: Municipality Characteristics in the 5 Years before the CCD

| | (1) Matched Sample | (2) T | (3) C | (4) T-C | (5) p |
|---|---|---|---|---|---|
| Population in 1991 | 15263.83 | 15522.71 | 15004.95 | 517.76 | 0.84 |
| N Establishments | 241.78 | 211.58 | 271.98 | -60.40 | 0.00 |
| N Firms | 232.59 | 203.59 | 261.59 | -58.00 | 0.00 |
| N Sole Proprietorship | 125.13 | 105.92 | 144.35 | -38.43 | 0.00 |
| N of Employees | 2226.94 | 1474.73 | 2979.15 | -1504.42 | 0.00 |
| Av. Daily Wage | 72.08 | 72.57 | 71.59 | 0.99 | 0.09 |
| Av. Daily Wage: Prev. Not Empl. | 63.05 | 64.23 | 61.88 | 2.35 | 0.00 |
| Av. Daily Wage: Prev. Empl. | 73.65 | 74.09 | 73.20 | 0.90 | 0.11 |
| Municipal Wage Bill (M of €) | 39.43 | 19.34 | 59.52 | 40.18 | 0.00 |
| Share New Entrants | 0.15 | 0.16 | 0.14 | 0.02 | 0.08 |
| Share Prev. Not Empl. | 0.27 | 0.28 | 0.25 | 0.03 | 0.03 |
| Share Prev. Not Empl. $<$ 30 y.o. | 0.16 | 0.17 | 0.15 | 0.02 | 0.11 |
| Share Firm Entries | 0.12 | 0.13 | 0.12 | 0.01 | 0.00 |
| Share Firm Exists | 0.08 | 0.09 | 0.08 | 0.01 | 0.45 |
| Turnout | 0.78 | 0.77 | 0.79 | -0.02 | 0.00 |
| Observations | 2110 | 1055 | 1055 | | |

*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 5 years 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. The statistics in column 4 are calculated as (2)-(3), and column 5 reports the pvalue on the null hypothesis that the difference in means is equal to zero.

Table G.3: Additional Municipality Characteristics in the 5 Years before the CCD

| | (1) Matched Sample | (2) T | (3 ) C | (4) T-C | (5) p | (6) N |
|---|---|---|---|---|---|---|
| *Panel A: Real Estate Prices* | | | | | | |
| Sale Price – Housing | 805.33 | 703.64 | 895.37 | -191.73 | 0.00 | 707 |
| Sale Price – Commercial Real Estate | 765.31 | 701.11 | 821.93 | -120.82 | 0.00 | 702 |
| Sale Price – Office Real Estate | 868.46 | 823.98 | 911.53 | -87.55 | 0.00 | 624 |
| Sale Price – Industrial Real Estate | 431.88 | 434.37 | 429.72 | 4.65 | 0.72 | 615 |
| Sale Price – Parking | 502.93 | 453.39 | 541.17 | -87.78 | 0.00 | 606 |
| *Panel B: Population and Public Finances* | | | | | | |
| Population | 14775.83 | 14653.34 | 14898.06 | -244.72 | 0.90 | 925 |
| Total Revenues | 18.65 | 19.09 | 18.22 | .88 | 0.75 | 925 |
| Taxes/Revenue | 0.26 | 0.24 | 0.28 | -0.05 | 0.00 | 925 |
| Expenditure/Revenue | 0.83 | 0.82 | 0.84 | -0.02 | 0.02 | 925 |
| *Panel C: Characteristics of Public Elected Officials* | | | | | | |
| Share of First-Time Politicians | 0.54 | 0.54 | 0.54 | 0.00 | 0.75 | 785 |
| Share of Male Politicians | 0.91 | 0.93 | 0.89 | 0.04 | 0.00 | 1022 |
| Education | 13.11 | 13.30 | 12.94 | 0.36 | 0.00 | 1020 |
| Age | 43.87 | 43.43 | 44.27 | -0.84 | 0.00 | 1022 |

*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 in the 5 years 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. The statistics in column 4 are calculated as (2)-(3), and column 5 reports the p-value associated with the null hypothesis that the difference in means is equal to zero. Column 6 reports the number of municipality-year observations.

Table G.4: Effects of CCDs on Municipality Employment, Wages, and Firms (Matching within Region)

| | (1)<br>Log(Empl) | (2)<br>Log(N Firms) | (3)<br>Log(Wage Bill) | (4)<br>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 |
| N | 11,400 | 11,400 | 11,400 | 11,400 |
| Muni FE | Yes | Yes | Yes | Yes |
| Reg-Year FE | Yes | Yes | Yes | Yes |

*Notes*: Matched municipality sample, INPS data (1983–2017). Treated municipalities are matched to potential control municipalities in the same region. This table reports the estimated $\theta_k$ 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.

Table G.5: Effects of CCDs on Municipality Revenue

| | (1) Log Total Revenue | (2) Taxes/ Tot. Rev. | (3) Transfers/ Tot. Rev. | (4) Loans/ Tot. Rev. | (5) Other Rev./ Tot. Rev. |
|---|---|---|---|---|---|
| On Impact | -0.0404 | 0.0169 | 0.0222 | -0.0067 | -0.0301 |
| | (0.0420) | (0.0133) | (0.0113) | (0.0142) | (0.0183) |
| Short Run | -0.0533 | 0.0174 | 0.0048 | -0.0362 | 0.0187 |
| | (0.0615) | (0.0217) | (0.0149) | (0.0207) | (0.0270) |
| Long Run | 0.0259 | -0.0163 | -0.0211 | 0.0335 | 0.0059 |
| | (0.0738) | (0.0241) | (0.0224) | (0.0326) | (0.0348) |
| Mean | 15.906 | 0.277 | 0.261 | 0.093 | 0.371 |
| N | 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 |

*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.

Table G.6: Effects of CCDs on Municipality Expenditures

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Tot. Exp./ Tot. Rev. | Admin/ Tot. Rev. | Justice Sys./ Tot. Rev. | Police/ Tot. Rev. | Educ./ Tot. Rev. | Culture/ Tot. Rev. | Sport/ Tot. Rev. | Tourism/ Tot. Rev. | Roads/ Tot. Rev. | Sanitat./ Tot. Rev. | Other Social Policies/ Tot. Rev. |
| On Impact | 0.0162 | 0.0085 | 0.0014 | -0.0006 | -0.0058 | 0.0056 | 0.0019 | -0.0004 | 0.0101 | 0.0056 | -0.0100 |
| | (0.0169) | (0.0110) | (0.0011) | (0.0016) | (0.0057) | (0.0041) | (0.0052) | (0.0035) | (0.0086) | (0.0163) | (0.0104) |
| Short Run | 0.0547 | 0.0019 | 0.0017 | 0.0016 | -0.0067 | 0.0029 | 0.0099 | -0.0023 | 0.0135 | 0.0420 | -0.0098 |
| | (0.0230) | (0.0158) | (0.0018) | (0.0022) | (0.0069) | (0.0060) | (0.0069) | (0.0051) | (0.0116) | (0.0194) | (0.0121) |
| Long Run | -0.0488 | -0.0275 | 0.0011 | 0.0000 | -0.0059 | 0.0018 | 0.0031 | -0.0142 | 0.0134 | -0.0047 | -0.0159 |
| | (0.0379) | (0.0178) | (0.0016) | (0.0039) | (0.0106) | (0.0056) | (0.0078) | (0.0083) | (0.0174) | (0.0290) | (0.0182) |
| 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 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Reg-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*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. Each outcome is normalized relative to the municipality's total budget. The first column reports total expenditures of a municipality relative to its total revenue. The remaining columns represent the different items on which the municipality can spend its money, again normalized relative to the overall budget. Column 9 reports expenditures on roads and other infrastructures. Column 10 reports expenditures on the environment, which is mainly allocated to garbage collection.