



Research paper



Neutralizing the tentacles of organized crime. Assessment of the impact of an anti-crime measure on mafia violence in Italy[☆]

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ABSTRACT

Organized crime tightens its corrupting influence on politics through violent intimidation. Anti-crime measures that increase the cost of corruption but not of the exercise of violence might, on the one hand, lead mafia-style organizations to retaliate by resorting to violence in lieu of bribery. On the other hand, this kind of anti-crime measure might also induce criminal clans to go inactive, owing to the lower expected payoff from the “business” of influencing politics, which would reduce violence. To determine which of these possible effects is prevalent, we undertake an empirical assessment of the impact of city council dissolution for mafia influence in Italy as prescribed by Decree Law 164/1991 in discouraging violence against politicians in the period 2010–2019. The difference-in-differences analysis shows that in the dissolved municipalities the enforcement of the Law reduces violence and that the effect persists (at least) for seven years after the compulsory administration. The most likely driving channel of this result is the renewed pool of politicians elected after compulsory administration.

1. Introduction

Organized crime influences politics by bribery and threats to “induce a given policy maker to change his action from that preferred by society to that preferred by the group” (Dal Bó and Di Tella, 2003, p. 1128). For instance, in El Salvador, Mexico and Colombia politicians are often physically attacked by criminal gangs (e.g., Melnikov et al., 2020; Sviatschi, 2019; Blattman et al., 2020; Daniele et al., 2020). Studies on developed countries have shown that criminal organizations use violence strategically around elections, when local governments are called on to make important decisions, such as appointments and political programs, and therefore criminal influence is likely to have the highest return (e.g., Daniele and Dipoppa, 2017; Dell, 2015; Olivieri and Sberna, 2014; Alesina et al., 2019; Acemoglu et al., 2020). The violence perpetrated during the electoral mandate to induce politicians to divert public resources to the advantage of local clans, resulting in significant losses in social welfare and economic development (Pinotti, 2015).

This pervasive phenomenon demonstrates the need for empirical assessment of the effectiveness of government anti-mafia policies, also from the standpoint of public safety. In this paper, we analyze the effectiveness of the Italian Law 164/1991 —

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empowering the national government to dismiss the city council and to institute a compulsory administration upon evidence of organized crime infiltration — in reducing the number of intimidatory acts by criminal clans against local politicians. Law 164 is an non-repressive measure in that it is directed to cutting off the economic resources of organized crime, not engaging in a battle against local clans through an expansion of police forces nor increased allocations for public safety (Mete, 2009; Cavaliere, 2004).

Dal Bó et al. (2006) has developed an influential theoretical framework for studying how non-repressive measures can affect the way pressure groups (in our case, criminal organizations) maximize the expected payoffs to political influence thanks to an optimal mix of bribery and threats of violent punishment. These actions entail costs, whose magnitude depends on institutional factors, including the quality of law enforcement. Non-repressive measures work as a sort of intensified law enforcement that increases the costs of corruption and reduces the amount of public resources that corrupt politicians can redirect to pressure groups. These measures lower the expected payoffs and so may well induce pressure groups to modify the mix of bribery and violence.

However, non-repressive measures have an ambivalent effect on mafias' violence. On the one hand, they may lead clans to discard bribery in favor of intimidation, raising the level of violence (a sort of retaliation for the anti-bribery measure). Indeed, the study by Pulejo and Querubín (2023), though not focused on anti-crime measures, shows that an increase in the cost of bribes due to higher remuneration of politicians in more populated municipalities induces a rise of violence by mafias. On the other hand, an increase in the costs of corruption may stiffen the entry barrier to political influence, so that some criminal groups might refrain from their illicit activities, lowering the level of violence. Hence, Dal Bó et al. (2006) do not offer a definite prediction on how non-repressive measures affect mafias' violence.¹ Which of the two effects prevails is an empirical question; to answer it is the aim of this paper.

In the Italian institutional framework, the application of Law 164/1991 can be interpreted as an unexpected increase in the cost of corruption to local clans, because the dismissal of the city council should presumably cut funding for public projects, especially in the sectors targeted by organized crime, such as manufacturing, waste management and construction (Acconcia et al., 2014). Moreover, the enforcement of Law 164 has been shown to increase the quality of public resource allocation (Di Cataldo and Mastrococco, 2022), and also to reduce irregularities in the public procurement award procedure (Ravenda et al., 2020). Hence, in the logic of Dal Bó et al. (2006)'s model, the enforcement of Law 164 increases the cost of corruption, by reducing the amount of public resources that corrupt or complicit politicians had previously diverted to criminal clans.

To gauge how increasing the cost of corruption affects the level of violence, here we estimate the causal effect (unexplored to date) of the enforcement of Law 164 on acts of violent intimidation against politicians by organized crime. We adopt a difference-in-differences (DiD) approach, comparing the change in the number of attacks against local politicians in dissolved and never dissolved municipalities after the city council dissolution. We estimate event-study models for count data using the Poisson panel regression with fixed effects (e.g., Azoulay et al., 2019; Wooldridge, 2023). Our identification strategy is based on three assumptions: (1) no-anticipatory effects (i.e., the dissolution of the city council is not anticipated by the organized crime); (2) parallel trends (i.e. control and treated municipalities would have the same growth in the means of the attacks in the absence of the measure against crime Wooldridge (2023)); (3) Stable Unit Treatment Value Assumption (SUTVA) (i.e., the status of treated municipalities does not affect the outcomes for the untreated).

Our empirical analysis relies on several data sources: municipal data on the attacks against local politicians from 2010 to 2019 in the regions of Calabria, Campania, Apulia and Sicily (where practically all the city council dissolutions occurred) provided by the NGO "Avviso Pubblico"; municipal data on the time and date of city council dissolution for mafia infiltration as well as for all other reasons; the municipal data provided by the Italian Ministry of Interior from which we derived the composition of city councils by gender, age, level of education and kind of position.

In order to check the validity of the Stable Unit Treatment Value Assumption (SUTVA) in the DiD design (Rubin, 1980) following a well published literature, we detected spillover effects in municipalities that share borders with those dissolved but not in the case of cities distant less than 20 km, 30 km, 40 km; accordingly, in the main analysis we excluded from the control group any neighboring municipalities.

The Poisson model estimates show that after the period of compulsory administration there was a considerably sharper reduction in attacks against politicians in the treatment group of dissolved municipalities than in the control group. The dynamic of the attacks shows pre-treatment coefficients that are not significant (corroborating the common trend assumption) and a sine curve of the post-treatment coefficients. The enforcement measure affects the number of attacks through the first two elections after compulsory administration, or over a period of 6–7 years or more. The drop in the number of attacks after compulsory administration is sharp: 82% at the first year after election and about 73% and 57% at the second and third, respectively. Our main result refers to the persistence of the effect from the fifth to the seventh year following compulsory administration, with an average decrease in the number of attacks of 67%. Over the entire period after compulsory administration, we record an overall reduction in the number of attacks of 55%. These results are confirmed under treatment effect heterogeneity (Borusyak et al., 2021); they are also confirmed by tests supporting the identification assumptions, and by a series of robustness checks.

As regards the ambivalent effect of an increase in the cost of corruption on organized criminal violence highlighted in Dal Bó et al. (2006), our findings indicate that the enforcement of Law 164 has in fact proved to be effective in stiffening the barriers to political influence by lowering the mafia's expected profits from corrupting local officeholders, thus reducing the violent activity of criminal organizations. These results complement the studies documenting the effectiveness of Law 164/1991 in discouraging organized crime bribery activities (e.g., Ravenda et al., 2020; Di Cataldo and Mastrococco, 2022).

¹ The "Conceptual framework" in Appendix discusses the relevant hypotheses.

To support this interpretation, we refer to robust evidence showing that the reduction in the profitability of corruption is a consequence of the renewal of the officeholders, which following compulsory administration tend to consist of more highly qualified individuals with a larger proportion of well-educated women (Daniele and Dipoppa, 2017; Baraldi and Ronza, 2022), typically less inclined to corruption and criminal activities (Lochner, 2004), as well as reflecting a new awareness on the part of citizens that the mafia is not invincible (Baraldi et al., 2022b). The renewed pool of officeholders tend to implement policies hostile to local clans. Moreover, we show that the alternative mechanism, whereby clans refrain from violent intimidation against local politicians due to a greater attention of police forces after the city council dismissal, does not find statistical support in our data.

This paper extends three main branches of literature. First, it contributes to the analysis of organized crime's use of violence to influence politics. Seminal contributions documented that mafias force citizens to vote for the candidates they back (e.g., Gambetta, 1996; Acemoglu et al., 2013). Other studies showed that clans use violence in the post electoral period to influence crucial decisions of the local government (e.g., Dal Bó and Di Tella, 2003; Dal Bó et al., 2006; Pinotti, 2013; Alesina et al., 2019; Daniele and Dipoppa, 2017). Our analysis provides evidence compatible with the hypothesis that increasing the corruption costs through city council dismissal reduces mafias' incentives to the strategic use of violence.

Second, several studies documented that the enforcement of Law 164 improves the municipal environment, reducing the level of corruption (Di Cataldo and Mastroiocco, 2022), favoring the election of better qualified politicians (Daniele and Geys, 2015; Baraldi et al., 2022b) and of women (Baraldi and Ronza, 2022), as well as boosting local economic performances (Fenizia and Saggio, 2023). None of them, however, analyzed the effectiveness of Law 164 in preserving public safety against mafias. This is important as clans' violence in Italy has been shown to reduce the quality of local politicians (Daniele, 2019) and to refrain women from politics (Daniele et al., 2023; Baraldi et al., 2023). This paper fills this gap.

Finally, since important studies showed that repressive policies increasing police forces trigger clans' violent reprisals against politicians (Calderón et al., 2015; Castillo and Kronick, 2020), our analysis allows assessing the effectiveness of a non-repressive government measure in fighting clans' violence against local administrators (Daniele and Dipoppa, 2022).

The rest of the paper is organized as follows. Section 2 describes the institutional framework and presents the data and the variables. Section 3 describes the empirical design and discusses selection bias as a possible threat to our identification strategy. Section 4 reports the results of our baseline analysis and their interpretation. Section 5 deals with possible alternative mechanism. Section 6 reports robustness checks. Section 7 concludes.

2. Institutional background and data

2.1. The anti-mafia measure

Law 164/1991 prescribes the dissolution of a city council upon evidence of direct or indirect links with organized crime that compromise the local administration's neutrality and autonomy, and more generally public safety. Introduced in response to the growing influence of organized crime on local administrations in the 1980s, the Law was designed to prevent or reverse criminal control of public procurement, public works, urban plants, and housing.

Dissolution entails the replacement of the mayor, the city council, and the executive board with commissioners appointed by the central government and drawn from outside the local area. They run the municipality for 12 to 24 months before new elections are held to reinstate a legal city council and restore public safety. In detail, the activity of the compulsory administration consists in an unexpected reduction in public investment projects, chiefly in the sectors targeted by organized crime (manufacturing, waste management, construction).

The Law lays down a rigid procedure from the initial discovery of evidence of mafia infiltration to the dissolution decree. Typically, there is some advance warning in the form of independent judicial or police inquiries. This evidence is reported to the Prefect, the provincial representative of the Ministry of the Interior, who, by virtue of powers of inquiry, can name a commission (*Commissione d'Accesso*) to investigate the extent of permeability of the local government to organized crime.

The investigation is kept secret until completed, or for a maximum of three months. Thus, the conduct of local administrators and criminal clans cannot be influenced by the procedure while it is under way. Once the investigation is concluded, the report of the *Commissione d'Accesso* is the basis of the Prefect's proposal for city council dismissal, which is submitted to the Ministry of the Interior for final decision. Therefore, the outcome of the procedure is independent of the partisan political dynamics of the municipality (Mete, 2009).

The dissolution provision is preventive, in that the replacement of local politicians by outside commissioners is intended to prevent future crimes in the public administration rather than prosecute past ones. Thus the evidence of links between civil servants and organized crime does not necessarily have to show a crime in order to trigger the procedure. In fact, the reports of the Prefect with a view to dissolution decrees very commonly involve evidence not of crimes but of personal relationships of kinship or friendship between local politicians and members of criminal organizations. The main triggers of the inspection procedure by the *Commissione d'Accesso* turn out to be electoral agreements, electoral participation of individuals linked to local clans, the exchange of packets of votes, and shared economic interests between politicians and criminal organizations.

For the purposes of our study, the intimidation of local politicians by organized crime (e.g., killings, physical attacks, threatening letters) may be a factor in activating the *Commissione d'Accesso*'s inquiry. This is plainly a threat to our identification strategy, as municipalities dissolved owing to evidence of violence are self-selected into the treatment group, creating a problem of reverse causation, since we may be unable to determine the causal link between the council dissolution and the variation in the number of attacks against local politicians.

However, violent intimidation cannot be taken as unequivocal evidence of collusion with local clans like the cases of electoral agreement, the delivery of packets of votes, and shared economic interests. Indeed, as the administrative courts have rightly pointed out, the simple fact of local roots of organized crime and criminal violence does not prove a connection with the local government such as to trigger an inspection procedure. Intuitively, in fact, one might well consider violence as an alternative to corruption as a way of influencing political action. Thus, acts of intimidation can be considered as a sign of city council permeability to mafia infiltration only insofar as they are combined with evidence of collusive behavior. For example, the municipalities of Bagnara Calabria and Delianuova were dissolved because local clans repeatedly attacked the majority of the city council with the complicity of its minority.² As a consequence, relatively few of the municipalities where attacks against local administrators occur undergo an inspection procedure. We will tackle this important question in sub-Section 2.3 and perform all regressions excluding from the sample those municipalities where the decree of dissolution cites cases of violence against local politicians.

2.2. Data and variables

The regressor of interest deal with the application of Law 164/1991 to dissolve a city council upon evidence of the infiltration of local politics by organized crime. Table D.1 in Appendix shows the distribution of city council dismissals by region: from 2010 to 2019 they totaled 120. The regions of Campania, Calabria, Sicily and Apulia count 115 dismissals (in 108 municipalities, of which 7 were dissolved more than once), or 95.8% of the total.

In line with the relevant literature analyzing the effectiveness of Law 164, we focus our analysis on these four regions where city council dissolution are concentrated in order to achieve the highest homogeneity between treatment — the dissolved municipalities — and control groups — the undissolved municipalities — required by the empirical design (e.g., Daniele and Geys, 2015; Galletta, 2017; Di Cataldo and Mastrococco, 2022; Baraldi et al., 2022a,b; Baraldi and Ronza, 2022).³ Moreover, we remove from the sample the municipalities dissolved because of mafia infiltration before 2010. Finally, to further increase the comparability between treated and never treated units, in sub-Appendix C.1 we replicate our analysis by implementing a propensity score matching.

In our sample, compulsory administration lasts on average 1.37 years. In order to correctly interpret the event-time dummies in the empirical analysis, we show a histogram reporting the frequency of elections in the years after the dissolution, as in Fig. E.1 in Appendix. The greatest part of municipalities dissolved between 2010–2019 shows only one election after the city council dismissal. Thus, given the average length of compulsory administration of 1.37 years, the first electoral round occurs between the first and the second year from the starting of the government held by external commissioners.

Fig. E.2 in Appendix shows the number of dissolutions for mafia by year (graph E.2(a)) and by province (graph E.2(b)). Except in 2012 and 2017, dissolutions are fairly evenly distributed; the same goes for provincial distribution in the four regions of interest: only the province of Reggio Calabria counts more than 30 dissolutions, while 4 provinces had none.

We construct a dataset of threats and violence against local politicians in Italy from 2010 to 2019. Official sources of data about attacks against politicians are not available; like Daniele and Dipoppa (2017), we use the yearly reports of “Avviso Pubblico”, a non-governmental organization that collects daily media reports of threats and attacks directed against Italian politicians. The dependent variable in this empirical analysis is the number of attacks against local politicians as reported by “Avviso Pubblico”. This organization distinguishes 20 types of attack and 11 types of politicians targeted for attacks as detailed in Figs. E.3 and E.4 for the regions of Campania, Calabria, Apulia and Sicily.

From 2010 to 2019, the most common types of attack were arson targeted at cars (505) or the City Hall or other municipal properties (466), followed by verbal or telephone threats (436). A non-negligible number of cases also involved threatening letters (306 + 159). Physical assaults and arson against politicians' homes too were reported in a substantial number of cases (respectively 292 and 152). Other types of attack are not so frequent; the worst, homicide, registered 28 victims. In the baseline specification of our empirical model, the dependent variable is the total number of attacks against politicians; we provide further evidence of the findings in the sub-samples of threats and violent intimidation.

Fig. E.4 shows 894 attacks against mayors (almost 30% of the total). City councilors, aldermen and deputy mayors display lower frequency. The last category in the figure, namely policemen and managers in city government or other public facilities, counts about 21% of the attacks. Violent intimidation against regional presidents and councilors are far less frequent. As is observed by Daniele and Dipoppa (2017), national politicians are not targeted by organized criminal violence, presumably thanks to their stronger protection owing to their greater public exposure.

Fig. E.5 in Appendix shows the trend in the mean number of attacks over the years, with a downward spike in 2012. As the National Coordinator of “Avviso Pubblico” explains, this actually reflects the organization's lack of human resources that year, so that the data were collected ex-post via internet search only. Given this problem, we replicate our analysis by excluding the year 2012.

Our data show that during compulsory administration we can observe only 12 attacks against politicians. Even though the coding of the data on attacks comprises also the attacks against commissioners (they are included in the category *Other institutions or institutional entities* in Fig. E.4), none of the 12 attacks during compulsory administration are against commissioners.

² Similar cases can be found in a number of other reports on dissolved municipalities. On this issue see Marotta (2019).

³ Focusing our analysis on this specific sample allows us also to control for economic shocks due, for example, to the Great Recession in 2009–2010, which is unlikely to have affected differently the four regions under analysis. However, in Section 4, we control for this aspect by including in the regression equation the interaction of Region and year fixed effects.

As Daniele and Dipoppa (2017) observes, the number of attacks on local politicians surveyed by “Avviso Pubblico” could be affected by a measurement error, in that the violence reported might be the work of private citizens and not necessarily organized crime. However, the parliamentary report by Moro et al. (2015), which documents the connection between attacks on politicians and organized crime using both open-source and restricted-access data, estimates that attacks driven by personal motives constitute a tiny fraction of the total.

Moreover, since organized crime deploys violence either to affect election results or for bargaining with the politicians elected, the attacks cannot be random but should be concentrated at election times. And research provides robust evidence that violent intimidation by organized crime is in fact concentrated around elections (Daniele and Dipoppa, 2017; Alesina et al., 2019). The strategic use of violence around elections corroborates the hypothesis that attacks are actually perpetrated by organized crime rather than private citizens.⁴

2.3. Main reasons for dissolution

In this section we will clarify that, although a city council can be dismissed only because of evidence of a collusive agreement between local clans and politicians, violent intimidation might be a reason to activate a procedure of investigation. We will, then, illustrate the main reasons motivating the Prefect's choice of city council dismissal to identify those municipalities whose motivation of dissolution was accompanied, among other reasons, *also* by episodes of violence against politicians. Therefore, we will *drop* these municipalities altogether from our sample.

In principle, any policy intervention whatever should be the response to some problem of public life. So while as we have seen the reasons for dissolution are much more wide-ranging, the procedure for city council dissolution can sometimes be associated with violent acts against local politicians. However, Section 2.1 makes it clear that violent intimidation *per se* is not conclusive evidence of infiltration by organized crime; it can only activate an investigation by the *Commissione d'Accesso* insofar as acts of intimidation are combined with evidence of collusion. Therefore, on the normative level, the decision to dissolve a city council depends exclusively on evidence of collusive behavior.

The Prefects' reports on the investigations of the *Commissione Parlamentare d'Inchiesta* (Parliamentary Commission of Inquiry) on the municipalities dissolved for mafia infiltration in our sample (in 2010–2019) make it possible to identify five main motivations for dissolution: (i) *Politicians' crimes*: investigations on crimes committed by administrators or politicians, not necessarily linked to their official functions⁵; (ii) *Extortion/irregular markets*: investigations on practices of extortion, illegal trafficking in arms and drugs, as well as wars among rival clans for the control of local territory; (iii) *Politicians' resignation*: inquiries prompted by the early dissolution of the city council due to the resignation of the mayor or of city council members, possibly indicating pressure from organized crime; (iv) *Shared economic interests*: investigations into irregularities in public procurement or the systematic distortion of public expenditure in sectors targeted by organized crime, indicating shared economic interests between local politicians and criminal clans; (v) *Violent intimidation*: investigations into attacks and threats against politicians.⁶

The histogram in Fig. E.6 gives an intuitive picture of the frequency of these five types of reasons for city council dissolution. The most common — found in 84% of the cases — is evidence of shared economic interests, followed closely by crimes committed by local politicians (70%). Resignation and extortion or irregular markets are relatively less frequent. Consistently with its ancillary nature, violent intimidation is the least common type of grounds for dissolution, occurring in 20% of the cases.⁷

Further corroboration that violence against politicians alone does not suffice to justify a dissolution decree can be found Table D.4 in Appendix, which bears on the 22 municipalities whose Prefect reports featured evidence of violent intimidation against politicians among other reasons (see the flags in Column 1). The Table clearly shows that this violence must be accompanied by other evidence that demonstrates some kind of collusive agreement between local clans and politicians. For example, for the dissolution of Cellino San Marco (Apulia) in 2011, the Prefect's report refers to “Repeated episodes of intimidation perpetrated against the directors of the entity”, together with evidence that “the mayor's behavior was reprehensible and indicative of proximity to the local criminal milieu, such as his attendance at the funeral of a convicted criminal known to be part of the local clan”, which indicates the existence of “long-standing relationships between the administration and the local clan that made the institution permeable to organized crime”. This confirms that violence against local politicians is strictly ancillary as a type of motivation for city council dissolution.

However, intimidatory violence might be a factor that increases the probability of dissolution. For instance, since the enforcement of Law 164/1991 can be grounded on evidence provided by the previous activity against mafias violence of the ordinary justice, those municipalities where mafias' intimidating power has been already weakened, are more likely to be treated. Hence, we estimate the baseline models excluding the 22 municipalities listed in Table D.4.

The remaining sample comprises those municipalities whose reasons of city council dissolution (e.g., extortion/irregular markets, politicians' crime, shared economic interests and politicians resignation) are arguably less related to organized crime violence. For example, politicians' resignation can well be a response to mafias' threats and intimidation. However, if the municipalities dissolved because of violence are removed from the sample, the remaining politicians resignations motivating the city council dismissal are more likely to indicate a collusion with local clans rather than a sort of defense against mafias reprisal. The same holds for the other motivations for the enforcement of Law 164/1991.

⁴ “Data provided by the Prefectures show that less than 8% of the acts of intimidation to which a motivation could be attributed refers to personal motives, private disputes that fall outside of the political and administrative engagement of the victim, and 3% are vandalism” (Moro et al., 2015).

⁵ This category comprises also investigations triggered by whistleblowers, informing on politicians' crimes not necessarily linked to organized criminal infiltration.

⁶ This definition is in line with the relevant juridical literature on this issue (e.g., Marotta, 2019).

⁷ The sum of the percentages in Fig. E.6 is more than 100% because the Prefect's reports often give more than one reason for the inquiry.

3. Empirical strategy

We create a yearly panel of all the municipalities in Calabria, Campania, Apulia and Sicily from 2010 to 2019. This panel structure allows to isolate the effect of the measure from any time-specific features. Further, the municipal-level fixed effects (FE) account for possible local factors. We take a difference-in-differences approach with staggered treatment and time and municipality FE. We record the number of attacks before and after city council dissolution for mafia infiltration, which is our treatment, in both dissolved and undissolved municipalities (i.e. in the treated and control groups). The baseline specification is the following event-study model (subscript i for municipalities, t years):

$$Y_{it} = \sum_{n=-m, n \neq -1}^{+m} \nu_n \cdot D_{i,t-n} + \alpha_i + \delta_t + \lambda' X_{it} + \epsilon_{it} \quad (1)$$

Y_{it} is the outcome variable in municipality i at year t . D_t is the set of event-time dummies, which take the value of 1 only for dissolved municipalities if year t is k periods before/after the dissolution (t_0). The omitted category, D_{-1} , is the year before the dissolution; the remaining ν_n coefficients measure the effects in the period before and after the application of Law 164 (t_0). In all the regressions we control for (1) municipality fixed effects (α_i); (2) time fixed effects (δ_t).

The dependent variable is the occurrence of an attack, and in our data-set the count of events has a large portion of 0 and a skewed distribution as shown in Table D.2 in Appendix. We specify the outcome variable as the total number of attacks in municipality i in year t and we estimate Eq. (1) using a Poisson regression model with fixed effects and robust standard errors clustered at the municipality level, which is more suitable for count data.⁸

The conditional quasi-maximum likelihood estimation of Poisson models (Wooldridge, 2010; Cameron and Trivedi, 2013) is consistent even when the assumption of a Poisson distribution of events is not correct (i.e. over- or under-dispersion). Municipality FE control for heterogeneity in the cross-section dimension and account for unobserved time-invariant factors that could engender omitted-variable bias. Time FE account for unobserved year-specific events that affect all municipalities. In Poisson regression, the coefficients can be interpreted as an elasticity, according to the following expression: $\text{Exp}(\text{coef}) - 1$ (Cameron and Trivedi, 2013).

The set of control variables (X_{it}) comprises firstly the municipal population size (hereafter *Pop*). The dismissal is de-facto aligning the electoral cycle of all treated municipalities, while the elections in control municipalities are likely distributed uniformly over the event time range. This implies that treatment is correlated with the electoral cycle, and this correlation has to be taken into account. Therefore, we control for the possible importance of the electoral cycle via a set of dummy variables from the election years up to the three years after, for all municipalities in the sample.⁹ ϵ_{it} is the idiosyncratic error term.

The validity of the empirical design requires the satisfaction of the parallel trends assumption, that cannot be tested. However, the presence of common trends can be verified by the estimation of the coefficients of the pre-treatment dummies of the event study model in Eq. (1) (Mora and Reggio, 2019). The event study estimates also permit assessment of the dynamic effect of the measure, i.e. the trajectories of the attacks in treated and untreated municipalities in each year after the dissolution.

We also estimate the mean impact of the measure using a panel Poisson FE regression model where the regressor of interest is Policy_{it} , which is equal to 1 for treated municipalities in all the years between compulsory administration and 2019 and to 0 in the period from 2010 to the year of the dissolution; for municipalities in the control group, it is 0 for the entire period 2010–2019.¹⁰

3.1. SUTVA

The “Stable Unit Treatment Value Assumption” (SUTVA) (Rubin, 1980), requires that the treatment status of one municipality does not affect outcomes for untreated municipalities. Since mafias often operate in areas larger than a single town, the shock to mafia infiltration due to the enforcement of Law 164 in one municipality may generate spillover effects on neighboring municipalities. For example, Galletta (2017) shows the presence of spillover effects in neighboring towns due to the dissolution for mafia infiltration in Calabria, Campania and Sicily in 1998–2013 in terms of a reduction in public investment. In the same spirit, Cingano and Tonello (2020) show spillover effects on the local petty crimes in municipalities in the neighborhood of the dissolved ones. Hence, we can likely consider that the enforcement of Law 164 may generate spillover effects in neighboring towns.

In Appendix B we provide evidence of the presence of spillovers effects in municipalities neighboring the dissolved ones; this means that the SUTVA assumption is probably violated. This can be a source of bias for our baseline estimates, because these neighboring municipalities are part of the control group. In order to correct for this potential bias in the estimates, following Kline and Moretti (2014a) among others, we exclude these neighboring municipalities from the control group.

⁸ On staggered panel Poisson DiD event studies see Azoulay et al., 2019 and Wooldridge, 2023.

⁹ The four dummies are the following: *Electoral cycle_0* taking the value of 1 in election year and 0 in other years; *Electoral cycle_1* taking the value of 1 in the first year after the election year and 0 in other years; *Electoral cycle_2* taking the value of 1 in the second year after the election year and 0 in other years; *Electoral cycle_3* taking the value of 1 in the third year after the election year and 0 in other years. The characteristics of the politicians in office, such as their education (as a proxy of quality), could be important to control for because these help determine whether and how the mafia affects politicians (see, among others, Daniele and Geys, 2015). Indeed, highly qualified politicians are more likely to manage a higher amount of public resources that organized crime can be interested to reap by attacking local administrators (Dal Bó et al., 2006). However, since the education of politicians is an intermediate outcome, introducing it in the regression equation would mean to introduce “bad controls”.

¹⁰ Since municipalities are defined as receiving treatment in the year they are dissolved, we need to exclude the 5 municipalities whose administrations were dissolved in 2010 in order to have at least one observation (a 0) before the dissolution.

4. Results

4.1. Baseline results

We first estimate the event study as in Eq. (1) using a Poisson model and calculate the trajectory of the effects of the enforcement of Law 164 on the attacks against politicians year-by-year before and after city council dissolution. The dependent variable is the total number of attacks against all the categories of politicians reported by “Avviso Pubblico”, in each municipality and in each year from 2010 to 2019. In this specification, the treatment group drops the 22 municipalities where, in the Prefect’s dissolution decree, violence against politicians was accompanied by evidence of collusion with mafia clans; and the control group excludes municipalities neighboring those dissolved. Moreover, we remove the municipalities dissolved because of mafia infiltration before 2010.

The results are in Fig. 1 and Table 1, which displays the results of alternative specifications of Poisson FE regressions (to save space, we remove the coefficients of pre-treatment interaction). As mentioned in Section 3, in the event study the excluded category is the year before the city council dissolution. Depending on the month of the dissolution, the year of the city council dismissal might be partially treated. For example, if the council is dismissed in January the whole event year t_0 would be treated; instead, if the council is dismissed in December the whole event year t_0 would be untreated. To account for this monthly-staggered treatment we multiply the event dummy t_0 by the fraction of the year that is treated.¹¹ Notice that, although the number of municipalities in the full sample is 1015 (with a total number of observations of more than 10,000), in Table 1 the Poisson fixed effects model determines a substantial reduction in the number of municipalities and observations, because the log-likelihood maximization excludes the units that always have zero values of the dependent variable for the entire period (Cameron and Trivedi, 2013). In this restricted sub-sample the number of city council dismissal is 57 instead of 85 as in the full sample. Nevertheless, while in the full sample of municipalities the dissolved municipalities are the 8% of the total, in the Poisson sample they are the 21%. In Table 1, robust standard errors are clustered at the municipal level.¹²

In this restricted sub-sample the number of city council dismissal is 57 instead of 85 as in the full sample. Nevertheless, while in the full sample of municipalities the dissolved municipalities are the 8% of the total, in the Poisson sample they are the 21%.

The graphs 1(a) and 1(b) in Fig. 1 depict the results reported in Columns 1 and 2 of Table 1 respectively. The coefficients for the pre-treatment period are not statistically significant, suggesting the absence of any anticipatory effect and of divergent patterns between the two groups of municipalities before the treatment. The parallel trends assumption is also supported by the formal test that all the pre-treatment coefficients in the event study model are jointly equal to zero, as the last row of Table 1 shows.¹³

At t_0 , since only the fraction of the year after the dissolution is treated, the coefficient is noisily estimated delivering a null effect. The coefficient after the dissolution at $t_0 + 1$ estimates a 80.8% ($e^{-1.654} - 1$) reduction in the number of attacks (see Column 1 Table 1). Since in our sample at $t_0 + 1$ no elections are held, the estimated effect is due to a mechanical reduction in the violence level as most of local politicians are out of office during compulsory administration.

A clearer effect of the anti-mafia measure appears at $t_0 + 2$ and at $t_0 + 3$, respectively with a decrease of 72% and 54% in the number of attacks after compulsory administration. Since in these years new elected governments are in office, we can gauge the effect of the anti-crime measure on the number of attacks against local politicians net of the mechanical reduction due to local politicians out of office during compulsory administration.

The effect of the measure then weakens, and is not statistically significant, up to $t_0 + 4$. Our main result refers to the persistence of the effect of the measure. Indeed, between $t_0 + 5$ and $t_0 + 7$, the attacks still decrease more in the treatment than in the control group; the estimated effect shows a decrease in the number of attacks against local politicians of 61% at $t_0 + 5$, 68% at $t_0 + 6$ and 73% at $t_0 + 7$.

The estimates of the average impact of the policy can be found in Columns 3 and 4 of Table 1. The negative sign of the treatment coefficient (*Policy*) is robust across specifications, confirming that the dissolution of a local administration for mafia infiltration reduces attacks against local politicians. The Poisson coefficients suggest that the measure diminishes the occurrence of attacks by 54.2% (Column 3). The coefficient of *Policy* remains negative and highly significant when relevant controls are included in Column 4.

The coefficients of the electoral cycle dummies in Table 1 are never significant (except for a weak significance of *Electoral cycle_0*), indicating no electoral cycle effect on the level of violence. This result is as expected, given the findings of Daniele and Dipoppa (2017) that in the same sample of municipalities in Campania, Calabria and Sicily, the mafia violence against local politicians increases only in the first month after an election; this result precludes any annual electoral cycle effect. Graph 1(b) of Fig. 1 confirms the dynamic pattern of the measure against organized crime depicted in Graph 1(a).

Finally, in order to control for economic shocks in 2009–2010 which could have affected specifically the four regions under analysis, we include in Eq. (1) the interaction between Region and year FE; the related results, displayed in Fig. E.10 in Appendix, confirm the dynamic pattern of the anti-mafia measure as in the main analysis.

¹¹ Therefore, in the event study graphs, the event dummy t_0 is not equal to 1 but, instead, to the fraction of the year that is treated (e.g. Angrist and Pischke (2014)).

¹² To compute these clustered robust standard errors at municipal level in the Poisson FE model we use the Stata command *xtpqml*. We also perform estimations with standard errors clustered at provincial level. They confirm the previous and are available upon request.

¹³ The last row of Table 1 shows the *P*-value of the Wald test that suggests the null hypothesis that all the pre-treatment coefficients in the event-study model are jointly equal to zero cannot be rejected.

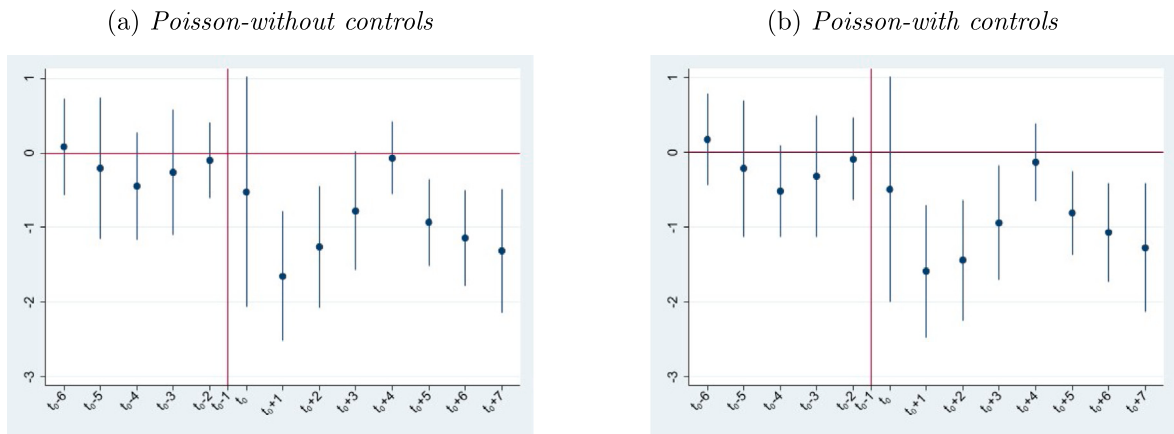


Fig. 1. Event study graphs. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1). The dependent variable is the total number attacks against local politicians. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. We include event-time dummy variables for 6 years before and 7 years after the dissolution. In all regressions we control for municipality FE and year FE. In Graph 1(b) we also control for the four dummies for electoral cycle and the log of resident population. Period: 2010–2019.

Table 1

Baseline estimates.

Dep. Var.: No. attacks	(1)	(2)	(3)	(4)
$t_0 + 1$	−1.654*** (0.527)	−1.588*** (0.539)		
$t_0 + 2$	−1.259** (0.497)	−1.442*** (0.493)		
$t_0 + 3$	−0.778 (0.483)	−0.941** (0.467)		
$t_0 + 4$	−0.070 (0.297)	−0.142 (0.315)		
$t_0 + 5$	−0.932*** (0.354)	−0.809** (0.343)		
$t_0 + 6$	−1.143*** (0.389)	−1.070*** (0.398)		
$t_0 + 7$	−1.311*** (0.503)	−1.275** (0.522)		
Policy			−0.783*** (0.249)	−0.798*** (0.266)
Ln(pop)		0.832 (2.592)		1.119 (2.794)
Electoral cycle_0		0.273* (0.148)		0.277** (0.139)
Electoral cycle_1		0.119 (0.174)		0.168 (0.162)
Electoral cycle_2		0.00132 (0.154)		0.107 (0.144)
Electoral cycle_3		−0.209 (0.172)		−0.194 (0.161)
Observations	3190	3174	3190	3174
No. Municipalities	319	319	319	319
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
P-value Wald test	0.69	0.43		

Note. The table reports coefficients estimated according to Eq. (1). The dependent variable is the total number attacks against local politicians. Standard errors are clustered at municipal level. *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 in previous years. In estimating Eq. (1) we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). All regressions include municipality FE and year FE (coefficients not reported). Standard errors are in brackets. The last row of the table reports the *P*-value of the Wald test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Interpretation. These effects point to the effectiveness of Law 164 in reducing the profitability of collusion between politicians and organized crime. On the one hand, the costs associated to the city council dissolution (e.g., being caught by police) should induce

politicians to step away from colluding with mafias, thereby being less a target of attacks. On the other hand, the city council dismissals reduce the amount of public resources to be redirected to local clans, thereby lowering the profitability to attack local politicians in order to have favorable policies.

In the event study, the dynamic of the effect of the policy on the number of attacks against politicians year-by-year after dissolution follows an inverted U-shaped path, suggesting an intriguing interpretation: after the state intervention, criminal clans try to restore their power on local politics by increasing the number of attacks, but the new legal context induces them to give up this goal. City council dissolution firstly weakens the connection between mafia and the local political class; moreover, by dismissing the city council, the central government sends a strong signal that mafia infiltration in the local government should not be tolerated, making citizens confident about the will of the State to fight organized crime.

Several studies have documented that after compulsory administration newly elected councils are made up of more highly educated politicians (e.g., Baraldi et al., 2022a; Daniele and Geys, 2015; Fenizia and Saggio, 2023) and with a larger number of women (Baraldi and Ronza, 2022), who are typically less inclined to corruption (e.g., Decarolis et al., 2023), as well as younger politicians at their first term in office (Daniele and Geys, 2015).

Such characteristics make this renewed pool of politicians less incline to be corrupted (e.g., Decarolis et al., 2020, 2023; Lochner and Moretti, 2004; Lochner, 2004). Indeed, the city council dissolution has been shown to entail a reallocation of public funds away from sectors targeted by organized crime (e.g. construction and waste management), an increase in tax collection, as well as a reduction of the irregularities in public procurement awarding procedure (e.g., Di Cataldo and Mastroiocco, 2022; Ravenda et al., 2020).

This renewal of officeholders is driven by a change in voters' behavior (Baraldi et al., 2022b), which documents an increased trust of local communities in the state's capacity to fight corruption (Kline and Moretti, 2014b).

Under this renewed climate, after the dismissal, mafia might try to infiltrate again by planning a new mix of bribes and punishment to define the new level of state capture (Dal Bó et al., 2006). Facing less corruptible politicians, mafia gradually increases the level of violence until a threshold, visible at $t_0 + 4$ where there is no longer difference in the attacks between treated and untreated municipalities. After that it seems to be no longer profitable for mafias to perpetrate violence. Indeed, from $t_0 + 5$ onward, as a consequence of this new climate of trust combined with the greater honesty of the new politicians, mafia gradually reduces their level of violence, documenting the persistent effectiveness of the city council dissolution prescribed by Law 164.

This result is consistent with the evidence that organized crime's *modus operandi* radically changed in the last 30 years, indicating that a violent confrontation with central government might be considered as a strategy with detrimental consequences for its economic businesses (Di Girolamo, 2012).

4.2. Threats and violence

Here we replicate our analysis by splitting the total number of attacks into threats and actual violence, to assess the effectiveness of Law 164 in reducing both categories of attacks. *Violence* comprises set the car on fire, set City Hall on fire, set the house on fire, physical assault, homicide and physical assault in public places; *Threats* all the remaining attacks.

Since the "Avviso Pubblico" data are drawn from media reports, this distinction allows us to further mitigate two measurement errors of the dependent variable. First, media can under-report attacks denounced by politicians. Second, not all the reported attacks can be imputed to organized crime. The first concern is not serious because in most cities attacks against politicians are uncommon and when they occur they generally do get reported (Daniele and Dipoppa, 2017). For while threats may be hidden, outright violence is visible and, thus, unlikely to go unreported. As for the second issue, attacks driven by personal motives are a tiny fraction of the total (Moro et al., 2015). Moreover, this issue mainly concerns threats; e.g., online threats are unlikely to be perpetrated by organized crime. Unfortunately, we cannot distinguish between these two types of threats. *Violence*, instead, comprises types of attacks (e.g., set the car/City Hall/the house on fire, shootings against the City Hall and/or the politicians' car) that are typical of mafias intimidation strategy.

Graphs E.11(a) and E.11(b) of Fig. E.11 in Appendix show the effect of the measure on the two categories of attacks. The dynamic pattern of the number of threats against local politicians after the city council dismissal is very similar to that of violence. The persistence of the measure's effect after the second electoral round after the city council dissolution is confirmed for both the subgroups of attacks. Columns 1 and 2 of Table D.5 show a statistically significant average effect of the measure for the two categories of attacks. The decrease in *Violence* was greater than that in *Threats*, with an elasticity of the average treatment effect over the entire period of 52% for *Threats* and 58.6% for *Violence*.

4.3. Heterogeneous treatment effects

The event-study models that we estimate allow for different effects of the measure against organized crime over the years since the onset of the treatment. A recent strand of the literature on DiD (De Chaisemartin and d'Haultfoeuille, 2023) highlights how the heterogeneity of treatment effects across units or groups can be a serious problem for aggregate estimates. In event-study regressions with staggered treatments (Sun and Abraham, 2021) show that the estimate of each dynamic effect can be expressed as a weighted average of the effects that differ by treatment cohort. This kind of heterogeneity can cause biased aggregate estimates if it is not correctly taken into account.

Several approaches to robust estimation have been recently proposed for linear models (De Chaisemartin and d'Haultfoeuille, 2023). The case of count panel data has been considered in Wooldridge (2023) where a general panel data framework is advanced

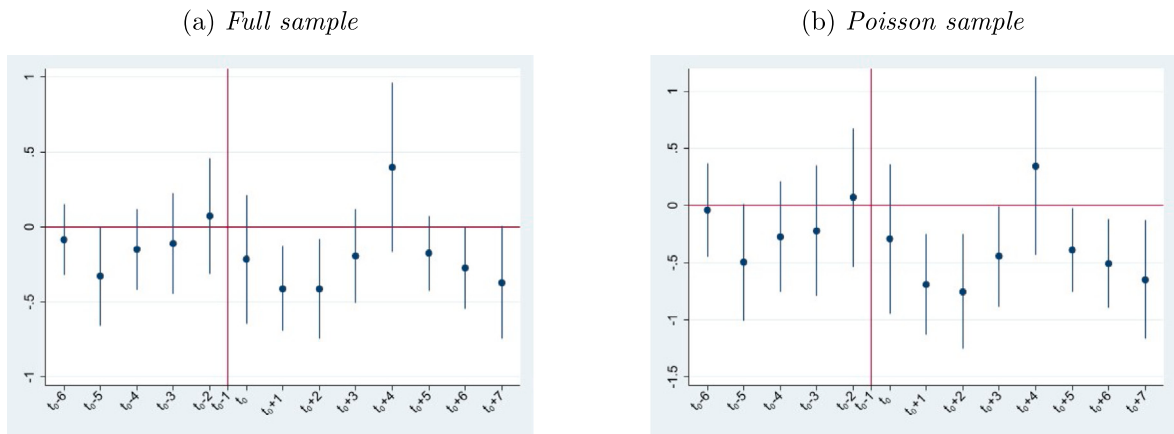


Fig. 2. Event study graphs — Linear estimates. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1). The dependent variable is the total number attacks against local politicians. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. We include event-time dummy variables for 6 years before and 7 years after the dissolution. In all regressions we control for municipality FE, year FE, the four dummies for electoral cycle and the log of resident population. Graph 2(a) refers to the full sample of municipalities for linear estimates while Graph 2(b) refers to municipalities in the sample used for Poisson estimates as in Graph 1(b). Period: 2010–2019.

to deal with heterogeneity in nonlinear DiD models. Assuming both parallel trends and no anticipation, Wooldridge (2023) demonstrates that pooled quasi-maximum-likelihood can be used to identify and estimate a large variety of DiD models when the heterogeneity of treatment effects is suitably considered in model specification. In the case of staggered treatment onset, possible heterogeneity of the effects across the cohorts of units implies the specification of different dynamic average effects for each cohort. The estimation of such a general model allows the test of aggregate specifications as the event study we use in this paper.

Our data refer to the dissolution of about 85 city councils in Southern Italy over ten years. Every year but the first sees the policy adoption by a small number of municipalities, and the outcome variable (i.e., the number of attacks to politicians) shows a very large number of zeros, greater than ninety percent of the total. These features of the data make quite hard the estimation of a nonlinear model with a large number of binary regressors as that advanced in Wooldridge (2023). Accordingly, we chose to investigate the robustness of our event-study results to cohort heterogeneity applying the methods recently proposed for linear models.

We, firstly, show in Fig. 2 that the dynamic of the violence against politicians (measured as the number of attacks against politicians, as in baseline Poisson estimations) displays the same pattern as in Fig. 1 when we perform linear model estimations on the full sample of municipalities (Graph 2(a)) and on the sample of Poisson estimates as in Graph 1(b) (Graph 2(b)).¹⁴

Then, in order to address the concern of heterogeneous treatment effects in such context of count data, we use the estimator implemented by Borusyak et al. (2021) for linear models that is robust to treatment effect heterogeneity.¹⁵ Results are displayed in Fig. 3 and the point estimates of parameters are in Table 2. A glance to the figure shows that the dynamic of the effects of council dissolution on violence against politicians when heterogeneous treatment effects are addressed replicates that of the main analysis. The confidence interval of all the pre-treatment coefficients include zero supporting the no-anticipatory effect of the measure against organized crime. The parallel trends assumption is also supported by the F-test that suggests the acceptance of the null hypothesis that all the pre-treatment coefficients are jointly equal to zero, as the last row of Table 2 shows.¹⁶ Graph 3(a) shows estimation over the full sample of municipalities (the number of observations for such estimation is equal to 10,921) while in Graph 3(b) we restrict the sample to that for Poisson estimates.¹⁷

5. Alternative mechanism

The evidence set forth to this point indicates that our results are consistent with the prediction of Dal Bó et al. (2006) that, in equilibrium, non-repressive measures reduce the profitability of corruption and with it the overall resort to violence. To support this interpretation, here we show that the implementation of the policy reduces the attacks against city council politicians and not against politicians outside the council. Furthermore, we provide suggestive evidence that the alternative mechanism of an increased perceived deterrence after city council dismissal does not find statistical support in our data.

¹⁴ The number of observations for estimation in Graph 2(a) is equal to 10,885 while in Graph 2(b) is equal to 3174.

¹⁵ We use the unofficial Stata command *did_imputation* (Borusyak, 2023).

¹⁶ The *P*-value of the F-test is equal to 0.638 for estimation in Graph 3(a) and equal to 0.661 for estimation in Graph 3(b).

¹⁷ In Graph 3(a) we were able to estimate until 6 leads after the treatment, while in Graph 3(b) we were able to estimate until 5 leads.

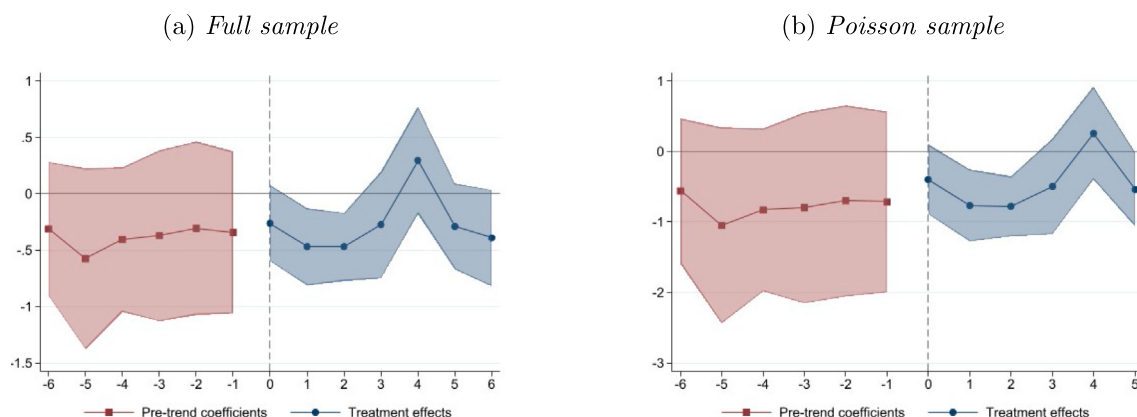


Fig. 3. Event study graphs — Heterogeneous treatment. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1). The dependent variable is the total number attacks against local politicians. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. We include event-time dummy variables for 6 years before and 7 years after the dissolution. In all regressions we control for municipality FE and year FE. Graph 3(a) refers to the full sample of municipalities for linear estimates while Graph 3(b) refers to municipalities in the sample used for Poisson estimates as in Graph 1(b). Period: 2010–2019.

Table 2

Estimates robust to heterogeneity treatment effects.

Dep. Var.: No. attacks	(1)	(2)
$t_0 + 1$	−0.471*** (0.175)	−0.766*** (0.260)
$t_0 + 2$	−0.470** (0.153)	−0.775*** (0.217)
$t_0 + 3$	−0.275 (0.241)	−0.495 (0.644)
$t_0 + 4$	0.297 (0.243)	0.260 (0.336)
$t_0 + 5$	−0.291 (0.194)	−0.540** (0.269)
$t_0 + 6$	−0.392* (0.217)	
Observations	10,921	3126
P-value F-test	0.638	0.661

Note. The table reports coefficients estimated according to Eq. (1). The dependent variable is the total number attacks against local politicians. Standard errors are clustered at municipal level. In estimating Eq. (1) we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). All regressions include municipality FE and year FE (coefficients not reported). Standard errors are in brackets. The last row of the table report the P -value of the F-test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Refining the target of clans' violence. In Dal Bó et al. (2006)'s conceptual framework, the enforcement of Law 164 increases the costs of corruption for city council politicians as they run the serious risk of being ousted from their office. Therefore, this model would predict a reduction in the number of attacks against city council politicians *but not* against politicians that are not part of the council, because the latter are not hit by the enforcement of Law 164.

Within the limits of our dataset, we try to disentangle these two mechanisms by estimating the event study model of Eq. (1), distinguishing between members of the city council (mayor, deputy mayor, councilors and aldermen) and other figures within the municipality but *outside* the city council (former mayor, relatives of politicians, the city hall). The results are in Fig. 4, comparing the dynamic effect of the enforcement of Law 164 on attacks against city council members (Graph 4(a)) and other political/institutional figures (Graph 4(b)). The analysis suggests that the overall reduction in violence against politicians is driven mainly by a decrease in attacks against city council members (Graph 4(a)). No statistically significant results, instead, appear after the compulsory administration period for politicians outside city council (Graph 4(b)).

Perceived deterrence. The estimated decrease in violence might be due to greater attention of police forces to defending the territory after the city council dismissal. Hence, criminal behavior can be influenced by the perception of an increase in the risk associated to political infiltration due to more certain or more severe punishment (Sah, 1991; Lochner, 2007). Within the limits of our dataset, we try to provide suggestive evidence making this alternative mechanism less likely.

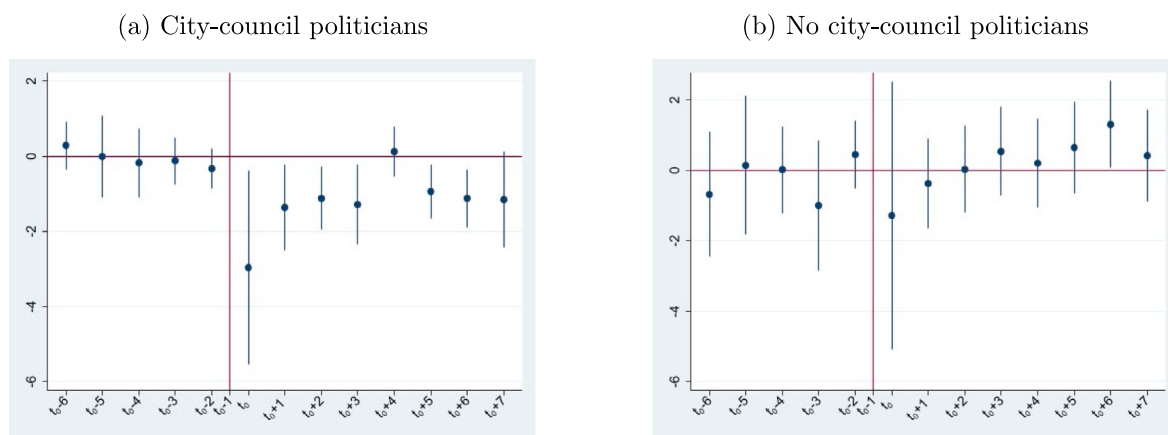


Fig. 4. Attacks to city-council politicians and no city-council politicians. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1). Graph 4(a) shows the Poisson FE model estimates where the dependent variable is the total number attacks against city-council politicians; Graph 4(b) shows the Poisson FE model estimates where the dependent variable is the total number attacks against other politicians or institutions. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. All regressions include municipality FE, year FE, the log of resident population. Period: 2010–2019.

Table 3

Types of politician.

No. attacks	(1) Municipal politicians/institutions	(2) Other institutions
Policy	−0.602** (0.280)	−0.374 (0.294)
Observations	3304	1787
No. Municipalities	332	179
Municipality FE	Yes	Yes
Controls	Yes	Yes

Note. The table reports estimates of the coefficients of a Poisson FE model. *Policy* is a dummy taking the value of 1 for all the years from the first after compulsory administration to the end of the period and 0 for previous years. We control for the log of resident population, municipality FE and year FE whose coefficients are not reported. Standard errors adjusted for clustering at the municipal level are in brackets. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

As discussed in Section 2.1, the enforcement of Law 164 does not directly imply stiffer sanctions or stepped-up police deployment. Here, we address this issue by exploiting the fact that the targets of mafia attacks are both municipal politicians/institutions and politicians/institutions outside local government (see Fig. E.4). We then replicate the analysis dividing politicians and institutions into two groups: *Municipal*, including deputy mayors, mayors, councilors, aldermen, former mayors, relatives, and the city hall building; and *other institutions or institutional entities*. If the reduction in violence is due to the perception of greater enforcement, then we should observe a decrease in the number of attacks in the municipality as a whole, that is, in *both* the sub-samples of politicians.

The estimation results are in Columns 1 and 2 of Table 3 respectively for the *municipal* and *other* institutions. This analysis suggests that the measure reduces the attacks against municipal entities by 44% on average (Column 1). As for institutional entities outside the municipality (Column 2), although we estimate a sizable negative coefficient, it is not statistically different from zero. Hence, with due caution for the general validity of this result, the measure seems to have no effect on institutional entities outside the municipality. This evidence indicates that an increase in *perceived deterrence* is not a likely driver of our results. Hence, the mechanism analyzed in Dal Bó et al. (2006) would appear to correspond more closely to our results.

6. Robustness

Appendix C of the Appendix presents additional evidence supporting our findings. Firstly, we replicate our analysis by implementing a propensity score matching procedure. We, then, test the validity of our main result on the full sample of municipalities in the treatment and control groups by Poisson RE estimates. We refine the analysis by excluding the year 2012, when there is a stark

drop in reporting the attacks against politicians due to a lack of human resources of “Avviso Pubblico”. We also investigate whether the political orientation of the local government has an impact on the effect of dissolution on the number of attacks. Moreover, we test whether dissolution unrelated to mafia infiltration affects our dependent variable. Finally, we investigate whether organized crime is able to restore its corrupting influence after the enforcement of Law 164 by excluding from the analysis municipalities that underwent multiple dissolution. These robustness analyses confirm our main results.

7. Conclusions

This paper offers an empirical assessment of the efficacy of the Italian Law 164/1991, mandating the dissolution of city councils in cases of mafia infiltration, in preserving public safety by deterring organized crime from acts of violence against politicians to influence local politics.

We referred to Dal Bó et al. (2006)’s model. In this framework, law enforcement that rises the cost of bribery without expanding the police presence could, on the one hand, induce criminal organizations to shift from corruption to violent intimidation, thereby raising the level of violence; or, on the other hand, it could have a negative effect on violence against politicians if it induces enough criminal groups to go inactive by substantially undercutting the profitability of bribery. In substance, empirical analysis of the impact of Law 164/1991 on organized crime violence against local politicians is essential to assessing its effectiveness in raising entry barriers to the mafias’ “business” of influencing politics.

Our DiD analysis shows that the enforcement of Law 164/1991 reduces the number of attacks against politicians substantially, with an effect that persists for (at least) seven years after compulsory administration. From the normative stand point, while city council dismissal is an *ad hoc* measure responding to specific circumstances within the local government, the persistence of the damping effect on violence against local politicians constitutes powerful evidence of the medium-run efficacy of Law 164/1991 in restoring the autonomy of the local government and public safety. These results complement recent studies documenting the effectiveness of Law 164/1991 in weakening organized crime corrupting influence on politics (e.g., Ravenda et al., 2020; Di Cataldo and Mastroiocco, 2022).

To provide a coherent interpretation of our findings, we discuss evidence suggesting that the most likely driver of our results is the renewal and improvement of the pool of elected politicians that implement policies weakening the economic power of local clans. To the extent that the cost to a criminal organization of influencing politics increases as the quality of officeholders improves (Dal Bó et al., 2006), our evidence indicates that the newly elected local governments represent a target for organized crime with higher negotiation costs, implying lower expected payoffs to corruption, thereby inducing mafias to lower the level of violence against politicians. Moreover, we show that the alternative mechanism, such as increased deployment of police forces and the perceived deterrence implicit in the enforcement of Law 164, does not find statistical support in our data.

Our results expand on recent studies documenting the effectiveness of city council dissolution prescribed by Law 164/1991 in improving long-run economic performance (Fenizia and Saggio, 2023), female political empowerment (Baraldi and Ronza, 2022) and the quality of elected officials (Daniele and Geys, 2015). Moreover, since several studies have shown that repressive measures (i.e., measures expanding police forces) tend to provoke violent reprisals by criminal organizations (e.g. Calderón et al., 2015; Castillo and Kronick, 2020), this paper contributes to the empirical analysis of the effectiveness of non-repressive government measures in fighting organized crime violence (Daniele and Dipoppa, 2022). Finally, the evidence presented here is in line with Daniele and Dipoppa (2017) and Alesina et al. (2019), showing that mafias use violence around elections to reinforce corruption. Our results document the efficacy of the city council dismissal in deterring violence by organized crime when it would be of the greatest strategic value in wielding political influence. This helps clarify how criminal organizations redirect public resources to their own advantage.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Conceptual framework

In this paragraph we derive the empirical hypothesis to be tested, summarizing the relevant aspects of the model of Dal Bó et al. (2006). In this framework, politicians have discretionary power over public resources that can be re-directed to the pressure group as a lump-sum transfer (π). To acquire these resources, pressure groups decide whether or not to influence politicians at a cost, through bribery (b) and credible threats of punishment (p).

In the absence of pressure groups, politicians’ income is equal to their salary w , but where pressure groups are active and the politician accepts bribery, this will be supplemented by bribes (b) and reduced by the costs of corruption c (moral costs, say, or the risk of detection). If instead the politician refutes the bribe offer, their salary will be reduced by the cost of punishment (p). Thus,

the politician will accept the bribe offer if $w + b - c \geq w - p$. Accordingly, active pressure groups will set the optimal bribe offer equal to the difference between the cost of being corrupted for the politician and the cost of punishment ($b^* = c - p^*$).

To choose the optimal levels of bribes and threats of violent punishment, pressure groups maximize the following payoff function: $\text{Max}_{b,p} \Pi(b, p) = \gamma[\pi - \beta\Phi(b)] - (1 - \gamma)\rho\Psi(p)$, subject to the constraint $b^* = c - p^*$, where γ and $(1 - \gamma)$ are respectively the probability of the politician's accepting and rejecting the bribe; π is the fraction of public resources that politicians can redirect to the pressure group; $\beta\Phi(b)$ and $\rho\Psi(p)$ respectively are the costs of delivering a bribe offer and carrying out a punishment¹⁸. The parameters β and ρ capture institutional features (i.e. law enforcement) that affect the costs of bribery and punishment respectively. The implementation of non-repressive measures that reduce the economic sources of organized crime are an instance of a change in the parameters β and π (i.e. the amount of public resources that complicit politicians can redirect to the pressure group). The expansion of police forces, instead, represents a change in the parameter ρ .

When the politician accepts the bribe, the pressure group obtains $\pi - \beta\Phi(b)$, the fraction of public resources managed by the politician net of the cost of the bribe. Otherwise, the pressure group bears the cost $\rho\Psi(p)$ of punishing the politician who rejects the offer. Whenever $\Pi(b, p) \geq 0$, the pressure group will be active; otherwise it remains inactive and makes no profit. Hence, setting $\Pi(b, p) = 0$ allows calculation of the threshold amount of public resources to be redirected to the pressure group ($\bar{\pi}$) below which the business of influencing politics is no longer profitable.

On this account, if the amount of public resources that can be redirected (π) is lower than $\bar{\pi}$, the pressure group's expected payoff would be negative. Hence, $\bar{\pi}$ can be interpreted as an inverse measure of “state capture”, i.e. the degree of the pressure group's influence on politics: the greater $\bar{\pi}$, the stiffer the entry barriers to the business of influencing politics and the lower the degree of “state capture”. Any change in law enforcement that affects the costs of “bribery and threats of punishment” (i.e., β and ρ) will alter the entry barriers and induce an active pressure group to modify its optimal mix of bribes and threats.

Intriguingly, for this study an increase in the cost of delivering a bribe — i.e., an increase in the β parameter — has a theoretically ambivalent effect on the level of criminal violence. On the one hand, if a pressure group is active both before and after the increase in β , it will substitute punishments for bribes, since the relative cost of the former diminishes: that is, the active mafia-style organization will step up its violent intimidation as a form of retaliation for the higher costs of bribery. On the other hand, an increase in the cost of bribes stiffens the barriers to entry into the business of influencing politics as such, as measured by the increase in $\bar{\pi}$. Some pressure groups within the range of variation of $\bar{\pi}$ would earn negative profits and will accordingly refrain from seeking influence either through bribery or through threats. If enough groups lie within this range, overall the rise in β will decrease the resort to threats of violent punishment. This is implied by Proposition 6 in Dal Bó et al. (2006). However, the model does not allow any straightforward prediction of what happens when the costs of bribery increase. Thus, which of the foregoing effects prevails is ultimately an empirical question; and this is what our analysis investigates.

City council dissolution by enforcement of Law 164/1991 imparts an exogenous shock to the pressure group's cost of making a bribe (i.e. an exogenous shock to the β parameter) and to the amount of public resources that politicians can redirect to organized crime (i.e. to the π parameter). Compulsory administration by external commissioners will presumably bring a sudden cut in funding for public projects, especially in the sectors targeted by organized crime such as manufacturing, waste management and construction (Acconcia et al., 2014). The Law, that is, is intended to lower the level of corruption by imposing an economic cost on organized crime's exercise of political influence, while it does not entail any stiffening of formal deterrence by such measures as expansion of police forces or increases in funds allocated to public safety (that is, it does not entail any change in the ρ parameter).

In the logic of Dal Bó et al. (2006), to the extent that city council dissolution increases the cost of corruption, it can curb overall violent intimidation only if it substantially cuts the amount of public resources to be redirected (π), thereby reducing the expected payoffs from influencing politics and inducing some criminal groups to go inactive. So if we are to assess the efficacy of Law 164/1991 in strengthening the barriers to influencing politics, empirical analysis of the effect on violent intimidation by organized crime is essential.

Appendix B. Spillover effects

The “Stable Unit Treatment Value Assumption” (SUTVA) (Rubin, 1980), requires that the treatment status of one municipality does not affect outcomes for untreated municipalities. In this section we provide evidence of the presence of possible spillover effect that determine the violation of the SUTVA.

In this respect, we estimate a Poisson FE model as in Eq. (1), taking as treated municipalities those with at least one neighbor experiencing a city council dissolution in the period 2010–2019; the control group comprises all the other municipalities except the dissolved ones that are excluded from the analysis.¹⁹ The neighboring municipalities are assumed to start receiving the treatment in the year of the first dissolution of a contiguous municipality. The dependent variable is the total number of attacks against politicians by year.

Fig. E.7 in Appendix shows the results of the estimation of the Poisson event study with standard errors clustered at municipal level.²⁰ Since we expect spillovers effects in neighboring municipalities to be delayed over time, we include the post-treatment

¹⁸ $\Phi(\cdot)$ and $\Psi(\cdot)$ are the cost functions respectively for delivering a bribe and carrying out a punishment. They are assumed to be twice continuously differentiable; also, $\Phi(0) = \Psi(0) = 0$, $\Phi'(0) = \Psi'(0) = 0$, $\Phi' > 0$, $\Phi'' > 0$, $\Psi' > 0$ and $\Psi'' > 0$.

¹⁹ Our source for selection of the neighboring municipalities is the “Matrici di contiguità, distanza e pendolarismo” database provided by the Italian Institute of Statistics (ISTAT).

²⁰ We also perform the same estimation by clustering standard errors at provincial level; results are unchanged and available upon request. Clustering at province level could improve the estimation of standard errors when spatial spillovers are significant.

event-time dummies up to $t + 9$. The coefficients for the pre-treatment period are not significantly different from zero, corroborating the validity of the common trend assumption for neighboring municipalities. The dynamic of the number of attacks suggests that for neighboring municipalities the measure substantially reduces organized criminal violence against politicians. As expected, the trajectory of the attacks in neighboring municipalities displays a lagged response to the dissolution. Although all the post-treatment coefficients are negative, they are not significant until $t_0 + 3$ and $t_0 + 4$. The spillover effect of the measure is persistent through time, as documented by the strongly significant coefficients up to $t_0 + 9$.

However, spillover effects could extend beyond the neighboring municipalities. Thus, we will check for spillover effects in a more rigorous manner. Fig. E.8 shows the municipalities in the control group according to the distance (based on the scale of blue) from the dissolved municipalities (the red points). Among such distances, for each treated (dissolved) municipality, we select all the never-treated municipalities in a 20, 30 and 40 km radius. As for the neighboring municipalities, the treated municipalities are those with at least one experiencing a city council dissolution in a radius of 20/30/40 km in the period 2010–2019 (hereafter *new treated*). The *new treated* municipalities are assumed to start receiving the treatment in the year of the municipality's dissolution in the 20/30/40 km radius. The control group comprises all the other municipalities except the dissolved ones.

Fig. E.9 reports the results for the estimation of the Poisson event study model (Eq. (1)). Graphs E.9(a), E.9(b) and E.9(c) in Appendix show the event study estimates with standard errors clustered at municipal level. For any radius in between 20 and 40 km, the event study graphs suggest the absence of any anticipatory effect of the anti-mafia measure as well as of any the spillover effects.

However, the presence of spillovers effects in municipalities neighboring the dissolved ones means the SUTVA assumption is probably violated. This can be a source of bias for our baseline estimates, because these neighboring municipalities are part of the control group. In order to correct for this potential bias in the estimates, following Kline and Moretti (2014a), we exclude these neighboring municipalities from the control group.

We further investigate possible spillover effects of the anti-mafia measure on violent intimidation against local politicians by following the new methodological approach proposed by Butts (2021b) that provides unbiased estimation of the *total effect* (the treatment effect on the treated) and the *spillover effects* (treatment effect on the untreated in the treated clusters, i.e., the units in the control group that receive spillover effects — partially treated units) over the entire sample of units. Following Butts (2021b), we estimate a linear dynamic model as in Eq. (1) on the whole sample of municipalities. The Butts (2021b)'s model includes the dynamic effects of the treatment variable and a spillover variable. Because the effects of council dissolution may vary across groups (Sun and Abraham, 2021), Butts (2021b) proposes the application of the two-stage estimator by Gardner (2022) that identifies the average treatment effects of the event times dummies.²¹

Fig. E.16 shows the plots of the event study estimated coefficients with treatment (*total effect*) and spillover average effects (*spillover effects*) when standard errors are clustered at city level. In our model, the geographic spillovers (the spillover variable) are captured by a dummy variable taking value 1 if in year t , municipality i is located no farther than a given distance from the nearest city with dissolved council, and 0 otherwise. As in Section 3.1, we consider possible spillovers in neighboring municipalities and in municipalities located at 20 km/30 km/40 km to define the largest ring.

The four graphs in Fig. E.16 refer to contiguous cities. Although Poisson estimates in Section 3.1 showed statistically significant spillovers in neighboring municipalities to the dissolved ones, the Butts (2021b)'s procedure provides slightly different results. Indeed, Graph E.16(b) does not provide evidence of spillover effects in neighboring municipalities. The same result is obtained when we adopt the other three values of distance.²² Furthermore, the estimates of the *total effect* of the council dissolution on political violence as in Graph E.16(a) confirms the main findings (Graphs in Fig. 1), showing that spillovers are not a serious concern in our analysis.

Appendix C. Robustness

C.1. Propensity score matching

In the matching procedure, the first step is to estimate the propensity score corresponding to the probability of each municipality's being treated, that we obtain by fitting a probit model of treatment status using as covariates (at the municipal level): population, average female and male educational attainment, female and male unemployment rates, a dummy for mayor's gender (1 if female) and the share of female politicians within the city council, measured as the average over the full period.²³

The t-tests for the means difference of the covariates for matched municipalities in the treatment and control groups highlight a good balance given that for all the covariates we do not reject the null (see Table D.6 in Appendix). The goodness of the match for constructing a suitable control group is also confirmed in Fig. E.12, which shows the propensity scores for the treatment and comparison groups before and after the matching.

²¹ We use the unofficial Stata command *did2s* by Kyle Butts (Butts, 2021a).

²² Regression results are not shown but are available upon request.

²³ We use the Stata command *psmatch2* developed by Leuven and Sianesi (2003) to calculate the nearest-neighbor propensity score matching where the matching is one-to-four. For each municipality, we calculate the mean, over year, of each variable to construct the cross-section for the propensity score procedure. In the propensity score matching procedure the $R^2 = 0.005$.

Fig. E.13 and Table D.7 show the estimation results of the Poisson event study and DiD model. The baseline findings are confirmed despite the substantial reduction in the number of observations, inevitable in propensity score matching. First of all, the common trend assumption is definitely corroborated, as is shown by the insignificance of the coefficients before the dissolution. The post-dissolution dynamic shows a significant negative effect just after compulsory administration ($t_0 + 2$ and $t_0 + 3$). The estimates also display a durable effect of the measure after $t_0 + 4$.

Column 2 of Table D.7 shows that after a city council dissolution the number of attacks decreases on average by 48% more in the treatment than in the control group. The coefficient is smaller (in absolute value) than that in the full sample estimates.

C.2. Poisson RE estimates

In Section 3 we well documented the validity of the Poisson empirical model to estimates count data. Indeed, with count data, applying an ordinary linear regression model can lead to two kinds of problems. First, being our distributions of count data positively skewed with many observations having a value of 0, it prevents the transformation of a skewed distribution into a normal one. Second, it is quite likely that the regression model will produce negative predicted values, which are theoretically impossible.

However, as pointed out, in the model that controls for any omitted time-invariant variables, Poisson log-likelihood maximization excludes the units that always have zero values of the dependent variable for the entire period, leading to a substantial reduction in the number of observations. To face this issue, in this section, for robustness we present Poisson RE estimations (Table D.8 and Fig. E.14). The results are similar but less strong than those with fixed effects.

C.3. Excluding year 2012

As noted in Section 2.2, the downward spike in attacks for 2012 simply reflects the lack of data collection by “Avviso Pubblico” for that year. Accordingly, 2012 is excluded from the analysis, but the results are unchanged (see Table D.9 and Graph E.15(a) of Fig. E.15).

C.4. Political party in power

In view of the literature that has documented that mafias are more connected with center-right parties (Buonanno et al., 2016), we assess whether our results are affected by the political orientation of the victims. In Italy, at local level, only in the largest municipalities is the electoral competition between national parties with a well-defined political orientation. In most municipalities, instead, it is between civic lists, often with no clear political orientation.

Applying a broader code of center-right and center-left parties, we divide the parties in local government into three categories: parties and civic lists of right and center-right (i.e., all those whose names contain words clearly ascribable to a rightist political group — *Center-right*), parties and civic lists of left and center-left (i.e., those with names containing words clearly ascribable to a leftist political group — *Center-left*), and parties and civic lists with no definite political orientation (*Civic lists*). In our sample of municipalities, 83.74% of municipal administrations are headed by civic list mayors, 9.63% by center-left mayors and the remaining 6.57% by center-right mayors.

According to this broader coding, in the Poisson FE estimations of the event study and DiD models we control for the dummies *Center-right*, *Center-left* and *Civic lists*.²⁴ The results (Graph E.15(b) of Fig. E.15 and Table D.11) are consistent with the baseline, and they indicate that the mayor's political orientation does not affect the outcome variable.

C.5. Placebo test on governments dissolved for reasons unrelated to mafia infiltration

As a further check of our findings, we run a placebo test on municipalities dissolved for reasons other than mafia infiltration, which is a common occurrence. The regular term in office of the mayor and the council is five years²⁵; under certain circumstances — permanent impediment, removal, lapse of appointment, or death preventing the mayor or the majority of the council from performing their duties; violation of the Constitution or national law; failure to pass the budget — the mandate may be terminated early and new elections held. We estimate Poisson FE as in Eq. (1), considering the dynamics of the number of attacks in municipalities dissolved for non-mafia reasons before and after the city council dismissal. Obviously municipalities dissolved for mafia infiltration are excluded. Graph E.15(c) of Fig. E.15 shows the year-by-year Poisson FE estimated coefficients, which are not statistically different from zero.²⁶ In short, the number of attacks against local politicians is plainly not significantly affected by city council dissolution *per se*.

²⁴ Given that the Poisson FE algorithm does not estimate the constant term, we can add all the dummies without falling into the dummy variable trap.

²⁵ Until 2000, shortened to four years by Law 81/1993, Art.2; restored to five by Legislative Decree 267/2000, Art.51.

²⁶ In our sample, 420 municipalities were dissolved for reasons unrelated to mafia infiltration.

Table D.1
City council dismissal by regions.

Region	City council dismissals
Calabria	59
Campania	21
Sicily	25
Apulia	10
Piedmont	2
Lombardy, Emilia Romagna, Liguria	1
Rest of Italy	0
Total	120

Note. The table displays the number of administrations dissolved in the period 2010–2019 by region. Lombardy, Emilia Romagna, and Liguria had 1 dissolution each.

Table D.2
Distribution of attacks.

No. attacks	Frequency	Percentage	Cumulative
0	10,363	94.64	94.64
1	243	2.22	96.86
2	178	1.63	98.48
3	76	0.69	99.18
4	27	0.25	99.42
5	26	0.24	99.66
6	10	0.09	99.75
7	7	0.06	99.82
8	8	0.07	99.89
9	5	0.05	99.94
10	2	0.02	99.95
11	1	0.01	99.96
14	2	0.02	99.98
15	1	0.01	99.99
17	1	0.01	100
Total	10,950	100	

Notes. Distribution of number of attacks against politicians in each municipality. Period: 2010–2019.

Table D.3
Descriptive statistics.

	Obs	Mean	Std. Dev.	Min	Max
Attacks	10,950	0.125	0.702	0	17
Pop	10,950	6.894	15.61	0.158	243.97

Notes. Descriptive statistics of variables. Population in thousands/1000. Period: 2010–2019.

C.6. Ineffectiveness of the city council dissolution

It might be argued that the decrease in attacks might indicate that organized crime is able to restore its corrupting influence, thereby reducing the need of violence to coerce them in the subsequent years. This would be at odds with the literature documenting the effectiveness of Law 164 in reducing the corruption of local politics in the long run (e.g., [Fenizia and Saggio, 2023](#)). Nonetheless, we address this concern by excluding from the sample 7 municipalities (see [Fig. E.2\(b\)](#)) that underwent multiple dissolution for mafia infiltration. By focusing on those municipalities where the city council dismissal was more effective in breaking the ties between clans and local politics, we investigate whether there is any change in the dynamic pattern of the regression results. [Table D.10](#) and [Graph E.15\(d\)](#) of [Fig. E.15](#) document that our main results are confirmed, making it less likely that clans violence at the second electoral round is successful in restoring the corrupting influence on politics.

Appendix D. Tables

See [Tables D.1–D.11](#).

Appendix E. Figures

See [Figs. E.1](#) and [E.16](#).

Table D.4Dissolved municipalities *also* because of violent intimidation against politicians.

Municipality	(1) Violent intimidation	(2) Politicians' crimes	(3) Extortion/irregular markets	(4) Shared economic interests	(5) Politicians' resignations
Altavilla Milicia	✓		✓	✓	✓
Bagnara Calabra	✓			✓	
Borgetto	✓		✓	✓	✓
Brancaleone	✓	✓			✓
Camastra	✓	✓		✓	
Carmiano	✓	✓		✓	
Cellino San Marco	✓	✓	✓	✓	
Corleone	✓	✓	✓	✓	
Crispano	✓	✓	✓		
Manduria	✓	✓	✓	✓	✓
Mazzarrà Sant'Andrea	✓	✓	✓	✓	✓
Monte Sant'Angelo	✓	✓	✓	✓	✓
Montelepre	✓	✓	✓		
Plati'	✓		✓	✓	✓
Rizziconi	✓	✓			✓
San Ferdinando	✓	✓	✓		✓
Scafati	✓	✓	✓	✓	✓
Scicli	✓	✓	✓		✓
Siderno	✓		✓		✓
Strongoli	✓	✓		✓	✓
Surbo	✓	✓	✓	✓	✓
Tropea	✓			✓	

Note. The table reports the reasons for city council dismissals in the 22 municipalities where such reasons accompanied the evidence of attacks against politicians.

Table D.5

Types of attacks.

	(1) Threats	(2) Violence
Policy	−0.704** (0.326)	−0.843*** (0.285)
Observations	2044	2397
No. Municipalities	206	241
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes

Note. The table reports coefficients estimated according to Eq. (1) by Poisson FE model. *Violence* comprises set the car on fire, set City Hall on fire, set the house on fire, physical assault, homicide and physical assault in public places; *Threats* comprises all the other attacks. *Policy* is a dummy taking the value of 1 for all the years from the first after compulsory administration to the end of the period and 0 for previous years. We control for municipality FE, year FE and the log of resident population whose coefficients are not reported. Standard errors adjusted for clustering at the municipal level are in brackets. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table D.6

Mean difference tests.

Variable	Mean		t-test	
	Treated	Control	t	p > t
Mayor's gender	0.03465	0.03738	−0.35	0.726
Average male education	0.84838	0.84566	0.83	0.408
Average female education	0.80245	0.79963	0.67	0.506
Average male unemployment	0.23641	0.23778	−0.25	0.8
Average female unemployment	0.34789	0.34532	0.33	0.743
Population	16.668	19.931	−0.57	0.568
Share of female politicians	11.478	11.587	−0.25	0.804

Note. The Table shows the value of the t and the p-value of the covariates used for the probit estimation of the treatment status. We use the Stata command *pstest*.

Table D.7
Propensity score matching.

Dep. Var.:	(1) No. attacks	(2) No. attacks
$t_0 + 1$	–1.566*** (0.533)	
$t_0 + 2$	–1.078** (0.493)	
$t_0 + 3$	–0.587 (0.479)	
$t_0 + 4$	–0.014 (0.338)	
$t_0 + 5$	–0.823** (0.362)	
$t_0 + 6$	–0.954** (0.428)	
$t_0 + 7$	–0.897* (0.541)	
Policy		–0.625** (0.278)
Observations	1534	1534
No. Municipalities	154	154
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
P-value Wald test	0.86	

Note. The table reports coefficients estimated according to Poisson FE model in Eq. (1). The dependent variable is total number of attacks against politicians. In estimating Eq. (1) in Column 1 we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). We report only the event study dummies that are significantly different from 0. *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 for previous years. We control for municipality FE and year FE and the log of resident population whose coefficients are not reported. Standard errors adjusted for clustering at the municipal level are in brackets. The last row of the table report the *P*-value of the Wald test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table D.8
Poisson RE.

Dep. Var.:	(1) No. attacks	(2) No. attacks
$t_0 + 1$	1.328** (0.548)	
$t_0 + 2$	–0.957* (0.512)	
$t_0 + 3$	–0.409 (0.504)	
$t_0 + 4$	0.276 (0.341)	
$t_0 + 5$	–0.598 (0.373)	
$t_0 + 6$	–0.735* (0.423)	
$t_0 + 7$	–0.887 (0.543)	
Policy		–0.558** (0.276)
Observations	10,876	10,885

(continued on next page)

Table D.8 (continued).

Dep. Var.:	(1)	(2)
	No. attacks	No. attacks
No. Municipalities	1091	1094
Year FE	Yes	Yes
Controls	Yes	Yes
P-value Wald test	0.71	

Note. The table reports coefficients estimated according to Poisson RE model in Eq. (1). The dependent variable is total number of attacks against politicians. In estimating Eq. (1) in Column 1 we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 for previous years. All regressions includes year FE (coefficient not reported). We control for the log of resident population. We also control for the level of education and unemployment of the municipalities that proxy for fixed effect given that they are constant over 2010–2019 (census data). Standard errors adjusted for clustering at the municipal level are in brackets. The last row of the table report the *P*-value of the Wald test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table D.9
Excluding 2012.

Dep. Var.:	(1)	(2)
	No. attacks	No. attacks
$t_0 + 1$	–1.634*** (0.558)	
$t_0 + 2$	–1.388*** (0.469)	
$t_0 + 3$	–0.991** (0.495)	
$t_0 + 4$	–0.160 (0.301)	
$t_0 + 5$	–1.151*** (0.356)	
$t_0 + 6$	–1.265*** (0.416)	
$t_0 + 7$	–1.458*** (0.551)	
Policy		–0.781*** (0.277)
Observations	2829	2829
No. Municipalities	316	316
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
P-value Wald test	0.63	

Note. The table reports coefficients estimated according to Poisson FE model in Eq. (1). We drop the year 2012. The dependent variable is total number of attacks against politicians. In estimating Eq. (1) in Column 1 we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). We report only the event study dummies that are significantly different from 0. *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 for previous years. We control for municipality FE, year FE and the log of resident population whose coefficients are not reported. Standard errors adjusted for clustering at the municipal level are in brackets. The last row of the table report the *P*-value of the Wald test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table D.10
Multiple dissolutions.

Dep. Var.:	(1) No. attacks	(2) No. attacks
$t_0 + 1$	−1.660*** (0.532)	
$t_0 + 2$	−1.344*** (0.517)	
$t_0 + 3$	−0.786 (0.481)	
$t_0 + 4$	−0.075 (0.306)	
$t_0 + 5$	−0.934*** (0.350)	
$t_0 + 6$	−1.170*** (0.409)	
$t_0 + 7$	−1.253** (0.536)	
Policy		−0.808*** (0.262)
Observations	3134	3134
No. Municipalities	315	315
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
P-value Wald test	0.68	

Note. The table reports coefficients estimated according to Poisson FE model in Eq. (1). We drop municipalities dissolved for mafia infiltration more than once. The dependent variable is total number of attacks against politicians. In estimating Eq. (1) in Column 1 we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). We report only the event study dummies that are significantly different from 0. *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 for previous years. We control for municipality FE, year FE and the log of resident population whose coefficients are not reported. Standard errors adjusted for clustering at the municipal level are in brackets. The last row of the table report the *P*-value of the Wald test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table D.11
Political party of administration.

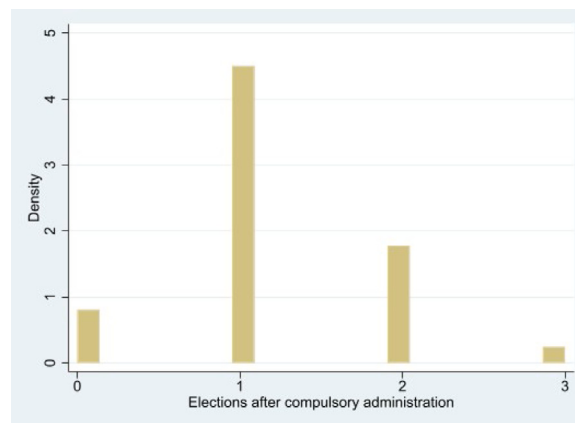
Dep. Var.:	(1) No. attacks	(2) No. attacks
$t_0 + 1$	−1.661*** (0.543)	
$t_0 + 2$	−1.305*** (0.488)	
$t_0 + 3$	−0.855* (0.464)	
$t_0 + 4$	−0.147 (0.297)	
$t_0 + 5$	−1.012*** (0.336)	
$t_0 + 6$	−1.141*** (0.397)	
$t_0 + 7$	−1.309** (0.514)	
Policy		−0.791*** (0.275)
Center-right	−0.00515 (0.194)	−0.0148 (0.197)
Center-left	0.171 (0.265)	0.171 (0.267)

(continued on next page)

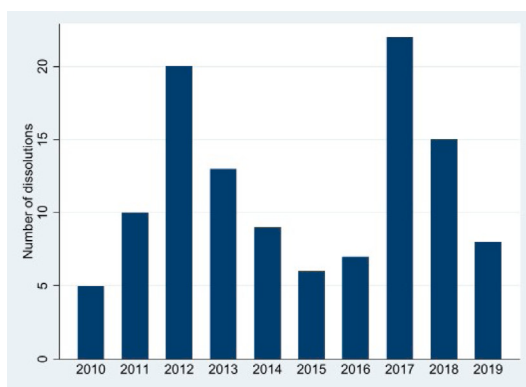
Table D.11 (continued).

Dep. Var.:	(1)	(2)
	No. attacks	No. attacks
Civic lists	0.0354 (0.170)	0.0186 (0.171)
Observations	3174	3174
No. Municipalities	319	319
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Controls	Yes	Yes
P-value Wald test	0.66	

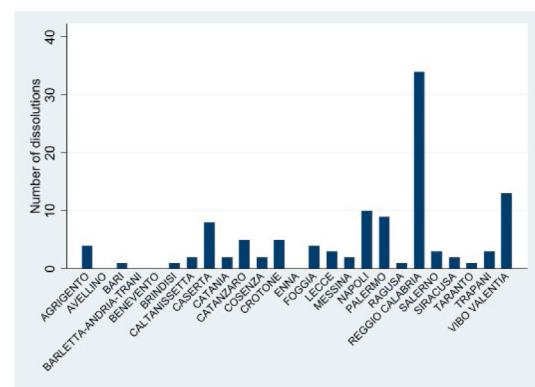
Note. The table reports coefficients estimated according to Poisson FE model in Eq. (1). The dependent variable is the total number of attacks against politicians. We control for dummies for mayor's party affiliation. In estimating Eq. (1) in Column 1 we include event-time dummy variables for 6 years before and 7 years after the dissolution; the omitted category is the year before the dissolution ($t_0 - 1$). We report only the event study dummies that are significantly different from 0. *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 for previous years. We control for municipality FE, year FE and the log of resident population whose coefficients are not reported. Standard errors adjusted for clustering at the municipal level are in brackets. The last row of the table report the *P*-value of the Wald test whose null hypothesis is that all the pre-treatment coefficients are jointly equal to zero. Period: 2010–2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

**Fig. E.1.** Frequency of elections after compulsory administration. *Notes.* Frequency of elections after compulsory administration. 2010–2019.

(a) By year



(b) By province

**Fig. E.2.** Dissolutions by year and province. *Note.* Graphs E.2(a) and E.2(b) show the distribution of city council dissolutions for mafia infiltration, by year and by province, respectively, in Calabria, Campania, Apulia and Sicily.

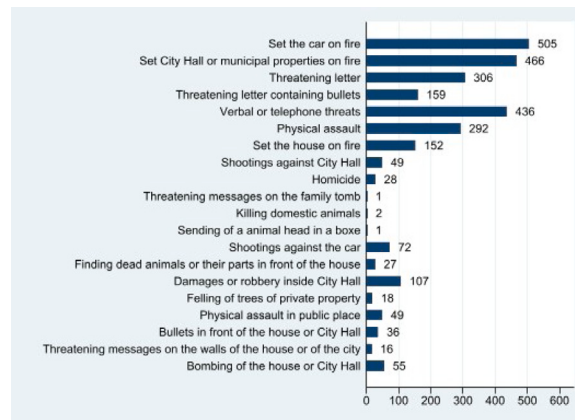


Fig. E.3. Types of attacks. 2010–2019. Notes. The Figure shows the types of attack against politicians in Campania, Calabria, Apulia and Sicily. 2010–2019.

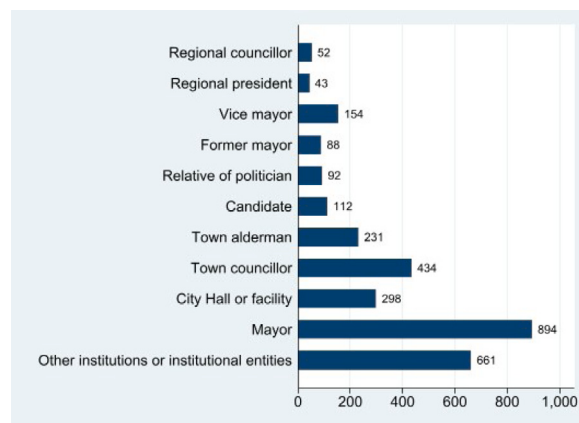


Fig. E.4. Victims of attack. 2010–2019. Notes. The Figure shows the types of victim of mafia attacks in Campania, Calabria, Apulia and Sicily. 2010–2019.

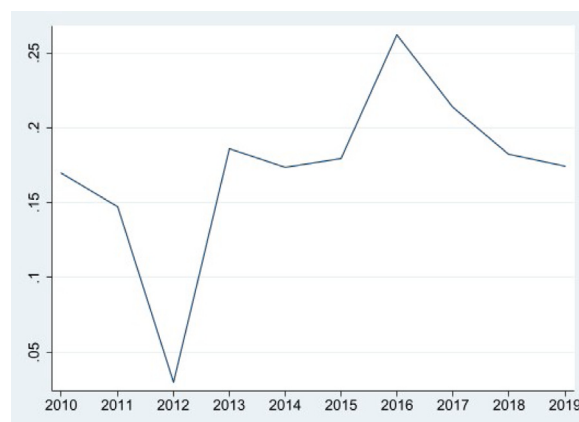


Fig. E.5. Yearly distribution of attacks. Notes. The Figure shows the trend in the mean, over municipalities, of attacks against politicians. 2010–2019.

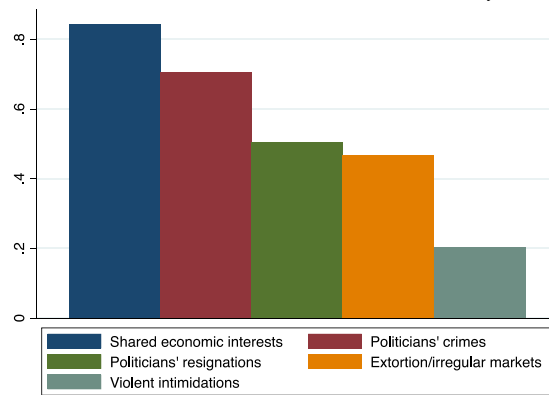


Fig. E.6. Reasons for City council Dismissals. Percentage. *Notes.* The histogram reports the percentage of occurrence (on the y axis) of all five categories of reason motivating the Prefect's decision for city council dissolution. Time-span 2010–2020.

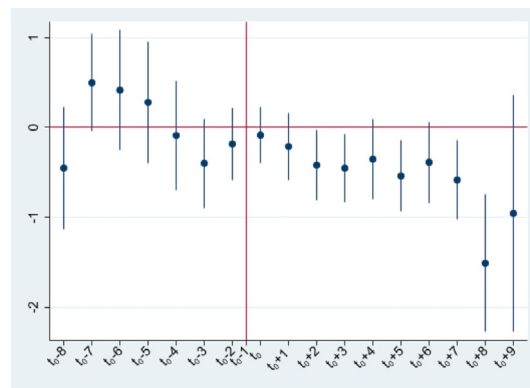


Fig. E.7. Spillovers — Event study. *Notes.* The graph reports coefficients and confidence intervals estimated according to Eq. (1). Graphs show the Poisson FE model estimates where the dependent variable is the total number attacks in neighboring municipalities. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). We include event-time dummy variables for 8 years before and 9 years after the dissolution. All regressions include municipality FE, year FE, and the log of resident population. Period: 2010–2019.

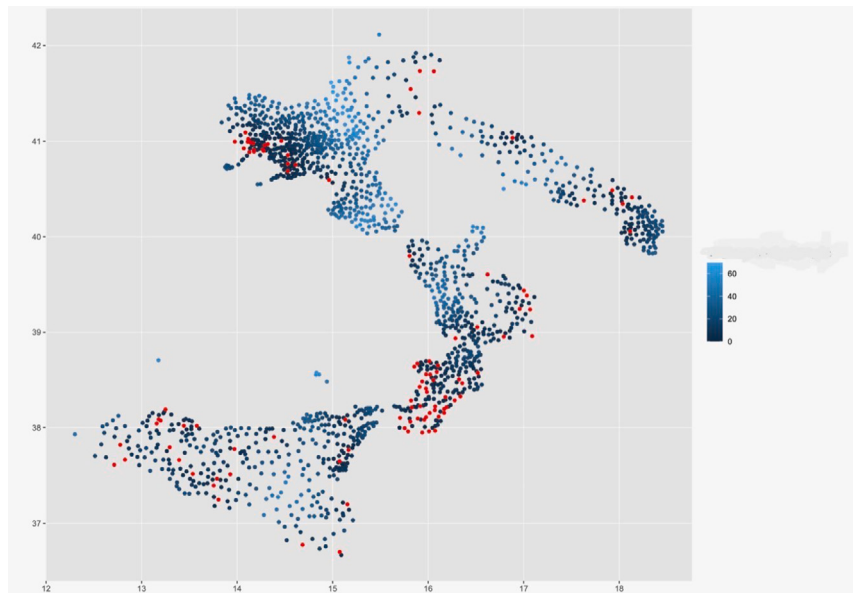


Fig. E.8. Distance to the dissolved municipalities. *Notes.* The figure shows the distance of the never dissolved municipalities in the control group from the dissolved municipalities (the red points) according to the distance expressed in scale of blue. The picture refers to year 2019. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

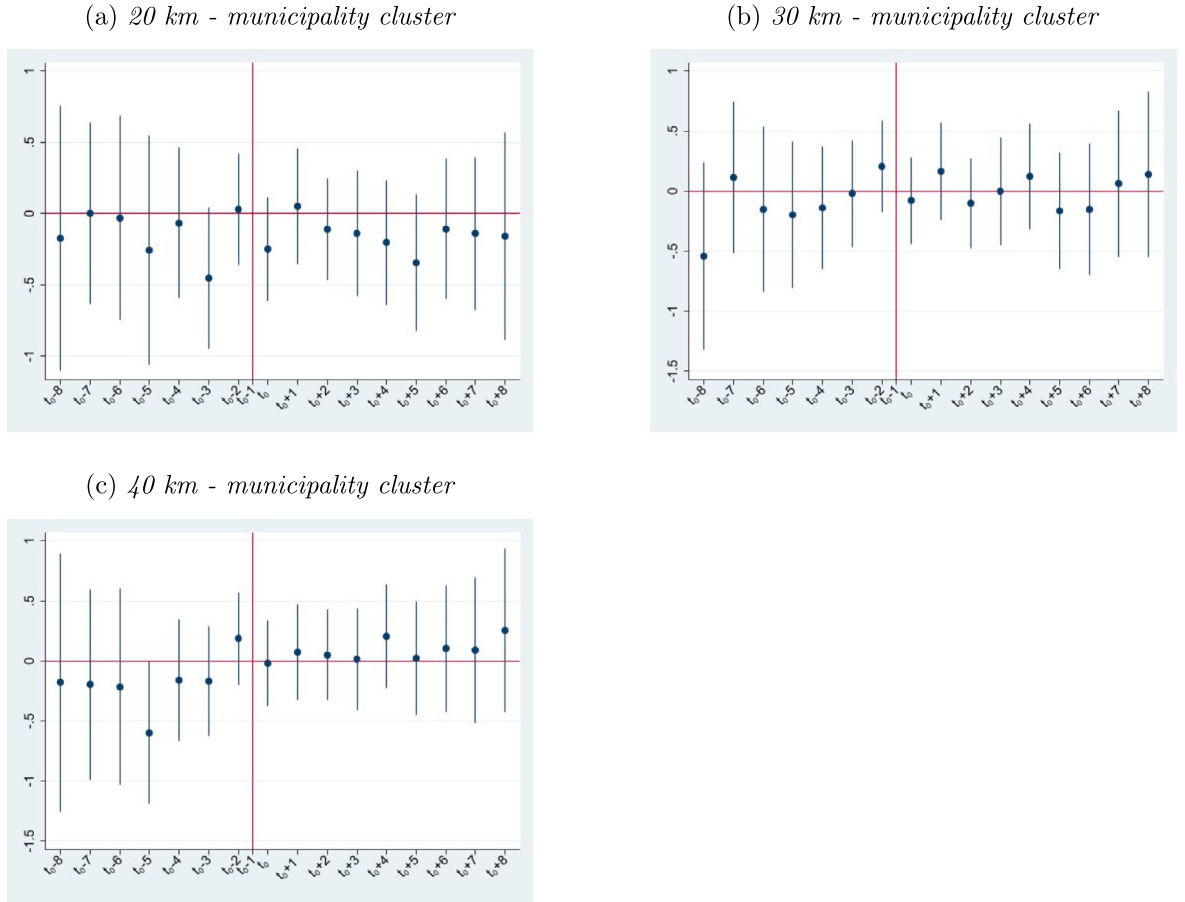


Fig. E.9. Spillovers — 20/30/40 km radius. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1). Poisson FE model estimates, dependent variable is total number attacks against local politicians. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). Standard errors are clustered at municipal level. We include event-time dummy variables for 8 years before and 8 years after the dissolution (the ninth year dummy is dropped because of collinearity). We control for municipality FE, year FE, the log of resident population. Standard errors are clustered at municipal level. Period: 2010–2019.

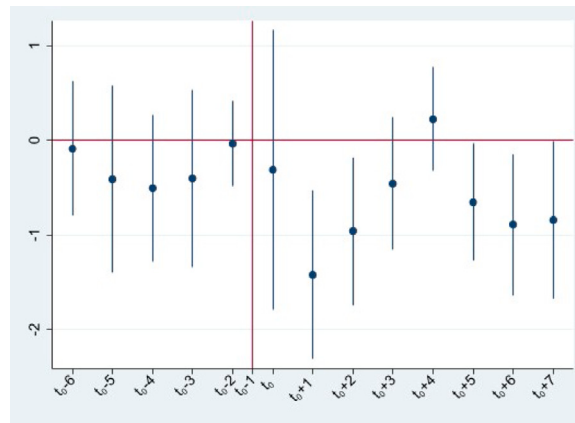


Fig. E.10. Event study — Region $\times$ year FE. *Notes.* The graphs report coefficients and confidence intervals estimated according to the Poisson model of Eq. (1). Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. The regression include municipality FE, year FE, Region $\times$ year FE and the log of resident population. Period: 2010–2019.

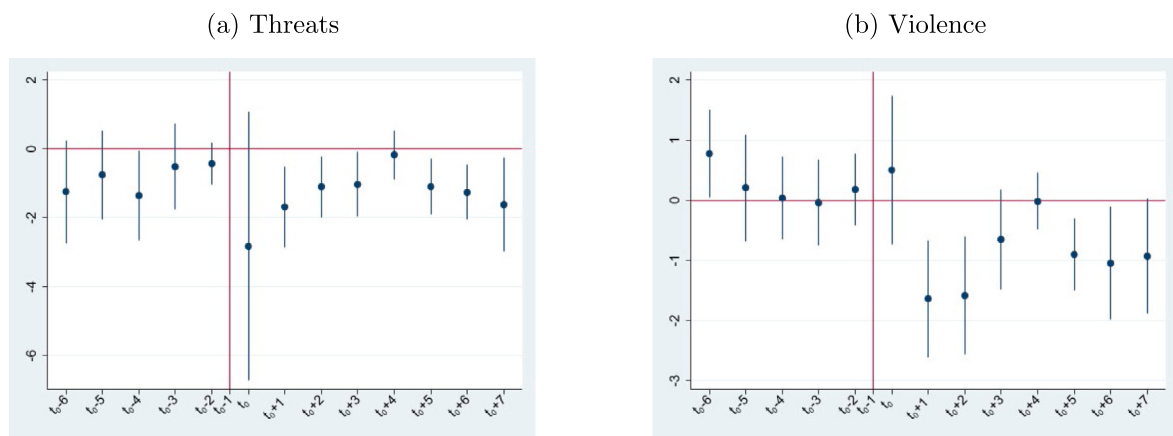


Fig. E.11. Types of attacks. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1). Graph E.11(a) shows the Poisson FE model estimates where the dependent variable is the total number of *Threats* against local politicians; Graph E.11(b) shows the Poisson FE model estimates where the dependent variable is the total number of *Violence* against local politicians. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. All regressions include municipality FE, year FE and the log of resident population. Period: 2010–2019.

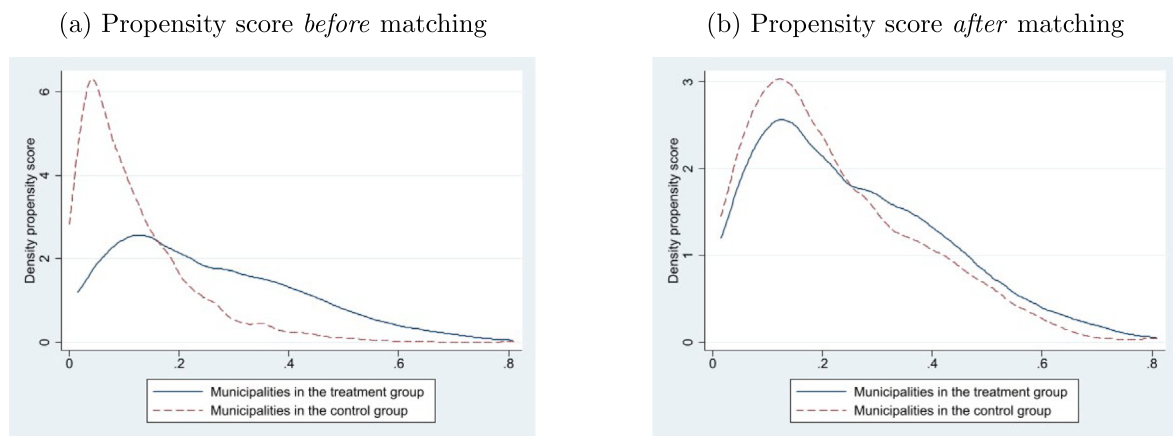


Fig. E.12. Propensity score graphs. *Note.* Overlap in propensity scores in treated and matched samples of municipalities before and after the propensity score matching.

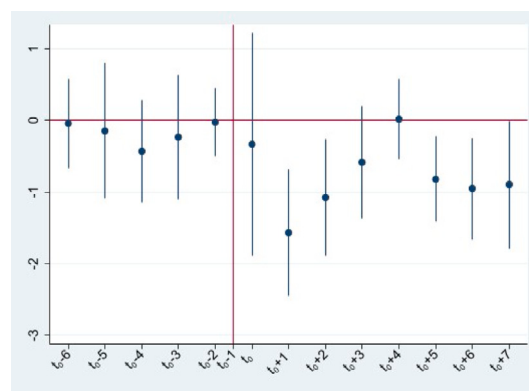


Fig. E.13. Propensity score matching — Event study. *Notes.* The graph reports coefficients and confidence intervals estimated according to Eq. (1) by Poisson FE model. The dependent variable is the total number of attacks against politicians. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. We include event-time dummy variables for 6 years before and 7 years after the dissolution. We control for municipality FE, year FE, the log of resident population, *Mayor degree*, *Councilors degree* and *Aldermen degree*. Period: 2010–2019.

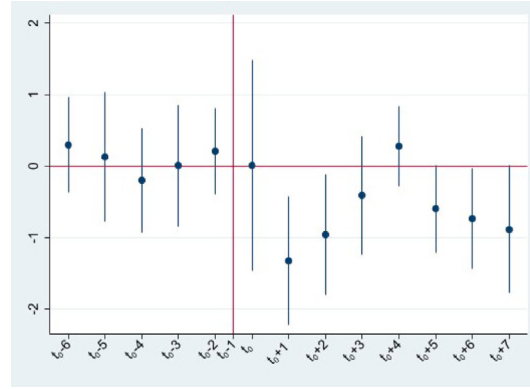


Fig. E.14. Poisson RE — Event study. *Notes.* The graph reports coefficients and confidence intervals estimated according to Eq. (1) by Poisson RE model. The dependent variable is the total number of attacks against politicians. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. We include event-time dummy variables for 6 years before and 7 years after the dissolution. We control for year FE, the log of resident population, the level of education and unemployment of the municipalities that proxy for fixed effect given that they are constant over 2010–2019 (census data). Period: 2010–2019.

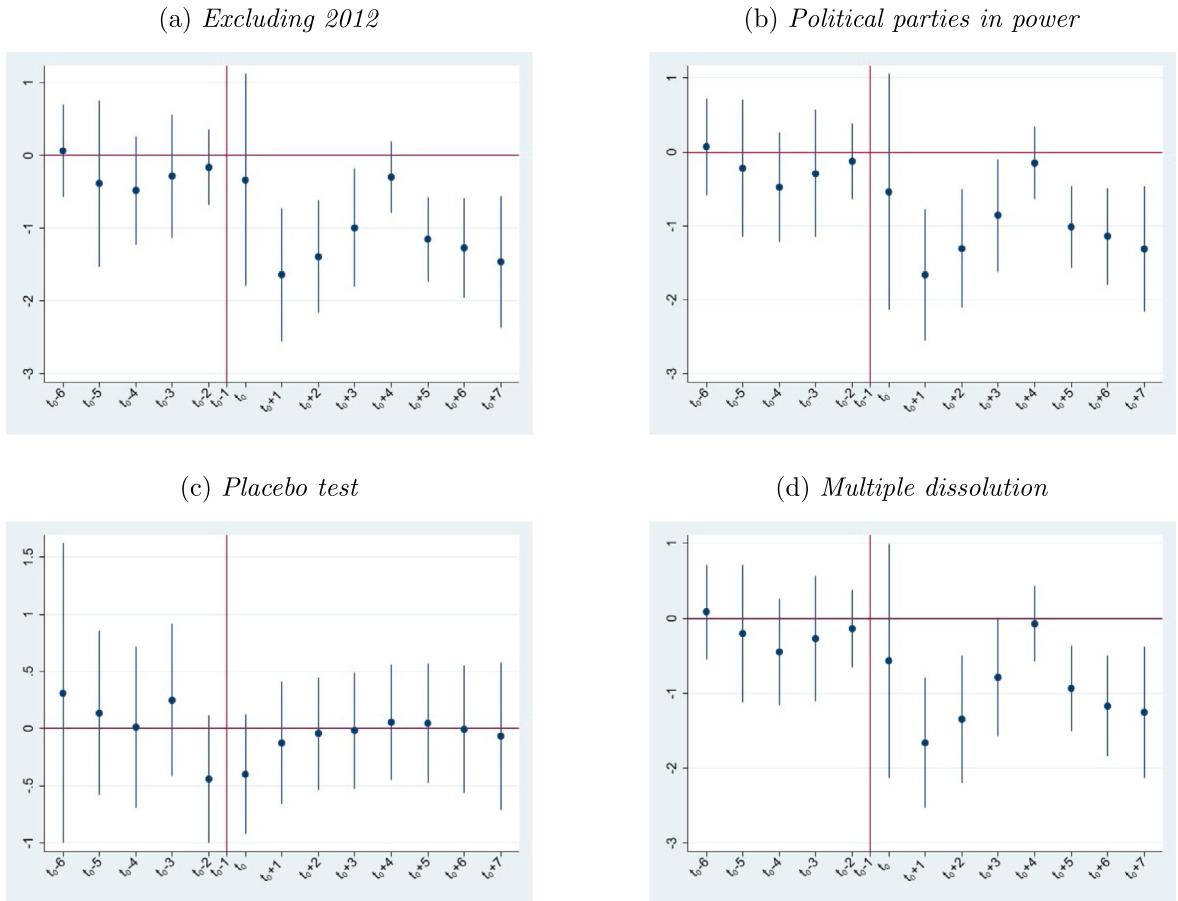


Fig. E.15. Robustness — Event study. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1), Poisson FE model estimates, dependent variable is total number attacks against local politicians. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). The event dummy t_0 is the fraction of the year that is treated. We include event-time dummy variables for 6 years before and 7 years after the dissolution. We control for municipality FE, year FE, the log of resident population. Standard errors are clustered at municipal level. Period: 2010–2019. In Graph E.15(a) we exclude 2012. In Graph E.15(d) we drop municipalities dissolved for mafia infiltration more than once. In Graph E.15(b) we control for dummies for mayor's party affiliation. In Graph E.15(c) we drop all the municipalities dissolved because of mafia infiltration.

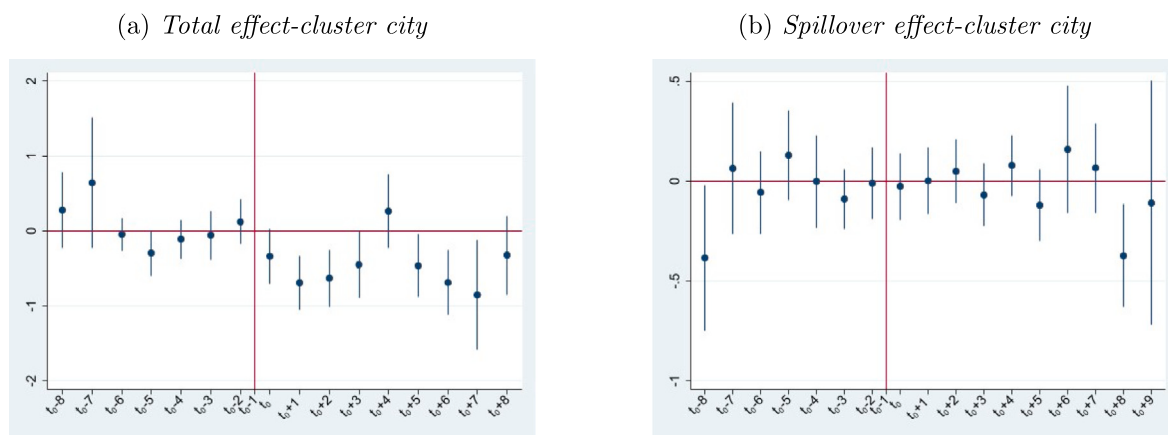


Fig. E.16. Total and spillover effects. *Note.* The graphs report coefficients and confidence intervals estimated according to Eq. (1) using the *did2s* Stata command. The dependent variable is the total number attacks against local politicians. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 90% confidence intervals. The omitted category is the year before the dissolution ($t_0 - 1$). We include event-time dummy variables for 8 years before and 9 years after the dissolution; in Graph E.16(a) the event dummy at $t_0 + 9$ has been dropped because of collinearity. In all regressions we control for municipality FE and year FE and the resident population. Period: 2010–2019.

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