For Online Publication

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

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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).

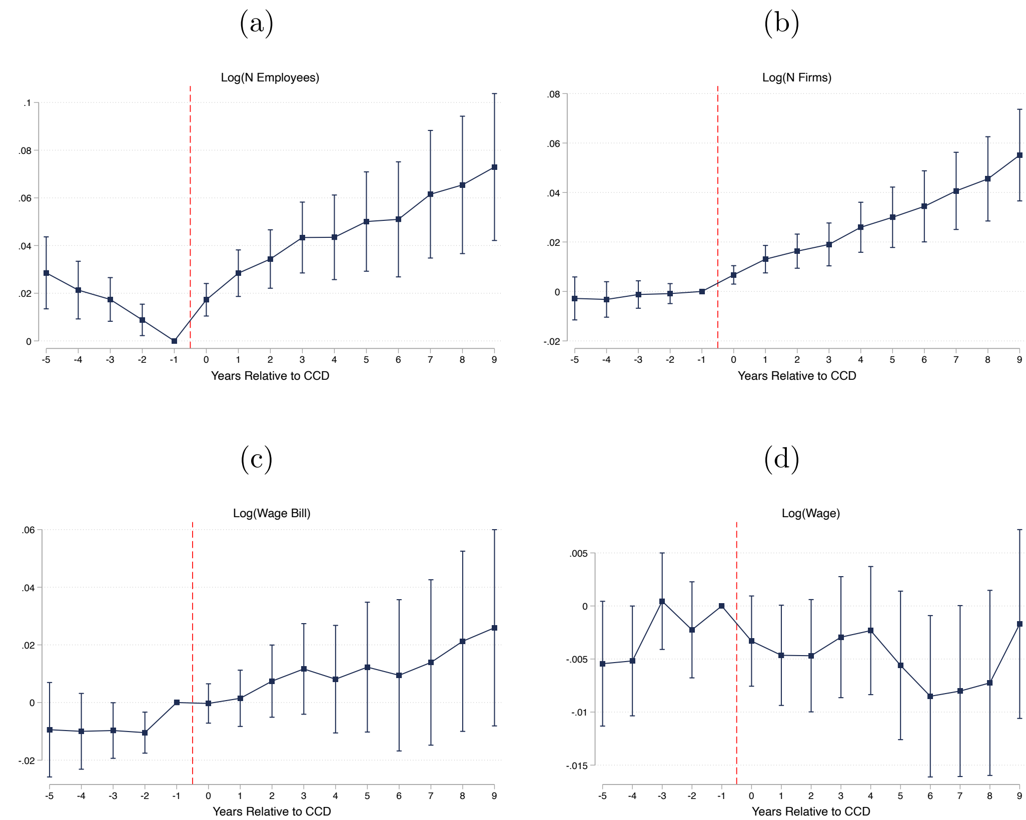
Wage bill (municipality level): the sum of all wages paid to formal private sector workers employed in municipality m in year t. The wage bill of workers employed at incumbent firms is constructed analogously.

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, 2017; 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., recentralization), we study the effect of CCDs that are caused by instances other than Mafia infiltration (Italian Ministry of Interior, 2017). 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 ten 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).[26] 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.

[26]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 (2017b) and De Feo and De Luca (2017a). 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 (2017b). 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

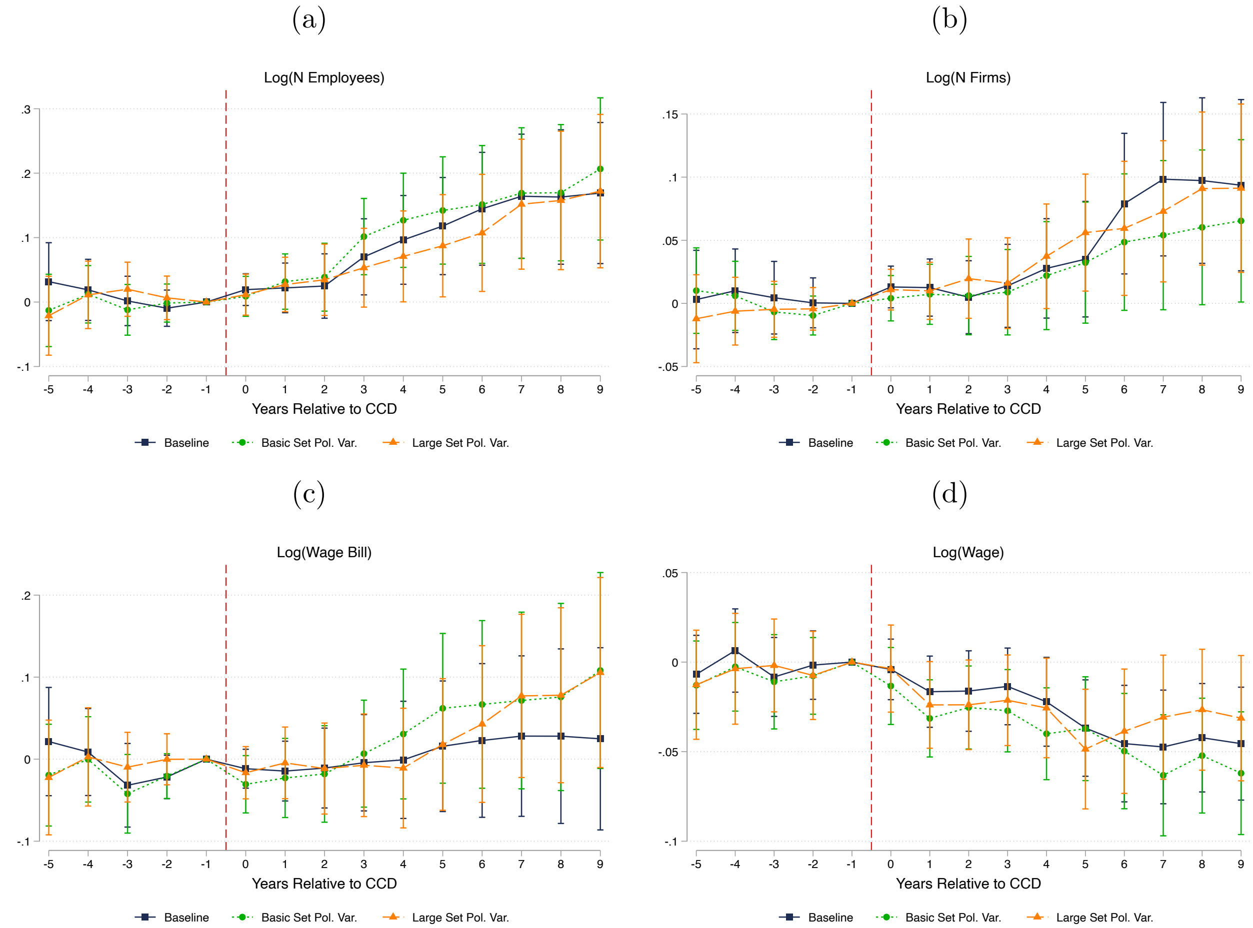
Because we document evidence of spillover effects in a radius of 20 km around treated municipalities, one may argue that matching out-of-region may be too restrictive. One may prefer instead to relax the out-of-region restriction and match treated municipalities with potential control units outside a 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.

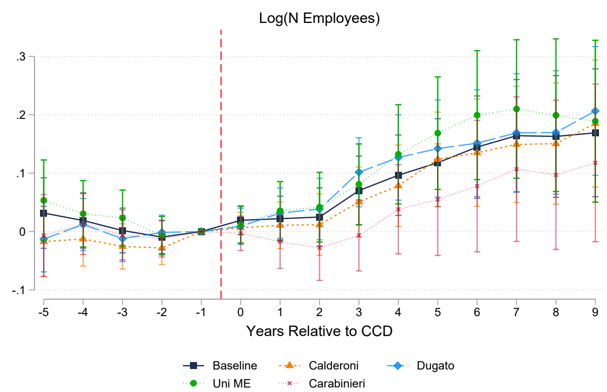
Figure E.1: Robustness: Alternative Matching Algorithms



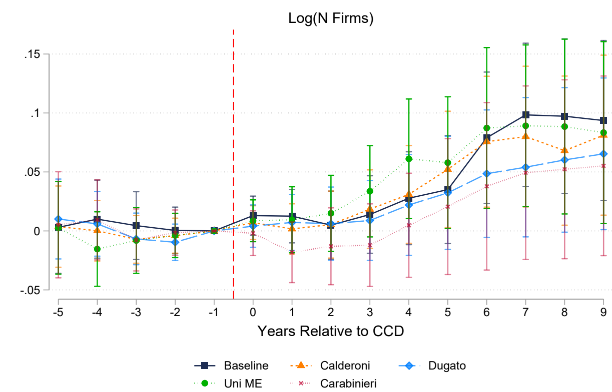
Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at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

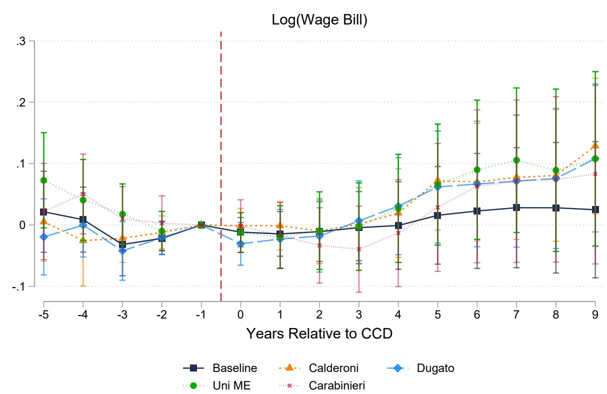
(a)



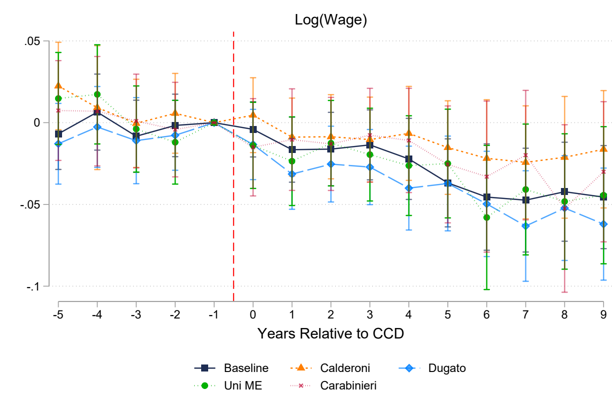
(b)



(c)

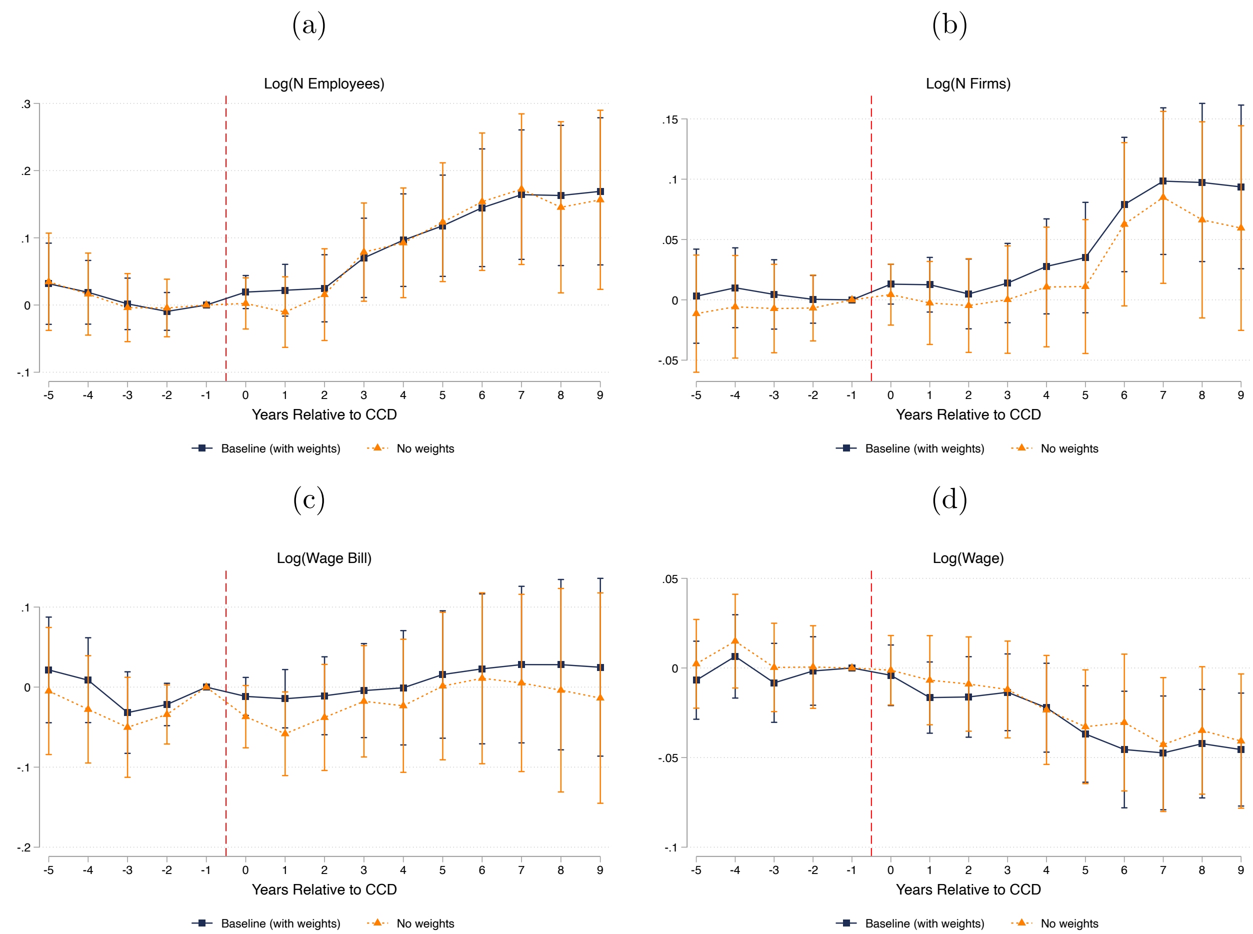


(d)



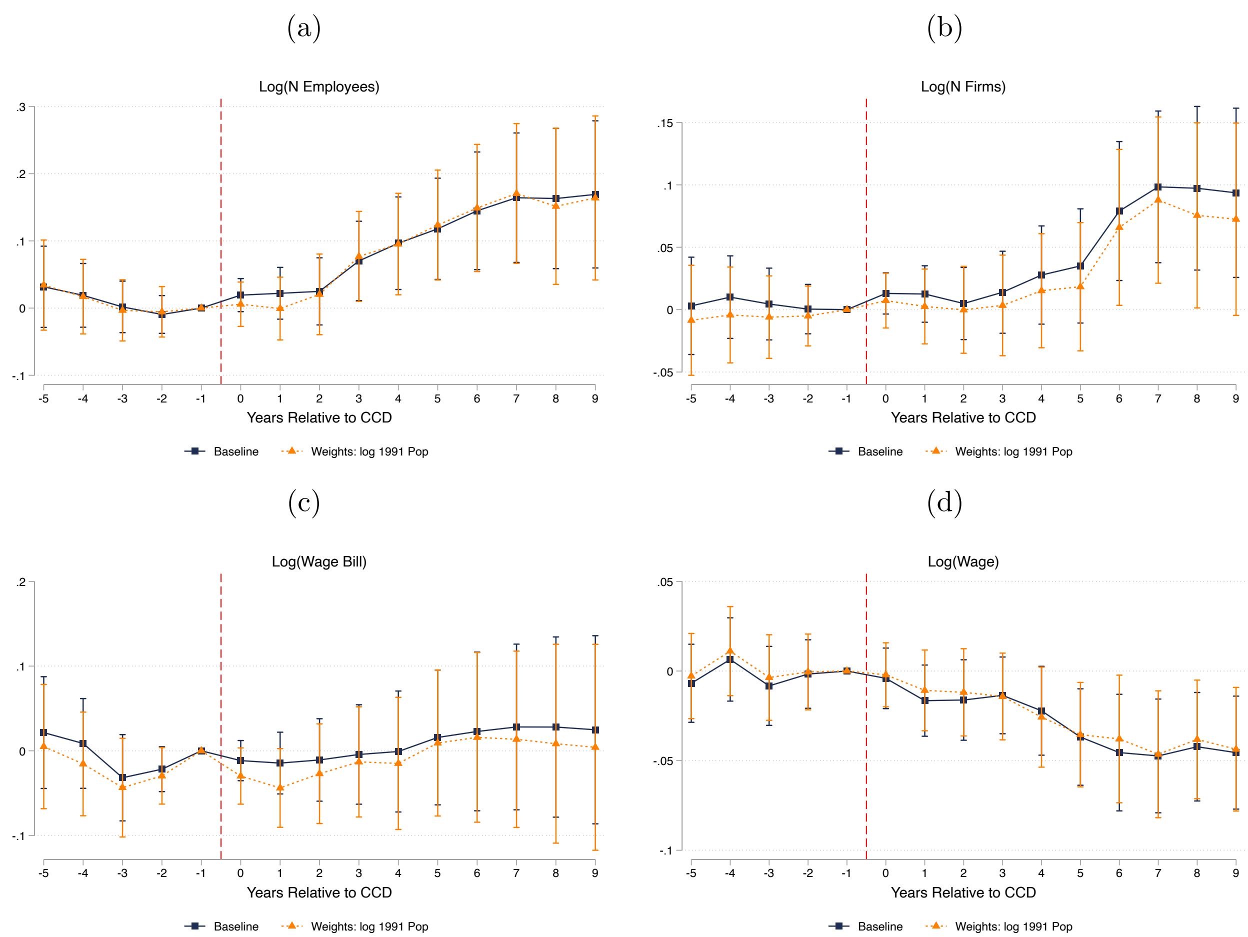
Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at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



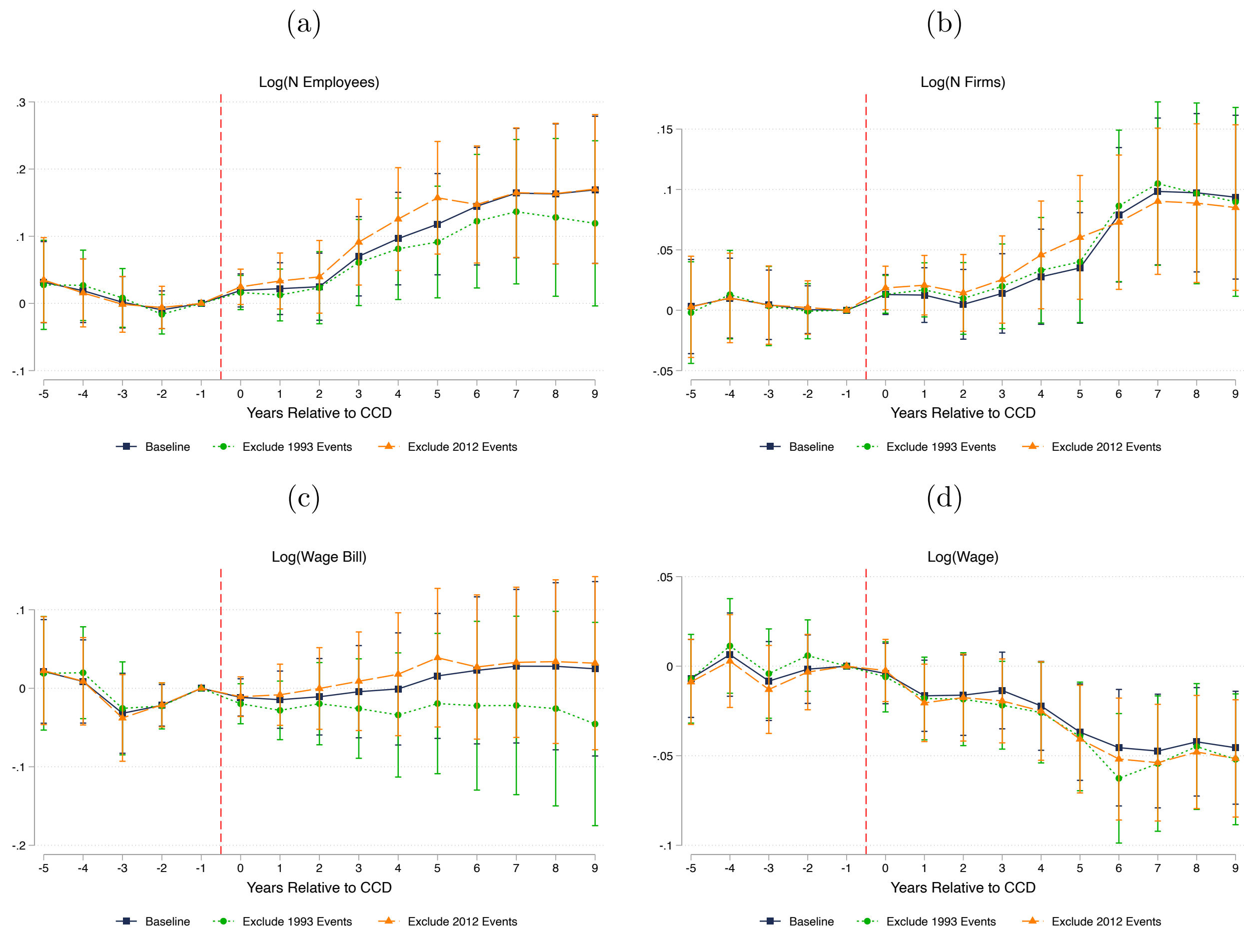
Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at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



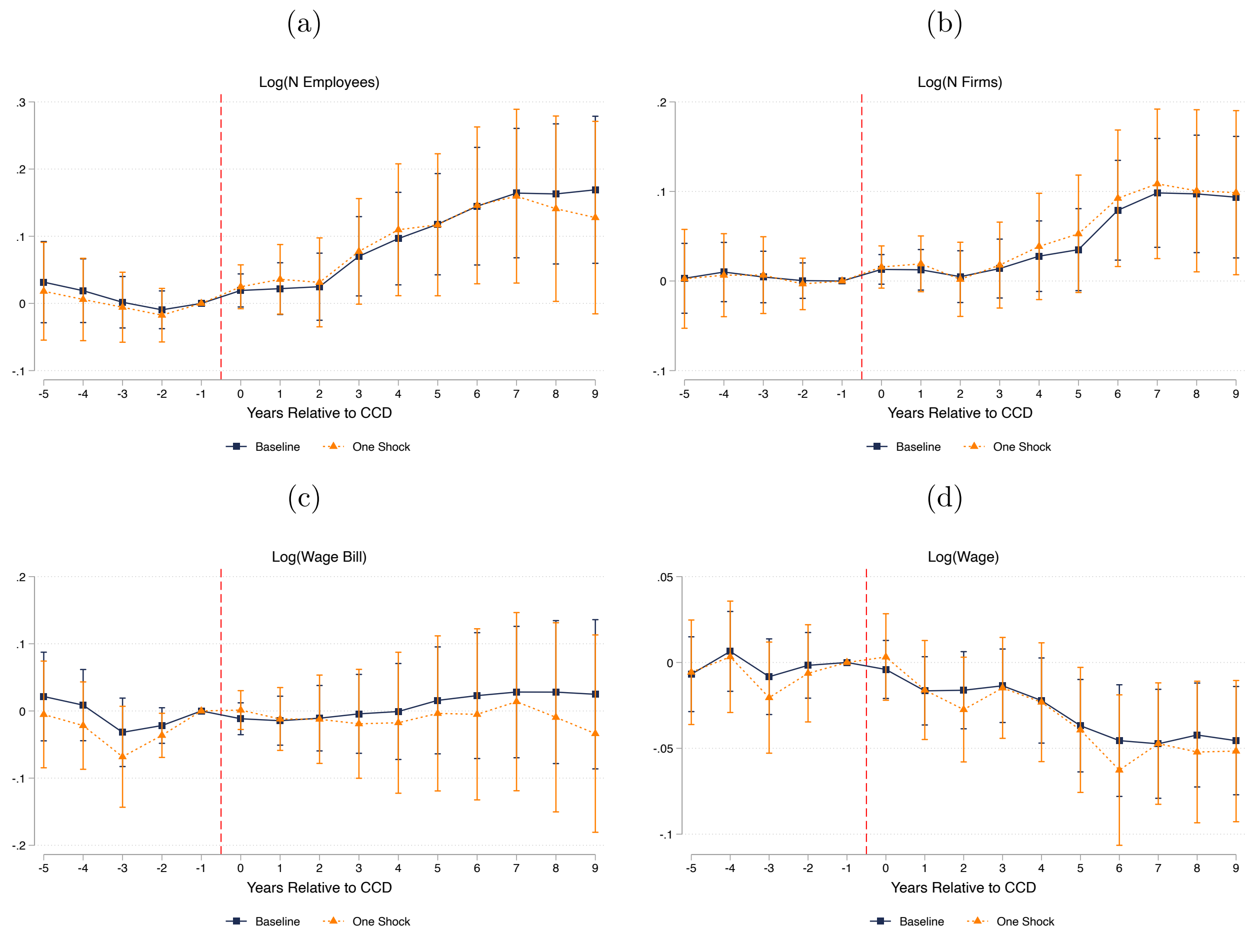
Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at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



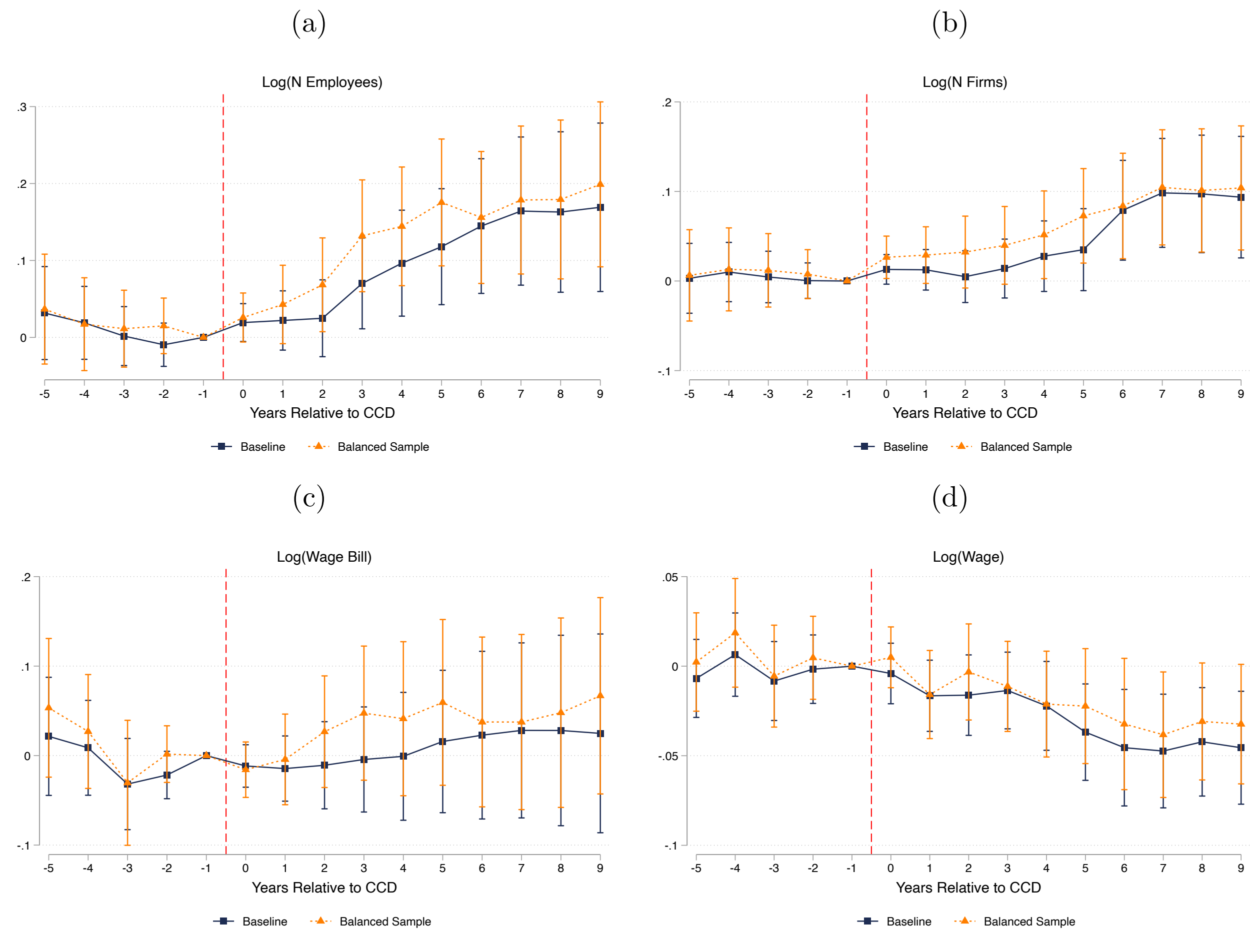
Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at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



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

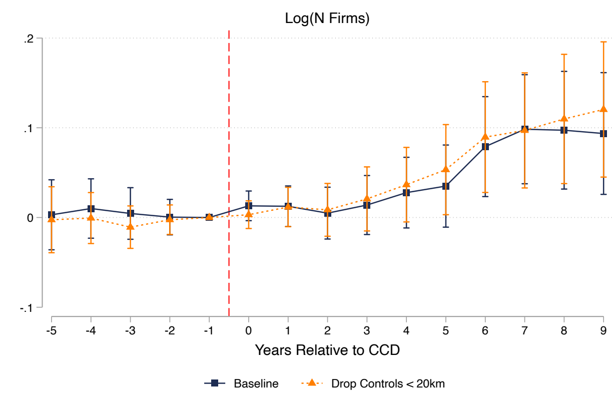


Notes: Matched municipality sample, INPS data (1983–2017). Panels a–d report the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients at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

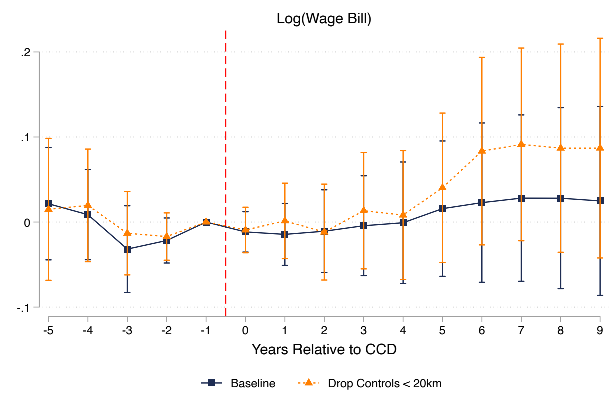
Figure E.8: Robustness: Dropping Potential Controls within 20 km

(a)

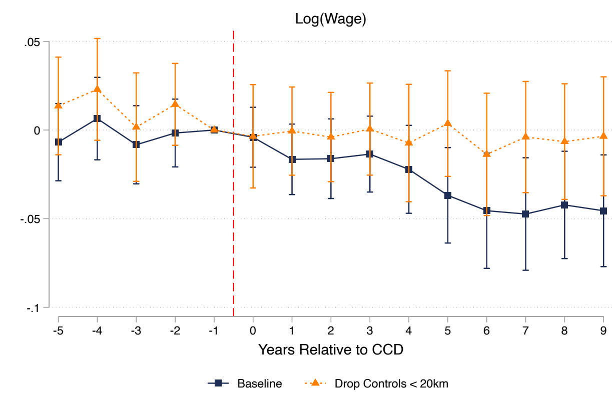
(b)



(c)

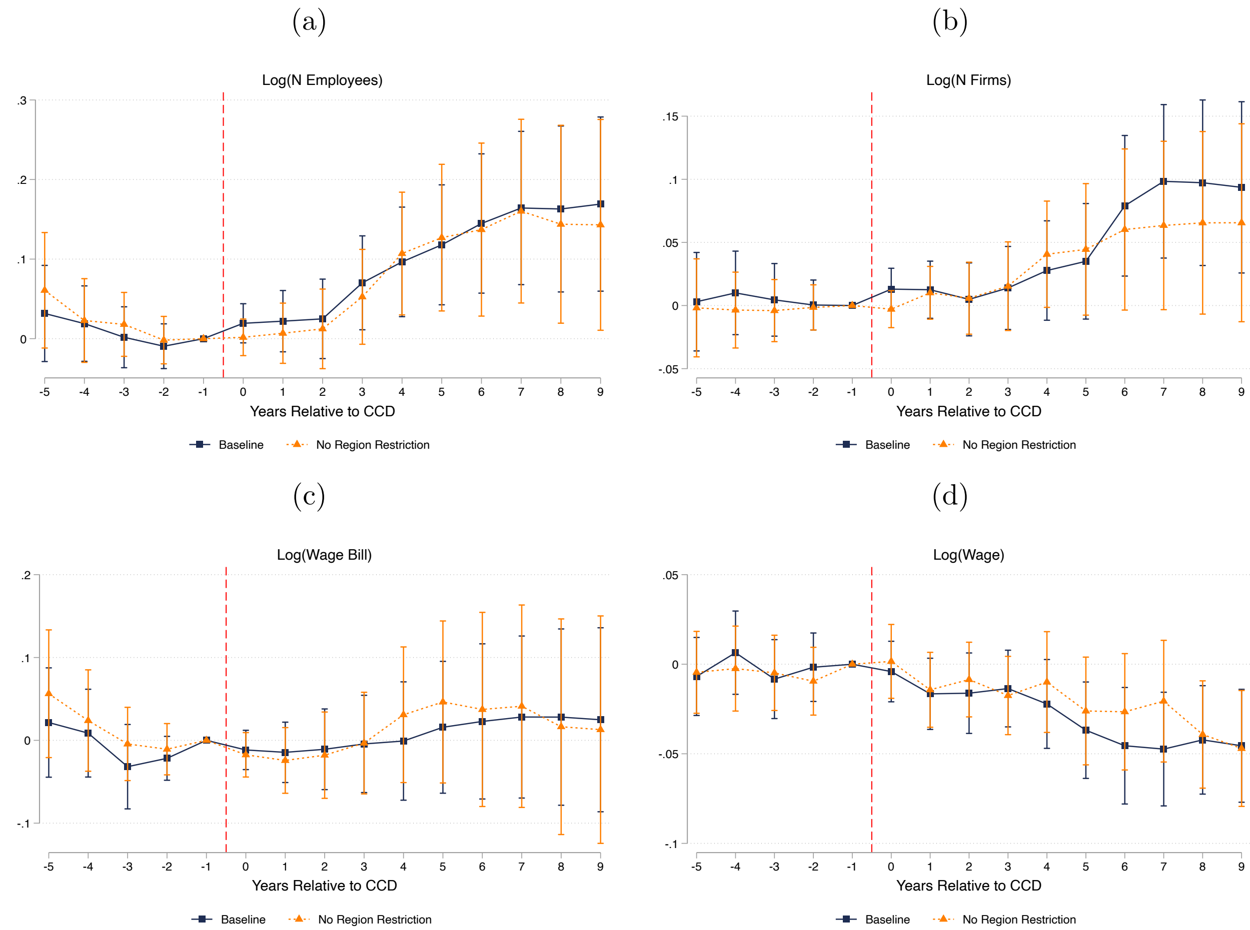


(d)



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 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

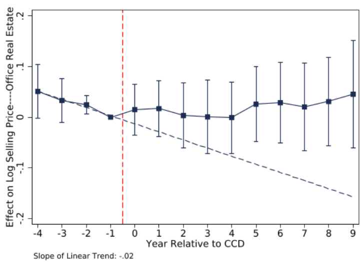
$$\Delta^{SD} \equiv \{\theta : |(\theta_{k+1} - \theta_k) - (\theta_k - \theta_{k-1})| \leq M\}. \tag{2}$$

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.

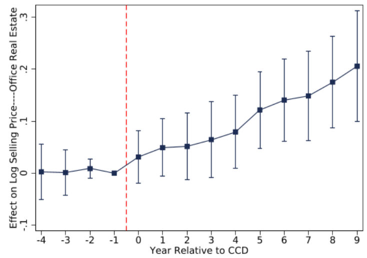
In conclusion, we assess the importance of differential pre-trends when analyzing the impact of CCDs on the price of office real estate and employment/wages spillover effects, outcomes for which the parallel trend assumption seems most likely to be violated. By extrapolating the estimated linear trend 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

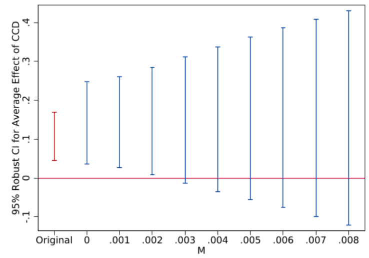
(a) Log Selling Price of Office Real Estate



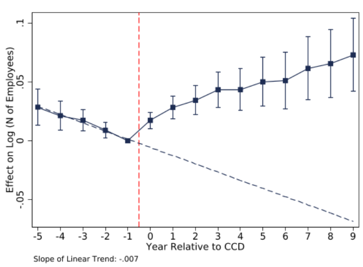
(b) Rotated Event-Study



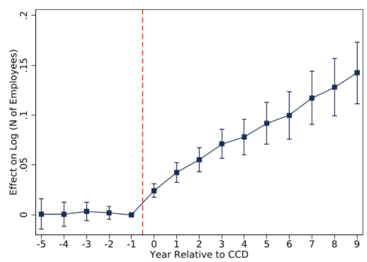
(c) Sensitivity



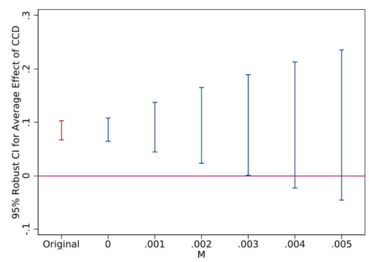
(d) Log Employment in Spillover Analysis



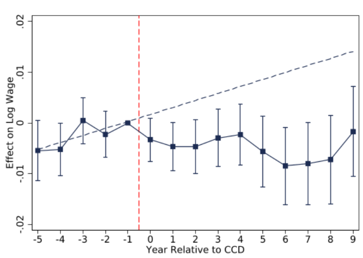
(e) Rotated Event-Study



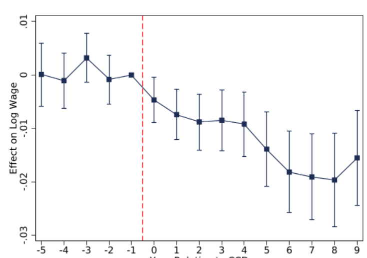
(f) Sensitivity



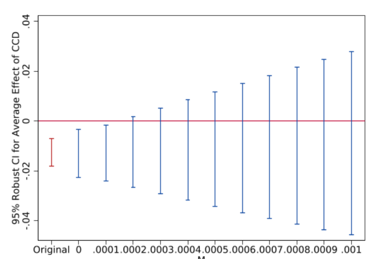
(g) Log Wages in Spillover Analysis



(h) Rotated Event-Study



(i) Sensitivity

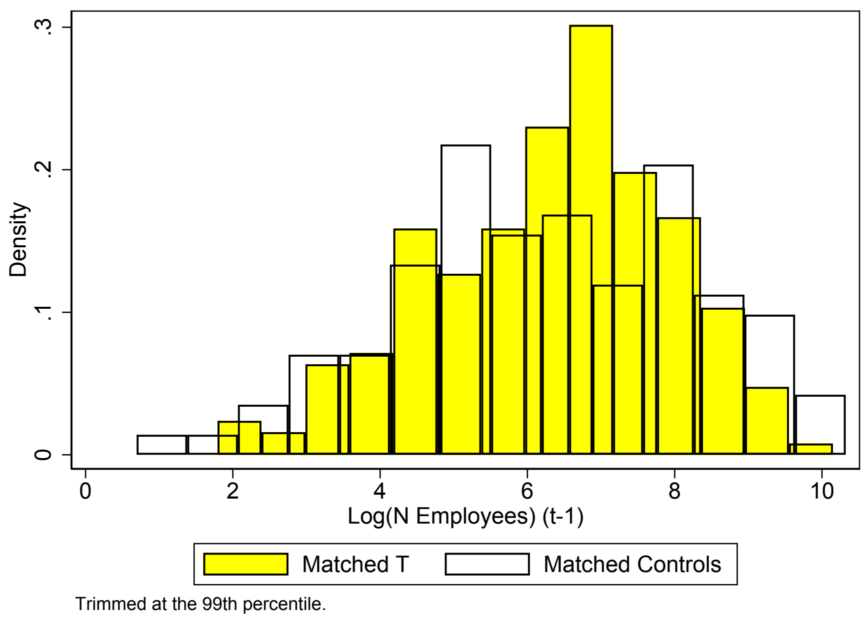


Notes: This figure analyzes potential violations of the parallel trend assumption in Figures 4b, C.1a, and C.1d. In the left panel, we overlay to the event-study coefficients a linear trend estimated using pre-CCD data and extrapolate it to the post-CCD era. The middle panel then reports the deviations 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.

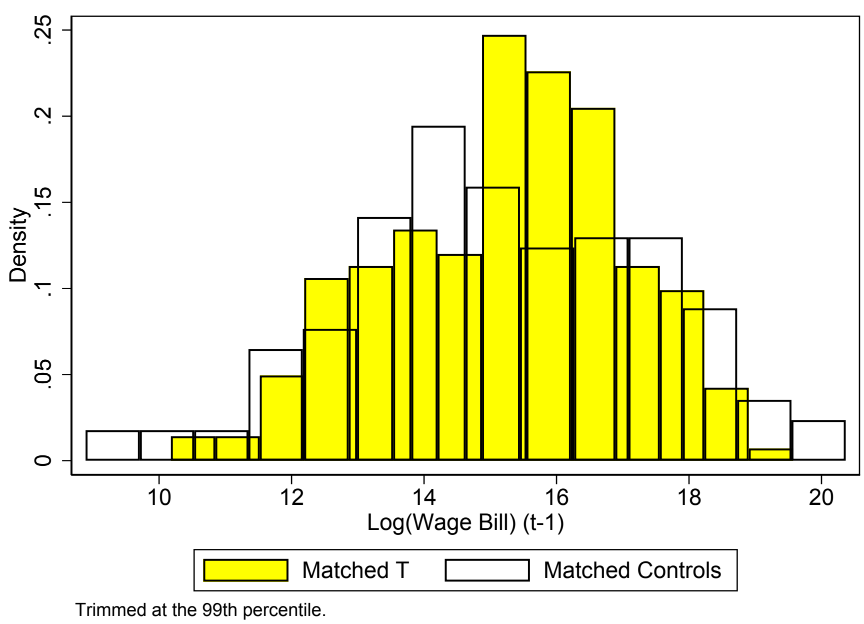
Appendix G Additional Figures and Tables

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

(a)



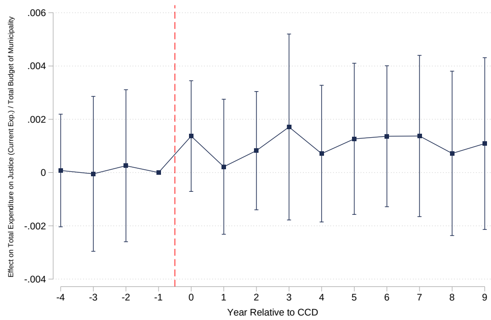
(b)



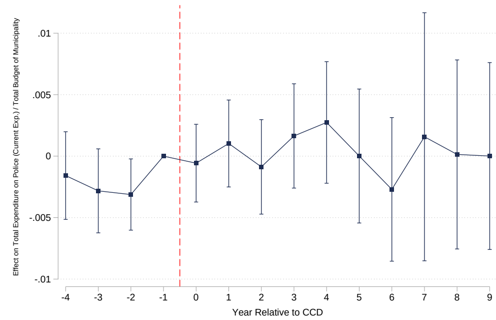
Notes: Matched firm sample, INPS data (1983–2017). Panels a and b display the distribution of log average earnings and log size for treated and matched control firms in the year before the CCD.

Figure G.2: Effects of CCDs on Expenditures

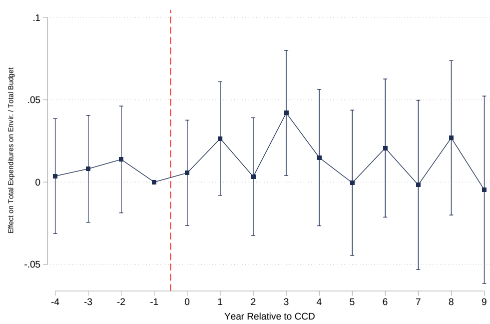
(a) Justice System



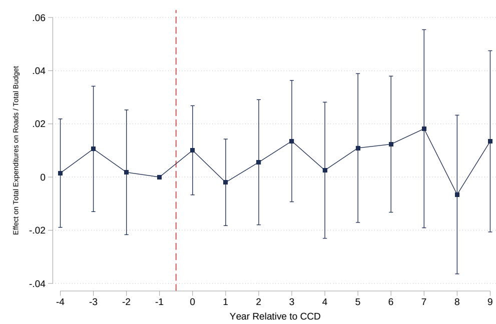
(b) Police



(c) Sanitation/Garbage Collection

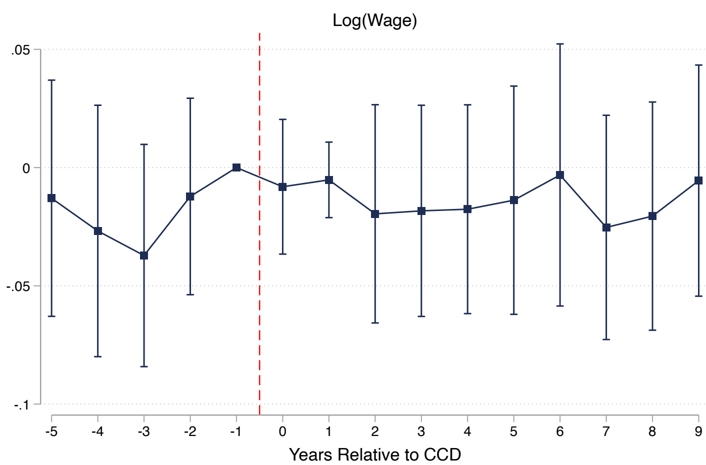


(d) Roads and Infrastructure



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.

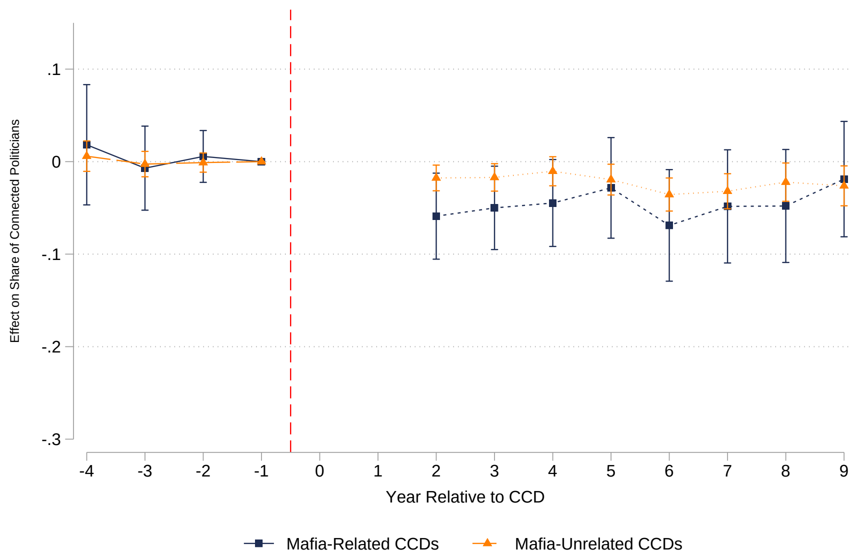
Figure G.3: Effects of CCDs on Incumbent Workers



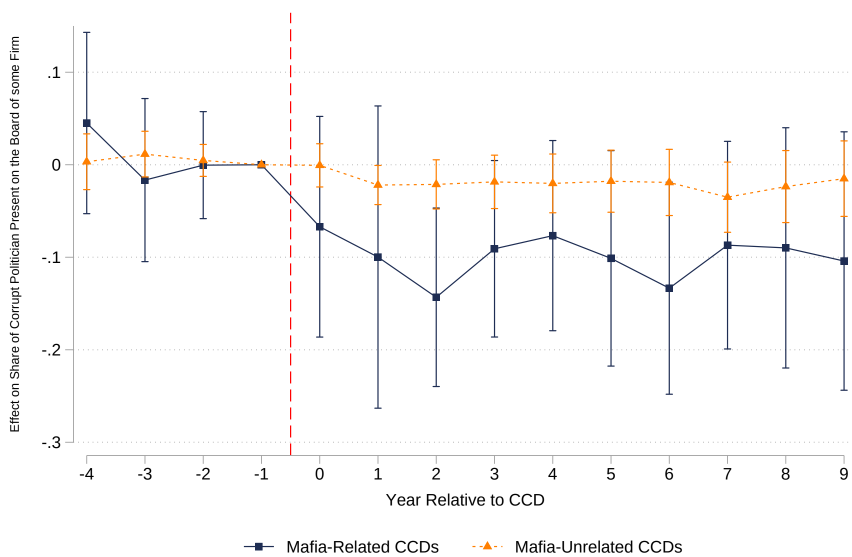
Notes: Matched municipality sample, INPS data (1983–2017). This figure reports the regression coefficients and the associated 95% confidence intervals for the difference between treated and control municipalities relative to the CCD year, i.e., the $\hat{\theta}^k$ from equation (1). The coefficients 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	354
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	1511
Share of Male Politicians	0.91	0.93	0.89	0.04	0.00	1974
Education	13.12	13.31	12.95	0.36	0.00	1972
Age	43.87	43.43	44.27	-0.84	0.00	1974

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) Log(Empl)	(2) Log(N Firms)	(3) Log(Wage Bill)	(4) 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 θ_k coefficients from (1). We define "on impact" as $k = 0$, "short run" as $k = 3$, and "long run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Table 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 θ_k coefficients from (1). We define "on impact" as $k = 0$, "short run" as $k = 3$, and "long run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD.

Table 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 θ_k coefficients from (1). We define "on impact" as $k = 0$, "short run" as $k = 3$, and "long run" as $k = 9$. "Mean" is the mean of the dependent variable. Standard errors are reported in parentheses and are clustered at the municipality level. Regression results are weighted by the logarithm of the number of firms in the year before the CCD. Each outcome is normalized relative to the 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.