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Anna Laura Baraldi, Erasmo Papagni, Marco Stimolo

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By **Anna Laura Baraldi** (Department of Economics, University of Campania)
Erasmo Papagni (Department of Law, University of Naples Federico II)
Marco Stimolo (Department of Economics, University of Campania)

Summary

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JEL Classification: C25, D73, D78, I38, K42

Keywords: Organized Crime, Violence, Anti-corruption measures, Spillovers

***Address for correspondence*:**
Marco Stimolo
Department of Economics, University of Campania "L. Vanvitelli"
C.so Gran Priorato di Malta
81043 - Capua (Caserta), Italy
email: marco.stimolo@unicampania.it

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Anna Laura Baraldi† Erasmo Papagni‡ Marco Stimolo §

Abstract

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†Department of Economics, University of Campania L. Vanvitelli, C.so Gran Priorato di Malta 81043 Capua, Italy. E-mail: annalaura.baraldi@unicampania.it

‡Department of Law, University of Naples Federico II, C.so Umberto I 40, 80138 Napoli, Italy. E-mail: papagni@unina.it.

§Department of Economics, University of Campania L. Vanvitelli, C.so Gran Priorato di Malta 81043 Capua, Italy. E-mail: marco.stimolo@unicampania.it

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 (see for instance 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 in the post-electoral period, in view of the potential "moral hazard" inherent in politicians' bargaining with clans (Daniele and Dipoppa, 2017; Daniele, 2019; Dell, 2015), and also before elections, in order to redirect votes to the candidate they back or discourage qualified adversary candidates from running Olivieri and Sberna, 2014 ; Alesina et al., 2019; Acemoglu et al., 2020. This pervasive phenomenon shows the need for empirical assessment of the efficacy of government policies in safeguarding public security.

Dal Bó et al. (2006) sets out one of the most influential theoretical frameworks for studying how pressure groups (in our case, criminal organizations) affect politics by optimally choosing the mix of bribes and threats of violent punishment that determines the level of "state capture". These actions have costs, which depend on institutional factors, including the quality of law enforcement. Stepped-up law enforcement increases the costs of corruption and strengthens the "entry barriers" to the "business" of influencing politics, thus modifying the optimal mix of bribery and violence from the mafia's viewpoint.

More specifically, a measure that increases the cost of corruption (the size of bribes), *but not* that of violence (by means of an expansion of police forces) has an ambivalent effect on organized criminal violence. On the one hand, the upward shock to bribes may induce criminal clans to discard them in favor of violent punishment, thus raising the level of violence. This can be seen as a sort of retaliation for the measure increasing the cost of bribes. At the same time, however, this kind of anti-corruption measure is likely to toughen the entry barriers to political influence, so that some criminal groups might actually refrain from their illicit activities and go "out of business", thereby lowering the level of violent intimidation and the overall degree of "state capture". Which of these effects prevails is ultimately an empirical question; to answer it is the aim of this paper.

The Italian institutional framework is suitable for this purpose. To counter the political influence of criminal organizations, in 1991 Parliament passed Decree Law 164/1991 (converted with amendments as Law 221 of 22 July 1991, but still known as Law 164). Law 164 prescribes, where there is evidence of organized criminal infiltration of a municipality, the dismissal of the elected officials and the institution of "compulsory administration" by three external commissioners designated by the central government. The application of this measure can be interpreted as an unexpected shock to the cost of corruption for local clans, in that the commissioners will 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). But this is an exclusively administrative act, entailing neither an expansion of police forces nor increased allocations for public security during the compulsory administration (Mete, 2009; Cavaliere, 2004).

In this regard, a relevant strand of literature has exploited the shocks to the cost of corruption produced by the enforcement of Law 164 to examine how organized crime affects the quality of politicians (Daniele and Geys, 2015; Baraldi et al., 2021; Baraldi et al., 2022) as well as economic performance and the allocation of public resources in the dissolved municipalities (Fenizia and Saggio, 2022; Di Cataldo and Mastrorocco, 2021).

In the light of these studies, here we estimate the causal effect (unexplored to date) of the enforcement of Law 164 on the exercise of violent intimidation by organized crime. We adopt a difference-in-differences method, comparing the change in number of attacks against local politicians in dissolved and undissolved municipalities, before and after the dissolution. Our empirical analysis is based on a dataset provided by "Avviso Pubblico", an NGO that records the attacks against local politicians carried out from 2010 to 2019 in the regions of Calabria, Campania, Puglia and Sicily, where practically all the city council dissolutions occurred.

The estimation results show a considerably sharper reduction, after the period of compulsory administration, in the number of attacks against politicians in the treatment group of dissolved municipalities compared with the undissolved control group. The results are robust to linear and Poisson FE specifications. The dynamic of the attacks under our preferred Poisson FE specification shows a pre-treatment coefficient that is 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: 77% at the first election and 65% at the second, with an overall mean effect (over the entire period after compulsory administration) of 50%. These results are confirmed by several robustness checks.

To assess the efficacy of this measure in a broader context, we also considered possible spillover effects on undissolved municipalities in the neighborhood of those dissolved. We find in fact that the measure had beneficial effects (i.e. a reduction in the number of mafia attacks) also in those neighboring municipalities; accordingly, we corrected our initial estimates for the bias generated by the presence of spillovers and modified the regression specification to comply with the Stable Unit Treatment Value Assumption (SUTVA).

Referring to 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 toughening the barriers to entry into the business of political influence, reducing the violent activity of criminal organizations and the overall degree of "state capture".

The main challenge for our diff-in-diff analysis is to validate the key identification assumption, namely the strict exogeneity of the treatment to the outcome, given that in principle city council dissolution itself might well be due to evidence of violence against politicians. Accordingly, we first focus on possible reverse causality, checking that the number of attacks does not Granger-cause city council dismissal; second, we exclude the municipalities dissolved *because of* violent attacks against politicians from the treatment group. We then address omitted-variable bias, applying a propensity score matching method and controlling for the repressive action of the central government through the presence of law enforcement

personnel. Finally, we adopt an instrumental variable (IV) approach by exploiting the personal characteristics of the Prefect (*Prefetto*), the provincial representative of the central government in charge of the procedure for the dismissal of the infiltrated municipality.

Our tests confirm the exogeneity of dissolution for mafia infiltration with respect to the number of attacks against local politicians. Interestingly, the IV analysis in the first-stage regression shows a significant correlation between the age of the Prefect and the probability of city council dissolution. Possibly, more experienced officers are better at picking up the warning signs of organized criminal infiltration. Using this latter instrument in a control function approach, we run a test to confirm the exogeneity of the policy measure studied here.

This study contributes to the abundant literature on the effects of Law 164/1991, producing novel empirical evidence that its enforcement reduces mafia violence against politicians. Our results thus complement and extend other recent work that documents the effectiveness of city council dissolution in favoring better long-run local economic performance (Fenizia and Saggio, 2022), fostering women's political empowerment (Baraldi and Ronza, 2022), enhancing the quality of elected politicians (Daniele and Geys, 2015), and improving tax collection and the allocation of public funds (Di Cataldo and Mastrorocco, 2021). Moreover, since some studies have shown that repressive policies bring about a violent reprisal from criminal organizations (e.g., Calderón et al., 2015; Castillo and Kronick, 2020), our analysis contributes to the literature studying the effectiveness of non-repressive government measures in fighting organized crime activities (Daniele and Dipoppa, 2022).

The rest of the paper is organized as follows. Section 2 sets out the relevant features of our theoretical framework. Section 3 describes the institutional framework to show that Law 164/1991 can be taken as a legitimate treatment and, further, presents the data and the variables used in the empirical analysis. Section 4 describes the empirical design and discusses selection bias as a possible threat to our identification strategy. Section 5 reports the results of our baseline analysis. Section 6 details the econometric analyses we use to address concerns over treatment endogeneity, and Section 7 deals with the IV approach. Section 8 reports robustness checks. Section 9 concludes.

2 Conceptual framework

In this Section 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 (π), which can be re-directed to the pressure group as a lump-sum transfer. 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, but where pressure groups are active this will be supplemented by bribes (b) and reduced by threats of punishment (p) and the costs of corruption c (e.g. moral costs or the risk of detection). Accordingly, active pressure groups will set the optimal bribe offer equal to the difference between the cost of corruption to 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: $Max_{b,p}\,\Pi(b,p) = \gamma[\pi - \beta\Phi(b)] - (1-\gamma)\rho\Psi(p)$, 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; $\beta\Phi(b)$ and $\rho\Psi(p)$ respectively are the costs of delivering a bribe offer and carrying out a punishment. The parameters β and ρ capture institutional features (i.e law enforcement) that affect the costs of corruption and punishment.

When the politician accepts the bribe, the pressure group obtains $\pi - \beta$ (i.e. the fraction of public resources managed by the politician net of the costs). Otherwise, the pressure group will bear the cost ρ 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:

$$\bar{\pi} = \frac{1-\gamma}{\gamma}\rho\Psi(p^*) + \beta\Phi(c - p^*) \tag{1}$$

$\bar{\pi}$ is an inverse measure of "state capture", i.e. the degree of pressure groups' influence on politics; thus, the greater $\bar{\pi}$, the higher 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" 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 a bribe (an increase in β) 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 pressure group 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). Which effect prevails is ultimately an empirical question; and this is what our analysis investigates.

City council dissolution by enforcement of Law 164/1991 delivers an exogenous shock to the cost to pressure groups of making a bribe. 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.

In the logic of Dal Bó et al. (2006), to the extent that city council dismissal increases

the cost of corruption, it can curb the overall use of violent intimidation only if it induces enough 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.

3 Institutional background and data

3.1 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.[1] 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.

This investigation is kept secret until completed, or for a maximum of three months. That is, the conduct of local administrators and criminal organizations cannot be influenced by the procedure while it is under way. Once the investigation is concluded, the report of the *Commissione d'Accesso* will form the basis of the Prefect's proposal for city council dissolution, which is submitted to the Ministry of the Interior for final decision. The outcome of the procedure, in other words, 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 to 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

[1] This is the reason for controlling for repressive action by government (Section 6.2.2).

interests between politicians and criminal organizations.

For the purposes of our study, the intimidation of local politicians by organized crime — 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 (violent) intimidation 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, we do not believe that this imperfection seriously compromises our analysis. The relationship between the number of attacks and the probability of dissolution is likely to be positive, while it is reasonable to expect a negative sign for the inverse correlation between council dissolution and *subsequent* attacks on politicians. This means that the self-selection might bias the estimates of the causal link between dissolution and number of attacks towards zero. In other words, our baseline analysis would indicate the lower bound of the downward effect of city government dismissals on criminal attacks on local politicians.

Moreover, violent intimidation certainly cannot be taken as unequivocal evidence of collusion with local clans like the above-mentioned 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. 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 behaviour. For example, the municipalities of Bagnara Calabra 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.

On the empirical level, this means that there should be no correlation between the regional distribution of the dissolutions (concentrated in the South) and that of per capita attacks on politicians (widespread throughout Italy). We test this hypothesis in Section 3.2. For more thorough statistical analysis of this issue, we conduct a number of endogeneity tests (Sections 6 and 7).

3.2 Data

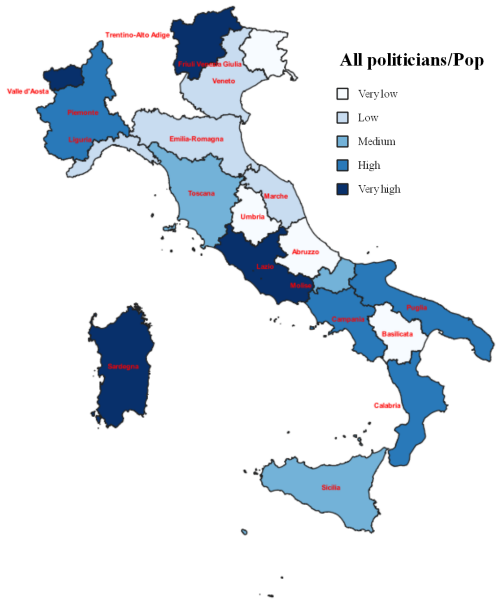
We construct a dataset of threats and violence against local politicians in Italy from 2010 to 2019. Official sources of data about attacks against politicians are not available; like Daniele and Dipoppa (2017), we use the yearly reports of "Avviso Pubblico", a non-governmental organization that collects daily media reports of threats and attacks directed against Italian politicians. The violent actions are sorted by type of attack and type of politician, in each of the municipalities where they occur.

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

"Avviso Pubblico" distinguishes 20 types of attack[3] and 11 types of politician targeted for attack.[4]

According to this source, Italy counted 4448 attacks against politicians from 2010 to 2019. Considering only city council politicians (deputy mayor, former mayor, relative of politician, candidate, alderman, city councillor, city or municipal company manager, mayor) the total is 3857. Figure 1 shows the geographical distribution of the attacks by region. For each region we calculate the mean number of attacks (in the period 2010-2019), reported in the first column of Table A.1. This mean is divided by the average regional population in the same period to produce the average number of attacks per capita, shown in Column 2 of Table A.1 and in Figure 1, which groups regions in classes from very low to very high (see the note on Figure 1 for details).

Figure 1: Geographical distribution of attacks against politicians



Notes. The figure shows the regional averages of attacks against local politicians per capita. Regions are grouped according to the following range of values: Very Low: 0.0006357-0.0019019; Low: 0.0021064-0.0022924; Medium: 0.0027108-0.0022926; High: 0.0031238-0.0037061; Very High: 0.0045485-0.0157233.

The per capita measure allows a more meaningful comparison of the intensity of the attacks between regions of differing population. Figure 1 shows that attacks against politicians are widespread throughout the country. That is, organized crime is now active well beyond the

[3] 1) Setting car on fire, 2) Setting City Hall or municipal property on fire, 3) Threatening letter, 4) Threatening letter containing bullets, 5) Verbal or telephone threats, 6) Physical assault, 7) Setting home on fire, 8) Shootings against City Hall, 9) Homicide, 10) Threatening messages on the family tomb, 11) Killing of domestic animals, 12) Delivery of an animal head in a box, 13) Shootings against car, 14) Leaving dead animals or their parts in front of home, 15) Damage or robbery in City Hall, 16) Felling of trees on private property, 17) Physical assault in public places, 18) Bullets in front of home or City Hall, 19) Threatening messages on walls of home or City Hall, 20) Bombing of home or City Hall.

[4] 1) Regional councillor, 2) Regional president, 3) Deputy mayor, 4) Former mayor, 5) Relative of politician, 6) Candidate, 7) Alderman, 8) City councillor, 9) City or municipal company managers, 10) Mayor, 11) Representative of other institutions or entities.

southern regions where it originated. For instance, northern regions like Valle D'Aosta have become a strategic territory for the business of the *'ndrangheta*, a criminal organization that originated in Calabria.

Table 1 shows the distribution of city council dismissals by region: from 2010 to 2019 they totalled 120. The regions of Campania, Calabria, Sicily and Puglia count 115 dismissals (in 108 municipalities),[5] or 95.8% of the total. For the greatest possible homogeneity in the sample of municipalities (which helps to reduce estimation bias due to unobservables), the analysis is limited to these four regions.

Table 1: City council dismissal by regions

Region	City council dismissals
Calabria	59
Campania	21
Sicily	25
Puglia	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.

As noted, the possible reverse causality between the number of attacks and dissolution for mafia infiltration is a serious concern. The issue of endogeneity will be treated extensively in the next sections, but we begin to address this concern here with a t-test comparing the per capita mean of attacks in Calabria, Campania, Puglia and Sicily (group 1) and in the rest of the country (group 2). The results (in Table 2) show that there is no statistical difference between the means of the two sets of regions. This suggests that while city council dissolutions for mafia are concentrated in Calabria, Campania, Puglia and Sicily, no such concentration is found for attacks on politicians, which precludes correlation between the two variables.

Table 2: t-test

Group	All politicians/Pop	City council politicians/Pop
1	0.0053	0.0038
2	0.0033	0.0025
Difference (p-value)	0.0019 (0.61)	0.0012 (0.52)

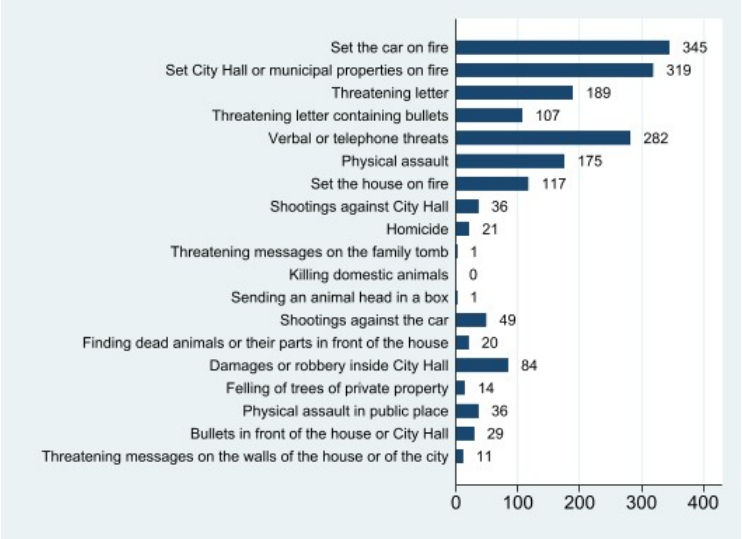
Note. The table displays the mean difference test (t-test) between regions in group 1 and 2 of the mean of per capita attacks on politicians. Group 1 comprises Calabria, Campania, Puglia and Sicily, group 2 the rest of the Italian regions. P-value is in brackets. The mean is calculated over regions and time (2010-2019) within each group.

3.3 Variables

The dependent variable in this empirical analysis is the number of attacks against local politicians as reported by "Avviso Pubblico". Figures 2 and 3 sort the data by type of attack and type of politician in our four regions.

[5] 7 municipalities in our database were dissolved more than once.

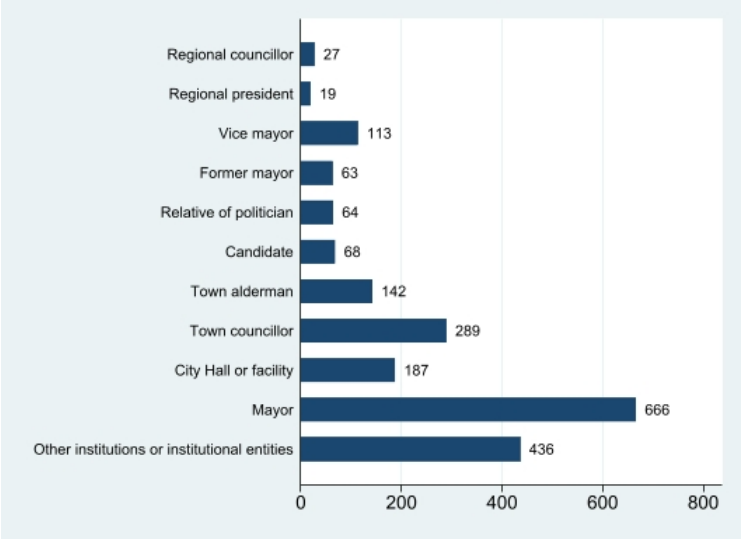
Figure 2: Types of attacks. 2010-2019



Notes. The Figure shows the types of attack against politicians. 2010-2019.

From 2010 to 2019, the most common types of attack were arson targeted at cars (345) or the City Hall or other municipal properties (319), followed by verbal or telephone threats (282). A non-negligible number of cases also involved threatening letters (189 + 107). Physical assaults and arson against politicians' homes too were reported in a substantial number of cases (respectively 175 and 117). Other types of attack are not so frequent; the worst, homicide, registered 21 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 as well as in that of the more visible attacks.

Figure 3: Victims of attack. 2010-2019



Notes. The Figure shows the types of victim of mafia attacks. 2010-2019.

Figure 3 shows 666 attacks against mayors (almost 32% of the total). City councillors, aldermen and deputy mayors display lower risk. The last category in the figure, namely policemen and managers in city government or other public facilities, counts about 21% of

the attacks. Attacks against regional presidents and councillors 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.

Figure B.1 in Appendix B shows the trend in the mean number of attacks over the years by type of attack (graph B.1a) and type of politician (graph B.1b). Graph B.1a merges the types of attack into two categories: *Threats* and *Violence* (see note to Figure B.1 for details). The number of *Threats* appears relatively constant over time, while actual *Violence* peaked in 2016, and with it the total number of attacks.

Graph B.1b merges the types of victim into *City council politicians* and *Other politicians* (see note to Figure B.1 for details). Clearly, the prime targets of mafia attacks are *City council politicians*. Both graph B.1a and graph B.1b in Figure B.1 show a downward spike of the attacks in 2012. As the National Coordinator of "Avviso Pubblico" explains, however, this is deceptive. The low value 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 exclude the year 2012 from the analysis.

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), which corroborates the hypothesis that the attacks are perpetrated by organized crime rather than private citizens. Nevertheless, the estimates of the linear models of attacks against politicians are robust to (consistent with) this kind of measurement error in the dependent variable.

Our regressor is the application of Law 164/1991 to dissolve a city council upon evidence of the infiltration of local politics by organized crime. Figure B.2 in Appendix B shows the number of dissolutions for mafia by year (graph B.2a) and by province (graph B.2b).

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 take this into account in the empirical analysis.

4 Empirical design

We create a yearly panel of all the municipalities in Calabria, Campania, Puglia and Sicily from 2010 to 2019. This panel structure, together with the exogeneity of the shock for organized crime due to the application Law 164/1991, makes it possible to isolate the ef-

fect of the measure from any time-specific features, such as long-term trends and seasonal patterns. Further, the municipal-level fixed effects account for possible local factors. We take a difference-in-differences approach. We record the number of attacks before and after city council dissolutions 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 for election years):

$$Y_{it} = \sum_{t=-n}^{+n} \nu_t \cdot D_t + \alpha_i + \delta_t + Pop_{it} + \epsilon_{it} \tag{2}$$

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. The omitted category, D_0, is the year of the dissolution; the remaining ν_t coefficients measure the effects in the period before and after the application of Law 164 (t_0). In all the estimations we control for 1) municipality fixed effects (α_i); 2) time fixed effects (δ_t); 3) municipal population (Pop_{it}). ϵ_{it} is the idiosyncratic error term.

Since the dependent variable is the occurrence of an attack, the count of events has a large portion of 0 and a skewed distribution as shown in Table A.3 (Appendix A). Consequently we specify the outcome variable in two ways: 1) as a dichotomous variable equal to 1 if an attack takes place in municipality i in year t and 0 otherwise; 2) as the total number of attacks in municipality i in year t. In the first case, we estimate eq. 2 by a linear probability model; in the second case, we use a Poisson model with fixed effects and standard errors clustered at the municipality level, which is more suitable for count data. Clustered standard errors are robust to heteroscedasticity and serial correlation, and they allow for consistent inference in a diff-in-diff framework (Bertrand et al., 2004).

Assuming the exogeneity of the explanatory variables, 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 fixed effects control for heterogeneity in the cross-section dimension and account for unobserved time-invariant factors that could engender omitted-variable bias. Time fixed effects account for unobserved year-specific events that affect all municipalities. In Poisson FE estimations, the coefficients are interpreted as an elasticity, according to the following expression: $Exp(coef) - 1$ (Cameron and Trivedi, 2013).

The validity of the empirical design requires satisfaction of the common trend assumptions, which can be tested by analysis of the pre-treatment dummies of the event study model in eq. 2 (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 the following specification:

$$Y_{it} = \beta_1 Policy_{it} + \alpha_i + \delta_t + Pop_{it} + \gamma' X_{it} + \epsilon_{it} \tag{3}$$

where Y_{it} is, as above, the number of attacks in municipality i in year t. The regressor 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. β_1 in eq. 3 measures the average treatment effect on the number of attacks against politicians. 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.

For our analysis it is important to control for the characteristics of the politicians in office, which we specify in the vector of control variables (X_{it}). These are a dummy for university degree of the mayor (*Mayor degree*) taking the value of 1 if the mayor has at least a university degree and 0 otherwise; a dummy taking value 1 if at least one city councillor has a university degree and 0 otherwise (*Councillors degree*); a dummy taking value 1 if at least one alderman in the executive board has a university degree and 0 otherwise (*Aldermen degree*); and the number of female councillors (*Female councilors*) and aldermen (*Female aldermen*).

It is also important to control for municipal context variables such as the average educational attainment of women and men (*Female education* and *Male education*) and the female and the male unemployment rates (*Female unemployment* and *Male unemployment*).[6] The inclusion of control variables offers an indication of the relevance of omitted-variable bias: where these additional controls have only marginal impact on the treatment coefficient (*Policy*), a causal interpretation of the results is justified. Moreover, the possible endogeneity of government dissolution with respect to the number of attacks is controlled for by covariates as well as municipality and time fixed effects.

4.1 Selection bias

A number of selection bias issues may arise. First, the regular term in office of the mayor and the council is five years;[7] 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. The large number of possible reasons for early termination of a municipal council could make it more likely for an infiltrated municipality to go undetected and therefore sorted into the control rather than the treatment group. We address this type of selection bias in the main analysis by excluding all councils terminated early from the control group.

Second, as is observed in the previous Section 3.2, 7 municipalities were dissolved more than once. To avoid post-treatment bias, in Section 5 we control for this by excluding them.

Third, no municipalities in the provinces of Avellino, Barletta-Andria-Trani, Benevento or Enna were dissolved (graph B.2b in Figure B.2, Appendix B). Municipalities within a province are likely to be similarly affected by shocks to provincial GDP or to the provincial government.[8] Finally, if criminal organizations are not the sole perpetrators of intimidation

[6] These data are taken from the censuses of 2001 and 2011.

[7] Until 2000, shortened to four years by Law 81/1993, art.2; restored to five by Legislative Decree 267/2000, art.51).

[8] Acconcia et al. (2014) show that provinces in which at least one city council was dissolved for mafia

and attacks, the impact of the dissolution measure should be smaller where mafias are less active. In order to control for all these aspects, in Section 5, we exclude these provinces from the analysis.

Fourth, the "Avviso Pubblico" data on threats and attacks against politicians are drawn from media reports. That is, our dependent variable captures only the attacks that are denounced by politicians, so possible bias due to media under-reporting could be a concern. However, as Daniele and Dipoppa (2017) note, this is not a serious danger: as in most cities attacks against politicians are uncommon, we can assume that when they occur, they generally do get reported. We address this issue in Subsection 8.2 by performing estimations on the restricted sample of the most sensational attacks — those that can be seen by people other than the victim and, thus, are unlikely to be hidden.[9]

5 The results

We first estimate eq. 2 and calculate the trajectory of the attacks on politicians year-by-year before and after the implementation of the anti-mafia dissolution measure. In the baseline specification, the dependent variable is the total number of attacks against all the categories of politician reported by "Avviso Pubblico", in each municipality and in each year from 2010 to 2019.

The results are displayed in Figure 4 and Table 3 (to save space, the latter shows only the significant coefficients). Table 3 shows (linear) panel FE estimations where the dependent variables are a dummy for the probability of an attack (Column 1) and the total number of attacks (Column 2). Columns 3 and 4 estimate a Poisson FE model.[10] All regressions present robust standard errors, which are clustered at municipal level,[11] except for the estimations shown in Column 4, clustered at province level.

The graphs from 4a to 4d in Figure 4 depict the results reported from Columns 1 to 4 in Table 3. Overall, the coefficients for the pre-treatment period are not significantly different from zero, which corroborates the common trend assumption. In all the graphs the coefficient immediately after the dissolution (corresponding to the period of compulsory administration) is negative but not statistically different from zero. The downward effect of the measure first appears at $t_0 + 2$, i.e. the year in which compulsory administration ends and a new local administration is elected.

In Column 1 of Table 3, at $t_0 + 2$ we observe a reduction of 10.8 percentage points (p.p.) in the probability of an attack in the treatment relative to the control municipalities. This differential reduction becomes more pronounced at $t_0 + 3$, when it peaks at 13.1 p.p., before the dependent variable turns back upward. Considering the total number of attacks (Column 2), the linear model shows decreases of 38.1 and 32.6 p.p. in the treated units in the periods

infiltration reduced their total investment in infrastructure by more than those with none.

[9] This test excludes all threatening letters and menaces and includes, for example, arson against City Halls, shootings at politicians' homes, bombings and homicides.

[10] The substantial reduction in the number of observations is due to Poisson log-likelihood maximization, which in a model with fixed effects excludes the units that always have zero values of the dependent variable for the entire period.

[11] To compute robust standard errors clustered at the municipal level in Poisson FE model we use the Stata command *xtpqml*.

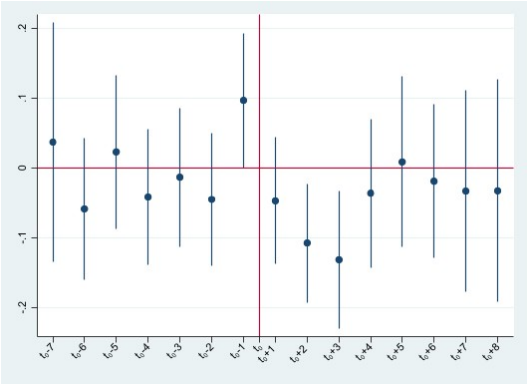
$t_0 + 2$ and $t_0 + 3$, respectively. The downward effect of the measure is observed again at $t_0 + 7$, in the run-up to the second election after compulsory administration, with a negative and significant coefficient of about 31 p.p.

Our preferred Poisson FE estimation shows a definite long-lasting effect of the dissolution measure on mafia attacks. In Column 3 of Table 4, the strongest effect of the measure appears at $t_0 + 2$ with a decrease of 76.7% ($e^{-1.459}$ - 1) in the number of attacks after compulsory administration compared to the control group of never-dissolved municipalities. The effect of the measure is increasing, though not statistically significant, up to $t_0 + 5$. When new elections are held between $t_0 + 6$ and $t_0 + 7$, the attacks still decrease more in the treatment than in the control group; the magnitude of the estimated effect in that period is smaller, with decreases of 46% (at $t_0 + 6$) and 65% (at $t_0 + 7$).

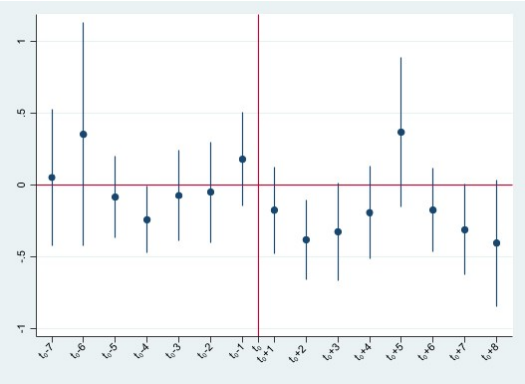
These results are in line with the evidence provided by Daniele and Dipoppa (2017) and Alesina et al. (2019) that in areas with a strong presence of organized crime the strategic use of violence against local politicians is concentrated in the weeks around the elections. Our findings thus indicate that the enforcement of Law 164/1991 is effective in discouraging intimidation of politicians exactly when this is of the greatest strategic importance to organized crime in its drive to influence politics.

Figure 4: Event study graphs

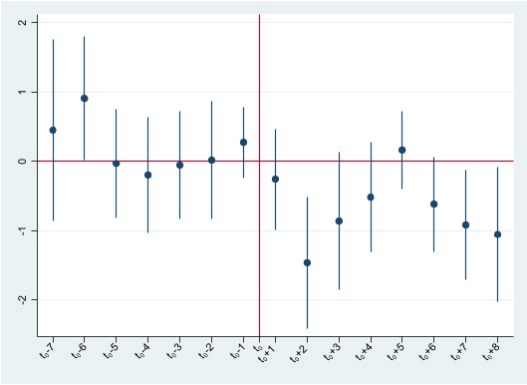
(a) *Linear (0/1) estimates*



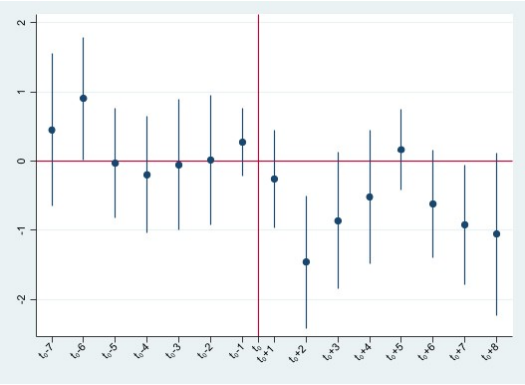
(b) *Linear estimates*



(c) *Poisson estimates*



(d) *Poisson estimates*



Note. The graphs report coefficients and confidence intervals estimated according to eq. 2. Graph 4a shows the linear probability model estimates where the dependent variable is the probability of being a target of violence. Graph 4b shows the linear probability model estimates where the dependent variable is the total number attacks. Graphs 4c and 4d show the Poisson FE model estimates where the dependent variable is the total number attacks. Standard errors are clustered at municipal level except for the estimations in graph 4d, where they are clustered at provincial level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 8 years after the dissolution. All regressions include municipality FE, year FE and the resident population. Period: 2010-2019.

The effect is long-lasting (for at least the first two council terms after compulsory administration), as shown by the u-shaped curve of the attacks, which means that the average impact of the policy can be examined by estimating eq. 3; the results are displayed in Columns 5 - 8, Table 3 (the four empirical specifications reproduce those of Columns 1 - 4.).

Table 3: Baseline estimates

	Linear estimation		Poisson FE estimation		Linear estimation		Poisson FE estimation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	Attacks(0/1)	No. Attacks	No. Attacks	No. Attacks	Attacks(0/1)	No. Attacks	No. Attacks	No. Attacks
$t_0 + 2$	-0.108**	-0.381***	-1.459***	-1.459***				
	(0.0435)	(0.141)	(0.485)	(0.489)				
$t_0 + 3$	-0.131***	-0.326*	-0.862*	-0.862*				
	(0.0503)	(0.173)	(0.506)	(0.505)				
$t_0 + 6$	-0.0195	-0.176	-0.618*	-0.618				
	(0.0561)	(0.149)	(0.349)	(0.395)				
$t_0 + 7$	-0.0334	-0.312*	-0.918**	-0.918**				
	(0.0736)	(0.161)	(0.405)	(0.441)				
$t_0 + 8$	-0.0327	-0.406*	-1.052**	-1.052*				
	(0.0814)	(0.225)	(0.496)	(0.599)				
Policy					-0.0747***	-0.243**	-0.700***	-0.700***
					(0.0264)	(0.0972)	(0.201)	(0.266)
Pop	0.0118**	0.123***	0.00985	0.00985	0.0120**	0.0902*	0.0106	0.0106
	(0.00478)	(0.0375)	(0.00992)	(0.0103)	(0.00486)	(0.0478)	(0.0102)	(0.0104)
Observations	16,002	16,002	5,731	5,731	16,002	16,002	5,731	5,731
R-squared	0.015	0.019			0.012	0.014		
No. municipalities	1,606	1,606	575	575	1,606	1,606	575	575
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table reports coefficients estimated according to eq. 2 (Columns 1 - 4) and eq. 3 (Columns 5 - 8). Columns 1 and 5 show the linear probability model estimates where the dependent variable is the probability of being a target of violence; in Columns 2 and 6 the dependent variable is the total number attacks. Columns 3, 4, 7, and 8 show the Poisson FE model estimates where the dependent variable is the total number attacks. Standard errors are clustered at municipal level except for the estimations in Columns 4 and 8 where they are clustered at provincial 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 2 we include event-time dummy variables for 7 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). All regressions include municipality FE and year FE (coefficients not reported). Standard errors are in brackets. The number of observations in the Poisson FE model as in Columns 3, 4, 7, and 8 is reduced to 5,731 because 1,033 groups (10,291 obs) were dropped, having all zero outcomes. Period: 2010-2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

One sees at a glance that 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. As to the probability of an attack, the enforcement of the Law lowers the probability of an attack by 7.47 p.p. (Column 5). The linear probability model estimated in Column 6 confirms the negative sign of *Policy*, with a magnitude of about 25 p.p. The Poisson coefficients suggest that the measure diminishes the occurrence of attacks by 50% (Columns 7 and 8). The significance of the main coefficient remains strong (at 1%) when we cluster the standard errors at province level (Column 8) rather than municipal level (Column 7).

Poisson FE estimates serve to control for any omitted time-invariant variables, but the algorithm of likelihood maximization drops the many municipalities that have zero outcomes in every year. However, this phenomenon does not occur when we perform the Poisson RE estimations (Table A.4 in Appendix A). These regression models include the cross-section level of education and the unemployment rate. The main coefficient remains negative and significant at conventional levels with an average effect of 37% (Column 1). It is robust to the inclusion of province FE, and significance increases when controls are included (Column 3). The estimates indicate no significant impact of the female and male unemployment rates; however, the female education level has a positive and significant impact on the number of attacks, while the male education level has a negative sign.

The downward effect of city council dissolution on violence against local politicians raises the issue of the trade-off posited in the model of Dal Bó et al. (2006). It offers support for the hypothesis that in the Italian situation the application of a measure that increases the cost of bribery — Law 164 — discourages criminal groups from seeking to influence politics and thereby lowers the overall level of violence.

As noted in Subsection 3.3, the downward spike in attacks depicted in 2012 was due

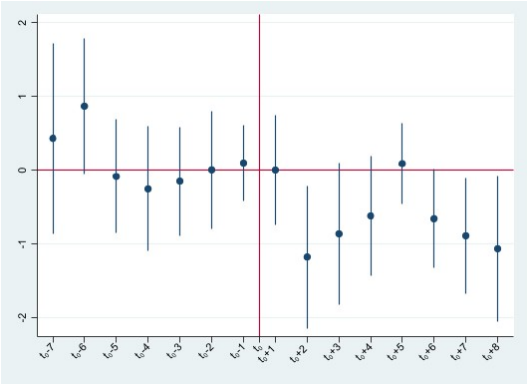
to a lack of data collection by "Avviso Pubblico". Accordingly, 2012 is excluded from the analysis, but the results are unchanged (see Table A.5 and Figure B.3).

Moreover, in Section 4 we underscored the importance of controlling for the characteristics of local politicians, in particular education and gender. The theoretical model of Dal Bó et al. (2006) and its empirical testing by Daniele and Geys (2015) show how organized crime negatively affects the quality of politicians, as proxied by education level. Another important strand of literature documents how gender matters for the level of political corruption (Beaman et al., 2009, Brollo and Troiano, 2016, Dollar et al., 2001, Jha and Sarangi, 2018, Swamy et al., 2001). Accordingly, we re-estimate eq. 2, now adding the foregoing control variables: *Mayor degree*, the *Councillors degree*, the *Aldermen degree*, *No. Female councillors*, and *No. Female aldermen*.

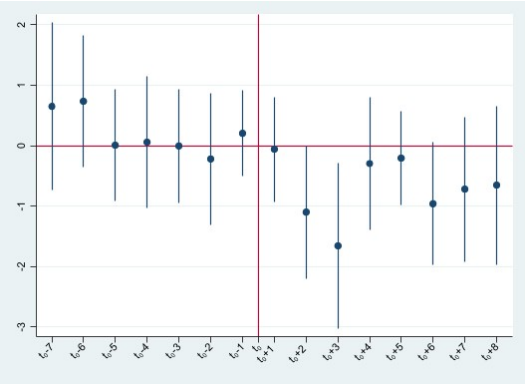
The Poisson FE estimation results, reported in graph 5a in Figure 5 and in Column 1 of Table 4, show that city council dissolution reduces acts of violence by mafias sharply in the period around the election of the first new government after compulsory administration (between $t_0 + 2$ and $t_0 + 3$) and around the next election (between $t_0 + 6$ and $t_0 + 7$). Importantly, controlling for politicians' characteristics does not alter the dynamics of the dependent variable. Only the dummies for the mayor's education and for the number of female aldermen appear to have a positive and significant impact on the dependent variable. Column 5 shows the mean impact of the measure when the relevant controls are included: the coefficient of *Policy* remains negative and highly significant, if somewhat smaller in magnitude than in the baseline estimates.

Figure 5: Poisson event study estimates

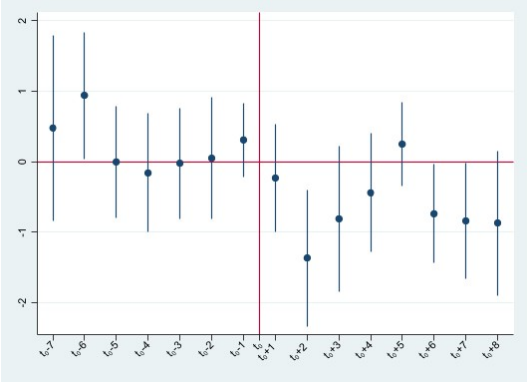
(a) *Control variables*



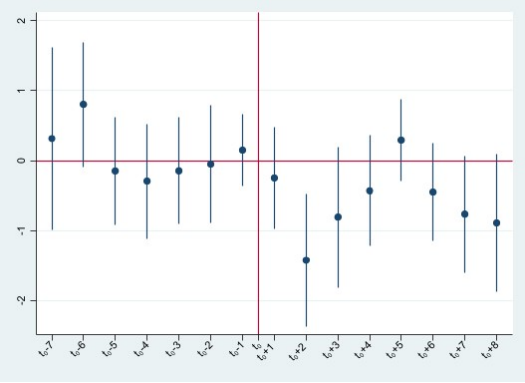
(b) *Early termination*



(c) *Multiple dissolutions*



(d) *Province without dissolutions*



Note. The graphs report coefficients and confidence intervals estimated according to eq. 2 by a Poisson FE model. The dependent variable is the total number of attacks against politicians. Graph 5a shows the estimates including controls for politicians' characteristics: *Mayor degree*, *Councillors degree*, *Aldermen degree*, *Female councillors* and *Female aldermen*. Graph 5b shows estimates excluding municipalities where an administration was terminated early for reasons unrelated to mafia infiltration. Graph 5c shows estimates excluding municipalities dissolved more than once. Graph 5d shows estimates excluding municipalities in provinces where there was no dissolution for mafia. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 8 years after the dissolution. All regressions include municipality FE, year FE and resident population.

As is observed in Section 4.1, there could be a problem of selection bias in cases of early terminations for reasons unrelated to mafia, municipalities dissolved more than once, and those located in provinces with no city council dissolutions. We address this problem in graphs 5b, 5c and 5d of Figure 5 for the event study dummies and in Columns 2, 3 and 4/6, 7 and 8 of Table 4, for the mean impact estimates. The results show a very similar path of the attacks and again corroborate the average negative effect of the measure.

Table 4: Poisson FE estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks
$t_0 + 2$	-1.178**	-1.102*	-1.365***	-1.425***				
	(0.494)	(0.562)	(0.495)	(0.483)				
$t_0 + 3$	-0.860*	-1.656**	-0.807	-0.809				
	(0.490)	(0.698)	(0.525)	(0.512)				
$t_0 + 6$	-0.654*	-0.957*	-0.735**	-0.451				
	(0.345)	(0.522)	(0.356)	(0.356)				
$t_0 + 7$	-0.886**	-0.720	-0.838**	-0.768*				
	(0.399)	(0.609)	(0.417)	(0.423)				
Policy					-0.516**	-0.671**	-0.690***	-0.559***
					(0.227)	(0.294)	(0.208)	(0.212)
Mayor degree	0.228**				0.242**			
	(0.0959)				(0.0957)			
Councillors degree	0.232				0.249			
	(0.604)				(0.601)			
Aldermen degree	-0.0196				-0.00562			
	(0.107)				(0.108)			
No. female councillors	-0.000972				-3.42e-05			
	(0.0174)				(0.0174)			
No. female aldermen	0.124***				0.123***			
	(0.0414)				(0.0415)			
Pop	0.00726	0.0106	0.00969	0.00885	0.00788	0.0109	0.0104	0.00938
	(0.00862)	(0.0104)	(0.00990)	(0.0103)	(0.00889)	(0.0104)	(0.0102)	(0.0105)
Observations	5,705	3,941	5,681	5,126	5,705	3,941	5,681	5,126
No. municipalities	574	396	570	516	574	396	570	516
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table reports coefficients estimated according to the Poisson FE model in eq. 2 (in Columns 1 - 4) and in eq. 3 (in Columns 5 - 8). The dependent variable is the total number of attacks against politicians. Columns 1 and 5 show estimates adding controls for politicians' characteristics (*Mayor degree*, *Councillors degree*, *Aldermen degree*, *No. Female councillors* and *No. Female aldermen*). Columns 2 and 6 show estimations excluding municipalities terminated early for non-mafia reasons. Columns 3 and 7 show estimations excluding municipalities dissolved more than once. Columns 4 and 8 show estimations excluding municipalities in provinces with no mafia-related dissolutions. *Policy* is a dummy taking the value of 1 for all the years from the dissolution onward and 0 in the others. In estimating eq 2 we include event-time dummy variables for up to 7 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). Coefficients of municipality FE and year FE 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).

5.1 Negative weights

The most recent econometric literature has shown that in a staggered diff-in-diff design, when the effects of the measure are dynamic (as in our setting), the coefficient of the average treatment effect (*Policy* in Table 3) represents a weighted average of these dynamic effects; and some of the weights can be negative, generating possible bias in the estimates (Goodman-Bacon, 2021). When the treatment effect is not constant over time, negative weights arise because the already-treated units move into the control group, so that changes in their treatment effects over time are no longer factored into the diff-in-diff estimate. De Chaisemartin and d'Haultfoeuille (2020) propose a test to compute the number of negative weights in this setting.[12] The test shows that the number of negatively-weighted Average Treatment Effects on the Treated (ATTs) is 0, suggesting that the heterogeneity is chiefly between treated and never-treated municipalities. Therefore, our initial estimates are not biased due to the negative weight issue.

[12] We use the *twowayfeweights* Stata command, developed by De Chaisemartin and d'Haultfoeuille (2020), for linear estimations.

6 About the exogeneity of the dissolution measure

The main assumption of the diff-in-diff methodology is the strict exogeneity of the policy with respect to the dependent variable. In this section, we tackle this methodological issue by addressing two important threats to our identification strategy: reverse causality and omitted variable bias. All Tables with the prefix "A." are in Appendix A. All Figures with the prefix "B." are in Appendix B.

6.1 Reverse causality

In principle, any policy intervention could be the response to some problem of public life. Hence, although (as argued earlier) the motivations for dissolution are much broader in scope, city council dissolution could be justified also by violent actions against politicians by organized crime . The event study model eq. 2 lends itself to a first check of the exogeneity of the treatment through a test of the Granger causality hypothesis (Granger, 1969). We also search for further evidence of Granger causality by regressing the treatment dummy — city council dissolution — on contemporaneous and past levels of attacks. Moreover, we estimate the baseline model excluding the municipalities where the dissolution procedure was initiated explicitly owing to attacks against politicians.

6.1.1 Granger causality

Granger causality implies that the treatment is not anticipated by current attacks on politicians. Hence, city council dissolution should come before violence and threats, not after. Granger causality does not imply policy exogeneity if the government intervention is influenced by the expectation of future attacks against politicians. However, such behavior on the part of the institutions in charge of the decision can be easily ruled out. Indeed, council dissolution means that the mayor and councillors lose their legally held office, and in democracy the rule of law does not admit any role for expectations in legal decisions.

The estimation of the dynamic model eq. 2 shows that the policy intervention Granger causes the reduction in the number of criminal attacks, because the coefficients of the event time dummies t_0, $t_0 - 1$, $t_0 - 2$ are not statistically significant.

The exogeneity of policy further implies that current and past values of the outcome do not explain the policy intervention. Table A.6 shows results of the regression of the dummy *Policy* on the total number of attacks in municipality i at times t, $t-1$ and $t-2$ (in Column 1) and at times $t-1$ and $t-2$ (in Column 2). The coefficients indicate that the *historical* number of attacks has no significant effect on the probability that a municipality will be dissolved.

We also estimate a linear probability model for the *hazard rate* of the policy, that is the probability of a municipality's being dissolved at time t when it has never been dissolved until then, as is common in duration analysis. In this case, the dependent variable *Policy* is a dummy taking the value of 0 in the years before the city council dissolution in treated municipalities and 1 in the year of dissolution, and 0 in the control municipalities. Accordingly, in these estimates we remove all the observations of treated municipalities after the year of

dissolution. The results in Columns 3 and 4 of Table A.6 show that mafia-related violence has no effect on the conditional probability of a municipality's being dissolved.

6.1.2 Investigation due to violent intimidation

As is noted in Section 3.1, violent intimidation of local politicians by organized crime can be the starting point of the investigation by the *Commissione d'Accesso*, which could end in city council dismissal. To address this issue, we reviewed the juridical literature (e.g. Marotta, 2019) that has used official reports to identify 31 municipalities whose dissolution procedure in the period 2010-2018 was initiated because of evidence of a collusion between organized crime and politicians *accompanied* by evidence of violent intimidation. These municipalities are then excluded from the treatment group; the results of estimating Poisson FE model as in eq. 2 and eq. 3 are reported in Figure B.4 and in Table A.7.

Consistent with the Granger causality test, the event study dummies confirm that the common trend assumption is again verified and that the effect of measure exhibits a pattern similar to that in graph 4c in Figure 4. The comparison between the mean estimated effect of the measure *Policy* (by the Poisson FE procedure) in Column 7 of Table 3 and that in Column 2 of Table A.7 offers reassurance that the reverse causality does not substantially bias our baseline estimates.

6.2 Omitted variable bias

Strict exogeneity of the treatment requires that the timing of treatment exposures in the diff-in-diff design must be statistically independent of the potential outcome distributions, conditional on the group and time fixed effects. We provide evidence of robustness to omitted variable bias with a propensity score matching analysis and controls for important variables that proxy for the level of government public security intervention.

6.2.1 Propensity score matching

The matching procedure first estimates a propensity score corresponding to the probability of each municipality's being treated, which is then regressed on covariates at the municipal level: population, average female and male educational attainment, female and male unemployment rates, a dummy for the sex of the mayor (1 if female) and the share of female politicians within the city council.[13]

The goodness of the match for constructing a suitable control group is confirmed in Figure B.5, which shows the distributions of the estimated propensity scores for the treated municipalities (right-hand side) and the matched control municipalities (left-hand side).

Figure B.6 and Table A.8 show the estimation results of eq. 3 by the Poisson FE procedure. 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

[13] We use the Stata command *psmatch2* developed by Leuven and Sianesi (2003).

after compulsory administration ($t_0 + 2$ and $t_0 + 3$). The estimates also display a durable effect of the measure, lasting at least until the second election afterwards.

Column 2 of Table A.8 shows that after a city council dissolution the number of attacks decreases on average by 47% more in the treatment than in the control group. The coefficient is a bit smaller (in absolute value) than that in the full sample estimates.

6.2.2 Controlling for government repressive action

According to the theoretical argument set out in Section 2, the reduction in violence against politicians is an indirect effect of the policy measure under examination, which is designed to increase the cost of corruption (i.e., bribes) but not that of violent punishment, which would require an expansion of law enforcement bodies. Thus government repression of criminal activities through police forces could be an important source of omitted variable bias in our analysis. The reduction in attacks in the treated relative to the untreated municipalities after city council dissolution could be due to an increase in the police presence rather than to the application of Law 164/1991.

To address this concern, let us note first that the application of the Law does not entail police control of the city government in territories where organized crime is especially active. Previous studies clearly show that owing to stringent budget constraints, neither police forces nor funds for public security are increased during compulsory administration (Mete, 2009; Cavaliere, 2004). In fact, examination of the national data reveals that both the number of policemen and national expenditure for public security declined between 2010 and 2019.[14]

In any case, we address the concern over omitted variable bias statistically by controlling for government repressive action. Data on central government control of the territory at municipal level are not available,[15] so we looked at the data of the Ministry of Interior on a large number of crimes reported by police to the judicial authorities at provincial level from 2006 to 2019. These crimes – homicides, sexual abuses, fencing of stolen goods, extortion, smuggling, drug trafficking, etc. – can be considered as a good proxy for central government repressive action against criminality.[16]

However, the number of crimes reported can be a good proxy for government actions against crime only if it is not affected by the treatment itself (Wooldridge, 2005). To verify this basic condition, we regress all of these crimes on our treatment variable. Since the number of crimes is reported at province level, we construct the treatment variable as taking value 1 if at least one municipality has been dissolved in the province, from the year of dissolution onward, and 0 otherwise.

We estimate a panel FE model for all provinces (with both year and province FE), where each of the 55 crimes recorded by the Ministry of the Interior is regressed one-by-one on the treatment variable defined above. The estimates [17] allowed us to pick out 35 crimes that are

[14] See the Eurostat website: http://ec.europa.eu/eurostat.

[15] For example, national expenditure for police and number of policemen are confidential information, not available at lower levels of government.

[16] The analysis of Cingano and Tonello (2020) shows that crimes are a good proxy for government repressive action. In fact, city council dissolution is associated with a persistent fall in thefts, but has no effect on such mafia-related crimes as homicide, extortion, drug trafficking, and usury.

[17] Results are available upon request.

not affected by the treatment. We then estimated the Poisson FE model in the four regions of Calabria, Campania, Puglia and Sicily as in eq. 3, controlling for each of the crimes selected. The coefficient associated with the treatment variable *Policy* remains negative and highly significant.

Among the 35 felonies, ten — beatings, intentional injury, threats, corruption of minors, robbery in homes, robbery in shops, car crime, art theft, robberies in post offices, and bombings — have a significant effect on our outcome variable. For these crimes we report the estimation results of the average impact of the measure in Table A.9, confirming the significant negative effect of the dissolution measure on the level of violence against politicians.[18] The magnitude of the coefficient associated with the variable *Policy* is very close to that given in Table 3, further supporting the exogeneity of the treatment with respect to the outcome variable.

7 Testing the exogeneity of the measure: IV approach

This Section presents additional statistical evidence of the exogeneity of the application of Law 164 through a test based on the instrumental variables approach. The following analysis relies on a robust complementary approach to the econometric issue of identification of policy effects (Besley and Case, 2000) and is quite new in the literature on the effects of Law 164/1991. First, we focus on variables that proxy for some relevant features of the institutional decision-making process and then use them as IVs in the testing procedure included in the control function approach (Blundell and Powell, 2003).

7.1 Factors affecting city council dissolution

As is discussed at some length in Subsection 3, Law 164/1991 assigns a key role to the Prefect, who chooses to start the investigation procedure by appointing the *Commissione d'Accesso* on the grounds of preliminary evidence provided by the judiciary or the police. The findings of the investigation then form the basis for the Prefect's proposal to the Minister of the Interior for city council dissolution. In short, it is the Prefect, as central government representative in a province, who judges whether the evidence of the link between mafia-type organizations and local public officials justifies the dissolution of the city council. The Prefect's role in determining the final decision of national institutions is paramount.

Given the preventive character of Law 164/1991, the Prefect has to make the decision on the basis of inferences and clues as to the infiltration of local politics by organized crime, not necessarily evidence of crime as such. Hence, the choice to initiate the inquiry cannot be based exclusively on the penal code but instead requires the subjective assessment of the Prefect. This means that the latter's personal characteristics are likely to influence the decision to appoint the *Commissione d'Accesso*.

The Italian Ministry of Interior provides information on the gender, date and place of birth of the Prefect in each province over the years. The Prefect's gender may have an effect on application of Law 164, as women are generally believed to have higher ethical

[18] The corresponding event study estimates are available upon request.

standards than men and to be more concerned with the common good (Bolzendahl and Coffé, 2009, Dollar et al., 2001, Goetz, 2007). Also, women have been credited with a greater propensity for altruistic and moral behavior (Eagly and Crowley, 1986, Eckel and Grossman, 1998, Glover et al., 1997) and with a more public-spirited attitude (Goertzel, 1983, Ones and Viswesvaran, 1998), and are held to be more effective in promoting honest government. In line with this literature, we believe that female Prefects are inclined to be more severe in the enforcement of Law 164 than their male counterparts.

The Prefect's age too could affect enforcement of the Law. The decision to dissolve a city council is a risky one, as the Prefect could suffer reputational damage for a wrong decision. More experienced Prefects, during their careers, have presumably learned how to reduce this risk and acquired a better capability to interpret the information produced by the local political and criminal context. Older officials may also have more experience in detecting the warning signs of mafia infiltration, drawing hints and clues from preliminary investigations. Furthermore, older Prefects, towards the end of their career in civil service, may be more inclined to "clean" the local territory of organized crime to the benefit of younger generations. This form of inter-generational altruism can be intensified by a psychological attitude that makes older people less tolerant of corruption (Hirschi and Gottfredson, 2000, Torgler and Valev, 2006). These arguments suggest that the age of the Prefect may have a positive effect on the probability of a decision to dissolve a city council.

Finally, we can conjecture that civil servants who were born and raised in a territory more severely plagued by organized crime may be more concerned with this social issue than men and women from outside the area. Thus, one may well expect that Prefects from the four regions studied here are more likely to formulate a dissolution proposal.

To sum up, our priors are that the probability of city council dissolution increases: 1) if the Prefect of the province is female; 2) where the age of the Prefect is greater; and 3) where the Prefect comes from Calabria, Campania, Puglia or Sicily.

Accordingly, we construct: 1) a dummy taking value 1 if the Prefect is a woman; 2) a dummy for the age of the Prefect, subtracting from each year 2010-2019 the birth year of the *Prefetto* (*Age prefetto*); 3) a dummy taking value 1 if the Prefect was born in a municipality in Calabria, Campania, Publia or Sicily and 0 otherwise (*Birth-place prefetto*).

The endogeneity test exploits the control function approach, a two-stage IV procedure that requires, first, regressing the potential endogenous variable (i.e., *Policy*) on all the assumed exogenous explanatory variables and the instruments just defined; and second, using the predicted residuals as an additional regressor in the main equation with the potential endogenous variables.

The results from the first stage estimates are shown in Table 5.

Table 5: First stage estimations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.:	Policy	Policy	Policy	Policy	Policy	Policy	Policy
Sex prefect	-0.00155			0.0016		0.00199	-0.0289
	(0.003)			(0.003)		(0.003)	(0.0705)
Age prefect		0.00184***		0.0019***	0.0017***	0.00181***	0.00171**
		(0.0005)		(0.0006)	(0.0005)	(0.0006)	(0.0007)
Birth-place prefect			0.00346		0.0056	-0.00593*	
			(0.003)		(0.003)	(0.003)	
Sex prefect*age prefect							0.000509
							(-0.0012)
No. observations	15938	15938	15435	15938	15435	15435	15938
F-stat	0.26	13.08	1.23	6.63	5.23	3.53	4.76

Note. The table reports the coefficients of the first stage estimations: estimates of a linear equation where the dependent variable is the dummy *Policy* (the endogenous one) over the regressors specified. All regressions include municipality FE, year FE and population, whose coefficients are not reported. Standard errors, in brackets, are clustered at municipal level. Period: 2010-2019. F-stat refers to the Wald test of the null that the coefficients of the variables Sex prefect, Age prefect, Birth-place prefect, and their interactions, are null. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

The first-stage regressions strongly confirm the prior for age. Indeed, in Columns 2, 4, 5, 6 and 7, the coefficient of *Age prefect* is always positive and highly significant: the older the Prefect, the higher the probability of city council dissolution. Place of birth, although significant (weakly) only in Column 6, has the expected positive sign. Contrary to our expectation, however, the gender of the Prefect does not affect the probability of dissolution. In Column 4 we add pairwise the Prefect's age and gender and in Column 5 place pf birth; Column 6 adds all the regressors together. Column 7 evaluates the combined effect on the dissolution of the age and the gender of the Prefect, constructing an interaction variable. However, the estimated coefficient is not significantly different from zero.

The value of the F-statistic testing the relevance of the instruments is greater than 10 (the "rule of thumb") only for the variable *Age prefect* (Column 2). Accordingly, we take this as our instrumental variable for the computation of the endogeneity test.

As regards the exclusion restriction, no link is found between either the activity or the personal characteristics of the Prefect and threats against local politicians by organized crime. Therefore, the enforcement of the Law is affected by the Prefect, whose action, however, is not correlated with the dependent variable of our empirical model.

7.2 Testing exogeneity

As noted above, in order to control for the endogeneity of the measure (the variable *Policy*), the control function procedure requires that the predicted values of the first-stage residuals, as estimated in Table 5, be added to the set of the other explanatory variables in the second-stage estimates of the outcome variable. If the coefficient of the predicted residual is not significantly different from zero, then the *Policy* variable can be considered exogenous.

Following Lin and Wooldridge (2019), we apply the OLS fixed effects panel regression to estimate the reduced form of *Policy*, including among the regressors *Age prefect*, and as IV the municipal *Population* and the year dummies. In the second stage, the structural eq. 3 for the number of mafia attacks, augmented with the predicted first-stage residuals, is estimated using FE Poisson maximum likelihood. For both the first- and the second-stage estimates, robust standard errors, clustered at the municipal level, are bootstrapped. The null hypothesis of a zero coefficient for first-stage residuals cannot be rejected ($z = 0.39$;

$P - value = 0.69$), which provides support for the assumption of exogeneity of the city council dissolution.

Although we found statistical evidence that *Age prefect* is a good instrument in the reduced form equation of *Policy* (as in Column 2, Table 5), the fundamental requirement for IV exogeneity, i.e. for the correct specification of the model, must be fulfilled. Accordingly, as usual in the IV procedure, we perform the Hansen over-identification test, which requires estimating the panel FE Poisson model as in eq. 3 using the Generalized Method of Moments (GMM) (Cameron and Trivedi, 2013). The variable *Policy* is assumed endogenous to the number of mafia attacks; the instrumental variables are *Age prefect* and *Sex prefect*.[19] The estimate produces a J-statistic that suggests that the hypothesis that the model is correctly specified cannot be rejected ($J = 0.5338$; $P - value = 0.4650$): the orthogonality conditions hold.

8 Spillovers and Robustness

This Section presents additional evidence in support of our findings. To assess the overall efficacy of Law 164 as an anti-mafia measure, we show the presence of spillover effects of city council dissolution on the level of violent intimidation in neighboring, undissolved municipalities. We refine our analysis by distinguishing among politicians by gender and office, as well as among the types of attack. We also investigate whether the political orientation of the local government makes a difference for the effect of the city council dismissal on the number of attacks. Finally, we test whether dissolution unrelated to mafia infiltration affects our dependent variable.

8.1 Spillover effects

Since it is plausible that mafias operate in an area larger than a single town, the shock to mafia infiltration due to the enforcement of Law 164 in one municipality may generate spillover effects in neighboring towns. This prediction is also supported by Galletta (2017), who shows the presence of spillover effects due to the dissolution for mafia infiltration in Calabria, Campania and Sicily in 1998-2013 in terms of a reduction in public investment. The analysis of the spillover effects also enables us to test the "Stable Unit Treatment Value Assumption" (SUTVA) (Rubin, 1980), namely that the treatment status of one municipality does not affect outcomes for untreated municipalities — which is crucial to the validity of the diff-in-diff methodology; otherwise estimation could be biased.

In this respect, we estimate a Poisson FE model as in eq. 2 and eq. 3, 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.[20] The neighboring municipalities are assumed to start receiving the treatment in the year of the near-by municipality's dissolution. The results

[19] The F-test on both variables in the estimates of the reduced form as in Column 4, Table 5, shows a value that satisfies the requirement.

[20] Our source for selection of the neighboring municipalities is the "Matrici di contiguità, distanza e pendolarismo" database provided by the Italian Institute of Statistics ($ISTAT$).

are shown in Figure B.7 and Table A.10, where the dependent variable is the total number of attacks against politicians by year.

Graphs B.7a and B.7b show the event study estimates of the Poisson FE model with standard errors clustered at municipal and provincial level, respectively.[21] The common trend assumption continues to hold for neighboring municipalities. The dynamic of the number of attacks suggests that for neighboring municipalities too the measure substantially reduces organized criminal violence against politicians. Interestingly, 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 (columns 1 and 2, Table A.10). Also for neighboring municipalities, the effect of the measure is long-lasting, as strongly significant coefficients persist up to t_0+9.

Columns 3 and 4 Table A.10 shows the average treatment effect on attacks against politicians in neighboring municipalities. Given the two-year lag in the response to a dissolution in a neighboring municipality, we construct the treatment dummy *Policy* (for neighboring municipalities), which takes value 1 in the second year after the dissolution of the neighbour municipality onward and 0 for other years. The shock to the political infiltration of mafia-type organizations due to the enforcement of Law 164 reduces mafia attacks in the neighbors of the dissolved municipalities by 25.6%.

The verified presence of municipality spillovers means the SUTVA assumption is violated, so the baseline estimates could be biased, because these neighboring municipalities are part of the control group. In order to correct for this bias in our original estimates, we exclude these neighboring municipalities from the control group. The results are presented in Figure B.8 and Table A.11. They confirm both the validity of the common trend assumption and the dynamics of the dependent variable.

Comparison of the average treatment effect in Column 7 of Table 3 and that in Column 2 of Table A.11 shows a clear underestimation of the coefficient of *Policy*, confirming the violation of the SUTVA in our initial results. Correcting for the spillover effects, the mean effect of the measure in reducing the number of attacks comes to 56.4%.

8.2 Types of politician and attacks

In this section, we provide some additional evidence confirming our main findings. We first investigate whether politicians' gender matters for the use of violence by organized crime. In the sample period, the average share of women in city councils is 21% and of female aldermen, 25%, female councillors 22% and female mayors, 5.7%. While this descriptive evidence suggests greater male participation in politics, women might be differentially engaged in sectors targeted by organized crime and thus more likely to be victims of attack. Columns 1 and 2 of Table A.12 do not confirm this thesis; instead, there is a significant and negative average impact of Law 164 for male politicians only: after the dissolution because of mafia infiltration, the number of attacks against men drops by an average of 58% more in the treatment than in the control group (Column 1). Using both these subsets of politicians, the trend of attacks is substantially confirmed for men (see graph B.9a of Figure B.9), with a

[21] To analyse spillover effects, clustering at province level is also very important.

reduction of some 81% (−1.69 in the figure) in the number of attacks in the year of the first election after compulsory administration (t_0+2). However, there is considerable volatility in the coefficients in the graph B.9b referring to female politicians, owing to the small number of attacks in this subgroup.

We replicate our analysis by distinguishing among different kinds of politician. As is shown in Figure 3, the targets of mafia attacks are city council politicians as well as politicians outside local government. In this respect, we evaluate the effectiveness of Law 164 over the two groups of politicians: *City council politicians*, including mayors, councillors, aldermen and deputy mayors; *Other politicians*, comprising regional councillors, regional presidents, former mayors, candidates and representatives of other institutions and institutional entities. The estimation results are given in Columns 3 and 4 of Table A.12 respectively for the two sets of politicians. The policy affected only city council politicians, and the attacks on them after the compulsory administration declined on average by 46% more in the treatment than the control group. Graph B.9c in Figure B.9 shows that the sharper reduction in the attacks on city council politicians occurs in the second elections after compulsory administration (that is, in t_0+6 and t_0+7) rather than in the first election.[22] Along the same lines, we also consider attacks against mayors only. Similarly, for them, the Poisson FE coefficient suggests a decrease of 82% in the number of attacks immediately after compulsory administration. The elasticity of the average treatment effect over the entire period is about 45% for the number of attacks against mayors.

We also investigate whether Law 164 is effective in reducing both threats and violence, which are the main instruments of violent intimidation, as detailed in Figure 2. In this respect, we split the types of attack into the two categories of *Threats* and *Violence*. We define as *Threats* the following: threatening letters, threatening letters containing bullets, verbal or telephone threats, bullets left in front of a politician's home, threatening messages on the family tomb, bullets left in front of the home or City Hall, and threatening messages on the walls of the home or City Hall. We define as *Violence*: setting fire to car, City Hall, home or municipal property, physical assault, shootings against City Hall, homicide, killing of domestic animals, delivery of animal head in a box, shootings against cars, leaving dead animals or their parts in front of the home, damage or robbery inside City Hall, felling of trees on private property, physical assault in public places, bombings of home or City Hall. This distinction also allows us to overcome, at least partially, the measurement error of the dependent variables due to the possibility of under-reporting. For while threats may be hidden, outright violence is visible and, thus, unlikely to go unreported. Columns 6 and 7 of Table A.12 show the effect of the measure for the two categories of attack respectively. While the coefficient for *Threats* is not significant, textitViolence is reduced on average by 45% more in the treatment than in the control group. The event study graph in B.9g of Figure B.9 confirms the inverted u-shaped trajectory of the subset of *Violence* following the Law's application in the treated compared with the untreated municipalities. Here, the highest coefficient is recorded at t_0+3, with a decrease in visible attacks of 78%(−1.51 in the graph). The coefficients of *Threats* in graph B.9f are also quite volatile, owing to the

[22] In graph B.9d the considerable volatility of the coefficients is due to the very low number of attacks on non-city-council politicians.

very small size of this sub-sample.

As noted in Section 4.1, the number of attacks according to "Avviso Pubblico" corresponds to those reported in the media. For example, as is suggested by Daniele and Dipoppa (2017), news of attacks may depend on the electoral period. Although these possible measurement errors concerning the dependent variable (see Subsection 3.3) are not a real concern in the empirical analysis, we replicate previous findings by considering the most salient attacks, defined as those most likely to gain media attention (cars burned, physical assaults, homicides, shootings against cars). Figure B.9 and Column 8 of Table A.12 show the estimation results of the preferred Poisson FE model, which are substantially similar to the baseline findings. The difference in the decrease in salient attacks between the dissolved municipalities and the control group peaks at $t_0 + 3$ after treatment, with an elasticity of 85% (column 1). The elasticity of the average treatment effect over the entire period is about 47%.

8.3 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 as in eq.2 and 3 we control for the dummies *Center-right*, *Center-left* and *Civic lists*.[23] The results (Figure B.10 and Table A.13) are consistent with the baseline, and they indicate that the mayors' political orientation does not affect the outcome variable.

8.4 Placebo test on governments dissolved for reasons unrelated to mafia infiltration

As a further support to our findings, we run a placebo test on municipalities dissolved for reasons unrelated to mafia infiltration, which is a quite common occurrence (see subsection 4.1).

We estimate Poisson FE as in eq. 2, considering the dynamics of the number of attacks in municipalities dissolved for reasons unrelated to mafia infiltration before and after the city

[23] 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.

council dismissal. Obviously municipalities dissolved for mafia infiltration are excluded.

Figure B.11 shows the year-by-year Poisson FE estimated coefficients, which are not statistically different from zero. In short, the number of attacks against local politicians is plainly not significantly affected by city council dissolution per se.

9 Conclusions

This paper offers an empirical measure of the efficacy of Law 164/1991 for city council dissolution in cases of mafia infiltration in preserving public security by deterring organized crime from violent intimidation to influence local politics.

We referred to Dal Bó et al. (2006)'s model. In this framework, law enforcement aimed at increasing the cost of corruption without expanding the police presence could in theory induce criminal organizations to shift from corruption to violent intimidation, thereby increasing the level of violence; or, on the other hand, it could have an overall negative effect on violence against politicians if it induces enough criminal groups to switch to inactivity. In substance, empirical analysis of the effect of Law 164/1991 on organized criminal violence against local politicians is essential to assessing its effectiveness in raising entry barriers to the mafias' "business" of influencing politics.

Our diff-in-diff analysis demonstrates that the enforcement of Law 164/1991 reduces the number of attacks against politicians substantially, with an effect that persists for at least the first two elections after compulsory administration. To evaluate the Law's effectiveness in preserving public security in a broader sense, we provide evidence that city council dissolutions also have appreciable spillover effects, lowering the number of attacks also in neighboring, undissolved municipalities. We validate the assumption of the exogeneity of the treatment to the outcome by controlling for reverse causality and omitted variable bias, and by an instrumental variable approach.

This paper makes two key contributions to the literature. First, it addresses the possibly ambivalent effect of a positive shock to the cost of corruption as highlighted in Dal Bó et al. (2006) by showing that the enforcement of Law 164/1991 is effective in raising the barriers to entry into the business of influencing politics, so as to induce criminal clans to reduce violence against politicians. Second, it adds to the substantial literature on the effects of Law 164/1991, with novel empirical evidence that the Law's enforcement diminishes mafia violence against politicians. In this regard our results expand on recent studies documenting the effectiveness of city council dissolution in terms of long-run economic performance (Fenizia and Saggio, 2022), female political empowerment (Baraldi and Ronza, 2022), the quality of elected officials (Daniele and Geys, 2015), and the allocation of public resources and tax collection (Di Cataldo and Mastrorocco, 2021). Finally, since several studies have shown that repressive policies bring about a violent reprisal from criminal organizations (e.g., Calderón et al., 2015; Castillo and Kronick, 2020), our analysis contributes to the literature that analyses empirically the effectiveness of non-repressive government measures in fighting organized crime activities (Daniele and Dipoppa, 2022).

From the normative perspective, while city council dismissal is meant to be an *ad hoc* measure responding to specific circumstances within the local government, the persistence of

the dampening effect on violence against local politicians up to at least the second electoral round after compulsory administration is powerful evidence of the medium-run efficacy of Law 164/1991 in restoring the autonomy of the local government and local public security.

The evidence presented here is in line with Daniele and Dipoppa (2017) and Alesina et al. (2019). The sine curve of the post-treatment coefficients between the first two elections after compulsory administration is consistent with the thesis that the electoral pressure exerted by criminal clans is a consequence of violent intimidation against politicians as a mechanism of reinforcement of corruption. Our findings document the efficacy of the city council dissolution in deterring violence by organized crime precisely when it would be of the greatest strategic value in wielding political influence. This helps clarify the way in which criminal organizations redirect public resources to their own advantage.

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APPENDIX

A *Tables*

Table A.1: Mean of attacks on politicians by region

Regions	All politicians (1)	All politicians/Pop (2)
Abruzzo	1.55	0.00389
Basilicata	1.66	0.011588
Calabria	2.39	0.004972
Campania	2.29	0.002642
Emilia Romagna	1.81	0.001324
Friuli Venezia Giulia	1.06	0.002163
Lazio	2.76	0.00253
Liguria	2.07	0.002915
Lombardy	1.62	0.000657
Marche	1.05	0.002209
Molise	1.20	0.010791
Piedmont	1.80	0.001747
Puglia	2.28	0.002704
Sardinia	1.93	0.00477
Sicily	2.66	0.002949
Tuscany	1.74	0.001644
Trentino Alto Adige	0.82	0.002947
Umbria	0.85	0.002943
Valle D'Aosta	1.00	0.031447
Veneto	1.70	0.00142

Note. The table gives the number of dissolved administrations among those elected from 1991 to 2016 in Italy by region. Piedmont, Lombardy, Lazio, Liguria, Emilia Romagna, and Basilicata had 1 dissolution each.

Table A.2: Descriptive statistics

	Obs	Mean	Std.Dev.	Min	Max
Attacks	16080	0.17	0.841	0	21
Pop	15982	10.396	35.018	0.158	989.111
Female education	11207	0.838	0.043	0.635	0.929
Male education	11207	0.886	0.029	0.756	0.962
Female unemployment	11207	0.233	0.076	0.014	0.537
Male unemployment	11207	0.155	0.051	0.014	0.351
Mayor degree	16080	0.574	0.495	0	1
Councillors degree	16080	0.005	0.069	0	1
Aldermen degree	16080	0.085	0.279	0	1
No. female councillors	15938	2.685	2.23	0	14
No. male councillors	15938	0.919	0.882	0	5

Notes. Descriptive statistics of variables. Population in thousands. Period: 2010-2019.

Table A.3: Distribution of attacks

No. Attacks	Frequency	Percentage	Cumulative
0	14,942	92.92	92.92
1	462	2.87	95.8
2	341	2.12	97.92
3	151	0.94	98.86
4	60	0.37	99.23
5	47	0.29	99.52
6	22	0.14	99.66
7	11	0.07	99.73
8	16	0.1	99.83
9	9	0.06	99.88
10	3	0.02	99.9
11	3	0.02	99.92
12	5	0.03	99.95
14	4	0.02	99.98
15	1	0.01	99.98
17	1	0.01	99.99
18	1	0.01	99.99
21	1	0.01	100
Total	16,080	100	

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

Table A.4: Poisson RE estimations

	(1)	(2)	(3)
Dep. Var.:	No. Attacks	No. Attacks	No. Attacks
Policy	0.465**	-0.563***	-0.709***
	(0.220)	(0.201)	(0.224)
Ln(Pop)	0.817***	0.930***	0.848***
	(0.0422)	(0.0400)	(0.0543)
Female education			8.822***
			(3.031)
Male education			-13.90***
			(4.211)
Female unemployment			0.536
			(1.593)
Male unemployment			1.536
			(2.491)
Observations	16,002	16,002	11,217
No. municipalities	1,606	1,606	1,603
Year FE	Yes	Yes	Yes
Province FE		Yes	Yes

Note. The table reports coefficients estimated according to eq. 3 by Poisson RE model. The dependent variable is the total number of attacks against politicians. Column 2 includes province FE; Column 3 adds municipality controls: *Female education*, *Male education*, *Female unemployment* and *Male unemployment*. 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 for previous years. All regressions include municipality FE, year FE, and population (in log). The coefficients of municipality FE, year FE, and province FE are not reported. Standard errors are in brackets. Period: 2010-2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table A.5: Excluding 2012

	(1)	(2)
Dep. Var.:	No. attacks	No. attacks
$t_0 + 2$	-1.638***	
	(0.462)	
$t_0 + 3$	-1.041**	
	(0.510)	
$t_0 + 6$	-0.745**	
	(0.340)	
$t_0 + 7$	-1.046***	
	(0.398)	
$t_0 + 8$	-1.183**	
	(0.480)	
Policy		-0.690***
		(0.200)
Pop	0.00855	0.00925
	(0.00985)	(0.0101)
Observations	5,094	5,094
No. municipalities	568	568
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2 (in Column 1) and in eq. 3 (in Column 2). We drop the year 2012. The dependent variable is total number of attacks against politicians. In estimating eq. 2 in Column 1 we include event-time dummy variables for 6 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). 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. The coefficients of municipality FE and year FE 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 A.6: Granger causality

	(1)	(2)	(3)	(4)
Dep. Var.:	Policy	Policy	Policy	Policy
Attacks	-0.00335		-0.000170	
	(0.00226)		(0.00160)	
$Attacks_{-1}$	-0.00218	-0.00189	6.33e-05	7.91e-05
	(0.00198)	(0.00185)	(0.00158)	(0.00159)
$Attacks_{-2}$	-0.000300	-1.31e-06	0.00258	0.00260
	(0.00211)	(0.00205)	(0.00195)	(0.00197)
Observations	12,796	12,796	12,400	12,400
R-squared	0.028	0.028	0.008	0.008
No. municipalities	1,606	1,606	1,599	1,599
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note. The table reports coefficients of panel FE estimations. For estimations in Columns 1 and 2 the dependent variable, *Policy*, is a dummy taking the value of 1 for all the years from the first year after compulsory administration to the end of the period and 0 for previous years. In Columns 3 and 4 *Policy* is a dummy taking the value of 1 in treated municipalities for the years of dissolution only and 0 previously, and 0 for untreated municipalities. *Attacks* is the total number attacks in municipality i. All regressions include municipality FE, year FE, and population, whose coefficients are not reported. Robust standard errors are in brackets. Period: 2010-2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table A.7: Investigation due to violent intimidation

	(1)	(2)
Dep. Var.:	No. attacks	No. attacks
$t_0 + 2$	-1.373**	
	(0.596)	
$t_0 + 3$	-1.119**	
	(0.540)	
$t_0 + 6$	-0.834**	
	(0.373)	
$t_0 + 7$	-0.947**	
	(0.446)	
$t_0 + 8$	-1.021**	
	(0.510)	
Policy		-0.708***
		(0.269)
Pop		0.0101
		(0.0104)
Observations	5,491	5,491
No. municipalities	551	551
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2 (in Column 1) and in eq. 3 (in Column 2). We exclude from the treatment group the 31 municipalities where the investigation was initiated also for violent intimidation. The dependent variable is the total number of attacks against politicians. In estimating eq. 2 in Column 1 we include event-time dummy variables for 6 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). 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. The coefficients of municipality FE and year FE 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 A.8: Propensity score matching

	(1)	(2)
Dep. Var.:	No. attacks	No. attacks
$t_0 + 2$	-1.384***	
	(0.475)	
$t_0 + 7$	-0.885**	
	(0.411)	
$t_0 + 8$	-0.923*	
	(0.493)	
Policy		-0.632***
		(0.197)
Pop	0.00941	0.00984
	(0.0104)	(0.0107)
Observations	3,281	3,281
No. municipalities	329	329
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2 (in Column 1) and in eq. 3 (in Column 2) on the matched control sample of municipalities. The dependent variable is the total number of attacks against politicians. In estimating eq. 2 in Column 1 we include event-time dummy variables for 7 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). 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. The coefficients of municipality FE and year FE 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 A.9: Controlling for crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dep. Var.:	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks	Attacks
Policy	-0.655***	-0.634***	-0.651***	-0.618***	-0.664***	-0.636***	-0.635***	-0.622***	-0.628***	-0.614***	-0.650***
	(0.212)	(0.209)	(0.213)	(0.212)	(0.213)	(0.211)	(0.211)	(0.213)	(0.214)	(0.210)	(0.218)
Beatings	0.00237**										0.00312**
	(0.00112)										(0.00154)
Intentional injury		0.000711**									-0.000872
		(0.000322)									(0.000796)
Threats			0.000494**								2.73e-05
			(0.000231)								(0.000493)
Corruption m.				0.0565**							0.0387
				(0.0223)							(0.0244)
House robbery					0.000379***						0.000374**
					(0.000132)						(0.000162)
Shop robbery						0.000674**					0.000630**
						(0.000298)					(0.000319)
Car crime							0.000236***				0.000152
							(8.62e-05)				(0.000149)
Art theft								-0.0168*			-0.00510
								(0.00875)			(0.0115)
Robberies in P.O.									-0.0157**		-0.0214***
									(0.00735)		(0.00759)
Bombings										-0.0131**	-0.00876
										(0.00553)	(0.00626)
Observations	5,697	5,697	5,697	5,697	5,697	5,697	5,697	5,697	5,697	5,697	5,697
Number of new_id	574	574	574	574	574	574	574	574	574	574	574
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2 (in Columns 1-5) and in eq. 3 (in Columns 6-10). The dependent variable is the total number of attacks against politicians. In Columns 1 and 6 we control for number of infanticides; in Columns 2 and 7, for the number of cases of corruption of minors; Columns 3 and 8, number of mafia-type criminal organizations; Columns 4 and 9, number of cases of drug production and trafficking; Columns 5 and 10, all those crimes together. Crimes are at province level in 2010-2016. 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. The coefficients of municipality FE and year FE 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 A.10: Spillovers

	(1)	(2)	(3)	(4)
Dep. Var.:	No. Attacks	No. Attacks	No. Attacks	No. Attacks
$t_0 + 3$	-0.429*	-0.429***		
	(0.242)	(0.155)		
$t_0 + 4$	-0.470**	-0.470***		
	(0.233)	(0.133)		
$t_0 + 6$	-0.524**	-0.524**		
	(0.223)	(0.233)		
$t_0 + 8$	-0.426	-0.426*		
	(0.261)	(0.252)		
$t_0 + 9$	-1.363***	-1.363**		
	(0.465)	(0.557)		
Policy			-0.296**	-0.296*
			(0.145)	(0.168)
Pop	0.0145	0.0145	0.0139	0.0139
	(0.0109)	(0.0116)	(0.0115)	(0.0126)
Observations	5,454	5,454	5,454	5,454
No. municipalities	547	547	547	547
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2. The dependent variable is the total number of attacks against politicians. *Policy* is a dummy taking the value of 1 from the second year after the dissolution onward and 0 for other years. All estimations include municipality FE and year FE, whose coefficients are not reported. Standard errors are adjusted for clustering at the municipal level; they are in brackets. Period: 2010-2019. Significant coefficients are indicated by * (10% level), ** (5% level) and *** (1% level).

Table A.11: Excluding neighboring municipalities

	(1)	(2)
Dep. Var.:	No. attacks	No. attacks
$t_0 + 2$	-1.832***	
	(0.473)	
$t_0 + 3$	-1.182**	
	(0.514)	
$t_0 + 4$	-0.807**	
	(0.394)	
$t_0 + 6$	-0.952***	
	(0.346)	
$t_0 + 7$	-1.272***	
	(0.402)	
$t_0 + 8$	-1.401***	
	(0.499)	
Policy		-0.831***
		(0.214)
Pop	0.0210	0.0400
	(0.0301)	(0.0345)
Observations	3,731	3,731
No. municipalities	465	465
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2 (in Column 1) and in eq. 3 (in Column 2). We exclude from the control group the municipalities neighboring those dissolved. The dependent variable is the total number of attacks against politicians. In estimating eq. 2 in Column 1 we include event-time dummy variables for 5 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). 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. The coefficients of municipality FE and year FE 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 A.12: Types of politician and attack

No. Attacks	(1) Male politicians	(2) Female politicians	(3) City council politicians	(4) Other politicians	(5) Mayor	(6) Threats	(7) Violence	(8) Salient politicians
Policy	-0.865***	-0.807	-0.608**	0.0446	-0.604**	-0.157	-0.592**	-0.643**
	(0.188)	(0.589)	(0.261)	(0.273)	(0.281)	(0.336)	(0.271)	(0.288)
Pop	0.00728	0.0516*	0.0399**	-0.0220	0.0371	0.0295*	0.00536	0.0141
	(0.0101)	(0.0296)	(0.0174)	(0.0141)	(0.0241)	(0.0170)	(0.0158)	(0.0225)
Observations	5,964	877	5,514	3,514	4,624	3,011	4,721	3,391
No. municipalities	598	88	553	353	464	302	473	395
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. The table reports coefficients estimated according to eq. 3 by Poisson FE model. Column 1 shows estimates when the dependent variable is the total number of attacks against male politicians. Column 2 shows estimates when the dependent variable is the total number of attacks against female politicians; Column 3, total number of attacks against city council politicians; Column 5, total number of attacks against mayors. Column 6 shows estimates when the dependent variable is the total number of threats against politicians. Column 7 shows estimates when the dependent variable is the total number of instances of actual violence against politicians, and Column 8 the total number of salient 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. The coefficients of municipality FE and year FE 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 A.13: Political party of administration

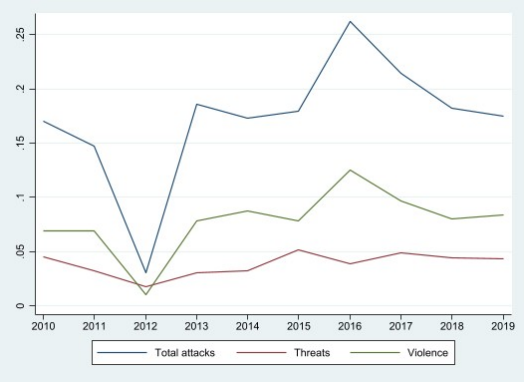
	(1)	(2)
Dep. Var.:	No. attacks	No. attacks
$t_0 + 2$	-1.429***	
	(0.500)	
$t_0 + 3$	-0.851*	
	(0.508)	
$t_0 + 6$	-0.658*	
	(0.340)	
$t_0 + 7$	-0.917**	
	(0.404)	
$t_0 + 8$	-1.052**	
	(0.496)	
Policy		-0.673***
		(0.210)
Center-right	0.0564	0.0669
	(0.148)	(0.146)
Center-left	0.230	0.261*
	(0.143)	(0.145)
Civic lists	0.0241	0.0423
	(0.115)	(0.111)
Pop	0.00683	0.00709
	(0.00856)	(0.00882)
Observations	5,731	5,731
No. municipalities	575	575
Municipality FE	Yes	Yes
Year FE	Yes	Yes

Note. The table reports coefficients estimated according to Poisson FE model in eq. 2 (in Column 1) and in eq. 3 (in Column 2). The dependent variable is the total number of attacks against politicians. We control for dummies for mayor's party affiliation. In estimating eq. 2 in Column 1 we include event-time dummy variables for 7 years before and 8 years after the dissolution; the omitted category is the year of dissolution (t_0). 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. The coefficients of municipality FE and year FE 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).

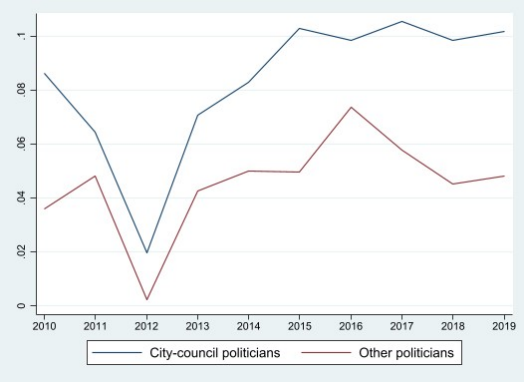
B *Figures*

Figure B.1: Yearly distribution of attacks

(a) By type of attack



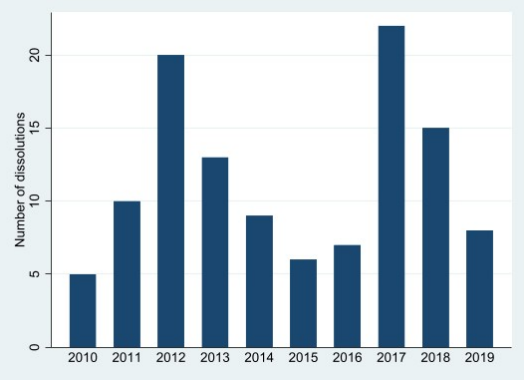
(b) By type of politician



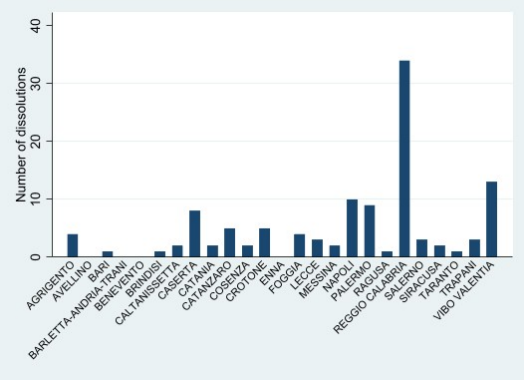
Note. The Figure shows the trend in the mean, over municipalities, of attacks against politicians. 2010-2019. Graph B.1a shows the trend in the total number of attacks and split into *Threats* (threatening letters, threatening letters containing bullets, verbal or telephone threats, bullets left in front of a politician's home, threatening messages on family tomb, bullets in front of home or City Hall, threatening messages on the walls of the home or City Hall) and actual *Violence* (car burning, burning of City Hall or municipal property, physical assault, burning of home, shooting against City Hall, homicide, killing of domestic animals, delivery of animal head in a box, shootings against car, leaving dead animals or their parts in front of home, damage or robbery in City Hall, felling of trees on private property, physical assault in public places, bombings of home or City Hall). Graph B.1b shows the trend of attacks on *City council politicians* (mayor, councillors, aldermen, deputy mayor) and *Other politicians* (regional councillors, regional presidents, former mayors, candidates and representatives of other institutions and entities).

Figure B.2: Dissolutions by year and province

(a) By year

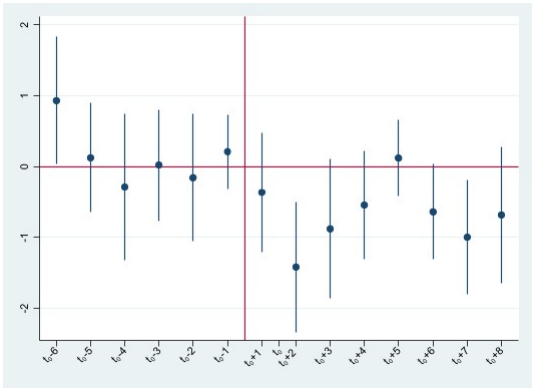


(b) By province



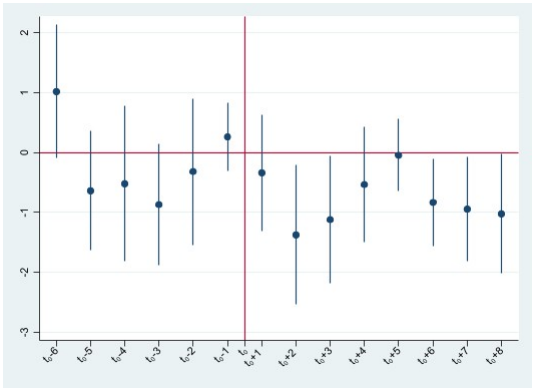
Note. Graphs B.2a and B.2b show the distribution of city council dissolutions for mafia infiltration, by year and by province, respectively, in Calabria, Campania, Puglia and Sicily.

Figure B.3: Excluding 2012 - Event study



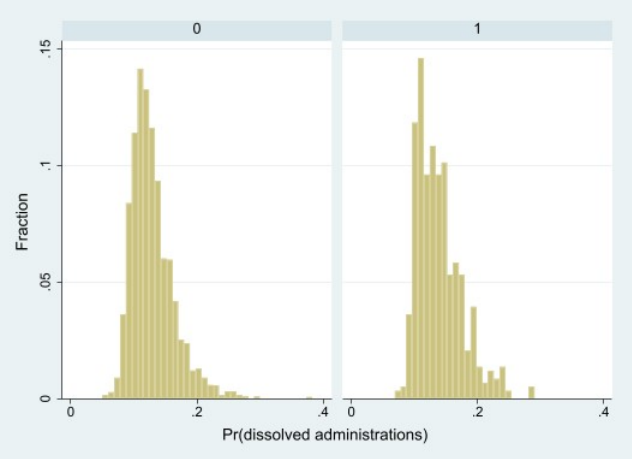
Notes. The graph reports coefficients and confidence intervals estimated according to eq. 2. Poisson FE model estimates, dependent variable is total number attacks. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 6 years before and 8 years after the dissolution. All regressions include municipality FE, year FE, and population. Period: 2010-2019.

Figure B.4: Investigation due to violent intimidation - Event study



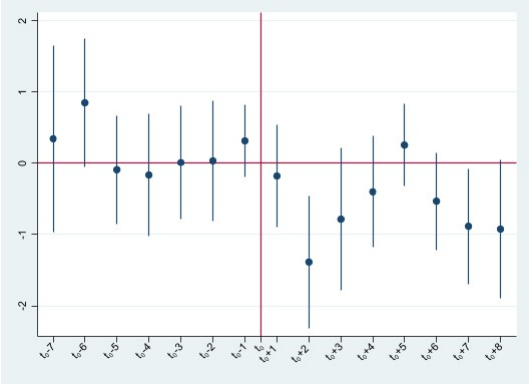
Notes. The graph reports coefficients and confidence intervals estimated according to eq. 2 by Poisson FE model. The dependent variable is the total number of attacks against politicians. We exclude from the sample of treatment units municipalities where the dissolution procedure was initiated owing to attacks against politicians in office. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 6 years before and 8 years after the dissolution. All regressions include municipality FE, year FE and population.

Figure B.5: Overlap in propensity scores in treated and matched samples



Notes. Distributions of the estimated propensity scores of dissolved municipalities for the treatment group (i.e. all municipalities put under compulsory administration; right-hand side) and the control group (i.e. the "nearest neighbor" of treated municipalities as derived from the matching procedure; left-hand side). 2010-2019.

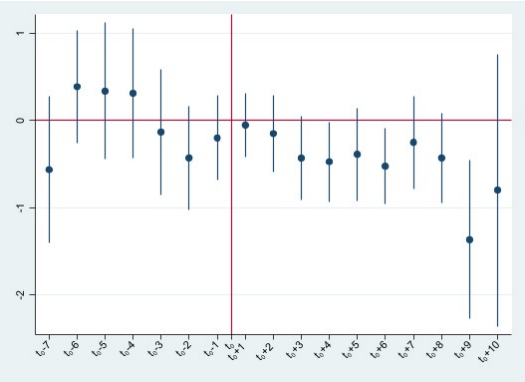
Figure B.6: Propensity score matching - Event study



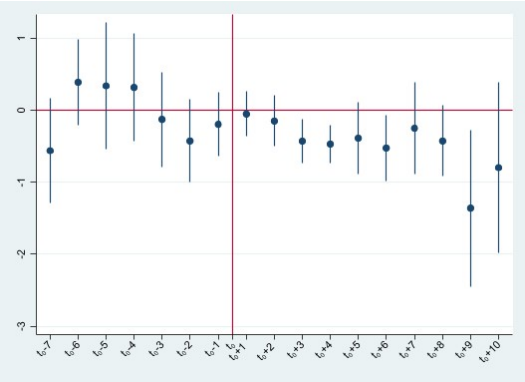
Notes. The graph reports coefficients and confidence intervals estimated according to eq. 2 by Poisson FE model. The dependent variable is the total number of attacks against politicians. Sample of treatment units excludes municipalities whose dissolution procedure was initiated owing to attacks against politicians in office. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 8 years after the dissolution. All regressions include municipality FE, year FE, and population.

Figure B.7: Spillovers - Event study

(a) *Poisson FE (municipality cluster)*

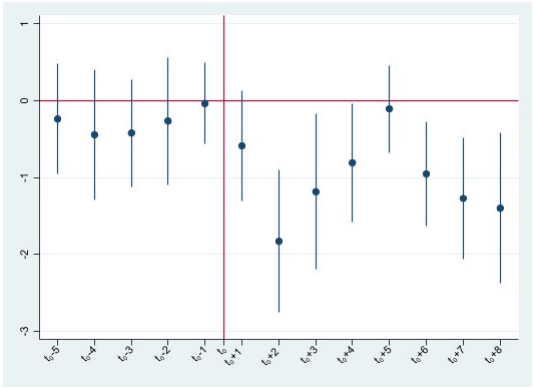


(b) *Poisson FE (province cluster)*



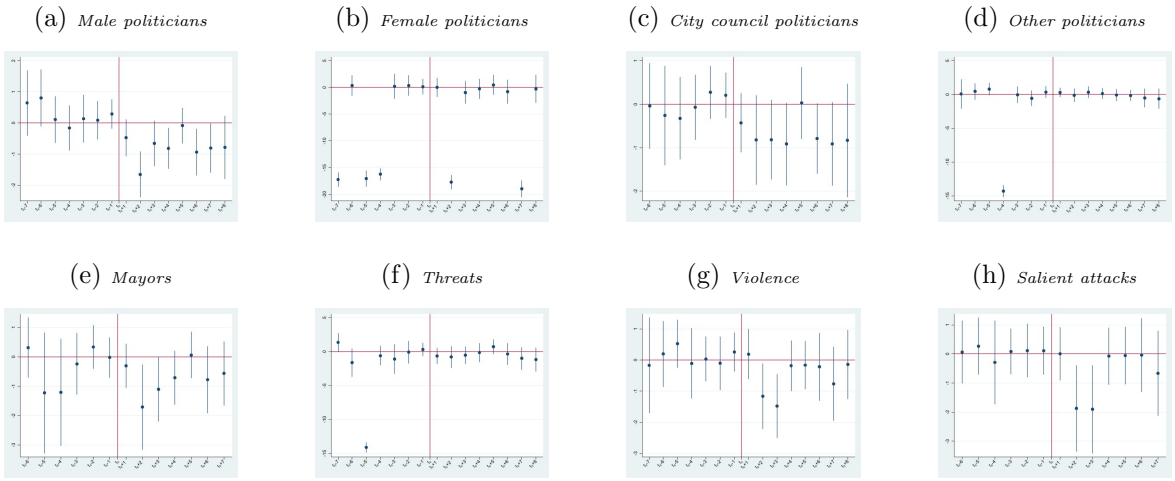
Note. The graphs report coefficients and confidence intervals estimated according to eq. 2. Graphs show the Poisson FE model estimates where the dependent variable is the total number attacks. Standard errors are clustered at municipal level in graph B.7a and at province level in graph B.7b. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 10 years after the dissolution. All regressions include municipality FE, year FE, and population. Period: 2010-2019.

Figure B.8: Excluding neighboring municipalities - Event study



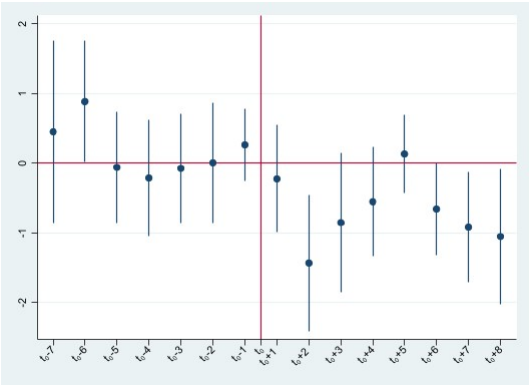
Notes. The graph reports coefficients and confidence intervals estimated according to eq. 2. Poisson FE model estimates, dependent variable is total number attacks. Control group excludes municipalities neighboring dissolved municipalities. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 5 years before and 8 years after the dissolution. All regressions include municipality FE, year FE, and population. Period: 2010-2019.

Figure B.9: Types of politician and attack - Event study



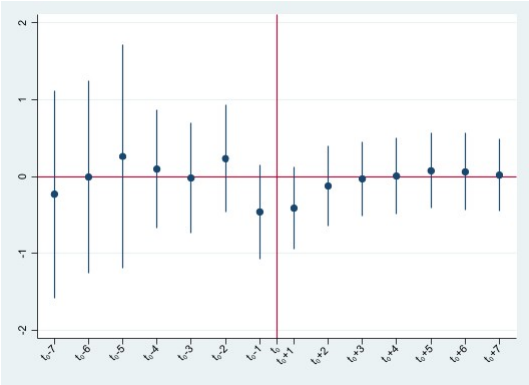
Note. The graphs report coefficients and confidence intervals estimated according to eq. 2 by Poisson FE model. Graph B.9a shows estimates when dependent variable is total number of attacks against male politicians. Graph B.9b: dependent variable is total number of attacks against female politicians. Graph B.9c: total attacks against city council politicians. Graph B.9d: total attacks against other politicians. Graph B.9e: dependent variable is total number of threats against mayors. Graph B.9f shows estimates when the dependent variable is the total number of threats against politicians. Graph B.9g: dependent variable is total acts of violence against politicians. Graph B.9h: total number salient attacks against politicians. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 8 years after the dissolution. All regressions include municipality FE, year FE, and population.

Figure B.10: Political party holding the administration - Event study



Notes. The graph reports coefficients and confidence intervals estimated according to eq. 2. Poisson FE model estimates where the dependent variable is the total number attacks. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 8 years after the dissolution. We control for dummies on mayor's party affiliation. All regressions include municipality FE, year FE, and population. Period: 2010-2019.

Figure B.11: Dissolutions unrelated to mafia infiltration - Event study



Notes. The graph reports coefficients and confidence intervals estimated according to eq. 2. Poisson FE model estimates where the dependent variable is the total number attacks. Standard errors are clustered at municipal level. Dots refer to point estimates, spikes to 95% confidence intervals. The omitted category is the year of dissolution (t_0). We include event-time dummy variables for 7 years before and 7 years after the dissolution. All regressions include municipality FE, year FE, and population. Period: 2010-2019.

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Fondazione Eni Enrico Mattei
Corso Magenta 63, Milano – Italia

Tel. +39 02 403 36934

E-mail: letter@feem.it
www.feem.it

Fondazione Eni Enrico Mattei