

Cingano, Federico; Tonello, Marco

Working Paper

Law enforcement, social control and organized crime.
Evidence from local government dismissals in Italy

GLO Discussion Paper, No. 458

Provided in Cooperation with:

Global Labor Organization (GLO)

Suggested Citation: Cingano, Federico; Tonello, Marco (2020) : Law enforcement, social control and organized crime. Evidence from local government dismissals in Italy, GLO Discussion Paper, No. 458, Global Labor Organization (GLO), Essen

This Version is available at:

<https://hdl.handle.net/10419/213325>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Law enforcement, social control and organized crime.

Evidence from local government dismissals in Italy*

Federico Cingano[†]

Marco Tonello[‡]

January 13, 2020

Abstract

Local governments suspected of Mafia infiltration can be dismissed in Italy through an administrative act not increasing formal deterrence but potentially signaling improved law enforcement among local communities. This paper finds that dismissals are associated to a persistent fall of petty crimes (e.g. thefts) but have no consequences on offenses more closely related to the activity of organized crime, as homicide, extortion, drug-trafficking or usury. Petty crimes are estimated to fall by around 10%, on average, a result that seems driven by the perception of enhanced deterrence (through media pressure, the signaling role of the policy, and other forms of social control) rather than induced by organized crime itself.

JEL Classification: K14, K42, D73

Keywords: crime, law enforcement, organized crime, social control

*We thank seminar participants at 14th Brucchi Luchino Labor Economics Workshop, Workshop on Quantitative Methods to Combat Organized Crime (Bocconi University and FIU–Bank of Italy), Workshop on Social Economy for Young Economists (University of Bologna), Catholic University Milan, Bank of Italy. We thank Magda Bianco, Paolo Buonanno, Maria De Paola, Vernon Henderson, Randi Hjaltmarsson, Ola Olsson, Paolo Pinotti, Giovanni Prarolo, Lucia Rizzica, Giulio Zanella for helpful discussions and suggestions. We are especially grateful to Raffaele Cantone for useful suggestions. Stefano Marucci provided excellent research assistance. The views expressed in this paper are those of the authors and do not necessarily reflect those of the institutions they belong to. The usual disclaimers apply.

[†]Bank of Italy, Directorate General for Economics, Statistics and Research, Structural Economic Analysis Directorate, Labour Market Division. E-mail: federico.cingano@bancaditalia.it.

[‡]Bank of Italy, Economic Research Unit, Firenze Branch & CRELI-Università Cattolica di Milano (**corresponding author**). E-mail: marco.tonello@bancaditalia.it; address: Via dell’Oriuolo 37/39, 50122 Firenze (Italy).

1 Introduction

The presence of organized crime is detrimental for local economic outcomes as growth and productivity, the allocation of public spending, corruption, let alone public safety and law offenses ([Barone and Narciso 2015](#); [Pinotti 2015](#)). And yet there is limited empirical evidence on the effectiveness of policies aimed at fighting organized crime, especially in the territories where its presence is pervasive.¹ In this paper we estimate the consequences for crime of an Italian policy imposing the dismissal of local administrations suspected of Mafia infiltration (a measure we label *local government dismissals* - LGDs). Introduced in the early 1990s as an unconventional policy tool to combat the sharp increase in the activity of organized crime in the South of Italy, LGDs have since then been implemented in dozens of cases ([Priolo 2004](#)).

Are LGDs effective in lowering crime? The measure implies the dismissed administration is replaced by an external commission which governs the municipality until new elections, typically after around two years. Hence, it induces a sharp (though possibly temporary) increase in law enforcement in territories where the presence of the state is perceived as extremely weak. Being largely an administrative act, however, LGDs do not imply the strengthening of formal deterrence, as increased police deployment or financial resources allocated to public order. In addressing the above question we therefore adopt a broad perspective looking at alternative potential channels. The first is that, by breaking its ties with local politicians, dismissals have a direct effect on the presence of mafia and its local criminal activity. Alternatively, LGDs might more generally affect local crime by inducing the perception of greater enforcement if - as posited by influential theories since [Sah \(1991\)](#) - perceptions matter for deterrence.

Quantifying the consequences of LGDs on crime is challenging given that Mafia-infiltrated municipalities may feature peculiar patterns of criminal offenses. For example, if dismissals occur as a consequence of high levels of crime then simple comparisons with other municipalities would likely over-emphasize their effectiveness. Absent suitable instruments, our analysis leverages on newly available time series of municipality level data on a detailed list of criminal offenses. This allows us to exploit within-municipality variation, comparing changes in crime rates around the intervention net of time-invariant differences in crime between municipalities. Because dismissals might still occur as a consequence of spikes in crime, we will extend the analysis to the immediate neighborhood of the dismissed municipalities, often excluding the latter from the sample. Identification arises from contrasting changes in crime

¹Forms of organized crime are not only diffused in Italy, but also in Russia, Japan and South America. On top of that, organized crime builds criminal and economic connections which extend far beyond the territories where they originally settled ([Lupo 1993](#); [Mastrobuoni 2015](#)).

in (neighbors of) a dismissed municipality with contemporaneous changes in (neighbors of) municipalities that are not dismissed in the same year. Finally, we exploit the fact that reverse causality should be more of a concern for mafia-related crime (homicide, threat, extortion, arson, drug-trafficking, usury, etc.) and examine them separately from *petty* crime, including sexual offenses and thefts. Preliminary inspections of the data reassuringly show that in treated areas the patterns of petty crime are fairly stable prior to the intervention and do not anticipate the dismissal. They do significantly drop in its aftermath, though.

We find that LGDs are associated with a significant and persistent fall in minor (petty) crimes, whose incidence is estimated to decrease by 9% on impact, and by 12.4% in the years following the dismissal. The fall in crime is detected irrespective of the dismissed municipality being included in the sample, and is robust to several sensitivity checks. Its geographical scope seems limited though, as dismissals do not affect crime rates beyond the municipality’s immediate neighbors. Our findings imply social gains from lower petty crimes ranging between 100 and 230k Euro per dismissal, depending on the specification and discount rate. By contrast, dismissals do not prove effective in reducing mafia-related (power and enterprise syndicate) offenses, implying little direct effects of the policy on the activity of organized crime.

Such heterogeneous responses suggest the fall in minor offenses should not be ascribed to a ‘lay-low’ strategy, whereas local crime syndicates command a generalized reduction of criminal activities following the intervention. With formal enforcement unlikely to be a relevant determinant either, we tentatively explore the role of perceived deterrence, as described for example by [Lochner \(2007\)](#). With LGDs, individual perceptions may change as a consequence of direct social learning (e.g. if friends, relatives, or affiliated to the same criminal organizations are caught), of indirect social learning (e.g. if some case determines a higher media pressure), or following changes in the surrounding environment (e.g. a greater urban order). Our results are consistent with forms of perception-based deterrence spurred by the signaling value of the policy or media attention. Moreover, LGDs look more effective where the endowment of civic capital is higher, consistently with social control theories ([Keizer et al. 2008](#)).

A large body of research studies the origins of Mafia-type organizations, their functioning and internal organization ([Bandiera 2003](#); [Buonanno et al. 2015](#); [Campaniello et al. 2016](#); [Dimico et al. 2017](#); [Mastrobuoni 2015](#)). Fewer works attempted to assess their economic consequences ([Barone and Narciso 2015](#); [Daniele and Marani 2011](#); [Mirenda et al. 2019](#); [Pinotti 2015](#)), and very little is known as to the effectiveness of policies attempting to increase law

enforcement in areas where organized crime is pervasive. We contribute suggesting that enforcement policies can successfully reduce the incidence of (minor) crime in Mafia-dominated regions, and trace the effect to perception-based mechanisms, whose effectiveness is not much investigated relative to formal enforcement. Other recent work focused on LGDs but evaluated their consequences on public spending (Acconcia et al. 2014; Cataldo and Mastroiocco 2019; Galletta 2017) and the quality of local politicians (Daniele and Geys 2015), rather than crime rates. More generally, this work contributes to the empirical literature evaluating the effects of increasing enforcement on local crime (Di Tella and Schargrodsky 2004; Machin and Marie 2011), although the specific policy considered does not imply an increase in actual deterrence (police deployment or additional transfers). By studying the consequences of LGD beyond the borders of the dismissed municipality, it also relates to empirical works on the cross-border effects of law enforcement (Bronars and Lott 1998; Dube et al. 2013; Gonzalez-Navarro 2013).

The paper is organized as follows. Section 2 presents the institutional setting. Section 3 describes the dataset, the variables construction and presents some descriptive statistics. Section 4 explains the empirical strategy, while Section 5 discusses the main results, presents the robustness checks, and provides an attempt to size the social benefits from the implementation of the policy. Section 6 discusses the possible mechanisms. Section 7 concludes and derives policy implications.

2 Institutional setting

In 1991, in response to a steep increase in the activity of organized crime in the South of Italy, the Italian Parliament passed a law establishing the possibility to dismiss the political body of a municipality (i.e. city council, major and governing council) in case of suspected or actual connections with criminal organizations.² Since its introduction in 1991, about 300 municipalities have been dismissed (Figure 1). Almost all the dismissals (278) took place in the three regions in Southern Italy where Mafia-type organizations were historically born and are still pervasive in the social, political and economic context (Campania, Calabria and Sicily).³ More than 3 millions citizens experienced at least one dismissal of the municipality where they reside because of Mafia infiltration: this figure corresponds to almost 6.5% of the

²See Mete (2009) for detailed historical description of the conditions and events which lead to the approval of this law and its application in the last two decades. The prescription - previously contained in the law 221/1991 - is now included in the Article 143 of Law 267/2000 (the so-called *Code for Local Governments*). Henceforth, we simply refer to it as *local government dismissal* or LGD.

³The count excludes municipalities for which the dismissal was later canceled by the Administrative Judiciary Authority. Fifteen dismissals occurred in other regions in Southern Italy, eight in regions in the rest of the country.

overall Italian population, and almost 19% of the population of Southern Italy.

[Figure 1]

In his analysis of dismissals occurred from 1991 to 2006, [Mete \(2009\)](#) observed that these usually started following some resounding event (e.g. political intimidation during the elections, attacks involving the mayor or members of the city council), corruption scandals (e.g. in auctions for public provisions or in the management of public services), or as incidental follow-ups from other investigations on the activity of criminal organizations. Such information is collected by the police and transferred to the Prefects (the local representative of the Ministry of Interior) who decides whether to start an investigation activity.⁴

The policy intervention is then implemented in two distinct steps. In a first phase, a team of external commissioners is appointed and sent to the municipality to proceed with formal investigations. At this stage, the existence of suspects of connections between Mafia-type organizations and the local government becomes publicly known. On the ground of the evidence collected, the decision to dismiss the local government is taken by the central government, and formalized by issuing an *ad hoc* Decree.⁵ We label this first step as the *investigation and dismissal phase*. In the second phase, three external commissioners appointed by the central government replace the dismissed local government and administrate the municipality for a minimum period of 18 months to a maximum of 24 (i.e. the *compulsory administration phase*), after which new elections take place. The compulsory administration aims at re-establishing the normal conditions of legality and public safety, the provision of public services, licensing, sectoral regulation and the collection of taxes. The presence of the police force is not increased during the compulsory administration ([Cavaliere 2004](#); [Mete 2009](#)).⁶

The implementation of this policy tool does not hinge upon a constant supervision by the police forces on all governing bodies in territories heavily influenced by the presence of

⁴In the period between 1991 and 2006, [Mete \(2009\)](#) documents that the reasons of the dismissals encompassed three main areas: (i) direct responsibility of the administrators, such as direct links with notorious criminals, legal prosecutions against administrators because of Mafia-related crimes, electoral support from criminal organizations, but also violent crimes suffered by administrators (i.e. extortions, threats, murders, arsons); (ii) direct or indirect links between the public employees (managers, bureaucrats, technical staff) and notorious criminals; (iii) illicit behaviors directly or indirectly linked to criminal organizations in business or administrative activities managed by the municipality, such as public auctions for service provision and outsourcing, building and garbage sector regulation, administrative permits.

⁵The Decree of dismissal is formally issued by the President of the Republic, and it usually contains a short report with some broad description of the reasons why the dismissal was decided. We use these reports to collect some information later exploited in the empirical analysis, such as whether the dismissal occurred with some persons arrested or not. In some cases, for investigation reasons, the report is not released.

⁶Additional information on the policy can be also found in [Acconcia et al. \(2014\)](#) and [Daniele and Geys \(2015\)](#). The former paper exploits LGD as an exogenous shock to public local spending, the latter evaluates the effects of the policy on the quality of the new politicians elected after the compulsory administration phase. [Galletta \(2017\)](#) also finds that LGDs have potential effects on municipality public finance.

organize crime. The police forces and the judicial authority are rather subject to stringent budget constraints which lead them to concentrate the investigation activities in cases in which the evidence for the dismissal are particularly resounding or when evidence of organized crime influence in the local governments is incidentally found. In the sample considered in the empirical analysis, the majority of the LGDs occurred because evidence of links between the local government and organized crime were incidentally found in investigations related public tenders run by the municipality. While in Italy, for public security reasons, it is not possible to obtain information on the deployment of police forces on the territory, all the works describing the establishment and the effects of LGDs show that starting from the mid 2000's, LGD has become a purely administrative act, thus not followed by an increase of control in the territory established by additional police forces (Cavaliere 2004; Mete 2009). This was also confirmed by conversations with former commissioners, judicial authorities, and police officials.

3 Data and descriptive statistics

3.1 Data and variables definition

We exploit the *Investigation System Database* (ISD), a newly available dataset containing information on a detailed list of offenses committed in all Italian municipalities on yearly basis, from 2004 to 2011. The ISD draws directly from the IT system used for investigation activities by the police, and it is collected and managed by the Italian Ministry of Interior.⁷ The number of offenses in each municipality includes those directly discovered by the police in its day-by-day investigation activity, as well as those reported to the police by citizens, or by the judiciary authority starting an investigation.

The dependent variables (C_{it}^j) are constructed as the sum of the offenses reported in each municipality (i) and year (t), for each offense category (j). We draw from the social science literature and exploit the offense categories based on the distinction among offenses linked to the *power syndicate*, *enterprise syndicate* of criminal organizations, and *petty crimes* (Block 1980; Mete 2009). In fact, the standard distinction between violent, property and economic-related crimes might not be suitable to study local crime dynamics in the areas with a pervasive and persistent presence of organized crime (Lupo 1993). Besides, the use of this categorization is new in the economic literature and offers interesting insights to the

⁷An anonymized version of the ISD is used by the Bank of Italy for research purposes on the basis of a joint agreement with Italian Ministry of Interior. ISD records are generally less spoiled by under-reporting issues as they contain offenses reported to the police by all possible sources (citizens, judiciary authorities and all police forces that operate on the Italian territory).

analysis.⁸ *Power syndicate* encompasses offenses which appear to be strongly linked to the power of organized crime associations to control the territory. For example, this category includes murderers, attacks, assaults, extortions, arsons. *Enterprise syndicate* includes offenses connected with the business of organized criminal associations, such as drug related crimes, prostitution, usury and smuggling. These are also offenses not necessarily linked to the territory where Mafia type organizations are historically settled. The sum of power and enterprise syndicate crimes constitutes what we define to as *Mafia-related* crimes. The *Petty* crimes category collects criminal acts that are not under the monopolistic control of organized crime and could be also linked to criminal behavior outside organized crime (e.g. thefts, auto thefts, sexual offenses).

Additional socio-economic and demographic information is obtained from several sources: the *Demographic Database* (ISTAT Demos) from the Italian National Bureau of Statistics, which includes information on the population resident in Italian municipalities on yearly basis; the *Public Finance Database* (PFDB), containing information on personal taxable income at the municipality level; the *Public Finance Reports* (PFR) collected by the Italian National Bureau of Statistics (ISTAT) and the Ministry of Interior, which contain information on dismissals. These data are linked to the ISD by year and municipality identifier so to build a panel dataset with time-variant measures of criminal activity, population and income dynamics at the municipality level for eight years (2004–2011). Finally, we collect additional information on the dismissals taken from the official Decrees (whenever available), and data from Google Trends to proxy for the media attention received by each dismissal (see Appendix B).

3.2 Descriptive statistics

We focus on the 55 dismissals of local governments occurred between 2004 and 2011 located in Southern Italy (Sicily, Campania and Calabria) and Lazio (see Figure 1).⁹ We identify 204 *neighboring municipalities*, those sharing at least one border with each dismissed municipality. All units are observed for eight years (so that $N = 1,632$).¹⁰

⁸The detailed list of offenses and a comparison between the standard distinction among property, violent and economic-related offenses, and the organized crime categories is shown in the Appendix Table A1. For instance, arson is a typical crime against property, which, however, in territories where organized crime is pervasive, is generally used as an intimidation tool to gain and maintain the control of the territory.

⁹The ISD data availability determines the sample. We do not consider dismissals of hospital government bodies (3), dismissals canceled by the Judicial Administrative Authority (6), and one dismissal in the North of Italy (Liguria). Five investigations did not lead to a dismissal, but could not be exploited in validation exercises as they all occurred at the end of our observational period (in 2010 and 2011). One such cases (Pompei) is included in our final sample (as neighbor), but this turns out to be immaterial for the results.

¹⁰When a municipality is neighboring to more than one dismissed municipality, the first dismissal is attributed. In the case in which the same municipality is subject to two or three contemporaneous dismissals,

[Table 1]

Table 1 contains descriptive statistics on the dependent variables (C_{it}^j) and the set of the control variables, distinguishing between the subsamples of neighboring and dismissed municipalities. For descriptive purposes we also compute the crime rates (i.e. crime counts by 1,000 resident population), which are easier to interpret. In both samples, petty crimes account for a substantial part of the local crime dynamics: we observe on average 12 petty crimes every 1,000 inhabitants, 9 enterprise syndicate crimes, and 1 power syndicate crime. These figures are quite similar across neighboring and dismissed municipalities, thus confirming the local crime dynamics do not differ substantially among the two groups: we further investigate this aspect in the next section. The set of control variables includes the share of male aged 15 to 24, the share of male aged 25 to 29, the average personal taxable income and the share of households below a poverty threshold. Figure 2 shows the distribution of the crime counts, by offense categories: following a count data generating process, the variables are highly left skewed with a non-negligible mass of zeros (Osgood 2000).

[Figure 2]

4 Empirical strategy

4.1 Empirical specification

One obvious concern when assessing the crime consequences of LGDs is that dismissed municipalities might differ from other municipalities in some fundamental and unobserved way. Put simply, the intervention might be a consequence of high levels of, or spikes in, Mafia-related, violent crimes which would not be observed in comparison groups. The empirical approach attempts to account for this major threat to identification in several complementary ways.

First, estimates will absorb time-invariant differences in crime with municipality-fixed-effects exploiting within-municipality variation in crimes over time. Second, the analysis allows the impact of LGDs to potentially spread beyond the borders of the dismissed municipality, and extend to the sample of neighbors (around 4 municipalities per dismissal, on average). In fact, to limit identification concerns our core results will be obtained excluding the dismissed from the sample. Hence, in the baseline specification identification arises from contrasting changes in crime in (neighbors of) a dismissed municipality with contemporaneous changes in (neighbors of) municipalities that are not dismissed in the same year. The

they are replicated. The the final dataset counts of 7 replications; results are robust to their exclusion (see section 5.1). If a municipality is both dismissed and neighboring, it is dropped from the the sample of the neighboring municipalities.

results will be subject to standard robustness and falsification tests. Third, the analysis will separately focus on three typologies of crime: power syndicate crimes, enterprise syndicate crimes, and petty crimes. The presumption is that while dismissals might be triggered by spikes in Mafia-related crimes (as murders, extortions, arsons, usury, drug related crimes, etc. which fall in the first two categories) they are much less likely to be correlated to the local dynamic of petty crimes (theft and sexual offenses). Indirect supportive evidence to this hypothesis will be discussed in the next subsection.

Taking also into account the count data nature of the dependent variables (C_{it}^j), the reduced form estimating equation is:

$$E[C_{it}^j] = \exp(\beta_1 Inst_{it} + \beta_2 Post_{it} + X'_{it}\alpha + \lambda \ln(Pop_{it}) + \gamma_i + \delta_y) \quad (1)$$

where C_{it}^j is the count of crimes of category j in municipality i , year t . Years t are centered on the year of the dismissal ($t = D$).¹¹ The specification allows the impact of the policy on crime to vary over time: $Inst_{it}$, is an indicator equal to one if $t = [I; D]$, thus measuring the consequences of *Inspection and dismissal*; $Post_{it}$ is a dummy variable capturing the *Post-dismissal* effects (i.e. covering the period of, and subsequent to, the compulsory administration, $t \geq 1$). Vector X'_{it} includes relevant time-variant characteristics (e.g. population characteristics and wealth in municipality i), while the (natural) logarithm of the population ($\ln(Pop_{it})$) is included as the standard exposure variable in count-data models (Cameron and Trivedi 2013). Finally, γ_i are municipality fixed effects and δ_y are calendar year fixed effects capturing aggregate shocks to crime.

While the analysis will provide a broad set of results on the crime consequences of dismissals, estimates exploiting changes in the patterns of petty crimes in the immediate proximity of the dismissed municipality are the least likely to reflect spurious correlation of crime rates with the intervention. Conditional on the identification assumption, the coefficients of interest can be interpreted as intention-to-treat (ITT) parameters, and section 6 will discuss the relative importance of alternative potential underlying mechanisms. Finally, the analysis will consider alternative metrics of distance and testing for the displacement of criminal activities from the center to the periphery.

¹¹The timing variable t is defined such that: $t \in [-3; -1]$ and $t \in [1; 3]$ indicate, respectively, the years before and after the inspection and the dismissal phase, while $t = I$ and $t = D$ indicate, respectively, the year in which the investigation phase presumably started and the year in which the formal dismissal decree is issued.

4.2 Preliminary evidence

The empirical setting allows quantifying the consequences of LGDs on crime if crime rates are orthogonal to the dismissal. It is therefore important to provide supportive evidence of this assumption. To this purpose, in Figure 3 we plotted the pre-intervention patterns of crime rates of dismissed municipalities (and their neighbors). Specifically, we grouped municipalities by dismissal year and computed the average crime rate of each group and year, focusing on the period before the inspection ($t = I$), which is potentially part of the treatment). In Panel A, for example, the yellow line plots the patterns of petty crimes for municipalities dismissed in 2010, in the period up to 2008. Similarly, the green line plots crimes for municipalities dismissed in 2009.¹² Panel B replicates the analysis for the case of mafia-related crimes. The pictures show that, when they overlap, the average patterns of crime rates before the interventions appear reasonably parallel, in particular starting in 2005.

[Figure 3]

Appendix Figure A1 deals with another potentially relevant source of confounds, showing that dismissals are not systematically related to a specific period of the year (seasonality) or to other relevant events, in particular the occurrence of local elections.

We also perform tests of spatial autocorrelation in the crime rates of the dismissed municipalities and its neighbors in the years before the LGD (see Appendix C for a detailed discussion of the approach and results). The null of spatial autocorrelation can be rejected for all dismissed municipalities in the case of petty crimes, and for most (all but 1 to 9 cases, depending on the test chosen) in the case of power and enterprise syndicate crimes. Our core findings are, however, robust to the exclusion of such municipalities.

5 Estimated consequences of dismissals on local crime

5.1 Baseline results on alternative typologies of crime

We estimate Equation (1) using Quasi-Maximum Likelihood (QML) methods for panel fixed-effects Poisson estimators (Cameron and Trivedi 2013; Osgood 2000), while in the robustness checks we also test alternative specifications. As $Inst_{it}$ and $Post_{it}$ are dummy variables, the corresponding estimated parameters from Poisson regressions (β_1 and β_2) have a straightforward interpretation as semi-elasticities.¹³

¹²For each set of municipalities, the graph plots simple averages of raw crime rates (with no partialling out of any variable). Dismissals occurred in the first two years (2004 and 2005) are not considered as they would have a too short pre-intervention window.

¹³More precisely, the percentage effect is given by $100 \times (e^{\hat{\beta}} - 1)$.

[Table 2]

Panel A of Table 2 reports our baseline estimates of the consequences of dismissals starting from the case of petty crimes (thefts, sexual offenses). Identification exploits within municipality variation and distinguishes immediate from delayed effects. Results in column (1) focus on neighboring municipalities, where the LGD has a statistically significant crime reducing effect: in the inspection and dismissal years petty crimes decrease by 9 percent relative to the pre-policy period; after dismissal they are more than 12 percent lower. The following two columns highlight that these effects are determined by the response of thefts (column 2). The last two columns extend the analysis to dismissed municipalities. The results suggest no major differences in the response pattern of crime relative to the neighboring areas (despite less precision in the estimate, see column 4). As a result, the estimated fall in petty crime does not change pooling the two groups of municipalities (column 5).

Panel B further distinguishes the post dismissal case into the compulsory administration phase (*Post-dismissal: short term*, $t = [1; 2]$), and the following years (*Post-dismissal: medium term*, $t \geq 3$). The estimates do not highlight any sizable difference between the two periods suggesting that the crime reducing effects from increased enforcement are not concentrated in the dismissal period or during the compulsory administration phase, but extend to the medium-term, when new elections take place.¹⁴

By contrast, government dismissals seem to have little consequences on the typical activities of organized crime. Table 3 summarizes the main findings when looking at Mafia-related crimes (power and enterprise syndicate crimes) in both dismissed and neighboring municipalities. This finding is confirmed if focusing on mafia-related crimes with little chance of going unreported (arsons and murders, see Appendix Table A2). Incidentally, this helps excluding the possibility that the result in Table 3 is the combination of a negative effect of dismissals on perpetrated crimes, and a positive effect on reporting (as would happen if the intervention weakened the code of silence known as *omertà*).¹⁵ This asymmetric response of different crime typologies is difficult to reconcile with the idea that organized crime syndicates can effectively determine all criminal activities in their territory, and are therefore the ultimate responsible of the observed decrease in, e.g., the incidence of thefts in dismissed and neighboring municipalities following the LGD.

¹⁴This result is somehow consistent to what found by Daniele and Geys (2015), who document an increase of the quality of politicians elected (as measured by their education level) after the compulsory administration phase.

¹⁵To further strengthen this case, Appendix Table A3 shows that that the level of mafia related crimes are not especially low (e.g due to under-reporting from *omertà*) before the dismissals: in fact they are higher than the national average.

[Table 3]

5.2 Robustness and falsification tests

Table 4 shows the results of several robustness checks for our baseline specification (i.e. column 1 in Table 2). The results in column 1 are obtained adding municipality-specific trends, thus exploiting deviations in crime rates from an underlying municipality-specific dynamic. This is a demanding specification given the short time span of the data, and indeed the estimated coefficients fall significantly relative to the baseline (a result that will be taken into account later in the analysis). Importantly, however, they remain statistically significant. The specification in column 2 accounts for potential spillover effects from crime in the dismissed municipality to the neighborhood. The results are not affected in this case. Columns 3 and 4 alter the underlying sample. Dropping duplicate observations, that is municipalities that happen to be neighbors of two dismissed at the same time, does not alter the core findings (column 3). In column 4, the sample is restricted to interventions occurred in the central years in the available time window (2005-2010) so to work with a similar number of observations before and after the dismissal. In this case, point estimates are slightly lower and less precisely estimated than in the baseline.

[Table 4]

Appendix Table A4 shows that the same results hold if the robustness checks are performed on the larger sample including the dismissed municipalities. It also reports robustness to changing estimation methods (again in both samples) and running a negative binomial, as opposed to the Poisson model. This check is meant to assess whether the Poisson standard errors are ‘too small’ due to over dispersion in the data (i.e. the conditional variance is larger than the conditional mean).¹⁶ While the point estimates can not be directly compared to those in the main text (Cameron and Trivedi 2013), the results confirm that dismissals induce a significant and persistent fall in petty crime following the policy intervention.

We also performed a falsification test looking at whether government dismissals *not* due to mafia infiltration end up affecting crime. Because they still imply the presence of technocrats, focusing on such cases test could help disentangle the sources of the deterrence effects. Similarly to other papers studying the same policy intervention, we focused on municipalities dismissed due to the mayor death for natural reasons, restricting to Southern Italian regions

¹⁶Negative binomial models are explicitly designed to handle over dispersion. Unlike Poisson models, however, they do not provide consistent estimates if the true data generating process is not distributed as a negative binomial (Cameron and Trivedi 2013). QML Poisson models are robust to distributional misspecification of the dependent variable and therefore usually preferred to other count data models.

for the sake of comparability. There are several advantages in focusing on mayor deaths. First, it is reasonable to assume that (natural) deaths are exogenous to the crime dynamics of the municipality (i.e. the treatment can be considered as-good-as random). Second, the number of occurrences (65) is in line with the number of dismissals for Mafia reasons (55). Table 5 replicates our baseline regressions, showing no significant changes in the patterns of petty and mafia-related crimes around dismissals of this kind. This suggests that our baseline finding in section 5.1 is associated to the detection of illegal activities, rather than the presence of commissioners per se. The next session will explore some potentially relevant underlying channels in greater extent.

[Table 5]

5.3 Do LGDs displace crime?

Spill-over effects of law enforcement are not new in the literature. The positive impact of LGD on neighboring municipalities is in line, for example, with findings on compliance with tax payments following an increase in inspections (Rincke and Traxler 2011). However, highly mobile crimes can also be easily displaced if prevention efforts are not uniformly implemented over the territory (Bronars and Lott 1998; Dube et al. 2013; Gonzalez-Navarro 2013). If this is the case, the observed fall in crime in and around dismissed municipalities might not imply a net aggregate reduction if accompanied by an increase in crime in further away areas. The core analysis was therefore replicated looking at municipalities located at further and increasing distances from the dismissed. In almost all cases, neighboring municipalities fall within a radius of 20 Km from the dismissed. Here, the sample is composed of those located within non-overlapping rings (*annuli*) with inner circle of radius $d=\{15, 20, 25, \dots, 45\}$ and larger circle of radius $d+5$.¹⁷

[Figure 4]

The results are summarized in Figure 4, which plots the *Inspection and dismissal* effect ($\hat{\beta}_1$) in Panel A, and the *Post-dismissal* effect ($\hat{\beta}_2$) in Panel B. The estimated coefficients are negative and marginally significant only in the case of the smallest circle (with 15 km radius), with point estimates close to those obtained for neighboring municipalities and reported in Table 2. In no other case we estimate positive and significant coefficients, suggesting that

¹⁷Distance is calculated based on geodetic coordinates for each municipality (ISTAT 2009). Notice that, for this reason, the group of municipalities included in the inner ring might not perfectly overlap with that of the neighboring municipalities.

displacement effects are not present (or not strong enough to offset the crime reducing spillover from the intervention) outside the closest area around the treated unit.

5.4 Some implications for welfare

While tackling minor offenses is not the main motivation behind dismissals, it certainly contributes to their economic effectiveness. A simple measure allowing to quantify the gains, in terms of social benefits (SB), from the intervention can be obtained as in [Machin and Marie \(2011\)](#): $SB = [(\hat{\beta}_1 + (1 + d)^{-1}\hat{\beta}_2) \cdot \bar{K}_{pre} \cdot USC]$, where $\hat{\beta}$ are the estimated policy impacts (Table 2, column 1), \bar{K}_{pre} is the average number of recorded offenses in the pre-policy periods, USC indicates the unitary social cost of petty crime (e.g. a theft) and d is a discount rate of the post-dismissal benefits.¹⁸ The unitary social cost for petty crimes (USC) is taken from [Detotto and Vannini \(2010\)](#), who produced detailed estimates for each of the offenses available in the ISD data underlying the present analysis. Clearly, this calculation does not allow for a full welfare assessment of dismissals, which should factor in the potential social benefits from the improved quality of local administration (e.g. public expenditure, tax collection, law enforcement) and the social cost of the policy.¹⁹

We report the calculation of social benefits in the Appendix Table A5 (figures are expressed in 2007 euros). Based on the baseline coefficients in Table 2 column 1 a dismissal implies an average reduction of about 140 petty crimes (essentially, thefts). The (monetary) cost of each such crime, USC , amounts to about 7,650 euros. The social benefits of dismissals would amount, in this case, to between 231k and 187k euros. However, the more demanding specification in Table 4 column 1 (including municipality specific linear trends) yielded to smaller coefficients. Based on those estimates the gains from lower crime incidence following the intervention would amount to between 128k and 102k euros.²⁰

For reference, the above figures amount to between 0.73 and 0.58 times (based on the most conservative estimates) the social benefits from the implementation of a policy (the *Street Crime Initiative*, SCI) explicitly targeted to high-density street crime areas of England and Wales (see [Machin and Marie \(2011\)](#)). While the estimated effect of the policy on crime rates is quite similar to that obtained here, the difference is explained by the larger size of

¹⁸We consider both the value used in the cost-benefit analysis of structural investments in the European Union proposed by [Evans \(2007\)](#) (3.3 per cent) and one proposed specifically for Italy (3.8 per cent). The *rule of thumb* rate suggested by the EU Commission before 2008 (5 per cent) is also considered as an upper bound.

¹⁹No official estimate of the social cost of a dismissal is available, unfortunately, but this is likely to be small ([Mete 2009](#)). For one thing, the law establishes that the three commissioners salary should not exceed one third of the overall wage bill of the dismissed council. While investigations certainly entail a cost, LGDs do not imply the deployment of additional police forces.

²⁰[Heaton \(2010\)](#) also provides an estimate of the average cost of thefts, based on US data. His estimates (58,400 euros) are considerably larger than those of [Detotto and Vannini \(2010\)](#), and would imply an aggregate benefit from LGD of about 780k euros, even using the conservative estimates of Table 4 column 1.

the population (and number of crimes) targeted by the SCI relative to the LGDs.

6 Potential channels

What explains the sizable reduction in petty crime following dismissals? As discussed in section 2, the policy does not imply an increase in actual deterrence, for example through higher sanctions, transfers or police deployment. Several alternative channels remain, however, whose relative importance can be indirectly tested.

Since Sah (1991) and Lochner (2007) it is known that, even absent changes in actual or formal deterrence, criminal behavior can be influenced by the perceived certainty or severity of punishment (perceived deterrence). Absent direct measures, one can test for heterogeneity in the effects of LGDs along dimensions that are plausibly correlated with the perception of law enforcement and the presence of the state among the the local population. One such dimension is media pressure, an important determinant of perceptions according to modern criminology (Jewkes 2010; Surette 2011). Variation in the degree of media attention across different dismissals can be measured through the Google Trend Index, which is available at the monthly and municipality level.²¹ Another plausible correlate of perceived deterrence across dismissals is the occurrence of arrests during the inspection and dismissal phase (which are reported in the formal Decree issued upon the dismissal, see section 3).

Figure 5 summarizes the results obtained running the baseline petty-crime regression on the two subsamples of municipality featuring, respectively, high and low intensity of each characteristic.²² The first panel clearly shows that the crime reducing effect of dismissals is statistically significant only when these attracted a lot of attention by the media (these coefficients are plotted as diamonds, with blue indicating the instantaneous effect (β_1), and red the subsequent effect (β_2)). While less clear-cut, the evidence on arrests is also consistent with perceptions-based deterrence channels.²³

[Figure 5]

The third panel tests whether the crime decreasing effects are higher in municipalities with higher *civic capital* endowment.²⁴ This should be the case if, as argued by many influential

²¹Technical details on the GTI construction and validation are contained in the Appendix B.

²²The detailed estimates are shown in the Appendix Tables A6, which also reports additional robustness checks using alternative measures. Notice that in the case of the arrests, we define as *High* the LGDs occurred with at least one arrested person, *Low* those occurred without arrests.

²³The reason why we do not detect a precise pattern in the post-dismissal phase may be due to the fact that we are able to construct the measure of LGDs occurred with arrests during the inspection or dismissal years, while we cannot retrieve adequate information on arrests in the post dismissal phase.

²⁴We define *civic capital* as ‘[...] those persistent and shared beliefs and values that help a group overcome the free rider problem in the pursuit of socially valuable activities’(Guiso et al. 2010).

theories, the consequences of increased (actual or perceived) enforcement are stronger where institutions (in this case, informal institutions measured by civic capital) are of better quality (Keizer et al. 2008). The results strongly support this hypothesis.²⁵

One last explorable explanation for the crime reducing effects of LGD is organized crime controlling, and determining the intensity of all type of offenses in their territory. Rather than a reaction to the increased law enforcement, the fall in the incidence of minor crime might just be a consequence of a Mafia-determined ‘lay-low’ strategy. Section 4 already discussed that this hypothesis is somehow inconsistent with the observed patterns of Mafia-related (power or enterprise syndicates) crimes, which, unlike minor crimes, do not experience any reduction in or around dismissed municipalities. Further evidence is provided in Table 6 which distinguishes municipalities based on the presence of immigrants (i.e. the share of immigrants in the population being higher or lower than the median in the sample). The idea is that, for reasons including their weaker links to the local community and their higher geographical mobility, immigrants (and their crime decisions) should be less likely to be influenced by organized crime. The results support this presumption, as petty crime falls only in municipalities with higher share of immigrants. Interestingly, Mafia-related crimes fall following the dismissal in municipalities where immigrant concentration is lower.

[Table 6]

7 Concluding remarks

This paper suggests that policies strengthening law enforcement mainly through perceptions-based deterrence or stimulating social control may have non-negligible effects on crime in territories characterized by the pervasive presence of criminal organizations. Our results show that the increase in law enforcement spurred by the dismissal determines a persistent reduction in petty crimes (e.g. thefts) in excess of 10%, on average. The consequences of the intervention are quantitatively similar in both the dismissed and neighboring municipalities, but no significant changes in the patterns of minor crime (in either direction) are observed at further distances. By contrast, dismissals do not have significant consequences on crimes that are more likely to be related to the activities of organized crime as murders, extortions, arsons, usury and drug related crimes.

²⁵In the Figure we make use of the measure of civicness taken from cheating in standardized test scores in primary schools (Guiso, Sapienza, and Zingales Guiso et al.; Paccagnella and Sestito 2014). The results are robust to the use of alternative measures of civic capital extensively used in the literature, such as the number of non-for profit organizations and electoral participation (see Appendix Table A6).

While not the main goal of the policy, the estimated reduction of minor crimes implies non-negligible social benefits which can be monetized in about 130 to 100k euro (in the more conservative specification). To reference this figure, we estimated it amounts to about two thirds the social benefits implied by a policy introduced in 2002 to counter petty crime in England and Wales ([Machin and Marie 2011](#)). This is of course also a consequence of LGDs being extremely cheap relative to alternative forms of deterrence ([Mete 2009](#)).

Administrative dismissals may impose other direct or indirect costs on local communities, and should obviously not be seen as panacea.²⁶ And yet, it seems important to be aware of the potential crime-reducing effect of policies strengthening law enforcement largely through perceptions-based deterrence or forms of social control ([Lochner 2007](#); [Sah 1991](#)). Moreover, we document the existence of positive spillovers in enforcement, which are not offset by the displacement of criminal activities. This aspect is rarely taken into consideration in the design and implementation of policies aimed at combating crime.

²⁶Some of the indirect effects of dismissals would nonetheless imply our exercises underestimate their crime impact. For example, LGDs are known to lower public investments both in the dismissed and in neighboring municipalities ([Galletta 2017](#)). And public expenditures seems negatively related to local crime ([Montolio 2018](#)).

References

- Acconcia, A., G. Corsetti, and S. Simonelli (2014). Mafia and public spending: Evidence on the fiscal multiplier from a quasi-experiment. *American Economic Review* 104(7).
- Bandiera, O. (2003). Land reform, the market for protection, and the origins of sicilian mafia: Theory and evidence. *Journal of Law, Economics and Organization* 19, 218–244.
- Barone, G. and G. Narciso (2015). Organized crime and business subsidies: Where does the money go? *Journal of Urban Economics* 86(C), 98–110.
- Bianchi, M., P. Buonanno, and P. Pinotti (2012). Do immigrants cause crime? *Journal of the European Economic Association* 10(6), 1318–1347.
- Block, A. (1980). *East side-West side. Organizing crime in New York, 1930-1950* (1 ed.). Cardiff: University College Cardiff Press.
- Bronars, S. G. and J. R. Lott (1998). Criminal deterrence, geographic spillovers, and the right to carry concealed handguns. *American Economic Review* 88(2), 475–479.
- Buonanno, P., R. Durante, G. Prarolo, and P. Vanin (2015). Poor institutions, rich mines: Resource curse in the origins of the sicilian mafia. *The Economic Journal* 125(586), F175–F202.
- Cameron, C. and P. Trivedi (2013). *Regression analysis of count data* (2 ed.). New York, NY: Cambridge University Press.
- Campaniello, N., R. Gray, and G. Mastrobuoni (2016). Returns to education in criminal organizations: Did going to college help Michael Corleone? *Economics of Education Review* 54, 242–258.
- Cataldo, M. D. and N. Mastrorocco (2019). Organised Crime, Captured Politicians and the Allocation of Public Resources. Trinity Economics Papers 1018, Trinity College Dublin, Department of Economics.
- Cavaliere, C. (2004). *Un vaso di coccio. Dai governi locali ai governi privati. Comuni sciolti per mafia e sistema politico istituzionale in Calabria* (1 ed.). Roma: Rubbettino.
- Choi, H. and H. Varian (2012). Predicting the present with Google trends. *Economic Record* 88, 2–9.

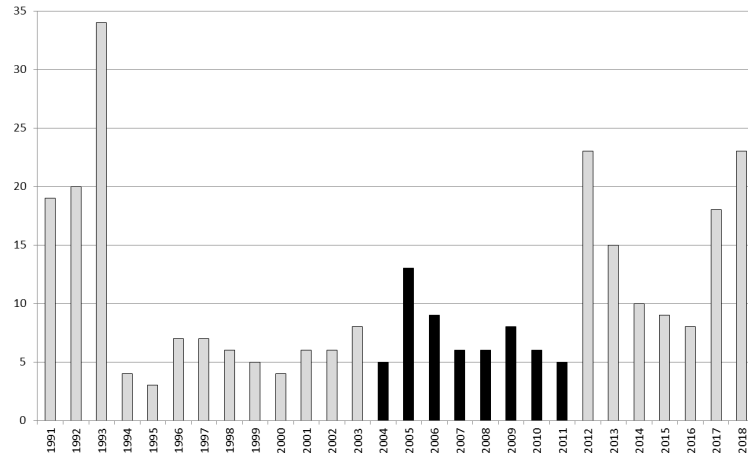
- D'Amuri, F. and J. Marcucci (2017). The predictive power of Google searches in forecasting US unemployment. *International Journal of Forecasting* 33(4), 801 – 816.
- Daniele, G. and B. Geys (2015). Organised Crime, Institutions and Political Quality: Empirical Evidence from Italian Municipalities. *The Economic Journal* 125(586), F233–F255.
- Daniele, V. and U. Marani (2011). Organized crime, the quality of local institutions and FDI in Italy: A panel data analysis. *European Journal of Political Economy* 27(1), 132–142.
- Detotto, C. and M. Vannini (2010). Counting the cost of crime in Italy. *Global Crime* 11(4), 421–435.
- Di Tella, R. and E. Schargrodsky (2004). Do Police Reduce Crime? Estimates Using the Allocation of Police Forces After a Terrorist Attack. *American Economic Review* 94(1), 115–133.
- Dimico, A., A. Isopi, and O. Olsson (2017). Origins of the Sicilian Mafia: The Market for Lemons. *The Journal of Economic History* 77(04), 1083–1115.
- Dube, A., O. Dube, and O. Garcia-Ponce (2013). Cross-Border Spillover: U.S. Gun Laws and Violence in Mexico. *American Political Science Review* 107(3), 397–417.
- Evans, D. (2007). Social discount rates for the European Union: New estimates. In M. Florio (Ed.), *CostBenefit Analysis and Incentives in Evaluation: The Structural Funds of the European Union*, pp. chapter 12. Cheltenham and Northampton, MA: Edward Elgar Publishing.
- Galletta, S. (2017). Law enforcement, municipal budgets and spillover effects: Evidence from a quasi-experiment in Italy. *Journal of Urban Economics* 101(C), 90–105.
- Gonzalez-Navarro, M. (2013). Deterrence and geographical externalities in auto theft. *American Economic Journal: Applied Economics* 5(4), 92–110.
- Guiso, L., P. Sapienza, and L. Zingales. Long-term persistence. *Journal of the European Economic Association* 14(6), 1401–1436.
- Guiso, L., P. Sapienza, and L. Zingales (2010). Civic capital as the missing link. In J. Benhabib, A. Bisin, and M. O. Jackson (Eds.), *Handbook of Social Economics*. Elsevier Science B.V.
- Heaton, P. (2010). What cost-of-crime research can tell us about investing in police. *RAND Center on Quality Policing Occasional Paper*.

- ISTAT (2009). *Atlante Statistico dei Comuni*. Roma: Istituto Nazionale di Statistica.
- Jewkes, Y. (2010). *Media and crime. Key approaches to criminology* (2 ed.). London, UK: SAGE Publications.
- Kearney, M. S. and P. B. Levine (2015). Media Influences on Social Outcomes: The Impact of MTV’s 16 and Pregnant on Teen Childbearing. *American Economic Review* 105(12), 3597–3632.
- Keizer, K., S. Lindenberg, and L. Steg (2008). The spreading of disorder. *Science* 322(5908), 1681–1685.
- Lochner, L. (2007). Individual perceptions of the criminal justice system. *American Economic Review* 97(1), 444–460.
- Lupo, S. (1993). *Storia della Mafia: dalle origini ai giorni nostri* (1 ed.). Roma: Donzelli.
- Machin, S. and O. Marie (2011). Crime and police resources: the street crime initiative. *Journal of the European Economic Association* 9(4), 678–701.
- Mastrobuoni, G. (2015). The Value of Connections: Evidence from the Italian-American Mafia. *The Economic Journal* 125(586), F256–F288.
- Mendes, D. A. and V. M. Mendes (2015). Parametric models in spatial econometrics: A survey. In P. Commendatore, S. Kayam, and I. Kubin (Eds.), *Complexity and Geographical Economics: Topics and Tools*. Springer.
- Mete, V. (2009). *Fuori dal comune. Lo scioglimento delle amministrazioni locali per infiltrazioni mafiose* (1 ed.). Roma: Bonanno.
- Mirenda, L., S. Mocetti, and L. Rizzica (2019). The real effects of ’Ndrangheta: firm-level evidence. Technical report, Bank of Italy. Mimeo.
- Montolio, D. (2018). The effects of local infrastructure investment on crime. *Labour Economics* 52, 210 – 230.
- Osgood, W. (2000). Poisson-based regression analysis of aggregate crime rates. *Journal of Quantitative Criminology* 16(1), 21–43.
- Paccagnella, M. and P. Sestito (2014). School cheating and social capital. *Education Economics* 22(4), 367–388.

- Pinotti, P. (2015). The economic costs of organised crime: Evidence from southern Italy. *The Economic Journal* 125(586), F203–F232.
- Priolo, G. (2004). La normativa di contrasto alle infiltrazioni mafiose negli enti locali con riferimento alla gestione commissariale. *Instrumenta* VII(22), 136–157.
- Rinke, J. and C. Traxler (2011). Enforcement spillover. *The Review of Economics and Statistics* 93(4), 1224–1234.
- Sah, R. K. (1991). Social osmosis and patterns of crime. *Journal of Political Economy* 99(6), 1272–1295.
- Surette, R. (2011). *Media, crime and criminal justice. Images, realities and policies* (4 ed.). Belmont, CA: Wadsworth.

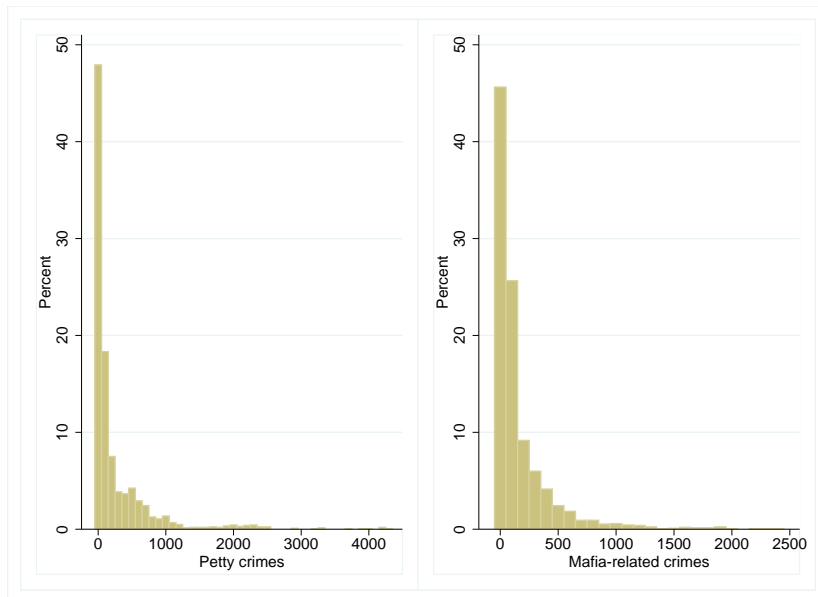
Figures

Figure 1
Number of Local Government Dismissals by year.



Notes. The bar chart indicates the number of Local Government Dismissals (LGD) occurred in each year, since the first implementation of the policy in 1991. The black bars highlight the years exploited in the empirical analysis (2004-2011). The year of dismissal refers to the year of the official publication of the Presidential Decree. **Source:** based on *The dataset of dismissed municipalities - AvvisoPubblico*. Last update: May 21, 2019.

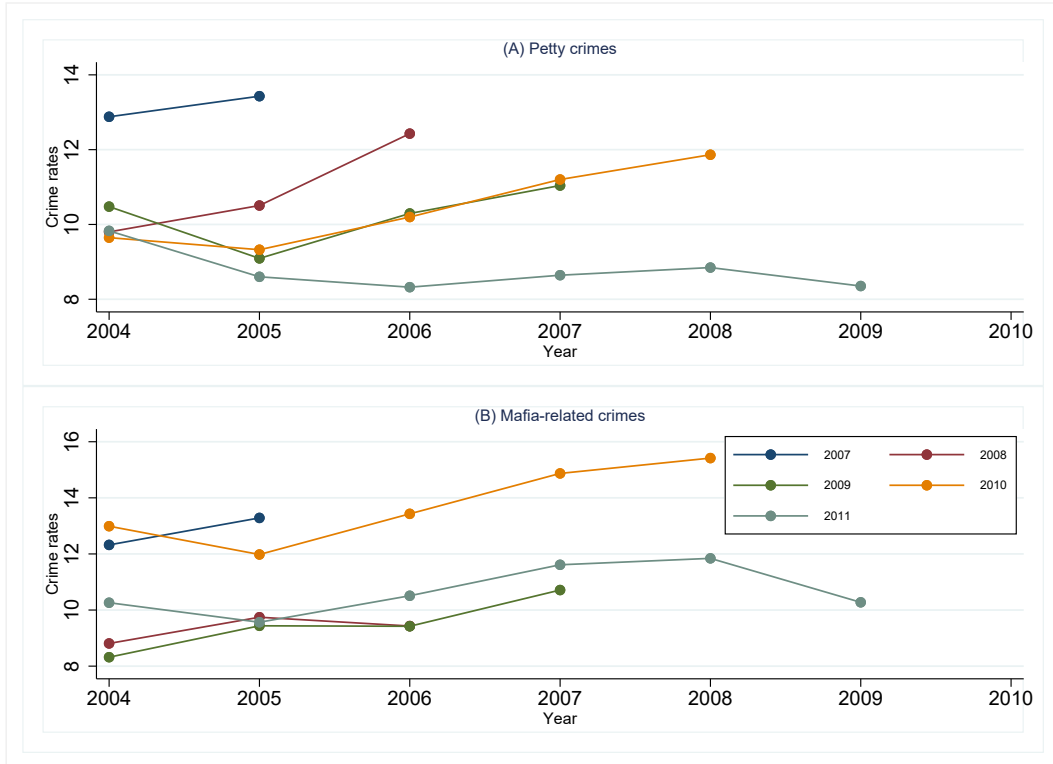
Figure 2
Distribution of crime counts by offense categories.



Notes. The histograms show the distribution of the crime counts by offense categories; outliers (defined as observations above the 99th percentile of each crime count distribution) are excluded from the plot. **Source:** based on ISD (2004-2011).

Figure 3

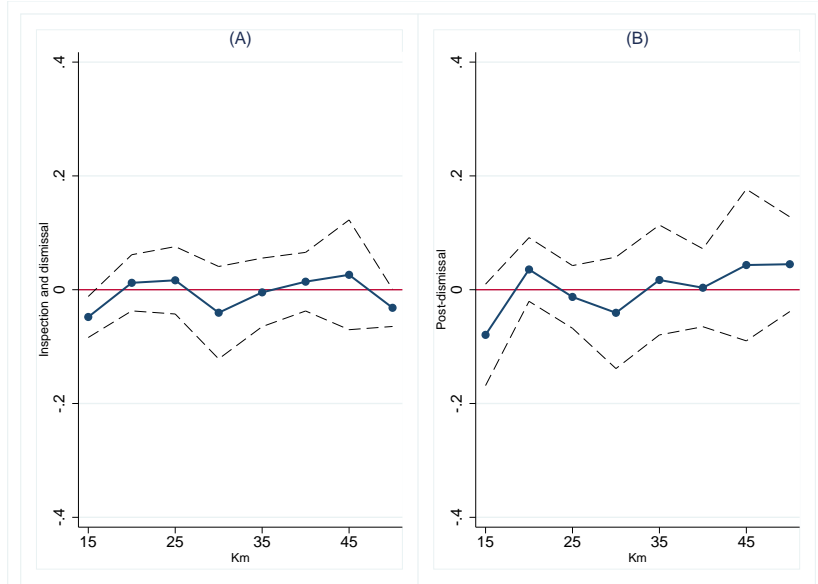
Petty and Mafia-related crime pre-trends for dismissed and neighboring municipalities.



Notes. The figure shows the average crime rates (Petty crime in Panel A and Mafia-related crime in Panel B) for neighboring and dismissed municipalities, grouped by the year of dismissal, in the years before the policy intervention (i.e., $t < I$). Dismissals occurred on 2004 and 2005 are excluded. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Figure 4

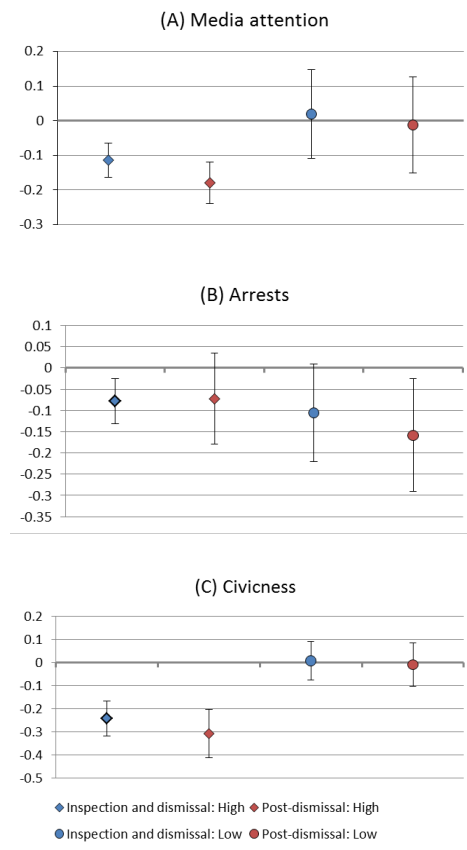
Displacement effects in petty crime.



Notes. The graphs show the estimated coefficients (blue dots) and the 90% confidence interval (dashed black lines) for the inspection and dismissal (Panel A, i.e. β_1 from Equation 1) and the post-dismissal effect (Panel B, i.e. β_2 from Equation 1) calculated using samples including municipalities at increasing distance from the dismissed one (i.e. annuli increasing their radius each time by 5 km) for petty crimes. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Figure 5

Potential channels: heterogeneous effects according to media attention, contextual arrests, civicness.



Notes. The graphs show the coefficients and 90% confidence intervals of the point estimates according to whether the neighboring municipality is characterized by a high or low level of media attention to the dismissal (Panel A), the dismissal occurred with (high formal deterrence) or without contextual arrests (low formal deterrence) (Panel B), the neighboring municipality is characterized by a high or low level of civicness (Panel C). Figure A refers to the estimates in Panel A columns 5 and 6 of Appendix Table A6, Figure B to the estimates in Panel B columns 3 and 4 of Appendix Table A6, Figure C to the estimates in Panel C columns 1 and 2 of Appendix Table A6. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database, Google Trends Index.

Tables

Table 1
Descriptive statistics for neighboring and dismissed municipalities.

	Neighboring			Dismissed		
	mean	sd	N	mean	sd	N
Panel A: Counts (dep. var.)						
<i>Petty crimes</i>	532.85	2784.95	1632	286.59	486.23	440
Thefts	531.62	2779.76	1632	285.91	485.39	440
Sexual offenses	1.23	6.29	1632	0.68	1.45	440
<i>Mafia-related crimes</i>	270.1	1141.22	1632	176.57	235.65	440
Power Syndicate	229.72	879.96	1632	155.67	212.24	440
Enterprise Syndicate	40.38	280.89	1632	20.9	30.1	440
Panel B: Rates (per 1000 inhabitants)						
<i>Petty crimes</i>	12.09	8.25	1632	12.64	7.18	440
Thefts	12.05	8.24	1632	12.6	7.16	440
Sexual offenses	0.05	0.14	1632	0.04	0.09	440
<i>Mafia-related crimes</i>	10.58	5.67	1632	10.84	5.17	440
Power Syndicate	9.69	5.34	1632	9.72	5.13	440
Enterprise Syndicate	0.89	1.13	1632	1.13	0.96	440
Panel C: Control variables						
Population	22555.37	83858.7	1632	16142.59	18864.82	440
Share of males aged 15-24	0.13	0.02	1632	0.14	0.02	440
Share of males aged 25-39	0.21	0.02	1632	0.22	0.02	440
Average personal taxable income	15633.65	2848.44	1632	15471.4	2496.51	440
Share of poor households	0.07	0.03	1632	0.07	0.03	440

Notes: *Petty crimes* include thefts and sexual offenses; *Mafia-related crimes* include *Power Syndicate* and *Enterprise Syndicate* crimes, as defined in section 3 and in Appendix Table A1. Rates per 1000 inhabitants (Panel B) are obtained dividing the corresponding counts by the resident population (Pop_{it}) and multiplying by 1,000. The share of poor households accounts for households with income below 5,000 euros per year. **Sources:** ISD (2004-2011), ISTAT-DEMOS, Ministry of Economics and Finance, Public Finance Database.

Table 2
Baseline results: LGD and petty crimes.

	(1)	(2)	(3)	(4)	(5)
	<i>Neighboring</i>			<i>Dismissed</i>	<i>Neighboring and dismissed</i>
	Petty	Thefts	Sexual offenses	Petty	Petty
<i>Panel A</i>					
Inspection and dismissal	-0.0944* (0.0515)	-0.0948* (0.0518)	0.1928 (0.1773)	-0.0951** (0.0474)	-0.0910** (0.0430)
Post-dismissal	-0.1328** (0.0604)	-0.1331** (0.0607)	0.2440 (0.2323)	-0.0795 (0.0888)	-0.1168** (0.0538)
<i>Panel B</i>					
Inspection and dismissal	-0.1026** (0.0460)	-0.1029** (0.0464)	0.1940 (0.1763)	-0.1261*** (0.0449)	-0.1037*** (0.0373)
Post-dismissal: short term	-0.1400** (0.0556)	-0.1402** (0.0559)	0.2453 (0.2317)	-0.1163 (0.0841)	-0.1285*** (0.0482)
Post-dismissal: medium term	-0.1566*** (0.0531)	-0.1567*** (0.0533)	0.2478 (0.2537)	-0.1777* (0.1035)	-0.1541*** (0.0452)
N	1632	1632	1144	440	2072
Year and municipality FE	✓	✓	✓	✓	✓
Municipality characteristics	✓	✓	✓	✓	✓

Notes. The dependent variables are municipality yearly counts (C_{it}) of petty crimes (columns 1, 4 and 5), thefts (column 2) and sexual offenses (column 3). The sum of thefts and sexual offenses corresponds to the petty crime category (see Appendix Table A1). The control variables include: the logarithm of resident population ($logpop$); the share of males aged between 15 and 24 ($male1524$), the share of males aged between 25 and 39 ($male2539$), the mean value of the personal taxable income in the municipality (PTI mean); the share of households with income below 5,000 euros ($poor$ households). *Panel A* contains estimates based on Equation 1; *Panel B* contains estimates based on Equation 1, in which the variable $Post_{it}$ is divided into two components: $Post_{it}^s$ equal to 1 for $t = [1; 2]$ (and zero otherwise) (short term effect), and $Post_{it}^m$ equal to 1 for $t \geq 3$ (and zero otherwise) (medium term effect). The estimates are obtained from Quasi-Maximum Likelihood Poisson fixed effects (FE) regressions, with robust standard errors clustered at the municipality level. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Table 3
Baseline results: LGD and Mafia-related crimes.

	(1)	(2)	(3)	(4)	(5)
	<i>Neighboring</i>			<i>Dismissed</i>	<i>Neighboring and dismissed</i>
	Mafia-related	Power	Enterprise	Mafia-related	Mafia-related
<i>Panel A</i>					
Inspection and dismissal	-0.0441 (0.0458)	-0.0565 (0.0527)	0.0329 (0.0656)	-0.0230 (0.0440)	-0.0428 (0.0382)
Post-dismissal	-0.0543 (0.0578)	-0.0706 (0.0668)	0.0269 (0.0725)	0.0399 (0.0763)	-0.0435 (0.0506)
<i>Panel B</i>					
Inspection and dismissal	-0.0553 (0.0404)	-0.0630 (0.0459)	0.0237 (0.0676)	-0.0617* (0.0323)	-0.0604* (0.0334)
Post-dismissal: short term	-0.0638 (0.0528)	-0.0761 (0.0609)	0.0193 (0.0747)	-0.0042 (0.0602)	-0.0589 (0.0457)
Post-dismissal: medium term	-0.0870* (0.0500)	-0.0897* (0.0537)	0.0007 (0.0929)	-0.0877 (0.0583)	-0.0954** (0.0434)
N	1632	1632	1584	440	2072
Year and municipality FE	✓	✓	✓	✓	✓
Municipality characteristics	✓	✓	✓	✓	✓

Notes. The dependent variables are municipality yearly counts (C_{it}) of Mafia-related crimes (columns 1, 4 and 5), power (column 2) and enterprise syndicate crimes (column 3). The sum of power and enterprise syndicate crimes corresponds to the Mafia-related crimes category (see Appendix Table A1). See Table 2 for the control variables and fixed effects included, and for the specifications. The estimates are obtained from Quasi-Maximum Likelihood Poisson fixed effects (FE) regressions, with robust standard errors clustered at the municipality level. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Table 4
Robustness and specification tests.

	(1)	(2)	(3)	(4)
<i>Panel A</i>				
Inspection and dismissal	-0.0522* (0.0268)	-0.0942* (0.0513)	-0.1004* (0.0526)	-0.0605 (0.0461)
Post-dismissal	-0.0769** (0.0329)	-0.1342** (0.0600)	-0.1474** (0.0642)	-0.0967* (0.0528)
<i>Panel B</i>				
Inspection and dismissal	-0.0575** (0.0268)	-0.1002** (0.0457)	-0.1226*** (0.0443)	-0.0704* (0.0420)
Post-dismissal: short term	-0.0821** (0.0334)	-0.1395** (0.0557)	-0.1746*** (0.0540)	-0.1017** (0.0515)
Post-dismissal: medium term	-0.1013** (0.0452)	-0.1518*** (0.0542)	-0.2149*** (0.0544)	-0.1285** (0.0513)
N	1632	1628	1576	1344
Year and municipality FE	✓	✓	✓	✓
Municipality characteristics	✓	✓	✓	✓
Specifications:				
(i) Municipality-specific trends	✓			
(ii) Add dismissed municipality crimes		✓		
(iii) Exclude replications			✓	
(iv) Dismissals occurred 2005 to 2010				✓

Notes. The dependent are municipality yearly counts (C_{it}) of petty crimes (baseline specification); see Table 2 for the control variables and fixed effects included. Specification (i) adds neighboring municipality specific year (linear) trends; specification (ii) adds as a control variable the petty crime count of the corresponding dismissed municipality; specification (iii) excludes the replications; specification (iv) excludes dismissals occurred in the year 2004 and 2011. Estimates obtained from QML Poisson fixed effects regressions, with robust standard errors clustered at the municipality level. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Table 5

Falsification test: dismissals occurred because of the (natural) death of the mayor.

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Neighboring and dismissed</i>					
	Petty	Thefts	Sex Off	Mafia-related	Power	Enterprise
<i>Death of the mayor:</i>						
Upon event	0.0045 (0.0271)	0.0046 (0.0272)	-0.0032 (0.1445)	-0.0243 (0.0309)	-0.0256 (0.0320)	0.0014 (0.0521)
Post-event	0.0056 (0.0466)	0.0056 (0.0467)	-0.0036 (0.2150)	-0.0425 (0.0513)	-0.0410 (0.0551)	-0.0458 (0.0769)
N	2135	2135	1113	2135	2135	2002

Notes. The dependent are municipality yearly counts (C_{it}) of crimes (as in the baseline specification); see Table 2 for the control variables and fixed effects included; 65 (natural) deaths of mayors are considered as source of dismissal, in the years from 2004 to 2010. As in the baseline analysis, the dummy *Upon event* includes two years (the year of the death and the following); the remaining years are included in the *Post event* dummy. The sample of municipalities is restricted to those in the South of Italy. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Table 6

Heterogeneous effects according to the share of non-native residents in the municipality.

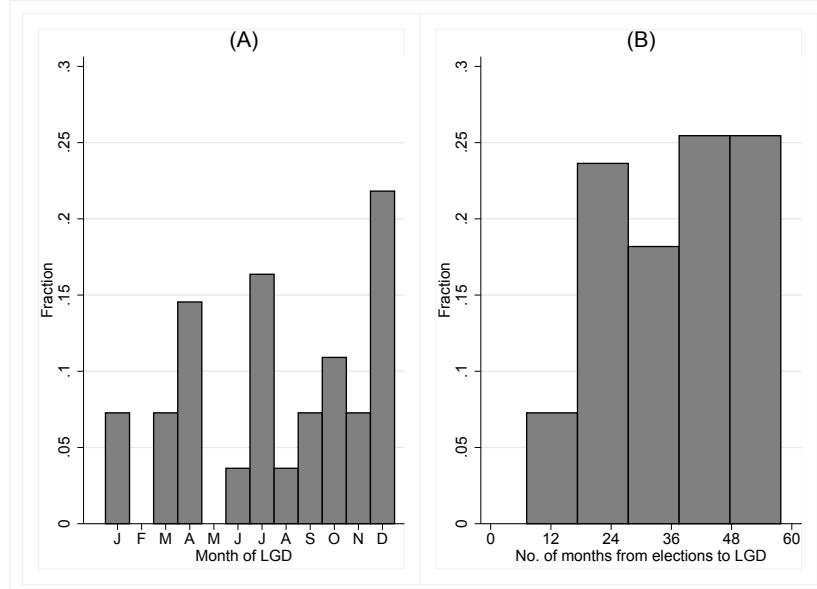
	(1)	(2)	(3)	(4)
	<i>Petty</i>		<i>Mafia-related</i>	
	High	Low	High	Low
<i>Panel A: neighboring</i>				
Inspection and dismissal	-0.1353*** (0.0431)	0.0618 (0.1238)	-0.0231 (0.0537)	-0.1147** (0.0545)
Post-dismissal	-0.1839*** (0.0560)	0.0470 (0.1199)	-0.0298 (0.0679)	-0.1571** (0.0652)
N	816	816	816	816
<i>Panel B: neighboring and dismissed</i>				
Inspection and dismissal	-0.1376*** (0.0392)	0.0179 (0.0767)	-0.0240 (0.0470)	-0.0825* (0.0475)
Post-dismissal	-0.1970*** (0.0527)	0.0695 (0.0794)	-0.0381 (0.0604)	-0.0672 (0.0684)
N	1000	1072	1000	1072
Municipality characteristics	✓	✓	✓	✓
Year and municipality FE	✓	✓	✓	✓

Notes. The dependent variables are Petty crimes counts (columns 1 and 2), Mafia-related crimes counts (columns 3 and 4). See Table 2 for the control variables and fixed effects included. *High* and *Low* refer to the subsamples of neighboring municipalities divided according to being above (*High*) or below (*Low*) the median of the share of male non-native residents over the total number of male residents in each municipality (Census 2001). Estimates obtained from QML Poisson fixed effects regressions, with robust standard errors clustered at the municipality level. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database, Census 2001.

Appendix A Additional Figures and Tables

Figure A1

Fraction of Local Government Dismissals by month of dismissal (A) and by distance (in months) from the local elections (B).



Notes. The histograms indicate the fraction of Local Government Dismissals (LGD) by calendar month in which the LGD took place (A), as derived from the formal Presidential Decree, and the fraction of LGDs by distance (in months) from the previous local elections (B); $N = 55$. **Source:** based on *The dataset of dismissed municipalities - AvvisoPubblico*. Last update: May 21, 2019.

Table A1
Offense categories definition.

	<i>Standard (S)</i>			<i>Organized crime (OC)</i>		
	Violent	Property	Economic	Power syndicate	Enterprise syndicate	Petty crimes
Attack	✓			✓		
Murder and assault	✓			✓		
Threat	✓			✓		
Robbery and extortion	✓			✓		
Kidnapping	✓			✓		
Sexual offense	✓					✓
Prostitution	✓				✓	
Fencing		✓				
Theft		✓				✓
Arson		✓		✓		
Vandalism		✓		✓		
Money related			✓		✓	
Smuggling			✓		✓	
Drug trafficking			✓		✓	
Patent and trademark infringements			✓		✓	

Notes. The sum of the power and enterprise syndicate crimes corresponds to the *Mafia-related crimes* category. The list of offenses is provided by the ISD; the *Standard categorization (S)* is taken from [Bianchi et al. \(2012\)](#); the *Organized Crime categorization (OC)* is taken from [Mete \(2009\)](#) (pp. 31-32); *Power Syndicate* also includes organized crime and organized crime Mafia-type criminal offenses (Articles 416 and 416bis of the Italian Penal Code); *money related* crimes include usury and money laundering.

Table A2
Additional results: separate effects on arson and murders.

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Neighboring</i>		<i>Dismissed</i>		<i>Neighboring and dismissed</i>	
	<i>Arsons</i>	<i>Murders</i>	<i>Arsons</i>	<i>Murders</i>	<i>Arsons</i>	<i>Murders</i>
Panel A						
Inspection and dismissal	-0.0366 (0.0666)	-0.0832 (0.1074)	0.0876 (0.1204)	-0.0270 (0.1383)	-0.0107 (0.0595)	-0.0538 (0.0837)
Post-dismissal	0.0360 (0.0969)	-0.1165 (0.1215)	0.2737 (0.1807)	0.1419 (0.2260)	0.0714 (0.0893)	-0.0330 (0.1020)
Panel B						
Inspection and dismissal	-0.0570 (0.0588)	-0.0698 (0.1139)	0.0620 (0.1114)	-0.0836 (0.1582)	-0.0376 (0.0526)	-0.0482 (0.0909)
Post-dismissal: short term	0.0185 (0.0907)	-0.1053 (0.1223)	0.2433 (0.1718)	0.0781 (0.2350)	0.0473 (0.0838)	-0.0282 (0.1047)
Post-dismissal: medium term	-0.0294 (0.0932)	-0.0767 (0.1536)	0.1862 (0.2135)	-0.0342 (0.3314)	-0.0147 (0.0885)	-0.0166 (0.1363)
N	1632	1376	440	416	2072	1792

Notes. The dependent variables are municipality yearly counts (C_{it}) of arson and murders; see Table 2 for the control variables and fixed effects included. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Ministry of Economics and Finance, Public Finance Database, Google Trends Index.

Table A3

Petty and Mafia-related crimes in neighboring and dismissed municipalities vs. all Italian municipalities.

Crime rates	<i>Neighboring and Dismissed (before LGD)</i>	<i>All Italian mu- nicipalities</i>
<i>Petty crimes</i>	11.40	12.87
Thefts	11.35	12.82
Sexual offenses	0.05	0.04
<i>Mafia-related crimes</i>	10.47	8.70
Power Syndicate	9.52	8.11
Enterprise Syndicate	0.95	0.58
N	1013	63019

Notes. Petty and Mafia-related crime rates (per 1000 inhabitants) in neighboring and dismissed municipalities (before LGD) and all Italian municipalities. **Sources:** based on ISD, ISTAT-DEMOS.

Table A4

Robustness and specification tests on dismissed and neighboring municipalities.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A</i>						
Inspection and dismissal	-0.0723*** (0.0245)	-0.0907** (0.0428)	-0.0952** (0.0439)	-0.0719* (0.0398)	-0.0462** (0.0225)	-0.0647*** (0.0196)
Post-dismissal	-0.0884*** (0.0308)	-0.1197** (0.0529)	-0.1259** (0.0576)	-0.0958** (0.0481)	-0.0565* (0.0308)	-0.0721*** (0.0272)
<i>Panel B</i>						
Inspection and dismissal	-0.0796*** (0.0243)	-0.0998*** (0.0369)	-0.1198*** (0.0361)	-0.0829** (0.0355)	-0.0527** (0.0241)	-0.0719*** (0.0213)
Post-dismissal: short term	-0.0959*** (0.0304)	-0.1279*** (0.0481)	-0.1551*** (0.0481)	-0.1036** (0.0456)	-0.0611* (0.0314)	-0.0778*** (0.0280)
Post-dismissal: medium term	-0.1198*** (0.0409)	-0.1460*** (0.0470)	-0.1997*** (0.0481)	-0.1310*** (0.0463)	-0.0781* (0.0424)	-0.0959** (0.0385)
N	2072	2068	2016	1720	1632	2072
Year and municipality FE	✓	✓	✓	✓	✓	✓
Municipality characteristics	✓	✓	✓	✓	✓	✓
Specifications:						
(i) Municipality-specific trends	✓					
(ii) Add dismissed municipality crimes		✓				
(iii) Exclude replications			✓			
(iv) Dismissals occurred 2005 to 2010				✓		
(v) Negative binomial					✓	✓

Notes. The dependent are municipality yearly counts (C_{it}) of petty crimes (baseline specification); see Table 2 for the control variables and fixed effects included. Specification (i) adds neighboring municipality specific year (linear) trends; specification (ii) adds as a control variable the petty crime count of the corresponding dismissed municipality; specification (iii) excludes the replications; specification (iv) excludes dismissals occurred in the year 2004 and 2011. Estimates for specifications from (i) to (iv) are obtained from QML Poisson fixed effects regressions, with robust standard errors clustered at the municipality level. Estimates for specification (v) are obtained from Negative Binomial FE regressions: results in column 5 focus on neighboring municipalities only; results in column 6 include the dismissed municipalities in the estimation sample. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Ministry of Economics and Finance, Public Finance Database, Google Trends Index.

Table A5

Welfare analysis: the social benefits from the LGD.

	(1)	(2)
	<i>Baseline specification</i>	<i>Lower bound specification</i>
Average no. of crimes in pre-policy years (a):	141.2	141.2
Average social cost of crime (c):	7,650.49	7,650.49
<i>Estimated percentage effect (b):</i>		
Inspection and dismissal	-9.01	-4.64
Post-dismissal	-12.44	-7.27
<i>Estimated crime reduction (b × a):</i>		
Inspection and dismissal	-12.72	-6.55
Post-dismissal	-17.56	-10.27
<i>Benefits from petty crime reduction (b × a × c):</i>		
Inspection and dismissal	97,310.17	50,112.22
i) Post-dismissal	134,339.49	78,555.97
ii) Post-dismissal (discounted, d=0.33)	101,007.14	59,064.64
iii) Post-dismissal (discounted, d=0.38)	97,347.46	56,924.61
iv) Post-dismissal (discounted, d=0.5)	89,559.66	52,370.64
<i>Total social benefits (SB) from petty crime reduction:</i>		
i) SB	231,649.66	128,668.18
ii) SB (d = 0.33)	198,317.30	109,176.85
iii) SB (d = 0.38)	194,657.63	107,036.83
iv) SB (d = 0.5)	186,869.83	102,482.86

Notes. Figures in 2007 euros. The estimated percentage effects (b) are obtained from the *Baseline specification* in Table 2, Panel A, column 1, and from the *Lower bound specification* in Table A4, Panel A, column 1. The average social cost of crime (c) is obtained from Detotto and Vannini (2010). Results denoted by i), ii), iii), and iv) apply, respectively, no social discount rate, a social discount rate of $d = 0.33$, $d = 0.38$, $d = 0.5$ to the calculation of the post-dismissal social benefits. **Sources:** based on ISD, ISTAT-DEMOS, Public Finance Database (2004-2011).

Table A6
Heterogeneous effects according to media attention, arrests, civicness.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Media attention</i>	<i>Baseline</i>	<i>GTI sample</i>	<i>GTI at dismissal</i>		<i>GTI 5 months window</i>	
			High	Low	High	Low
Inspection and dismissal	-0.0944* (0.0515)	-0.0657 (0.0425)	-0.1168*** (0.0336)	0.0002 (0.0679)	-0.1141*** (0.0297)	0.0190 (0.0781)
Post-dismissal	-0.1328** (0.0604)	-0.1169** (0.0460)	-0.1764*** (0.0423)	-0.0277 (0.0727)	-0.1796*** (0.0364)	-0.0129 (0.0839)
N	1632	1352	688	664	712	640
<i>Panel B: Arrests</i>	<i>Baseline</i>	<i>Arrests sample</i>	<i>LGD with arrests</i>	<i>LGD W/O arrests</i>		
Inspection and dismissal	-0.0944* (0.0515)	-0.1002* (0.0548)	-0.0785** (0.0325)	-0.1058 (0.0698)		
Post-dismissal	-0.1328** (0.0604)	-0.1356** (0.0648)	-0.0726 (0.0648)	-0.1584** (0.0805)		
N	1632	1448	592	856		
<i>Panel C: Measures of civicness</i>	<i>Cheating at school</i>		<i>Non-profit organizations</i>		<i>Electoral turnover</i>	
	High	Low	High	Low	High	Low
Inspection and dismissal	-0.2423*** (0.0452)	0.0071 (0.0507)	-0.1941*** (0.0471)	-0.0209 (0.0649)	-0.0758 (0.0566)	-0.0390 (0.0579)
Post-dismissal	-0.3074*** (0.0627)	-0.0096 (0.0566)	-0.1964*** (0.0658)	-0.0695 (0.0689)	-0.1194* (0.0625)	-0.0468 (0.0825)
N	592	1040	824	808	904	728
Controls	✓	✓	✓	✓	✓	✓
Year and municipality FE	✓	✓	✓	✓	✓	✓

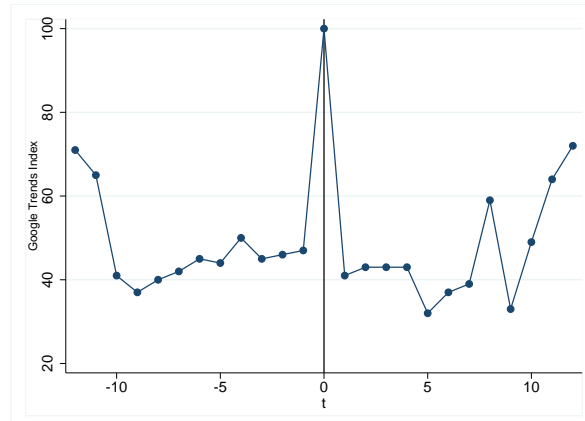
Notes. The dependent are municipality yearly counts (C_{it}) of petty crimes (baseline specification); see Table 2 for the control variables and fixed effects included. Panel A shows heterogeneous effects according to measures of media attention derived from Google Trends Index (GTI): column (1) reports the baseline estimates, column (2) reports the baseline estimates on the sample of municipalities for which it was possible to retrieve the GTI. In columns 3-6 the sample is split according to whether the dismissal received a media attention (proxied by the GTI) higher (*High*) or lower (*Low*) than the median of the GTI calculated at dismissal (columns 3 and 4), or of the GTI calculated on 5 months window period centered in the month of the dismissal (columns 5 and 6). Panel B shows the results according to whether the LGD occurred with or without contextual arrests: column (1) reports the baseline estimates, column (2) reports the baseline estimates on the sample of municipalities for which it was possible to retrieve information on arrests; column (3) reports estimates for the sample of municipalities in which the dismissal occurred with at least one person arrested, column (4) reports estimates for the sample of municipalities in which the dismissal occurred without persons arrested. Panel C shows heterogeneous effects according to different measures of civicness at the municipality level, where *High* (*Low*) refer to the group of municipalities with higher (lower) than the median level of civicness according to three different measures: *Cheating at school* considers statistical indicators of cheating in national assessment test (grades 5 and 6, school year 2009/2010); *Non-profit organizations* considers the number of non-profit organizations (with respect to the resident population in the municipality); *Electoral Turnover* considers referendum held between 1948 and 1968. Notice that in the case of the Cheating Indicator, the High (Low) civicness subsample corresponds to municipalities with cheating lower (higher) than the median. Estimates obtained from QML Poisson fixed effects regressions, with robust standard errors clustered at the municipality level. Asterisks denote statistical significance at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ levels. **Sources:** based on ISD, ISTAT-DEMOS, Ministry of Economics and Finance, Public Finance Database, Google Trends Index.

Appendix B The Google Trends Index for LGD

LGDs are usually resounding (but local) events, only sometimes reported in the national newspapers. To build a measure that would proxy the attention devoted by the media to each LGD we resort to the Google Trend Index (GTI, <https://www.google.it/trends/?hl=it>). Recent papers have shown that the GTI is a good predictor of near-term values of economic indicators, such as automobile sales, unemployment claims, travel destination planning, consumer confidence, and media attention (Choi and Varian 2012; D’Amuri and Marcucci 2017; Kearney and Levine 2015). The GTI provides a time series index of the volume of queries users enter into Google in a given geographic area. The index is based on query share: the total query volume for the search term in question within a particular geographic region divided by the total number of queries in that region during the time period being examined. The maximum query share in the time period specified is normalized to be 100 (Choi and Varian 2012).

We collect the GTI for each dismissal defining a time window of 25 months centered in the month of the dismissal ($t \in [-12; +12]$), and restricting the geographical area to Italy, the keyword for the search being the name of the dismissed municipality.²⁷ Figure B1 shows an example of the GTI for a dismissal occurred in 2013: the GTI peaks exactly in the month of the dismissal ($t = 0$). This is somehow confirming what previously found in existing works: the GTI is more appropriate to capture the short term attention of the media, in our case, the media attention around the dismissal (Choi and Varian 2012).

Figure B1
The Google Trends Index for LGD: an example.



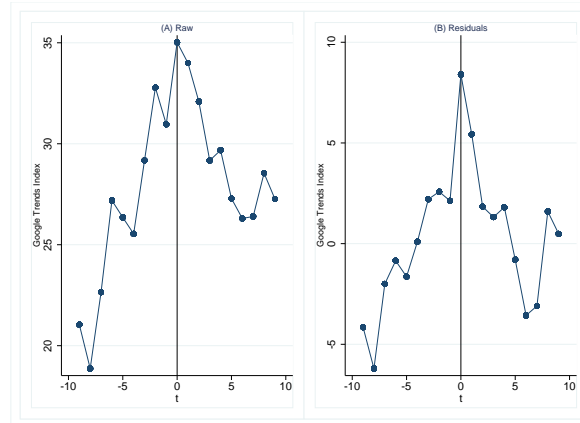
Notes. The graph plots the monthly GTI (vertical axis) by timing of the intervention (horizontal axis) derived from the municipality of Sedriano, dismissed in October 2013; the vertical line in $t = 0$ indicates the month of the dismissal.
Sources: Google Trend Index (GTI, <https://www.google.it/trends/?hl=it>).

The GTI is provided from the year 2004: for this reason, it was not possible to retrieve

²⁷For 17 dismissals, the search volume was insufficient to calculate the GTI: in these cases, without loss of generality, we set the GTI to zero (D’Amuri and Marcucci 2017).

the index for nine dismissals (occurred in 2004 and at the beginning of 2005) determining a reduction of 352 observations from the baseline sample (i.e. 35 neighboring municipalities per eight years). Moreover, municipalities located on the seaside displayed seasonal trends in the search volumes plausibly due to touristic flows. To clean out monthly and yearly trends, as well as trends linked to observable characteristic of the municipality, such as geographical location and dimension, we calculate a *clean* version of the GTI, taking the residuals from OLS regressions of the raw GTI on dummies for the municipality characteristics (population dummies and seaside location dummy), calendar month and calendar year fixed effects. A comparison between the *raw* and the *clean* version of the GTI is shown in Panel A and Panel B, respectively, of Figure B2. In the main analysis and in the reminder of the section we refer to clean version of the GTI, although the results do not change substantially using the raw index.

Figure B2
The Google Trends Index for LGD: a visual inspection.



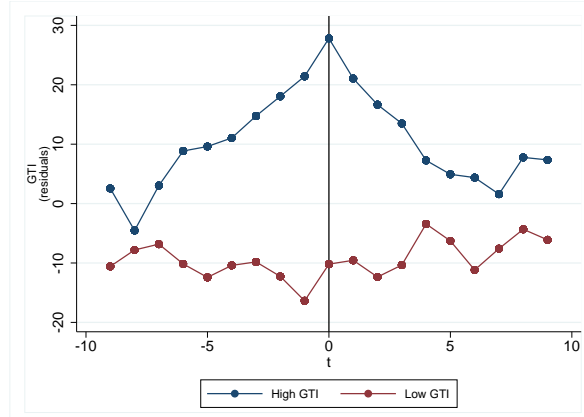
Notes. The graph plot the monthly GTI (vertical axis) by timing of the intervention (horizontal axis) for the subsample of dismissed municipalities for which it was possible to compute the GTI; the vertical line in $t = 0$ indicates the month of the dismissal. Panel A shows the raw index; Panel B shows the residuals from OLS regressions of the raw index on municipality characteristics (i.e. five dummies identifying municipalities with: (i) less than 5,000 inhabitants, (ii) 5,001–10,000 inhabitants, (iii) 10,001–20,000 inhabitants, (iv) 20,001–60,000 inhabitants, (v) more than 60,001 inhabitants, a dummy indicating whether the municipality is on the seaside), calendar month and calendar year fixed effects. **Sources:** Google Trend Index (GTI, <https://www.google.it/trends/?hl=it>) and ISTAT (2009).

As a final step, we build for each dismissal an average index of media attention, averaging the monthly GTI over a given number of months centered in the dismissal. Given that the GTI is suitable to capture the media attention in the short term (i.e. around the dismissal, as shown in Figures B1 and B2), the larger the time window the less accurate could be the measure. In our baseline empirical analysis (see Table A6) we use the average GTI in the month of the dismissal or over a window of 5 months centered in the dismissal.²⁸ Finally, we distinguish between LGDs with *High* or *Low* media attention defining two subsamples

²⁸Results over other time windows (available upon request) do not change significantly from those presented in Table A6, albeit becoming less precisely estimated the larger the time window.

according to whether the average GTI is, respectively, higher or lower than the median value. In Figure B3 we show the GTI separately for the subsamples of LGD with *High* or *Low* media attention: the group of high media attention LGD clearly shows a peak around the dismissal (indicated with the vertical line in $t = 0$), while LGD with low media attention do not show peculiar trends around the dismissal.

Figure B3
The Google Trends Index: LGD with high vs. low media attention.



Notes. The graph plots the monthly GTI (vertical axis) by timing of the intervention (horizontal axis) for the subsample of dismissed municipalities for which it was possible to compute the GTI, and by distinguishing among the groups of dismissals that received *High* or *Low* media attention. The groups are defined according to whether the average GTI (calculated over the GTI residuals in a time window of 5 months centered in the dismissal, $t = 0$) is, respectively, higher or lower than the median value observed in the data. **Sources:** Google Trend Index (GTI, <https://www.google.it/trends/?hl=it>) and ISTAT (2009).

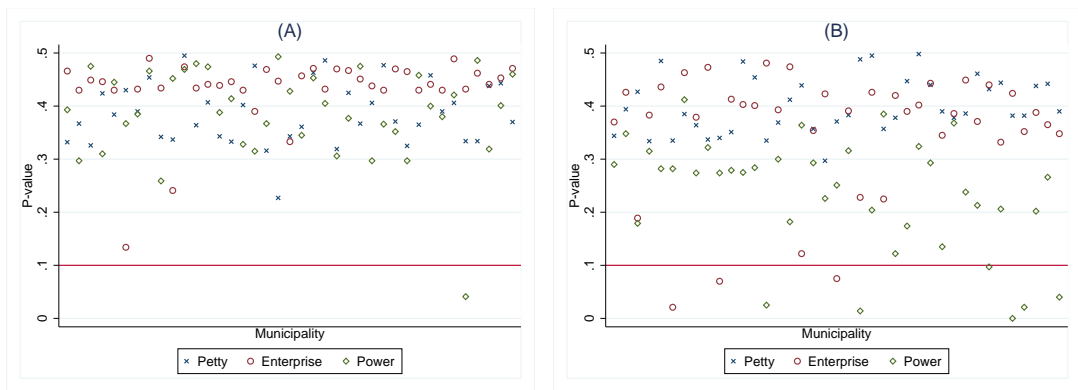
Appendix C A test of spatial autocorrelation

Figure C1 plots the results of testing for the presence of spatial autocorrelation in crime rates between the dismissed municipalities and their neighbors. The test (the Local Moran's I test, see Mendes and Mendes (2015)) is computed in the years before the intervention (i.e. $t \leq -1$). For each dismissed municipality, indicated on the horizontal axis, the graph reports the P-values of the test for each of the three typologies of crime considered in the paper; the 10% level of acceptance is indicated by a horizontal line.

The two panels differ in the definition of neighboring municipalities. The left panel focuses on the whole sample of neighbors, as in the core exercises in the main test (Figure C1.A). In this case the presence of spatial autocorrelation can be rejected in all but one municipality (in the case of power syndicate crimes). Figure C1.B refers to a more restrictive version of the test, focused on the sample of closest neighbors (i.e. municipalities having an Euclidean distance to the dismissed falling within a band of 0 – 5). The test is more restrictive as spatial correlation statistics are likely to decrease in sample size. In this case, the null of spatial autocorrelation can be rejected in all but few (9), and in any case in which petty crimes are considered.

This evidence supports the idea that the incidence of crimes in the sample of neighbors would likely be unrelated to the intervention, even if this was a response to the patterns of crime in the dismissed municipality.

Figure C1
Moran's I test for spatial autocorrelation.



Notes. The scatter plots depict the results of binary Moran's I tests between the crime rates of each dismissed municipality and those of the corresponding neighboring municipalities: the vertical axes indicates the p-values of test; the horizontal axes the anonymized (dismissed) municipality ID; the red horizontal line indicates the 10% level of acceptance in the p-values. Panel A performs the test including all neighboring municipalities (i.e. those having an Euclidean distance to the dismissed municipality falling within a band of 0 – 10); Panel B restricts to the closest neighbors (i.e. municipalities having an Euclidean distance to the dismissed lower than 5). **Source:** based on ISD (2004-2011).