NBER WORKING PAPER SERIES

PLATA Y PLOMO:
HOW HIGHER WAGES EXPOSE POLITICIANS TO CRIMINAL VIOLENCE

Massimo Pulejo
Pablo Querubín

Working Paper 31586
http://www.nber.org/papers/w31586

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2023

We wish to thank Gianmarco Daniele, Francesco Drago, Claudio Ferraz, Simone Ferro, Vincent Pons, Alessio Romarri, Shanker Satyanath, Silvia Vannutelli, Hye Young You, and seminar participants at EAFIT University, New York University, Erasmus University, MPSA 79th Annual Meeting, Yale University, AL CAPONE 10th Annual Meeting, Barcelona GSE Summer Forum, RIDGE-LACEA Workshop on Political Economy, WEOC 6, Italian Parliamentary Antimafia Commission, Northwestern University Kellogg School of Management, Stanford University Graduate School of Business, University of Milan and the CLEAN Unit for the Economic Analysis of Crime at Bocconi University for helpful comments and suggestions. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Plata y Plomo: How Higher Wages Expose Politicians to Criminal Violence
Massimo Pulejo and Pablo Querubín
NBER Working Paper No. 31586
August 2023
JEL No. D72,D74,H72

ABSTRACT

Adequate wages are an important tool to shield public officials from special interests and corruption. But what is the equilibrium effect of higher wages in the presence of criminal pressure groups, who use both bribes and violence? By means of a regression discontinuity design, we show that an increase in the remuneration of Italian municipal cabinets triggers a sizable and significant increase in criminal attacks against their members. We argue that this is triggered by higher-paid officials' lower likelihood of catering to criminal interests. In particular, we show that better-paid politicians are significantly more likely to prevent corruption in public procurement, a key area of illicit interactions between the state and criminal organizations. Additional analyses reveal that the disciplining effect of wages is driven by a change in incumbents' behavior rather than improved selection. These findings reveal how -- in the presence of criminal groups -- higher wages may limit corruption, but also foster the use of violence as an alternative tool to influence policymaking.

Massimo Pulejo
University of Milan and CLEAN
Via Festa del Perdono, 7
Milan 20122
Italy
massimo.pulejo@unimi.it

Pablo Querubín
Department of Politics
New York University
19 West Fourth Street, Room 428
New York, NY 10012
and NBER
pablo.querubin@nyu.edu

1 Introduction

The remuneration of public officials plays a key role in the performance of public institutions. Higher wages can enhance political selection, providing citizens with more competent candidates (Caselli and Morelli, 2004; Keane and Merlo, 2010; Gagliarducci and Nannicini, 2013) and shape the incentives of incumbents, affecting their performance and re-election prospects (Besley, 2004; Ferraz and Finan, 2009). Since Becker and Stigler (1974), economic theory has also linked higher compensation to lower levels of corruption, through three main channels. First, it may attract more honest individuals into the public sector (Besley and McLaren, 1993; Bond, 2008). Second, it raises the material cost of corruption scandals (Rose-Ackerman, 1999; Van Rijckeghem and Weder, 2001). Finally, by augmenting the income of politicians, it lowers the marginal utility of bribes (Di Tella and Schargrodsky, 2003; Chen and Liu, 2018).

Nevertheless, this theoretical link is yet to find causal empirical support. Research has shown that corruption may persist (Svensson, 2005), if interest groups are willing to match requests for higher bribes (Mookherjee and Png, 1995). Alternatively – in the presence of criminal pressure groups – favors can be obtained via the use of violence. In fact, criminal groups typically use both bribes and physical punishment – or the threat thereof – as means to obtain concessions (Dal Bó and Di Tella, 2003; Dal Bó et al., 2006; Lessing, 2015; Enríquez, 2022). When this type of influence is an option, reducing the appeal of bribes through higher salaries may have unintended consequences and raises important questions. Will higher wages lead criminal pressure groups to substitute bribes with violence? If yes, what is the net effect of increasing politicians' wages?

We take advantage of a natural experiment to illuminate the consequences of increasing politicians' wages on corruption levels and criminal attacks against elected officials.

We estimate the causal effects of higher wages by leveraging a unique feature of Italian legislation, which determines the compensation of public officials according to population thresholds. In particular, we employ a Regression Discontinuity Design (RDD) at the 5,000 inhabitants threshold, above which mayors and other members of the cabinet enjoy a sizable increase in their monthly salaries.

We begin by studying how politicians' remuneration affects the number of criminal threats and attacks to which they are subjected. To this end, we digitize data on 3,051 attacks on Italian public officials for the period 2014-2020, systematically collected by the NGO *Avviso Pubblico*. RDD analyses reveal a positive and statistically significant relationship between politicians' wages and criminal attacks on local politicians. On average, better-paid cabinets are seven percentage points – or 0.3 standard deviations – more likely to be targeted with violence. On the intensive margin, this implies that the average city at the cutoff will experience 0.15 additional criminal attacks against its cabinet. Consistent with their greater power and authority to set policies, the effect is stronger for mayors than for other members of the municipal cabinet. On the other hand, there is no significant impact on attacks towards municipal councilors and non-elected local officials, who do not enjoy a pay raise at the 5,000 residents cutoff.

These results are robust to a number of sensitivity checks, such as different transformations of the dependent variable, the use of Poisson regression, the inclusion of municipal controls, and alternative choices of bandwidths and polynomial orders. Also, we show that our estimates are not driven by any particular region and are unlikely to be explained by a higher likelihood that better-paid politicians will report violent attacks. Additional tests rule out that the coefficients we retrieve reflect other rules introduced at the 5,000 cutoff after 2013.

What explains the increase in attacks against better-paid politicians? Following the theoretical literature, we hypothesize that higher violence is caused by a lower propensity of better-paid politicians to favor corruption. We focus our empirical tests on public procurement, a key area of illicit interactions between the public sector and criminal organizations across the globe (Barone and Narciso, 2015; Boas et al., 2014). Using data from the Italian Anti-Corruption Authority (ANAC) on more than half a million procurement contracts, we document a significant improvement in anti-corruption practices in municipalities with more than 5,000 residents. Our exploration tracks all the stages of the procurement process and is guided by the insights of recent studies of Italian public finance and criminal organizations (Coviello et al., 2018; Daniele and Dipoppa, 2022; Decarolis et al., 2021).

We start at the contract-design stage. We investigate the tendency to concentrate the reservation values of contracts below three thresholds, that introduce stronger monitoring and more stringent competition requirements. RDD analyses provide evidence that municipalities with better-paid cabinets are significantly *less* likely to concentrate the reservation values of their contracts below these thresholds, thus fostering competition and preventing criminal infiltration. We then analyze the competition phase. At this stage, we find that municipal administrations with higher wages are more inclusive: The log average number of firms invited to bid in negotiated procedures increases by about a fourth of a standard deviation at the RDD cutoff. Finally, at the post-competition stage, we find that in municipalities with better-paid cabinets, winning firms are significantly less likely to subcontract public works. Since in Italy subcontracting is often used to enforce corrupt deals (ANAC, 2017; Decarolis et al., 2021), we interpret this as evidence that better-paid politicians are less prone to striking such deals.

Importantly, the information in the ANAC database allows us to test for heterogeneity by type of contract. For all the three sets of outcomes we analyze, the effects of higher wages are much larger among contracts for public works than among contracts for goods and services. Since criminal firms in Italy operate primarily in the construction sector (Transcrime, 2013; ANAC, 2017), this is consistent with better-paid cabinets being committed to avoiding the infiltration of these firms in their auctions.

To conclude our extensive empirical exploration, we inquire whether improved procurement practices under better-paid cabinets are driven by three potential mechanisms put forward in the theoretical literature. We start by evaluating the role of improved selection (Besley and McLaren, 1993; Bond, 2008). To this end, we repeat our RDD analyses, but use sixteen different characteristics of mayors elected since 2011 as outcome variables. The results – with the caveat of being unavoidably limited to observable mayoral features – suggest that selection does not play a key role in improving procurement practices in the context of our study.

A second possibility is that better-paid cabinets behave differently out of stronger re-election incentives, since a pay raise increases the monetary value of an additional term in office (Shapiro and Stiglitz, 1984; Van Rijckeghem and Weder, 2001). To explore this, we compare mayors in their first terms with those in their second terms, as the latter face a binding term limit. RDD analyses reject significant differences in procurement behavior across the two groups, suggesting that the effect of wages does not stem from stronger re-election incentives.

Finally, the effect of higher wages may induce a change in incumbents' behavior driven, for example, by a reduction in the marginal utility of bribes, as cabinets with better remuneration may not need to supplement their income through illicit means (Di Tella and

Schargrodsky, 2003; Chen and Liu, 2018). RDD analyses run on a restricted, panel version of our data provide evidence in favor of this potential mechanism. Holding constant the identity of the mayor, we find that municipalities with more than 5,000 inhabitants began to improve their procurement practices only after 2011 (i.e., when the most recent population figures were issued and the remuneration of their cabinet was raised following the existing compensation scheme).

This paper offers important new insights into two strands of scholarship. First, it adds to the literature on the effects of politicians' remuneration. Our results support long-standing theories linking a higher compensation for public officials to lower levels of corruption (Becker and Stigler, 1974; Besley and McLaren, 1993). Together with D'Andrea (2019), we are the first to find robust, causal evidence for this relationship, while extant studies had found ambiguous (Mookherjee and Png, 1995; Svensson, 2005; Chen and Liu, 2018) or even perverse effects of higher wages on government quality (Pique, 2019). Furthermore, our novel data set allows us to causally test each of the three mechanisms that may link higher wages to lower corruption. In this respect, our findings underscore the primacy of incentives over selection, by showing that higher compensation may induce incumbents not only to work harder (Ferraz and Finan, 2009), but also to behave more honestly.

Our study is also the first to document how such an improvement may come at the expense of politicians' safety. This complements our understanding of the political economy of organized crime, and highlights an issue with broad implications as politicians across the globe must deal with violent interest groups (Pinotti, 2015a). Our findings show how criminal pressure groups adapt to the institutional environment (Daniele and Dipoppa, 2022; Szucs, 2021; Palguta and Pertold, 2017), adjusting their use of bribes and violence (Enríquez, 2022). Moreover, unlike previous contributions, which mostly focus on criminals' reactions to rules directly affecting them, we document how they also respond to the incentives of the actors whom they wish to influence. This poses new challenges for both scholars and policymakers, with crucial implications for public safety and accountability.

2 Background

2.1 Italian Municipal Politics

There are 7,904 municipalities in Italy. Municipal administrations have responsibility over several policy areas: management of basic services (e.g., garbage collection, sewage, etc.), local police, transportation, and a wide array of social welfare services. Furthermore, and crucially for the scope of the present study, municipalities are in charge of all the public procurement procedures needed to carry out their tasks.

Citizens directly elect the mayor – who heads the municipal cabinet – and the members of the municipal council (i.e., the legislative assembly). Municipal councils are elected at the same time as the mayor, and each mayoral candidate is linked to lists of candidates to the council. After the election, the lists of candidates supporting the elected mayor are ensured an absolute majority in the council. In municipalities with over 3,000 inhabitants, mayors can serve for up to two consecutive five-year terms (up to three in smaller municipalities).

During the period of our study, the remuneration of municipal politicians was regulated by Law 266/2005. This law (minimally) updated Decree 119/2000 and the initial law on this subject matter, which dates back to 1960. Compensation levels change automatically at several population thresholds, as detailed in Table 1 below. For the purposes of identification, we follow Gagliarducci and Nannicini (2013), focusing on the 5,000 residents cutoff. In municipalities with more than 5,000 residents as of the latest census, the mayor earns a monthly wage of €2,511: 558 euros/month more (+28.6%) than her colleagues governing towns comprising just below 5,000 inhabitants. An even larger pay raise is awarded to members of the municipal cabinet: Their salary – calculated as a percentage of the mayor's – moves from €390.6 to €1,255.5 per month (+221%). The salary of local officials is updated every 10 years, when a new population census is released. Therefore, as our study focuses on the period 2013-2020, the relevant census used to construct our running variable is the one of 2011.

Table 1: Legislative Thresholds for Italian Municipalities, 2014-2020

Municipal Population	Wage Mayor	Wage Cabinet	Rate Council	Term Limit	Cabinet Size	Council Size	DSP Rules
Below 1,000	1,161	15%	18	3	4	12	No
1,000-3,000	1,305	20%	18	3	4	12	Yes
3,000-5,000	1,953	20%	18	2	4	16	Yes
5,000-10,000	2,511	50%	18	2	4	16	Yes
10,000-30,000	2,790	55%	22	2	6	20	Yes
30,000-50,000	3,114	55%	36	2	6	30	Yes
50,000-100,000	3,717	75%	36	2	6	30	Yes
100,000-250,000	4,509	75%	36	2	10	40	Yes
250,000-500,000	5,202	75%	36	2	12	46	Yes
Above 500,000	7,020	75%	36	2	14-16	50-60	Yes

Notes: Figures for wages and attendance rates are in euros. *Municipal Population* is based on the 2011 census. *Wage Mayor* and *Wage Cabinet* refer to the monthly gross wages of the mayor and the members of the municipal cabinet, respectively; the latter is expressed as a percentage of the former. *Rate Council* is the reimbursement per session paid to councillors and is measured in euros. The wage thresholds at 1,000 and 10,000 were introduced in 2000; all of the others date back to 1960. *Cabinet Size* is the maximum allowed number of cabinet members appointed by the mayor. *Council Size* is the number of seats in the city council. All of the cabinet and council size thresholds were set in 1960. Differences in term limits were introduced in 2014. Since 1993, mayors in towns with fewer than 15,000 residents are elected via plurality (with 60% premium), while mayors in towns above 15,000 residents are elected with runoff plurality (with 66% premium). Source: Gagliarducci and Nannicini (2013), with our edits.

As shown in Table 1, 50,000 inhabitants is the only other threshold at which rule changes concern the wage of mayors and members of the cabinet exclusively. However, the number of municipalities around the threshold is too small to allow for a credible RDD, as also noted in Gagliarducci and Nannicini (2013). Hence, the 5,000 residents threshold is the only one suitable for identifying our causal effects of interest, with some caveats.

First, until 2013, municipalities with populations below the cutoff were exempt from the Domestic Stability Pact (DSP), a set of constraints on spending imposed by the national government. Since such constraints can affect corruption levels (see Daniele and Giommoni, 2021), in this paper we focus on criminal attacks from 2014 onward, one year after the DSP was extended to all municipalities with more than 1,000 residents.

Second, Law 215/2012 introduced two rules concerning the gender of candidates for the municipal council that change discontinuously at the 5,000 residents threshold. In municipalities with more than 5,000 inhabitants, lists of candidates to the council cannot contain more than two thirds of candidates of the same gender. Also, voters expressing two preference votes for councillors must indicate candidates of different gender. As we will show in Section 5, however, these rules do not affect the rate at which municipal councillors – who do not enjoy any pay raise – are targeted by criminal attacks.

Third, according to Law 122/2010, municipalities with fewer than 5,000 residents should jointly plan and execute certain functions through unions of municipalities. Yet, as we will show in Subsection 5.3, this does not appear to have effectively restricted the value of municipal procurement. Moreover, in our procurement findings we focus exclusively on contracts offered by municipalities as independent entities. Lastly, our findings for criminal attacks against local politicians hold when we drop municipalities that are part of unions of municipalities.

Finally, as established by Law 148/2011, the budgets of municipalities with more than 5,000 inhabitants are reviewed by more experienced accountants.[1] However, in Subsection 5.3, we follow Vannutelli (2022) and exploit the staggered introduction of this rule to show that it is unlikely to be a main driver of the increase in criminal attacks that we observe at the RDD cutoff.

2.2 Organized Crime in Italy

The presence of criminal organizations has been a defining feature of Italy's social, economic, and political landscape for decades. Taken together, these organizations make yearly profits of about 1.7% of the Italian GDP (Unioncamere, 2017), and critically hamper the country's development (Pinotti, 2015b). After originating in the South, criminal groups have progressively expanded to the rest of the peninsula.

While previously focused on illicit operations, criminal organizations now source their revenues from both legal and illegal activities (Le Moglie and Sorrenti, 2020; Dipoppa, 2022). In the legal economy, they typically launder money by investing it in the retail, construction, and hospitality industries (Transcrime, 2013). Therefore, these organizations have high stakes in political decisions at all levels, and especially at the local level. In fact, as described in Section 6, Italian municipal administrations allocate a large amount of jobs and resources to firms and individuals.

Organized crime influences policy making in three main ways. The most common tool is corruption. Many firms have links to criminal groups (Decarolis et al., 2021; Mirenda et al., 2022), and there has been roughly one proven episode of corruption per

[1]For municipalities with up to 4,999 inhabitants, auditors must have been certified public accountants for at least two years. For municipalities with between 5,000 and 14,999 inhabitants, the minimum experience for auditors is five years, and auditors must have been previously appointed as municipal auditors at least once.

week in public procurement between 2016 and 2019 (ANAC, 2019). Notably, 41% of such cases involved municipal administrations, and were characterized by small bribes: typically between 2,000 and 3,000 euros, but occasionally even as low as 50-100 euros. This suggests that a sizable raise in salaries, like the one awarded at our RDD threshold, may well be effective in reducing the appeal of such modest bribes.

Second, criminal groups often use violence or intimidation against public officials. In the period we consider in this paper, close to 2,500 attacks of criminal nature were recorded by the NGO *Avviso Pubblico*, the overwhelming majority targeted municipal politicians. More details on these episodes are provided in Section 3 below. Finally, a third way to influence local policymaking is by infiltrating elective bodies (Di Cataldo and Mastrorocco, 2021). Since this is a relatively uncommon strategy, adopted almost exclusively in Southern regions, it is not analyzed in the present study.

Thanks to this rich toolkit, criminal groups are highly capable of affecting political decisions. According to recent estimates, about 40% of Italian procurement auctions are at risk of criminal influence (DNA, 2019). Though there are various ways in which public administrations favor organized crime, widespread strategies are to design ad-hoc contracts, or appeal to technical quibbles to prevent rival firms from competing (ANAC, 2019).

3 Data on Attacks against Politicians

To gauge the amount of acts of violence and intimidation against municipal politicians, we use yearly reports published by the NGO *Avviso Pubblico* for the years 2014-2020.[2] *Avviso Pubblico* is an independent organization, established in 1996 with the aim of raising

[2]The reports are publicly available for download at: `www.avvisopubblico.it/home/home/cosa-facciamo/pubblicazioni/amministratori-sotto-tiro/`.

awareness about undue influences on local governments. Among its main activities, the association tracks daily online and offline news about attacks against public officials. The resulting reports, which have been used by Daniele and Dipoppa (2017), constitute a highly reliable and detailed source of information. In addition to the exact date and location, they specify the type of attack, as well as the victim(s). For the purposes of this study, we first digitize all the 3,051 attacks that occurred between 2014 and 2020. Next, we manually check the reports to exclude attacks that were not perpetrated by criminal groups,[3] which leaves us with a data set of 2,464 episodes over seven years.

As depicted in Panel A of Figure 1, among elected municipal officials, mayors constitute the modal victim of criminal attacks (759 episodes), followed by municipal councillors (349) and members of the cabinet (270). This resonates with the fact that mayors are the highest municipal authorities, and those endowed with the most power in setting policies of interest to criminal groups. As to the nature of the episodes, Panel B of Figure 1 illustrates how they are fairly equally distributed between threats (1,097, or 45%) and actual attacks (1,367, 55%). Among the latter, attacks on property are much more frequent than those on individuals. Figure A1 in the Appendix maps the geographical distribution of the attacks. Consistent with the Italian criminal landscape, attacks are more frequent in the South, but there is a non-trivial number of municipalities targeted in each region. Finally, Figure A2 shows how the frequency of criminal attacks is quite stable across the years of our sample period.

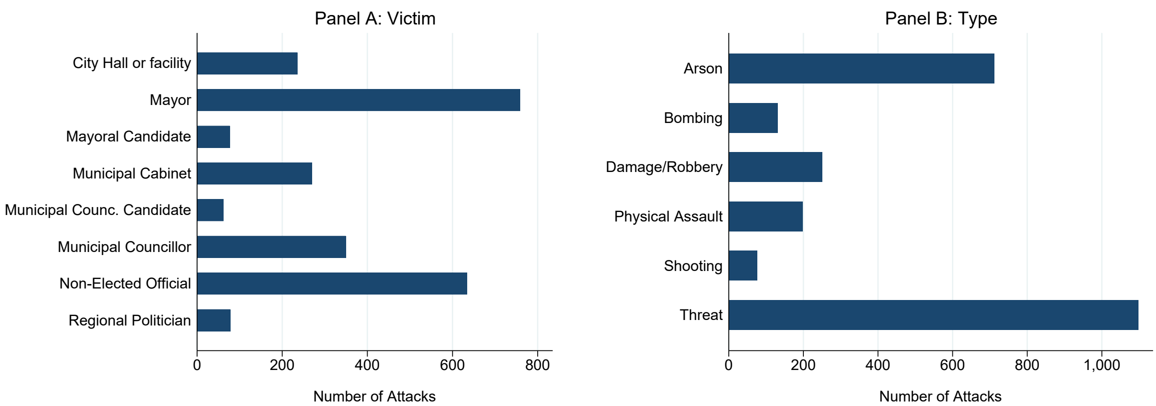
Three key facts are worth noting. First, the probability of being attacked is low: About 8% of municipal cabinets elected after 2011 ever experienced an attack. Second, attacks are meant to scare: Only around 10% are shootings and physical assaults, and

[3] Non-criminal episodes are instances of individual citizens insulting or physically attacking public officials to complain about the lack of services or the denial of a benefit.

there was only one homicide during our sample period. Third, attacks aim to send an early signal: The probability of being attacked is 9.2% for cabinets in their first terms and 4.9% for those in their second terms. Also, attacks tend to be concentrated in the early years within each term.

These patterns of criminal violence are helpful in understanding why, in such a context, an increase in local politicians' salaries may meaningfully curb corruption. Criminal attacks are rare and, conditional on happening, they are unlikely to be life-threatening. Thus – at least initially – better-paid politicians may commit to maintaining their integrity in government while accepting a limited risk of retaliation. In this way, they can avoid scandals and secure the benefits of higher salaries for a longer period of time.

Figure 1: Number of Criminal Attacks, by Victim and Type



Notes: In Panel A, *Municipal Cabinet* includes attacks on municipal cabinet members and vice mayors. Non-elected officials are mostly police officers and clerks of the municipal bureaucracy. All categories in Panel A also include attacks on family members of each type of public official.

Finally, in line with the discussion in Subsection 2.2, a closer look at the data suggests a strong link between criminal attacks and local procurement. In fact, as depicted in Figure A3, close to half of the attacks on members of the municipal cabinet concentrate on those in charge of public works and waste management, the two main interests of the

Mafia in the legal economy. Furthermore, as shown in Figure A4, attacks are frequent in the first four years of a municipal term (Panel A); while they drop in the fifth year, when only a few contracts are tendered due to limits on cabinets' scope of action in the final year of their electoral mandates (Panel B).

4 Empirical Strategy

Our argument posits that if higher wages curb corruption, criminal organizations will use more violence as an alternative tool to influence municipal cabinets. In what follows, we test our argument starting with the effect of wages on criminal attacks. Next, we investigate the impact of higher salaries on procurement practices, which constitutes the causal mechanism in the context of our study. We use a RDD at the 5,000 residents cutoff to identify the effects of an exogenous increase in the wages of municipal cabinets on our outcomes of interest. Our estimating equations have the form:

$$\begin{aligned} Y_i = \phi_p + \beta * \mathbf{1}[Population \geq 5,000]_i + \gamma f(Population)_i + \\ + \lambda(\mathbf{1}[Population \geq 5,000] \times f(Population))_i + \theta X_i' + \epsilon_i, \end{aligned} \tag{1}$$

where i is municipality and p is province. The parameter of interest is β (i.e., the effect of higher wages for the mayor and the other members of the municipal cabinet on outcome Y_i).[4] Given the RDD setup, the coefficient $\hat{\beta}$ measures this effect at the cutoff of 5,000 inhabitants. The main identifying assumption, tested below, is that other municipal characteristics prior to 2011 – the year of the relevant census – are smooth

[4] In the first part of the analysis, for a given municipality and class of official, Y_i will be one of three main outcomes: *(i)* An indicator equal to 1 if municipality i experienced at least one attack between 2014 and 2020, and 0 otherwise; *(ii)* The natural logarithm of the total number of attacks, augmented by 1; and *(iii)* The inverse hyperbolic sine of the total number of attacks. To address potential issues with log and inverse hyperbolic sine transformations (Chen and Roth, 2023; Norton, 2022), in the Appendix we also present results using maximum likelihood estimation and the raw count of attacks as an outcome.

around the cutoff.

To further tighten the identification of $\hat{\beta}$, Equation (1) has province fixed effects (ϕ_p), so we are comparing municipalities close to the cutoff that belong to the same province. The vector X_i contains pre-2011 municipal characteristics which may help improve the precision of our estimates.[5] For estimation, we employ the robust, data-driven method for optimal bandwidth selection proposed by Calonico et al. (2014), with a triangular kernel. In all specifications, we present robust bias-corrected standard errors, clustered at the province level, to take the spatial correlation in criminal presence across municipalities within a given province into account.

Before presenting the first set of results estimating the effect of wages on criminal attacks, it is important to check that the main RDD assumptions are met. The first is that the density of the forcing variable should be smooth around the cutoff. The opposite would imply that municipal administrations may falsify their population figures to sort above 5,000, thereby granting themselves higher compensation. This possibility is strongly ruled out by formal tests of the no-sorting assumption, proposed by McCrary (2008) and Cattaneo et al. (2018). Both tests cannot reject the null of equal density across the cutoff, as shown by their graphical representations in Figure A5 in the Appendix.

Next, we check whether observable, pre-2011 municipal characteristics are smooth at the cutoff. Table A1 in the Appendix reports the results of these tests. Reassuringly, of the thirty different covariates we consider, only one – the number of municipal foreign residents – exhibits a significant discontinuity around our RDD cutoff. Crucially, mu-

[5] In particular, X_i includes: a vector of indicators for the year of the last municipal election up to 2011 (Italian municipal elections are staggered); Mafia presence (see footnote 6); Log of: longitude, latitude, distance from regional capital, altitude, slope, surface, population density, foreign residents per 100 inhabitants, non-profits per 1,000 inhabitants; Share of: women, illiterate, high-school graduates, unemployed, workforce employed in agriculture; Average age, center-right vote share in the 2008 general election, turnout in the 2008 general election.

nicipalities with slightly more than 5,000 residents do not differ from those with slightly fewer than 5,000 residents in terms of Mafia presence prior to 2011,[6] nor do they exhibit different levels of procurement spending.[7]

Since wages are adjusted following each census, it is also important to note that municipalities above the RDD cutoff as of 2011 were not more likely to have more than 5,000 inhabitants as of the 2001 census. This is shown in more detail in Figure A6, which uses several RDD bandwidths to compare population size and likelihood of being above 5,000 residents as of the 2001 census, based on 2011 municipal population. This implies that, on average, for our study period differences in wages in municipalities near the cutoff begin in 2011.

5 Wages and Attacks on Municipal Cabinets' Members

We now turn to the results of our analyses. In this section, we begin with the effect of higher compensation for municipal politicians on criminal violence, accompanied by a series of identification and robustness checks. Next, in Section 6, we investigate our hypothesized mechanism by looking at the relationship between wages and procurement outcomes.

[6]Regarding Mafia presence there are three indicators from official sources that are available at the municipal level: *(i) Mafia Victims*, which is equal to 1 if municipality i experienced at least one Mafia-related homicide prior to 2011, drawn from: `www.vittimemafia.it/vittime/`; *(ii) Mafia Seizures*, equal to 1 if municipality i experienced at least one seizure of goods, properties, or firms belonging to Mafias, from cases of application of Law 646/1982: `www.benisequestraticonfiscati.it`; *(iii) Mafia Infiltrated*, which is equal to 1 if municipality i experienced at least one dissolution of its city council due to Mafia infiltration, from cases of application of Law 221/1991: `www.avvisopubblico.it/home/home/cosa-facciamo/informare/documenti-tematici/comuni-sciolti-per-mafia`. We code a municipality as having *Mafia Presence=1* if at least one of these indicators equals 1.

[7]Unfortunately, we cannot conduct meaningful, pre-treatment balance checks for criminal attacks on politicians, as the first *Avviso Pubblico* report is for the year 2010.

5.1 Main Results

Table 2 reports RDD estimates of the effect of higher wages on attacks against members of the municipal cabinet (mayor, vice mayor, and cabinet members). Across all specifications, we find strong evidence that raising the salary of members of the cabinet causes a significant increase in criminal attacks against them.

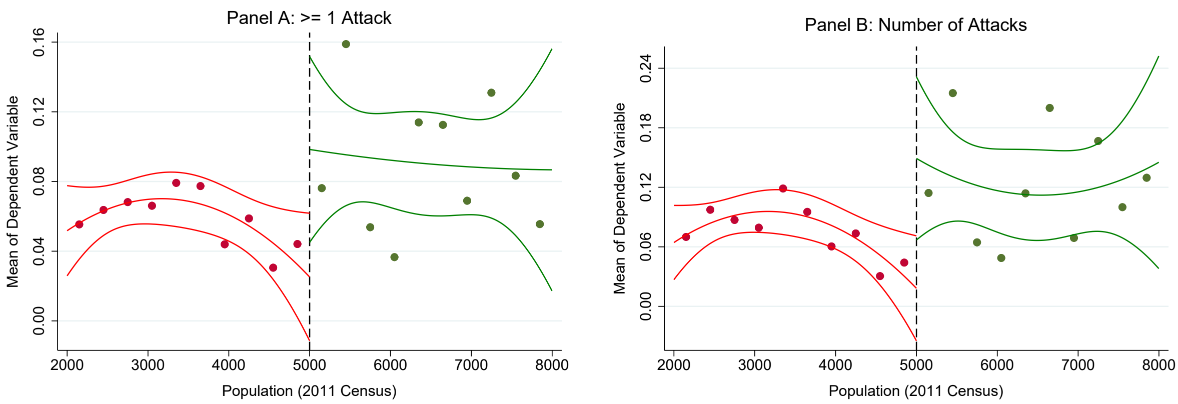
Table 2: Wages and Attacks on Members of the Municipal Cabinet

	(1)	(2)	(3)	(4)	(5)	(6)
	$\geq$ 1 Attack	$\geq$ 1 of Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population $\geq$ 5,000	.075*** (.028)	.064** (.028)	.076*** (.028)	.064** (.028)	.099*** (.037)	.083** (.036)
SD Depvar	.219	.223	.172	.173	.222	.222
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,390	1,449	1,360	1,346	1,359	1,335
Effective N	1,135	1,167	1,105	1,080	1,101	1,067
N Left	696	718	676	657	673	648
N Right	439	449	429	423	428	419

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

On the extensive margin, when including the full battery of pre-treatment controls (column 2), the estimated effect of the increase in salaries on the likelihood of being attacked is 6.4 percentage points, or 0.29 of a standard deviation. The magnitude of the effect is even larger on the intensive margin (columns 3 to 6), where it ranges between 0.37 and 0.45 of a standard deviation across the different specifications. These analyses use either the natural logarithm of attacks augmented by 1 (columns 3 and 4) or the inverse hyperbolic sine (columns 5 and 6) as the dependent variable. Figure 2 offers a graphical visualization of this effect. Consistent with the coefficients in Table 2, we notice a marked upward shift in the polynomial fit and an increase in variance at the 5,000 residents cutoff.

Figure 2: Wages and Attacks on Members the Municipal Cabinet, RDD Plots



Notes: In Panel A, each dot represents the average probability of observing at least one attack against politicians in the cabinet of municipality i between 2014 and 2020, for a given binned level of municipal population. In Panel B, each dot represents the average number of attacks against politicians in the cabinet of municipality i between 2014 and 2020, for a given binned level of municipal population.

While the pay raise we analyze concerns all members of the municipal cabinet, its magnitude varies by type of official (see Table 1). Also, mayors have different prerogatives with respect to the members of their cabinet. Therefore, it is interesting to separate the overall effects presented in Table 2. We do this in Tables A2 and A3 in the Appendix, where we report estimates for mayors and other members of the municipal cabinet separately. We see how the impact is relatively stronger for mayors than for vice mayors and cabinet members, despite the larger size of the wage increase for the latter (see Table 1). This is consistent with the descriptive evidence in Figure 1, suggesting that criminal groups focus primarily on influencing more powerful local politicians. While the sparse nature of the dependent variable makes the estimates fall short of statistical significance, it must be noted from Table A3 that the magnitude of the estimates for vice mayors and cabinet members remain sizable, ranging from 0.13 to 0.17 of a standard deviation.

5.2 Placebo and Robustness Checks

Our argument posits that the observed increase in attacks is driven by criminal groups, and concerns only politicians in the municipal cabinet who enjoy a pay raise in towns with more than 5,000 inhabitants. To corroborate this interpretation, we implement four placebo tests. First, Table 3 shows coefficients from estimating Equation (1) using the number of attacks on municipal councillors as the outcome. Since the compensation of councillors does not change at our RDD cutoff, if higher wages are what motivates increases in attacks there should not be any change in the amount of episodes involving municipal councillors. This is exactly what we see across all models of Table 3. Similar results are obtained when repeating this exercise using attacks on non-elected municipal officials (Table A7).

Alternatively, it could be that better-paid politicians – perhaps due to different policy choices – tend to create more generalized discontent that is not restricted to criminal groups. If this were the case, we would also expect an increase in non-criminal attacks (i.e., physical or verbal attacks perpetrated by ordinary citizens, see footnote 3) on members of the municipal cabinet above the RDD cutoff. Yet, Table A8 in the Appendix shows that this is not the case. A final possibility is that, rather than being the target of more attacks, politicians with higher wages are more likely to report these episodes. Under this scenario, the coefficients in Table 2 would reflect differential reporting rather than an actual increase in attacks. However, *Avviso Pubblico* sometimes flags attacks that were reported by the victim(s). If we repeat our analysis after excluding all the 271 episodes reported by the victim, our findings are virtually unchanged (see Table A9), suggesting that they are unlikely to be explained by reporting bias.

Table 3: Placebo Test - Attacks on Municipal Councillors

	(1)	(2)	(3)	(4)	(5)	(6)
	$\geq$ 1 Attack	$\geq$ 1 of Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population $\geq$ 5000	.007 (.012)	.000 (.011)	.004 (.008)	-.001 (.008)	.006 (.010)	-.001 (.010)
SD Depvar	.123	.115	.091	.081	.116	.103
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,417	1,353	1,471	1,314	1,474	1,311
Effective N	1,160	1,086	1,201	1,050	1,202	1,050
N Left	713	662	741	637	742	637
N Right	447	424	460	413	460	413

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: see footnote 4. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

We now test the robustness of the results presented in Table 2. First, we repeat our RDD estimation fitting a quadratic polynomial in municipal population around the 5,000 inhabitants threshold. As displayed in Table A4, the estimates are larger than those retrieved with a linear polynomial, although less precisely estimated.

Second, we test the sensitivity of the coefficients to the choice of the estimation bandwidth. Namely, we re-estimate Equation 1 using thirteen alternative bandwidths, covering a minimum of 800 to a maximum of 2,000 on each side of the cutoff. The results are presented graphically in Figure A7, and are reassuring: All coefficients remain positive and statistically significant even for the most extreme bandwidth choices.

Third, given issues with both the log and the inverse hyperbolic sine transformations (Chen and Roth, 2023; Norton, 2022), we check that our intensive-margin results are robust to other transformations of the dependent variable. Table A5 shows that coefficients remain positive and strongly significant when using the raw count of attacks, both in standard RDD regressions (columns 1 and 2) and when using maximum-likelihood estimators within the optimal bandwidth (columns 3 and 4). The same holds true when using the number of attacks per 5,000 inhabitants as an outcome (Table A6), which also facilitates a substantive interpretation of the results: The average city at the cutoff incurs 0.15 additional criminal attacks against members of the cabinet over the study period.

Fourth, given that criminal attacks are rare events, we must make sure that the coefficients are not driven by a handful of municipalities in a specific area. To this end, we repeat the estimation of $\hat{\beta}$ twenty times, excluding all municipalities from a given region in each iteration. As shown in Figure A8 in the Appendix, the estimate of the treatment effect is positive and strongly significant in all of the twenty replications. Finally, we check the validity of the RDD by reporting estimates around nominally irrelevant population cutoffs. To this end, we re-estimate Equation (1) thirty times, each time using a different value of population as a falsified criterion to assign the treatment. As shown in Figure A9, the highest jump in attacks toward the municipal cabinet is detected at the real cutoff of 5,000 residents.

5.3 Alternative Explanations

We close this Section with a battery of tests aimed at assessing the role of two reforms passed during our sample period that introduced provisions that changed discontinuously at the RDD cutoff of 5,000 inhabitants. The first is Law 122/2010, whose implementation was postponed multiple times and nominally enacted starting in 2017. This provision requires municipalities with fewer than 5,000 inhabitants to jointly deliver a set of services by forming a union of municipalities (or joining an existing one).[8] Yet, three related pieces of evidence suggest that this rule is unlikely to play a key role in explaining our results. First, as shown in the last two rows of Table A1, municipalities across the cutoff are equally likely to be members or leaders of a union of municipalities.

Second, in our analyses of procurement, we will focus exclusively on contracts offered by municipalities as independent entities, not as union members. In Table A10 we show that there is no robust evidence of differences in the amount of procurement at the cutoff. However, if anything, municipalities with just under 5,000 inhabitants appear to be offering *more* procurement contracts than their counterparts above the cutoff, suggesting that Law 122/2010 did not effectively constrain procurement spending during our sample period. Finally, as shown in Table A11, the results in Table 2 are robust to the exclusion of all municipalities – both below and above the 5,000 inhabitants threshold – that are part of unions of municipalities.

Next, we evaluate the role of Law 148/2011 that, among other provisions, introduced additional experience requirements for the accountants in charge of reviewing the budgets of municipalities comprising more than 5,000 inhabitants. To do so, we follow Vannutelli (2022) and exploit the staggered introduction of this rule across municipalities, determined by idiosyncratic differences in the end of the mandates of the last cohort of pre-reform auditors. Therefore, in Table A12 we re-estimate Equation (1) using only attacks occurring before the new auditors took office in each municipality as dependent variable.[9]

[8] Details at: https://dait.interno.gov.it/territorio-e-autonomie-locali/documentazione/obbligo-di-esercizio-associato-di-funzioni-fondamentali

[9] We are grateful to Silvia Vannutelli for sharing data on auditors' nomination dates.

While unavoidably more imprecise,[10] the estimates remain of similar magnitude to those reported in Table 2 (around 0.2 to 0.3 of a standard deviation).

6 Mechanisms: Wages and Public Procurement

In this section we provide evidence that an important channel through which higher wages trigger more attacks against local politicians is the lower likelihood of better-paid politicians to cater to criminal groups in the procurement process.

6.1 Italian Public Procurement

In what follows, we summarize the key features of procurement for public works in Italy during our period of observation (2013-2020). Italian public procurement is articulated in three stages: The contract-design stage, the selection stage, and the post-competition stage.

The contract-design stage consists of the call for tender, where the buyer details the object of the contract and the relevant technical specifications, and assigns a reservation value to the project. This represents the maximum value that the buyer would be willing to pay. At the municipal level, an engineer – who is appointed by the mayor on a contract-by-contract basis – estimates the reservation value and identifies the types and quantities of inputs needed.

In addition to the project's details and the reservation value, the call for tender must define the selection procedure and indicate whether subcontracting will be allowed.[11] These decisions are made by a contracting officer, called *Responsabile Unico del Progetto*

[10] For this exercise we can only use 30.4% of all attacks on municipal cabinets that took place throughout our sample period.

[11] When allowed, subcontracting cannot exceed 30% of the reservation value.

(RUP), who is again directly appointed by the mayor on a contract-by-contract basis.[12] There are three possible selection procedures: direct assignments, negotiated procedures, and open procedures. Direct assignment does not require any form of competition, and – absent exceptional conditions – is only allowed for contracts with reservation value below €40,000. Contracts below this value are not actively monitored by ANAC, and the contract's awardee does not need to be publicly announced.

Contracts above €40,000 can be assigned via either negotiated or open procedures. In negotiated procedures, once the call for tender has been issued, the RUP can choose a set of firms and consult with them to negotiate the terms of the contract. However, the RUP is still required by law to hear from any qualified firm that expresses interest in the contract. When these one-on-one negotiations are completed, the buyer solicits bids from a set of firms, thus determining both the size and the characteristics of the pool of bidders. Given the higher degree of discretion, the law restricts the use of certain negotiated procedures to contracts below a given reservation value. For the purposes of the present study, one important threshold is €200,000, above which the law forbids the use of the *Cottimo Fiduciario*. This negotiated procedure has a more expedited consultation phase, and allows the buyer to directly award the contract to one of the firms consulted.[13]

In open procedures, any interested firm can directly bid for the contract. For both negotiated and open procedures, firms send a sealed bid with their proposed rebate, expressed as a percentage of the reservation value. Before the bids are sent, Law 159/2011 requires firms bidding for contracts at values above €150,000 to undergo police screenings that check for possible links to organized crime. These screenings may result in exclusion

[12] The RUP is usually picked among the management-level bureaucrats within the municipal administration (Decarolis et al., 2021).

[13] The reader can refer to Article 3 of Dlgs 163/2006 for further details on *Cottimo Fiduciario*.

from the competition, and in criminal charges to the firms' owners and collaborators (Daniele and Dipoppa, 2022).

Once bids are received, contracts are assigned following one of two awarding criteria, as specified in the call for tender: the lowest price or the most economically advantageous offer (*Offerta Economicamente Più Vantaggiosa*, or OEPV). The lowest price criterion awards the contract to the firm bidding the highest rebate, after trimming extreme bids.[14]

OEPV, by way of contrast, considers several other parameters in addition to the proposed rebate, and assigns a score to each offer based on a combination of these parameters. The firm with the highest score is awarded the contract. To limit discretion and discrimination, the law prescribes that the relevant parameters must refer to the bid, not to the bidding firms. Yet, as noted in Decarolis et al. (2021), OEPV does allow for the use of more discretion on behalf of the buyer in identifying the winning firm.

6.2 ANAC Data on Procurement Contracts

To analyze the effects of local politicians' wages on municipal procurement, we leverage official procurement data from the Italian National Anti-Corruption Authority (ANAC).[15] We draw information on procurement by Italian municipalities between 2013 and 2020. This amounts to 544,828 contracts, offered by 8,063 unique municipalities.[16]

The mean reservation value of the contracts is €665,555, with a median of €75,806. Contracts offered by municipalities around the 5,000 residents cutoff are less valuable: The average reservation value is €228,972 and the median reservation value is €66,195.

[14]First, 10% of the bids with the lowest rebate and 10% of the bids with the highest rebate are excluded. Next, an anomaly threshold is calculated, and bids with a rebate higher than this anomaly threshold are also excluded. This threshold is the sum of the average rebate plus the average deviation of the other rebates from that average. See Coviello et al. (2018) and Coviello and Mariniello (2014) for details.

[15]Data sets available at: `dati.anticorruzione.it/opendata/dataset?q=cig-`.

[16]This figure exceeds the current number of Italian municipalities (7,904) due to mergers that occurred between 2013 and when this paper was written.

Contracts for public works are 35.8% of the total. The remaining are for the delivery of goods (15.7%) and services (48.5%). The selection procedure is direct assignment in 41.1% of cases, open in 16.3% of cases, and negotiated for the remaining 42.6%. Within the latter category, 51,699 contracts use *Cottimo Fiduciario* (22.3% of all those employing negotiated procedures).

For 204,860 contracts of relatively higher value, ANAC also reports information regarding the competition phase. These relate to 7,503 unique municipalities, and concentrate in the public works sector (52%). In negotiated procedures, the average number of firms invited to bid is thirteen. The awarding criterion is the lowest price for 58.8% of contracts, and OEPV in the remaining 41.2%. The awardee subcontracts in 10.5% of occurrences (19.2% for public works and only 1.1% for other contracts), or 28.6% of the times it was allowed to do so by the call for tenders.

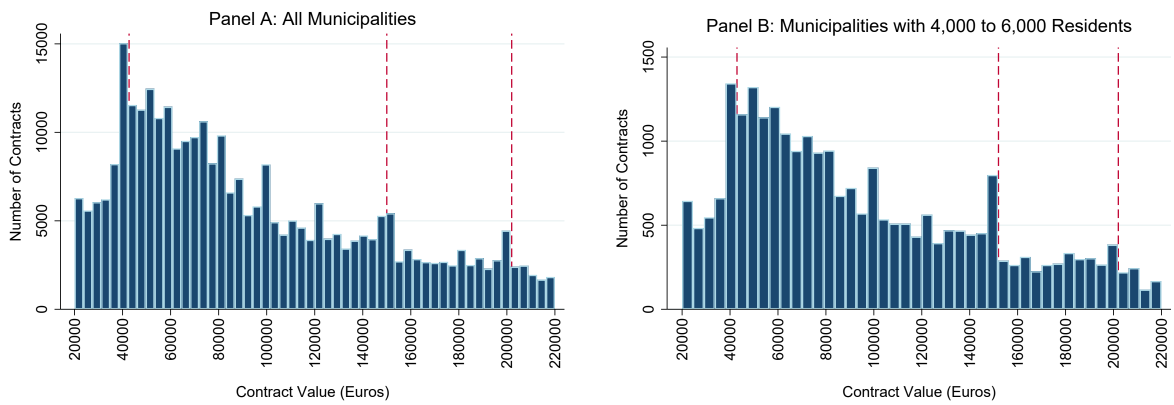
6.3 Measuring Corruption in Public Procurement

We define three sets of outcomes to capture the propensity of municipal administrations to favor corruption in procurement; each refers to one of the three stages described in Subsection 6.1. First, for the contract-design stage, we look at the tendency of municipalities to cluster the reservation values of their contracts just below each of the three thresholds previously mentioned: €40,000, €150,000, and €200,000. While these exact cutoffs are specific to Italian regulations, analogous thresholds exist in several countries, and the strategic manipulation of reservation values has indeed been studied in other contexts (see, e.g., Palguta and Pertold, 2017; Szucs, 2021).

As described in Subsection 6.2, reservation values are calculated by an engineer appointed by the mayor. The engineer chooses the quality and quantity of inputs to be used, but not their prices, which she must source from a standardized list common to all public administrations. As argued by Decarolis (2014) and Coviello et al. (2018), this does not grant the buyer full control over the final reservation price. Yet, manipulation remains very frequent, as shown by Baltrunaite et al. (2021) and Coviello et al. (2022).

Since these recent contributions focus on higher value thresholds than those analyzed in this paper, we hereby provide both descriptive and analytical evidence of manipulation around our three cutoff values of interest. As a first step, Figure 3 plots the distribution of reservation values within our sample, for all municipalities (Panel A) and for municipalities in the vicinity of the RDD cutoff of 5,000 inhabitants (Panel B). In Panel A, jumps are visible both below €40,000 – the threshold that forbids direct assignment of contracts – and below €200,000, the threshold that limits the use of *Cottimo Fiduciario*. In the more restricted sample around the RDD cutoff (Panel B), we also see a clear jump just below €150,000, the threshold above which anti-Mafia screenings of bidders are required.

Figure 3: Distribution of Reservation Values for Procurement Contracts, 2013-2020



Notes: Bins group reservation values into intervals of 4,000 euros. The dashed red lines correspond to €40,000, €150,000, and €200,000. Total number of contracts in full sample: 544,828. Total number of contracts for municipalities with 4,000 to 6,000 residents: 44,917.

In the Appendix – Figures A10 to A12 – we focus on contracts for public works and present more rigorous checks for manipulation, based on a series of McCrary density tests (McCrary, 2008). For both €40,000 and €150,000, these tests reveal very high levels of sorting, as shown by the largely negative and strongly significant difference in log densities across the cutoff. A negative log difference also is retrieved at €200,000 in the full sample, but it does not achieve statistical significance at conventional levels.

Having established that bunching of reservation values does take place in our sample of municipal contracts, we construct four outcome variables, which will be used to test whether the degree of bunching varies around the 5,000 residents cutoff. First, for each municipality i between 2013 and 2020, we proxy for bunching by computing the difference in the share of contracts within €10,000 to the left and the share of contracts within €10,000 to the right of each threshold k:

$$Bunching_{i,k} = \left(\frac{Contracts_i \in [k - 10000, k]}{Contracts_i}\right) - \left(\frac{Contracts_i \in [k, k + 10000]}{Contracts_i}\right), \quad (2)$$

for $k \in \{40000, 150000, 200000\}$. Furthermore, to simultaneously capture bunching occurring at all three thresholds in a given municipality, we compute:

$$Bunching_i = \sum_k Bunching_{i,k}, \;\; k \in \{40000, 150000, 200000\} \quad (3)$$

Regarding the selection stage, we are interested in establishing whether municipalities with better-paid cabinets invited more firms to bid in their negotiated procedures, a signal that such procedures are not being used instrumentally to curb competition (Decarolis et al., 2021). Thus, for each municipality, we calculate the log average number of firms invited to bid in all its negotiated procedures between 2013 and 2020.

Finally, at the post-competition stage, we calculate the share of contracts awarded by municipality i that are subcontracted by the awardee. Subcontracting is an important outcome to consider in the Italian context. In fact, in addition to manipulating auctions to obtain the direct assignment of a contract, criminal firms often reap public resources by subcontracting at a higher price than the one they bid (Caneppele and Martocchia, 2014). Recent work has demonstrated that firms under investigation for criminal infiltration are significantly more prone to subcontracting (Decarolis et al., 2021).

Importantly, for all three sets of outcomes, we distinguish between contracts for public works and other contracts, i.e., contracts for the delivery of goods or services. Contracts for public works, which constitute 35.7% of the contracts offered by municipalities during our period of observation, are known to be the main object of interest to Italian criminal firms, that are concentrated in the construction sector (Transcrime, 2013; ANAC, 2017). Therefore, the dependent variables based on these contracts will be our main outcomes of interests, while those relative to contracts for goods and services will serve as placebo outcomes.

6.4 The Effect of Wages on Procurement Outcomes

Table 4 reports RDD estimates from Equation 1, using our overall measure for bunching of reservation values as the dependent variable (see Equation 3). Columns (1) and (2) display estimates for the full sample of contracts, while columns (3) and (4) focus on the relevant subsample of public works contracts. Finally, columns (5) and (6) report coefficients for other contracts (i.e., contracts for the delivery of goods and services) which, as explained in Subsection 6.3, constitute a placebo category.

The negative and significant coefficients in columns (3) and (4) show that in municipalities above the cutoff – where members of the municipal cabinet enjoy the pay raise outlined in Table 1 – the reservation values of procurement contracts for public works are less concentrated below the three key thresholds described in Subsections 6.1 and 6.3. The magnitude of the effect is sizable – around one third of a standard deviation.

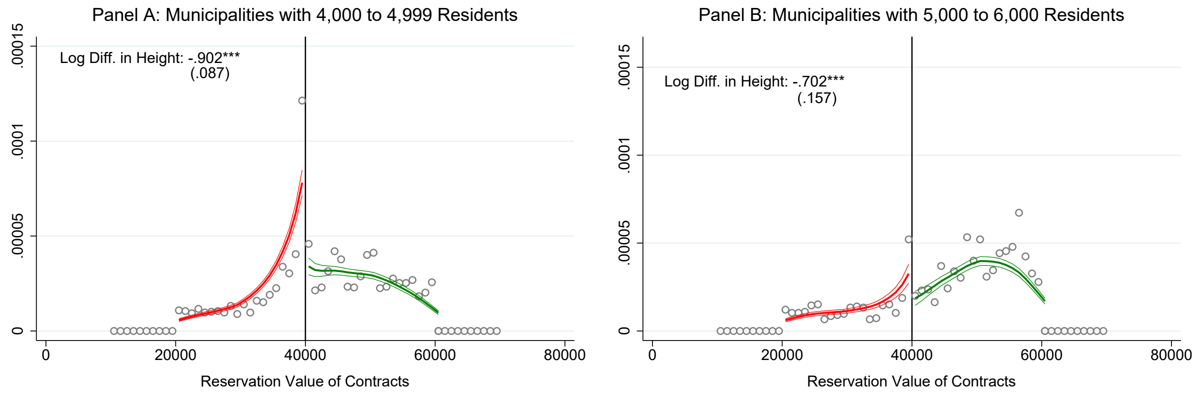
Table 4: Wages and Procurement - Bunching of Reservation Values

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Goods & Services	Goods & Services
Population $\geq$ 5000	-.026**	-.021*	-.057***	-.051**	-.011	-.006
	(.011)	(.012)	(.019)	(.020)	(.014)	(.017)
SD Depvar	.104	.106	.153	.154	.146	.142
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,150	914	1,090	979	1,420	984
Effective N	926	707	857	759	1,150	763
N Left	556	403	507	438	708	441
N Right	370	304	350	321	442	322

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is $Bunching_i$, as jointly defined by equations 2 and 3. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A graphical depiction of these dynamics, by means of RDD plots, is in Figure A13. Tables A13 to A15 in the Appendix show separate RDD coefficients for bunching below each of the three thresholds. Therefore, the results for the contract-design stage indicate that municipalities with better-paid cabinets are significantly *less* likely to strategically sort the value of their contracts to avoid more stringent regulations, particularly as it pertains to the selection of bidding firms. To illustrate and describe these regression findings more intuitively, Figure 4 uses McCrary density tests (McCrary, 2008) for bunching at €40,000, run separately on each side of the RDD cutoff (5,000 municipal residents). A quick comparison of the two panels shows how, while municipalities in both subsamples do tend to sort their contracts below 40,000 euros, that tendency is much attenuated for those just above 5,000 residents (Panel B), where cabinets enjoy higher wages.

Figure 4: McCrary Tests of Reservation Value Bunching Below €40,000, Contracts for Public Works, by Treatment Status



Notes: In both panels, densities refer only to contracts for public works drafted between 2013 and 2020. Number of contracts: 194,926. Number of unique municipalities between 4,000 and 6,000 residents: 467. Number of unique municipalities between 5,000 and 6,000 residents: 335.

We now move to the analyses using our second outcome, the log average number of firms invited to negotiated procedures. The results are displayed in Table 5. The RDD estimates in columns (3) and (4), which refer to contracts for public works, show how administrations with better-paid cabinets are more inclusive in their negotiated procedures. The coefficient in column (4), for instance, implies that the log average number of firms invited in municipalities with just above 5,000 residents is 0.25 standard deviations higher than in municipalities with just below 5,000 residents.

This is an important result, as the discretionary exclusion of firms is a well-known method to condition the assignment of contracts. Indeed, having too few firms invited to bid – especially when using negotiated procedures – has been shown to favor criminal infiltration (Decarolis et al., 2021). Similar to the analyses for bunching, this differential is detected only in procurement for public works, the primary target of Mafia-controlled firms (Transcrime, 2013; ANAC, 2017). RDD plots for this outcome are in Figure A14.

Table 5: Wages and Procurement - Firms Invited to Negotiated Procedures

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Goods & Services	Goods & Services
Population $\geq$ 5000	.153*	.164*	.222**	.207**	.093	.080
	(.086)	(.088)	(.093)	(.097)	(.108)	(.124)
SD Depvar	.830	.832	.826	.831	.930	.973
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,130	1,085	1,049	1,000	1,678	1,218
Effective N	825	774	720	686	855	619
N Left	492	455	418	387	510	360
N Right	333	319	302	289	345	259

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the log average number of firms invited to bid in the negotiated procurement procedures of municipality i between 2013 and 2020. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

We conclude our empirical exploration of the link between politicians' remuneration and public procurement by looking at the post-competition stage. Specifically, we examine the propensity to subcontract municipal contracts to a third party. As explained in Subsection 6.3, subcontracting is a well-known method that criminal firms employ to realize profits after obtaining procurement contracts (Caneppele and Martocchia, 2014; Decarolis et al., 2021). Thus, we use the share of subcontracted contracts as a proxy for the frequency at which criminal firms win procurement contracts in a given municipality.

Columns (3) and (4) of Table 6 show how higher wages for the municipal cabinet significantly reduce the likelihood that public works are partially subcontracted. This reduction is economically sizable, with magnitudes between 0.16 and 0.25 of a standard deviation. A graphical illustration of this result is in Figure A15.

Table 6: Wages and Procurement - Share of Contracts Subcontracted

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Goods & Services	Goods & Services
Population $\geq$ 5000	-.023**	-.045***	-.042***	-.027*	-.008	-.008
	(.012)	(.012)	(.015)	(.015)	(.006)	(.007)
SD Depvar	.117	.119	.170	.172	.067	.076
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,454	1,250	1,289	1,433	1,372	1,005
Effective N	1,154	978	1,002	1,109	971	698
N Left	714	593	610	683	591	407
N Right	442	385	392	426	380	291

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the share of contracts awarded by municipality i between 2013 and 2020 that were subsequently subcontracted. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The analyses presented to this point identify a tendency for better-paid cabinets to promote transparency and competition in procurement. But are these dynamics actually driven by increased wages? To answer this question, Table 7 presents a placebo exercise, using procurement outcomes measured in the pre-census period, i.e., for public works contracts offered between 2007[17] and 2011.[18] If the results documented in columns (3) and (4) of Tables from 4 to 6 stem from increased pay, they should not hold for contracts

[17]The earliest year for which ANAC data on procurement contracts are available is 2007.

[18]As shown in Table A1 and Figure A6, municipalities above the RDD cutoff of 5,000 residents in 2011 were not more likely to have more than 5,000 residents as of 2001, the year of the previous census.

that precede the adjustment in wages. This is indeed what we see for all three outcomes in Table 7. If anything, the coefficients flip signs, and are not statistically significant at conventional levels. One caveat is that, as explained in Section 2, municipalities with more than 5,000 inhabitants were under tighter budget constraints up to 2013, as a result of the DSP.

Table 7: Placebo - Wages and Procurement for Public Works, 2007 to 2011

	(1)	(2)	(3)	(4)	(5)	(6)
	Bunching Values	Bunching Values	Invited Firms	Invited Firms	Subcontr. Works	Subcontr. Works
Population $\geq$ 5000	.009 (.016)	.018 (.019)	-.168 (.144)	-.143 (.147)	.050 (.042)	.068 (.042)
SD Depvar	.124	.126	.821	.822	.320	.320
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,411	825	1,893	1,824	1,358	1,358
Effective N	1,141	641	716	671	846	835
N Left	703	365	460	430	519	512
N Right	438	276	256	241	327	323

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). Outcomes are measured on contracts for public works offered between 2007 and 2011. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

7 Selection or Behavior?

Taken together, the results presented in Section 5 and Section 6 indicate that increasing wages leads municipal cabinets to promote competition in public procurement, but also to suffer more criminal attacks. One remaining question is through which channels higher wages are capable of disciplining municipal politicians in our context. To shed light on this additional aspect, this section presents the results of several empirical exercises that test the three main existing theories for why higher pay should induce more honesty among public officials.

We start by evaluating the role of improved selection (Besley and McLaren, 1993; Bond, 2008). To do so, we repeat our RDD analyses, but using a data set at the municipality-term level, and employing sixteen different characteristics of mayors elected since 2011 as outcome variables. If selection is driving the observed difference in procurement administration, we would expect mayors elected in municipalities with just above 5,000 residents to possess different attributes than those governing municipalities with just below 5,000 residents.

The results are shown in Table 8, which displays coefficients for both personal (Panel A) and party (Panel B) characteristics of our pool of mayors. Mayors elected across the cutoff do not differ in terms of demographics, professional qualification or education. This latter null result differs from the findings of Gagliarducci and Nannicini (2013) for the years 1993-2001, likely due to the lower variation in mayors' education during our sample period. The only significant effect we retrieve is a lower likelihood of mayors to be supported by a right-wing or far-right party in municipalities that are above the 5,000 residents cutoff (Panel B, columns 5 and 6).

Does this discrepancy in political affiliation explain the differential management of procurement illustrated in Subsection 6.4? To answer this question, Tables A16 to A21 use an RDD to gauge the effects of the narrow victories of right-wing and far-right mayors on procurement outcomes. The only differential pattern we detect is a higher propensity of these mayors to sort reservation values below regulatory thresholds. While this may in principle explain part of our results on procurement, Tables A22 and A23 reveal no differences in criminal attacks in municipalities led by a right-wing or far-right mayor.

A natural limitation of this analysis for the role of selection is that we are restricted to observable and available measures of candidate attributes. We cannot test for differences in unobservable traits – such as intrinsic honesty and public-service motivation. However, we take this as suggestive evidence that selection does not play a key role in improving procurement practices or triggering greater criminal attacks in the context of our study.

Table 8: Selection - Wages and Features of Elected Mayors

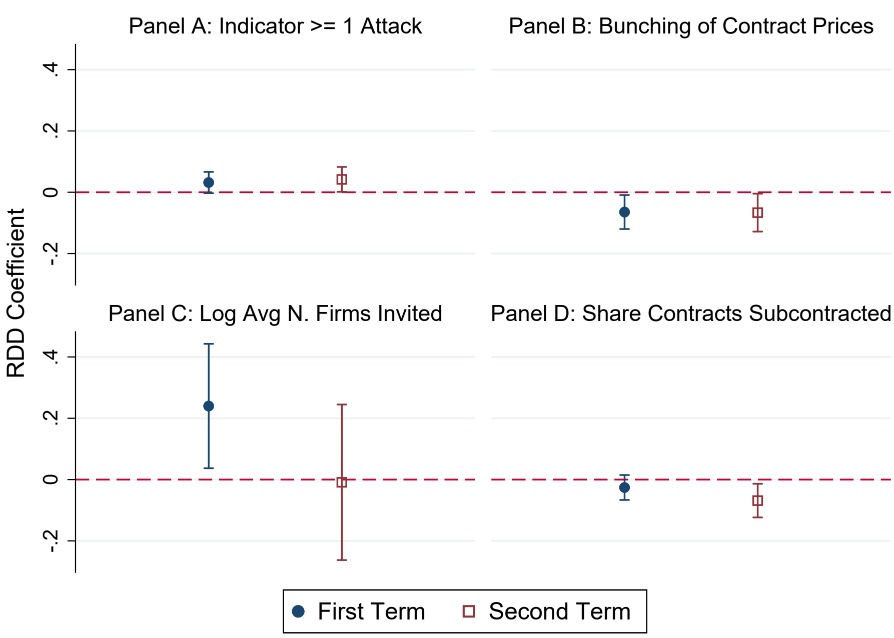
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Personal Features	Mayor Age	Mayor Female	Mayor Local	Mayor Educ.	Mayor College	Mayor Skilled	Mayor Tech	Mayor Incumb.
Population $\geq$ 5000	-.004	-.052	.045	.199	-.012	-.083	-.051	-.001
	(1.187)	(.042)	(.048)	(.315)	(.053)	(.058)	(.036)	(.036)
SD Depvar	10.43	.366	.470	3.21	.500	.496	.407	.473
EY FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov. FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No
Polynomial	1st	1st	1st	1st	1st	1st	1st	1st
Bandwidth	1,318	1,347	1,704	1,722	1,560	1,346	2,307	1,233
Effective N	2,187	2,240	2,891	2,801	2,508	2,116	3,962	2,060
N Left	1,331	1,364	1,836	1,780	1,555	1,297	2,639	1,243
N Right	856	876	1,055	1,021	953	819	1,323	817
B. Party Features	National Party	Far Left	Left	Indep.	Right	Far Right	Aligned Central	Aligned Region
Population $\geq$ 5000	-.037	-.002	.023	.037	-.068**	-.080***	-.002	-.029
	(.041)	(.001)	(.029)	(.041)	(.028)	(.028)	(.026)	(.032)
SD Depvar	.327	.001	.236	.327	.247	.237	.255	.287
EY FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov. FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	No
Polynomial	1st	1st	1st	1st	1st	1st	1st	1st
Bandwidth	1,732	1,292	1,478	1,732	1,919	1,666	1,955	2,107
Effective N	2,406	1,787	2,034	2,406	2,731	2,319	2,781	3,030
N Left	1,516	1,081	1,254	1,516	1,737	1,457	1,778	1,962
N Right	890	706	780	890	994	862	1,003	1,068

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the municipality level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A second prominent theory posits that higher wages improve officials' behavior by increasing their commitment to keep their job (Shapiro and Stiglitz, 1984; Van Rijckeghem and Weder, 2001). In our context, this means that better-paid cabinets may behave more honestly out of stronger re-election incentives, as the pay raise increases the value of an additional term. The intuition behind this hypothesis is that corruption hurts re-election prospects, an assumption that has found limited support (De Vries and Solaz, 2017).

To analyze the role of re-election incentives, we exploit the fact that mayors in our sample face a two-term limit (see Table 1). If re-election incentives are inducing more honest behavior, the effects of wages on procurement and attacks should be driven by cabinets led by a first-term mayor. However – as shown in Figure 5 – re-estimating our RDD equation separately for cabinets led by a first- and a second-term mayor does not show substantive differences. One exception is the tendency to invite more firms, which is indeed more pronounced in municipalities governed by a first-term mayor.

Figure 5: Re-Election Incentives? Attacks and Procurement, by Presence of Term Limit



Notes: In Panels B, C and D, the sample of contracts is restricted to contracts for public works.

A final possibility is that higher wages discipline politicians by reducing the marginal value of bribes. Research has argued that well-remunerated public servants may not want to risk prosecution by extracting illicit rents from their offices (Di Tella and Schargrodsky, 2003; Chen and Liu, 2018). To check whether this explains our results, we proceed in three steps. First, we expand our dataset to the municipality-year level. Next, for each municipality, we identify the incumbent mayor as of 2011 – the last year before wages were adjusted to the population of the new census – and only keep municipality-year observations associated to that mayor, before and after 2011. With this data in hand, we run separate regressions on procurement outcomes for years up to 2011 and after 2011.

This strategy, by keeping only observations associated to the same mayor for each municipality, shuts off any potential selection effects. Next, by conducting separate RDD analyses for years before and after the census, we compare the behavior of the exact same mayor, but before and after the increase in wages. The results, summarized in Table 9, are consistent with increased wages providing differential incentives, by reducing the marginal gains from dishonest behavior. In fact, the effects on procurement outcomes documented in Section 6.4 only emerge in post-census years (even-numbered columns).

Overall, the analyses in this section indicate behavioral improvements – motivated by lower incentives to engage in corruption – as the main driver of the effect of higher salaries on procurement outcomes. Nonetheless, a word of caution is necessary. First, similar to those in Table 7, the coefficients in Table 9 may incorporate the effects of the tighter budget constraints mandated by the DSP, which were not extended to municipalities with fewer than 5,000 inhabitants until 2013. Second, while the battery of mayoral features employed in Table 8 is certainly comprehensive, it is still possible that higher wages bring about differential selection along other unobservable traits of mayoral candidates.

Table 9: Incentives - RDD for Continuing Mayors, Pre vs. Post 2011

	(1)	(2)	(3)	(4)	(5)	(6)
	Bunching Pre2011	Bunching Post2011	Inviting Pre2011	Inviting Post2011	Subcontract Pre2011	Subcontract Post2011
Population	.029**	-.051**	-.069	.246*	.004	-.023
$\geq$ 5000	(.015)	(.020)	(.114)	(.141)	(.035)	(.029)
SD Depvar	.245	.284	1.08	1.10	.367	.319
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,193	955	1,866	993	1,438	1,444
Effective N	3,205	2,597	1,506	1,202	1,933	2,395
N Left	1,914	1,537	938	692	1,187	1,483
N Right	1,291	1,060	568	510	746	912

Notes: Observations are at the municipality-year level, and the dependent variables refer only to contracts for public works. Observations included are only the municipality-years with the same mayor who was in office as of 2011. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth.

8 Conclusion

This paper examines the effects of increasing politicians' wages in the presence of criminal pressure groups, which use both bribes and violence to influence policymaking (Dal Bó and Di Tella, 2003; Dal Bó et al., 2006; Lessing, 2015). To identify the causal effect of higher wages, we take advantage of the Italian scheme for remunerating municipal politicians, which attributes a sizable pay raise to municipal cabinets in towns housing more than 5,000 inhabitants. The analyses, based on a Regression Discontinuity Design, yield three key findings. On the one hand, by fostering competition for public works contracts, better-paid local cabinets are significantly more likely to prevent corruption in public procurement. On the other hand, and plausibly because of their effect on corruption in public procurement, higher wages also increase the number of criminal attacks against members of municipal cabinets. Finally, the disciplining effect of wages on corruption appears to be driven by better incentives rather than by improved selection.

These findings constitute an important contribution to two strands of literature in political economy. First, they provide causal empirical evidence of the link between higher public-sector wages and lower levels of corruption. While theoretically grounded for several decades (Becker and Stigler, 1974; Besley and McLaren, 1993), this relationship had received little empirical attention to date, and only weak support (Di Tella and Schargrodsky, 2003; Chen and Liu, 2018; D'Andrea, 2019). Second, they augment our understanding of the strategies of organized criminal groups. Recent studies have demonstrated that organized crime reacts quickly to policies that directly target its activities (Daniele and Dipoppa, 2022; Enríquez, 2022). The increase in violence toward better-paid politicians documented in our study adds to this literature, showing how criminal groups can also adapt to a number of other changes in the institutional environment.

Finally, the insights of our study raise important questions for policymakers, especially in contexts with a strong presence of organized crime. In fact, they demonstrate that better remuneration of public officials can constitute an important tool to curb corruption. However, they also show that such a tool can be undermined by criminal violence in the short- to medium-run. Furthermore, as argued in previous research (Dal Bó et al., 2006; Daniele, 2019), a boost in violence against politicians can scare honest and competent administrators away from public office. If this is the case, short-term gains in effort and honesty may be offset by a long-term reduction in the quality of elected politicians, with negative consequences for policy outcomes. Therefore, institutional reforms aimed at improving public-sector wages should account for such unintended consequences, and adopt appropriate countermeasures to ensure the safety and integrity of public officials.

References

ANAC (2017). Analisi istruttoria per l'individuazione di indicatori di rischio corruzione e di prevenzione e contrasto nelle amministrazioni pubbliche coinvolte nella politica di coesione. Technical report.

ANAC (2019). La corruzione in Italia (2016–2019): Numeri, luoghi e contropartite del malaffare.

Baltrunaite, A., Giorgiantonio, C., Mocetti, S., and Orlando, T. (2021). Discretion and Supplier Selection in Public Procurement. *The Journal of Law, Economics, and Organization*, 37(1):134–166.

Barone, G. and Narciso, G. (2015). Organized crime and business subsidies: Where does the money go? *Journal of Urban Economics*, 86:98–110.

Becker, G. S. and Stigler, G. J. (1974). Law Enforcement, Malfeasance, and Compensation of Enforcers. *The Journal of Legal Studies*, 3(1):1–18.

Besley, T. (2004). Paying Politicians: Theory and Evidence. *Journal of the European Economic Association*, 2(2-3):193–215.

Besley, T. and McLaren, J. (1993). Taxes and Bribery: The Role of Wage Incentives. *The Economic Journal*, 103(416):119–141.

Boas, T. C., Hidalgo, F. D., and Richardson, N. P. (2014). The Spoils of Victory: Campaign Donations and Government Contracts in Brazil. *The Journal of Politics*, 76(2):415–429.

Bond, P. (2008). Persistent Court Corruption. *The Economic Journal*, 118(531):1333–1353.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326.

Caneppele, S. and Martocchia, S. (2014). Italian Mafias, Public Procurement and Public Works in Southern Italy. In *Organized Crime, Corruption, and Crime Prevention. Essays in Honor of Ernesto U. Savona.* Edited by Stefano Caneppele and Francesco Calderoni.

Caselli, F. and Morelli, M. (2004). Bad politicians. *Journal of Public Economics*, 88(3):759–782.

Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation Testing Based on Density Discontinuity. *The Stata Journal*, 18(1):234–261.

Chen, J. and Roth, J. (2023). Log-like? Identified ATEs defined with zero-valued outcomes are (arbitrarily) scale-dependent. Technical report, Working Paper.

Chen, Y. and Liu, Q. (2018). Public-sector wages and corruption: An empirical study. *European Journal of Political Economy*, 54:189–197.

Coviello, D., Guglielmo, A., Lotti, C., and Spagnolo, G. (2022). Procurement with Manipulation. *CEPR Discussion Paper No. DP17063*.

Coviello, D., Guglielmo, A., and Spagnolo, G. (2018). The Effect of Discretion on Procurement Performance. *Management Science*, 64(2):715–738.

Coviello, D. and Mariniello, M. (2014). Publicity requirements in public procurement: Evidence from a regression discontinuity design. *Journal of Public Economics*, 109:76–100.

Dal Bó, E., Dal Bó, P., and Di Tella, R. (2006). “Plata o Plomo?”: Bribe and Punishment in a Theory of Political Influence. *American Political Science Review*, 100(1):41–53.

Dal Bó, E. and Di Tella, R. (2003). Capture by Threat. *Journal of Political Economy*, 111(5):1123–1154.

D'Andrea, A. (2019). Mayor's Wage and Public Procurement. *BAFFI CAREFIN Centre Research Paper*, (2019-125).

Daniele, G. (2019). Strike one to educate one hundred: Organized crime, political selection and politicians' ability. *Journal of Economic Behavior & Organization*, 159:650–662.

Daniele, G. and Dipoppa, G. (2017). Mafia, elections and violence against politicians. *Journal of Public Economics*, 154:10–33.

Daniele, G. and Dipoppa, G. (2022). Doing Business Below the Line: Screening, Mafias and Public Funds. *Forthcoming, The Journal of Law, Economics, and Organization.*

Daniele, G. and Giommoni, T. (2021). Corruption under Austerity. *CEPR Discussion Paper No. DP15891.*

De Vries, C. E. and Solaz, H. (2017). The Electoral Consequences of Corruption. *Annual Review of Political Science*, 20:391–408.

Decarolis, F. (2014). Awarding Price, Contract Performance, and Bids Screening: Evidence from Procurement Auctions. *American Economic Journal: Applied Economics*, 6(1):108–32.

Decarolis, F., Fisman, R., Pinotti, P., and Vannutelli, S. (2021). Rules, Discretion, and

Corruption in Procurement: Evidence from Italian Government Contracting. *NBER Working Paper 28209*.

Di Cataldo, M. and Mastrorocco, N. (2021). Organised Crime, Captured Politicians, and the Allocation of Public Resources. *Forthcoming, The Journal of Law, Economics, and Organization*.

Di Tella, R. and Schargrodsky, E. (2003). The Role of Wages and Auditing during a Crackdown on Corruption in the City of Buenos Aires. *The Journal of Law and Economics*, 46(1):269–292.

Dipoppa, G. (2022). How Criminal Organizations Expand to Strong States: Migrant Exploitation and Political Brokerage in Northern Italy. *Working Paper*.

DNA (2019). Relazione annuale sulle attività svolte.

Eggers, A. C., Freier, R., Grembi, V., and Nannicini, T. (2018). Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions. *American Journal of Political Science*, 62(1):210–229.

Enríquez, J. R. (2022). Democracy under Assault: Electoral Reform and Political Violence. *Working Paper*.

Ferraz, C. and Finan, F. (2009). Motivating Politicians: The Impacts of Monetary Incentives on Quality and Performance. Technical report, National Bureau of Economic Research.

Gagliarducci, S. and Nannicini, T. (2013). Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection. *Journal of the European Economic Association*, 11(2):369–398.

Keane, M. P. and Merlo, A. (2010). Money, Political Ambition, and the Career Decisions of Politicians. *American Economic Journal: Microeconomics*, 2(3):186–215.

Le Moglie, M. and Sorrenti, G. (2020). Revealing "Mafia Inc."? Financial Crisis, Organized Crime, and the Birth of New Enterprises. *The Review of Economics and Statistics*, 62(7):1–45.

Lessing, B. (2015). Logics of Violence in Criminal War. *Journal of Conflict Resolution*, 59(8):1486–1516.

McCrary, J. (2008). Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test. *Journal of Econometrics*, 142(2):698–714.

Mirenda, L., Mocetti, S., and Rizzica, L. (2022). The economic effects of mafia: firm level evidence. *American Economic Review*, 112(8):2748–73.

Mookherjee, D. and Png, I. P.-L. (1995). Corruptible Law Enforcers: How Should They Be Compensated? *The Economic Journal*, 105(428):145–159.

Norton, E. C. (2022). The inverse hyperbolic sine transformation and retransformed marginal effects. *The Stata Journal*, 22(3):702–712.

Palguta, J. and Pertold, F. (2017). Manipulation of Procurement Contracts: Evidence from the Introduction of Discretionary Thresholds. *American Economic Journal: Economic Policy*, 9(2):293–315.

Pinotti, P. (2015a). The Causes and Consequences of Organised Crime: Preliminary Evidence Across Countries. *The Economic Journal*, 125(586):F158–F174.

Pinotti, P. (2015b). The Economic Costs of Organised Crime: Evidence from Southern Italy. *The Economic Journal*, 125(586):F203–F232.

Pique, R. (2019). Higher pay, worse outcomes? The impact of mayoral wages on local government quality in Peru. *Journal of Public Economics*, 173:1–20.

Rose-Ackerman, S. (1999). *Corruption and Government: Causes, Consequences, and Reform*. Cambridge University Press.

Shapiro, C. and Stiglitz, J. E. (1984). Equilibrium Unemployment as a Worker Discipline Device. *The American Economic Review*, 74(3):433–444.

Svensson, J. (2005). Eight Questions About Corruption. *Journal of Economic Perspectives*, 19(3):19–42.

Szucs, F. (2021). Discretion and Favoritism in Public Procurement. *Working Paper*.

Transcrime (2013). Gli investimenti delle mafie.

Unioncamere (2017). La misurazione dell'economia illegale: sintesi delle principali esperienze sul tema.

Van Rijckeghem, C. and Weder, B. (2001). Bureaucratic corruption and the rate of temptation: do wages in the civil service affect corruption, and by how much? *Journal of Development Economics*, 65(2):307–331.

Vannutelli, S. (2022). From Lapdogs to Watchdogs: Random Auditor Assignment and Municipal Fiscal Performance. *Working Paper*.

Appendix

Additional Tables

Table A1: Balance Checks for Relevant Covariates at Cutoff

Dependent Variable	$\hat{\beta}$	(SE)	Dependent Variable	$\hat{\beta}$	(SE)
Mafia Presence (pre-2011)	-.013	(.056)	% Illiterate	.005	(.005)
Mafia Victims (pre-2011)	-.033	(.154)	% Female	.001	(.002)
Mafia Seizures (pre-2011)	.009	(.056)	% Unemployed	0.164	(0.140)
Mafia Infiltrated (pre-2011)	-.000	(.006)	% Agriculture	.029	(.018)
Log Surface	-.032	(.190)	% Industry	-.023	(.024)
Population in 2011	168	(125)	Average Age	-.402	(.459)
2011 Population $\geq$ 5,000	.069	(.087)	Turnout 2008	-.014	(.012)
Log Longitude	.074	(.050)	Center-Right 2008	-.012	(.026)
Log Latitude	-.021	(.015)	% % Center-Left 2008	-.005	(.020)
Log Kms to Region Capital	-.006	(.127)	Log Tot. Procurement	-.074	(.117)
Log Elevation	.078	(.223)	Log Tot. Procurement PW	-.551	(.337)
Log Slope	.055	(.294)	Log Avg. Procurement	.023	(.136)
Log Population Density	.057	(.203)	Log Avg. Procurement PW	.013	(.113)
Log Foreigners x 100 Inhab.	-.217**	(.110)	Log Spending in Police	.598	(.514)
Log NGOs x 1,000 Inhab.	-.030	(.074)	Municipal Union Member	-.098	(.086)
% High School	-.009	(.006)	Municipal Union Head	-.047	(.038)

Notes: The coefficients displayed are bias-corrected RD estimates of $\hat{\beta}$ from Equation 1, using a first-order polynomial, with robust variance estimator (Calonico et al., 2014). The outcome variable of each model is listed in each column's title. All outcomes are measured as of 2011 or as of the last available year prior to 2011. *** p<.01, ** p<.05, * p<.1.

Table A2: Wages and Attacks on Mayors

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population	.065**	.053*	.069***	.059**	.090***	.077**
$\geq$ 5000	(.027)	(.028)	(.026)	(.026)	(.034)	(.034)
SD Depvar	.176	.171	.129	.128	.166	.164
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,280	1,215	1,210	1,131	1,203	1,126
Effective N	1,037	972	980	904	974	900
N Left	627	584	591	539	587	535
N Right	410	388	389	365	387	365

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Wages and Attacks on Cabinet Members and Vice Mayors

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population $\geq$ 5,000	.019 (.018)	.018 (.019)	.016 (.013)	.013 (.013)	.020 (.016)	.017 (.017)
SD Depvar	.121	.132	.093	.100	.120	.128
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,352	1,162	1,282	1,134	1,276	1,132
Effective N	1,095	931	1,037	908	1,032	904
N Left	668	557	627	542	624	539
N Right	427	374	410	366	408	365

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Wages and Attacks on Municipal Cabinet
Robustness to Quadratic Polynomial

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population $\geq$ 5000	.079*	.062	.084**	.068*	.110**	.088*
	(.045)	(.045)	(.042)	(.041)	(.054)	(.053)
SD Depvar	.232	.234	.195	.195	.252	.253
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	2nd	2nd	2nd	2nd	2nd	2nd
Bandwidth	1,741	1,638	1,717	1,653	1,715	1,651
Effective N	1,440	1,329	1,419	1,345	1,415	1,344
N Left	911	833	897	842	894	841
N Right	529	496	522	503	521	503

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Wages and Attacks on Municipal Cabinet - Robustness to Count Dependent Variable and Maximum Likelihood Estimation

	(1)	(2)	(3)	(4)
	RDD	RDD	Poisson	Negative Binomial
Population $\geq$ 5000	.145***	.123**	.075***	.064**
	(.054)	(.053)	(.028)	(.028)
SD Depvar	.285	.286	.285	.285
Election FEs	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No
Polynomial	1st	1st	-	-
Bandwidth	1,372	1,316	1,372	1,372
Effective N	1,117	1,052	1,117	1,117
N Left	684	639	684	684
N Right	433	413	433	433

Notes: In all models, the dependent variable is the count of the number of criminal attacks against members of the municipal cabinet of municipality i between 2014 and 2020. Columns (1) and (2) present RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). Columns (3) and (4) present, respectively, Poisson and negative binomial estimates of the effect of municipality i having more than 5,000 residents as of the 2011 census, restricting the sample to municipalities within the optimal RDD bandwidth selected by algorithm for the model of column (1). The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth. Controlled specifications for Poisson and Negative Binomial estimators are not shown because of failures of the algorithm in converging. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Wages and Attacks on Municipal Cabinet
Robustness to Count of Attacks per 5,000 Inhabitants

	(1)	(2)
	Attacks per 5,000 Inhabitants	Attacks per 5,000 Inhabitants
Population $\geq$ 5000	.152***	.127**
	(.058)	(.056)
SD Depvar	.315	.315
Election FEs	Yes	Yes
Province FEs	Yes	Yes
Controls	No	Yes
Polynomial	1st	1st
Bandwidth	1,196	1,135
Effective N	969	908
N Left	582	542
N Right	387	366

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Placebo Test – Wages and Attacks on Non-Elected Officials

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population ≥ 5000	-.042 (.027)	-.043 (.028)	-.015 (.020)	-.019 (.021)	-.016 (.023)	-.028 (.026)
SD Depvar	.266	.267	.207	.209	.250	.248
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,329	1,314	1,493	1,325	1,692	1,249
Effective N	1,076	1,050	1,214	1,061	1,392	1,003
N Left	653	637	749	643	880	604
N Right	423	413	465	418	512	399

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Placebo Test – Non-Criminal Attacks on Municipal Cabinet

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 of Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population ≥ 5000	-.007 (.015)	.013 (.019)	.001 (.012)	.022 (.016)	.001 (.016)	.028 (.021)
SD Depvar	.141	.157	.108	.126	.138	.161
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,924	1,142	1,922	1,078	1,922	1,078
Effective N	1,634	915	1,631	851	1,631	851
N Left	1,042	547	1,039	499	1,039	499
N Right	592	368	592	352	592	352

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A9: Wages and Attacks on Municipal Cabinet
Excluding Attacks Denounced by the Victim

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population ≥ 5000	.059**	.063**	.054**	.060**	.070**	.078**
	(.026)	(.028)	(.024)	(.025)	(.031)	(.033)
SD Depvar	.225	.225	.186	.167	.240	.215
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,826	1,634	1,725	1,504	1,724	1,498
Effective N	1,530	1,327	1,425	1,209	1,425	1,203
N Left	974	831	899	745	899	741
N Right	556	496	526	464	526	462

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A10: Wages and Amount of Public Procurement

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Tot. Value	Log Tot. Value	Log Avg. Value	Log Avg. Value	Log. N. Contracts	Log N. Contracts
Population $\geq$ 5,000	-.150*	-.126*	-.080	-.094	-.070	-.060
	(.078)	(.075)	(.078)	(.079)	(.093)	(.082)
SD Depvar	.793	.794	.674	.670	.669	.667
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	997	1,046	1,173	1,088	1,033	1,276
Effective N	782	814	939	846	813	1,009
N Left	452	475	565	499	474	611
N Right	330	339	374	347	339	398

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The dependent variables refer to procurement contracts offered by each municipality in the period 2013-2020. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A11: municipal Wages and Attacks on Members the Municipal Cabinet Excluding Members of Unions of Municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
	$\geq$ 1 Attack	$\geq$ 1 of Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population $\geq$ 5,000	.102***	.095***	.104***	.099**	.134***	.129***
	(.033)	(.031)	(.031)	(.029)	(.040)	(.037)
SD Depvar	.222	.220	.181	.181	.232	.233
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,440	1,456	1,488	1,476	1,492	1,474
Effective N	739	743	766	756	770	756
N Left	445	448	464	459	466	459
N Right	294	295	302	297	304	297

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: municipal Wages and Attacks on Members the Municipal Cabinet
Only Attacks Before Appointment of New Auditors

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 of Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Population $\geq 5{,}000$	.013	.018*	.013	.013*	.016*	.016*
	(.010)	(.010)	(.008)	(.007)	(.010)	(.009)
SD Depvar	.067	.063	.065	.044	.053	.036
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,985	1,221	1,826	1,204	1,903	1,205
Effective N	1,402	832	1,264	814	1,336	814
N Left	895	496	797	486	842	486
N Right	507	336	467	328	494	328

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). For each municipality, the dependent variable only includes attacks perpetrated before the nomination of the first post-reform cohort of municipal auditors. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A13: Wages and Procurement
Bunching of Reservation Values Below €40,000

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Population	-.005	-.005	-.044***	-.043**	.018*	.020*
$\geq$ 5000	(.010)	(.011)	(.016)	(.018)	(.010)	(.011)
SD Depvar	.087	.087	.142	.143	.099	.113
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,034	946	1,062	977	1,528	1,059
Effective N	813	736	834	756	1,228	822
N Left	474	422	490	436	754	481
N Right	339	314	344	320	474	341

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the difference in the share of procurement contracts offered by each municipality in the period 2013-2020 with a value between €30,000 and €40,000 and the share of procurement contracts offered by each municipality in the period 2013-2020 with a value between €40,000 and€50,000. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Wages and Procurement
Bunching of Reservation Values Below €150,000

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Population $\geq$ 5000	-.010**	-.011*	-.003	.002	-.022**	-.025***
	(.005)	(.007)	(.008)	(.008)	(.010)	(.009)
SD Depvar	.053	.050	.071	.072	.072	.071
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	2,072	1,120	1,124	1,025	1,106	1,078
Effective N	1,760	886	895	796	874	840
N Left	1,143	528	534	464	519	494
N Right	617	358	661	332	355	346

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the difference in the share of procurement contracts offered by each municipality in the period 2013-2020 with a value between €140,000 and €150,000 and the share of procurement contracts offered by each municipality in the period 2013-2020 with a value between €150,000 and €160,000. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A15: Wages and Procurement
Bunching of Reservation Values Below €200,000

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Population $\geq$ 5000	-.007**	-.008**	-.010**	-.010**	-.003	-.003
	(.004)	(.004)	(.005)	(.005)	(.007)	(.007)
SD Depvar	.040	.040	.036	.036	.072	.074
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	1,362	1,267	1,032	924	1,195	1,110
Effective N	1,095	1,003	811	717	957	840
N Left	672	607	473	410	577	516
N Right	423	396	338	307	380	352

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the difference in the share of procurement contracts offered by each municipality in the period 2013-2020 with a value between €190,000 and €200,000 and the share of procurement contracts offered by each municipality in the period 2013-2020 with a value between €200,000 and €210,000. The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A16: Right-Wing Mayors and Bunching of Reservation Values of Contracts

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Right-Wing Mayor	.017 (.018)	.019 (.018)	.062** (.025)	.064*** (.023)	.012 (.023)	.011 (.024)
SD Depvar	.174	.177	.217	.215	.201	.201
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	18.97	19.15	16.61	17.77	15.33	14.84
Effective N	1,920	1,909	1,703	1,786	1,601	1,546
N Left	1,095	1,089	970	1,017	914	880
N Right	825	820	733	769	687	666

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is $Bunching_i$, as jointly defined by equations 2 and 3. The running variable is the margin of victory (or loss) of the most voted right-wing mayoral candidate. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A17: Far-Right Mayors and Bunching of Reservation Values of Contracts

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Far-Right Mayor	.015 (.019)	.014 (.019)	.064** (.027)	.064** (.026)	.002 (.024)	-.001 (.024)
SD Depvar	.169	.170	.218	.220	.190	.190
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	18.96	18.47	18.00	17.66	14.91	14.15
Effective N	1,672	1,612	1,600	1,554	1,372	1,291
N Left	959	922	918	885	787	730
N Right	713	690	682	669	585	561

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is The dependent variable is $Bunching_i$, as jointly defined by equations 2 and 3. The running variable is the margin of victory (or loss) of the most voted far-right mayoral candidate. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A18: Right-Wing Mayors and Firms Invited to Negotiated Procedures

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Right-Wing	-.104	-.127	-.080	-.150	.084	.003
Mayor	(.123)	(.121)	(.124)	(.128)	(.149)	(.174)
SD Depvar	.938	.963	.936	.934	1.03	1.01
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	11.04	11.87	13.20	11.15	14.85	8.51
Effective N	924	974	996	852	811	481
N Left	516	546	574	480	469	276
N Right	408	428	422	372	342	205

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the log average number of firms invited to the negotiated procurement procedures of municipality i between 2013 and 2020. The running variable is the margin of victory (or loss) of the most voted right-wing mayoral candidate. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A19: Far-Right Mayors and Firms Invited to Negotiated Procedures

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Far-Right Mayor	-.160 (.129)	-.287* (.150)	-.181 (.137)	-.226 (.139)	.045 (.154)	.010 (.172)
SD Depvar	.916	.973	.922	.908	1.07	.993
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	10.82	6.26	11.31	9.84	15.03	10.01
Effective N	798	458	766	674	712	491
N Left	448	254	436	375	410	284
N Right	350	204	330	299	302	207

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the log average number of firms invited to the negotiated procurement procedures of municipality i between 2013 and 2020. The running variable is the margin of victory (or loss) of the most voted far-right mayoral candidate. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A20: Right-Wing Mayors and Share of Contracts Subcontracted

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Right-Wing	-.008	.007	-.011	.007	0.001	-.007**
Mayor	(.022)	(.021)	(.031)	(.032)	(.008)	(.004)
SD Depvar	.146	.143	.265	.240	.052	.058
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	16.33	10.89	16.59	12.89	15.81	8.99
Effective N	1,509	702	1,400	783	1,225	508
N Left	860	394	800	447	502	294
N Right	649	308	600	336	723	214

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the share of contracts awarded by municipality i between 2013 and 2020 that were subsequently subcontracted. The running variable is the margin of victory (or loss) of the most voted right-wing mayoral candidate. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A21: Far-Right Mayors and Share of Contracts Subcontracted

	(1)	(2)	(3)	(4)	(5)	(6)
	All Contracts	All Contracts	Public Works	Public Works	Other Contracts	Other Contracts
Far-Right	-.017	-.009	-.017	.007	.002	-.008**
Mayor	(.020)	(.017)	(.032)	(.033)	(.009)	(.004)
SD Depvar	.144	.143	.237	.243	.029	.029
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	18.17	19.30	16.63	12.86	15.42	8.85
Effective N	1,448	1,036	1,232	688	1,042	441
N Left	837	594	708	392	602	250
N Right	611	442	524	296	440	191

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The dependent variable is the share of contracts awarded by municipality i between 2013 and 2020 that were subsequently subcontracted. The running variable is the margin of victory (or loss) of the most voted far-right mayoral candidate. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A22: Right-Wing Mayors and Attacks to Municipal Cabinet

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Right-Wing Mayor	.037 (.023)	.049* (.029)	.034* (.015)	.043* (.017)	.043* (.026)	.045* (.026)
SD Depvar	.298	.301	.259	.266	.256	.262
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	14.35	11.97	12.98	11.93	13.00	12.17
Effective N	1,584	943	1,453	942	1,454	1,340
N Left	890	536	813	536	814	742
N Right	694	407	640	406	640	598

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A23: Far-Right Mayors and Attacks to Municipal Cabinet

	(1)	(2)	(3)	(4)	(5)	(6)
	≥ 1 Attack	≥ 1 Attack	Log Attacks	Log Attacks	Asinh Attacks	Asinh Attacks
Far-Right Mayor	.035 (.023)	.052* (.029)	.030 (.022)	.044* (.027)	.039 (.028)	.040 (.028)
SD Depvar	.290	.286	.269	.261	.287	.286
Election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Province FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Polynomial	1st	1st	1st	1st	1st	1st
Bandwidth	13.67	13.29	11.86	12.70	11.81	11.14
Effective N	1,317	925	1,156	887	1,150	1,083
N Left	738	535	640	512	636	597
N Right	579	390	516	375	514	486

Notes: RDD estimates with triangular weighting kernel and data-driven optimal bandwidth selection (Calonico et al., 2014). The running variable is the municipal population as of the 2011 census, with a cutoff of 5,000 residents determining treatment assignment. The standard deviation of the dependent variable is measured within the left half of the optimal bandwidth selected by the algorithm for each model. Controls: See footnote 5. Robust bias-corrected standard errors clustered at the province level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figures

Figure A1: Share of Municipalities with One or More Attacks, by Region, 2014-2020

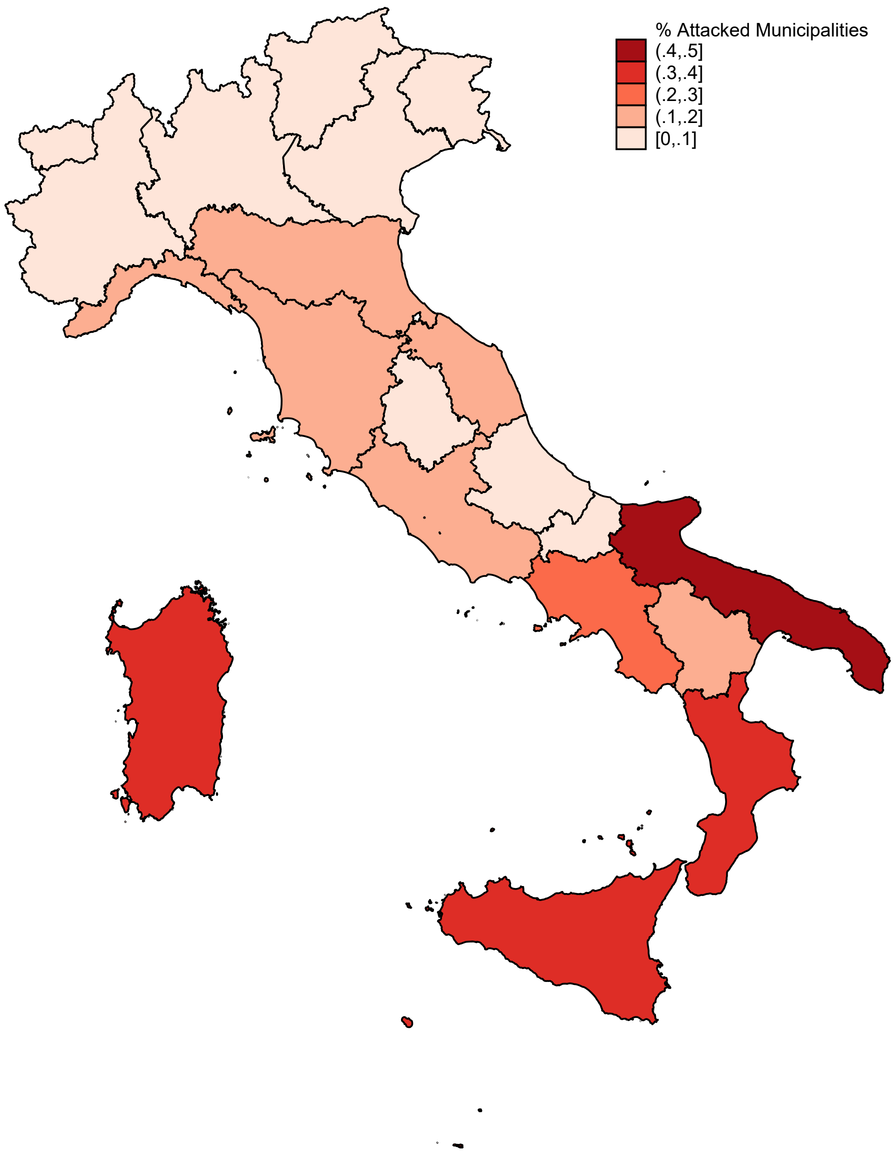
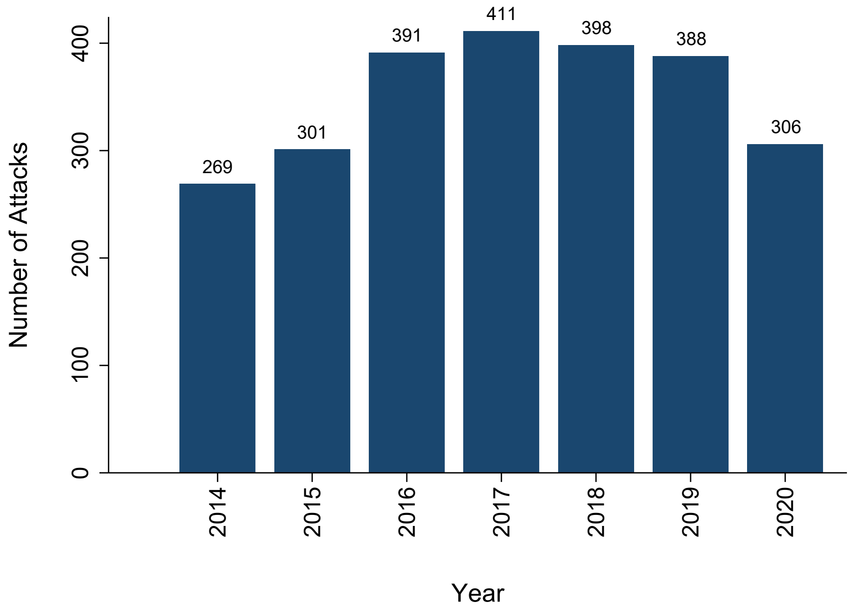
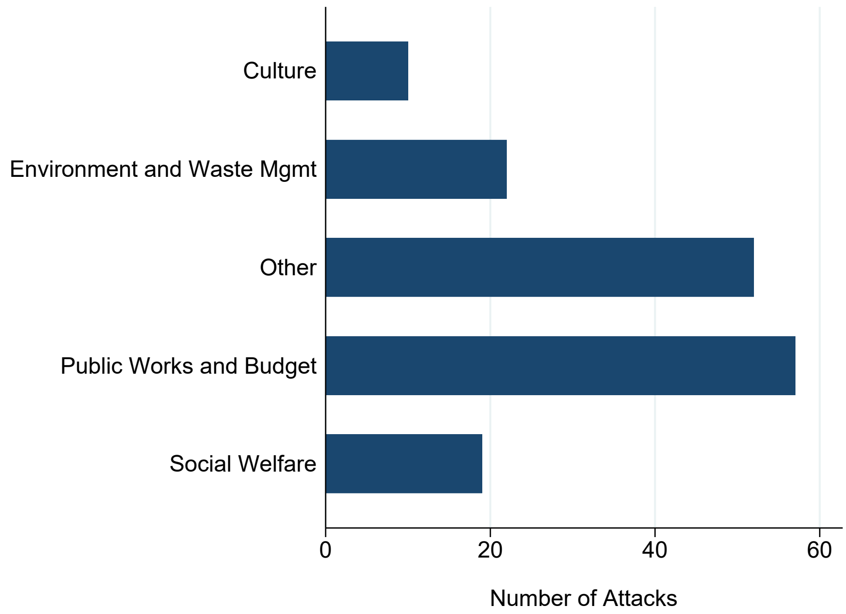


Figure A2: Attacks by Year, 2014-2020



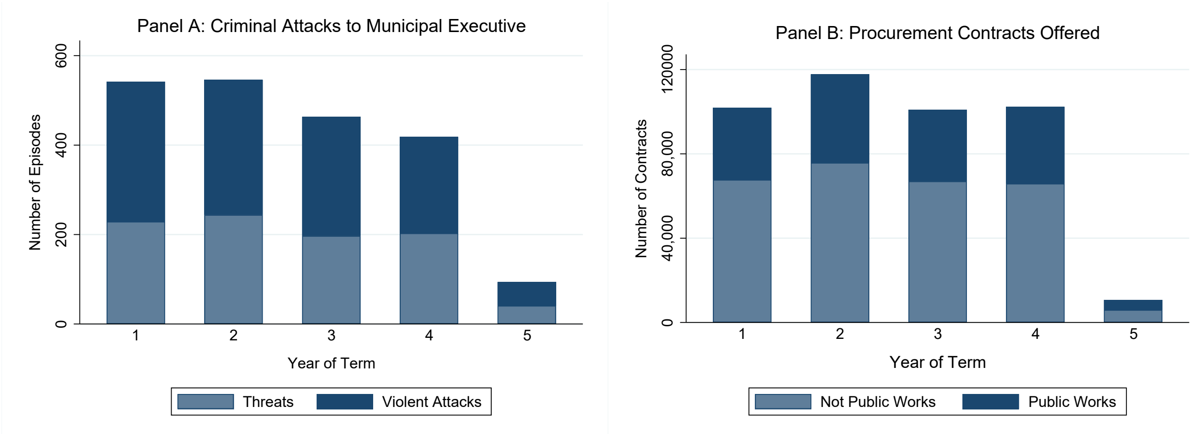
Notes: For each year, attacks by ordinary citizens are excluded from the count.

Figure A3: Criminal Attacks on Cabinet Members, by Policy Area



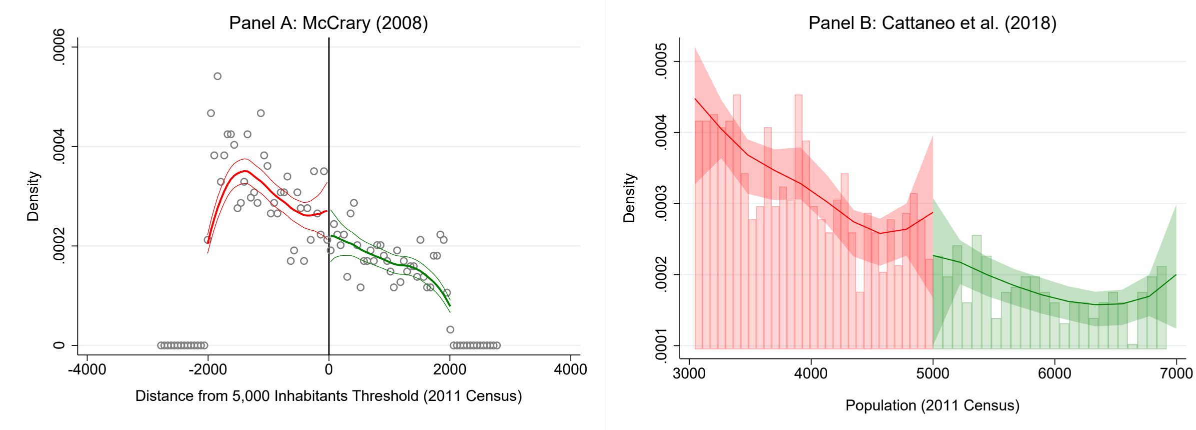
Notes: Data on attacks are from Avviso Pubblico, and refer to years 2014-2020. The figure only plots the 182 attacks on cabinet members whose policy area was specified on the report or could be retrieved via online searches.

Figure A4: Criminal Attacks and Procurement Contracts, by Year of Term



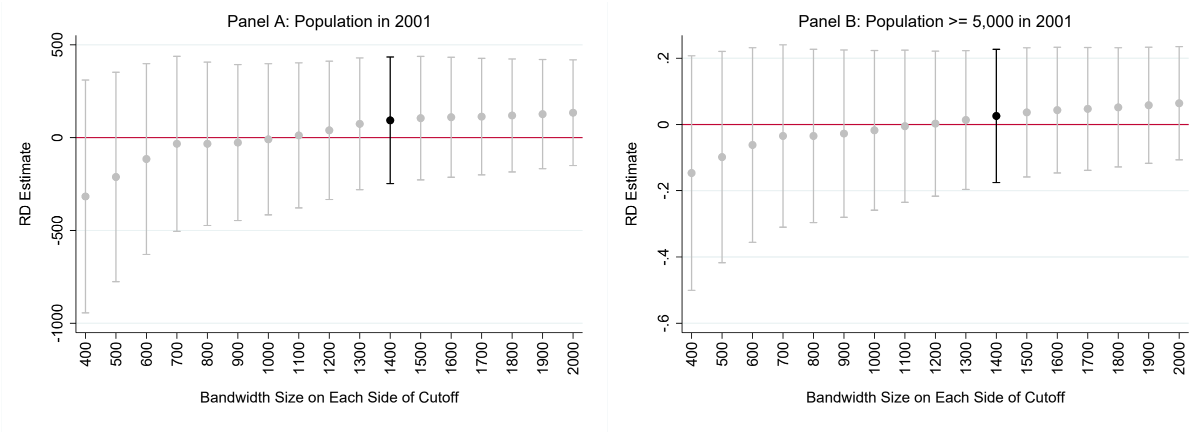
Notes: Data on attacks are from Avviso Pubblico, and refer to years 2014-2020. Data on procurement contracts are from ANAC, and refer to years 2013-2020.

Figure A5: Tests of No-Sorting Assumption



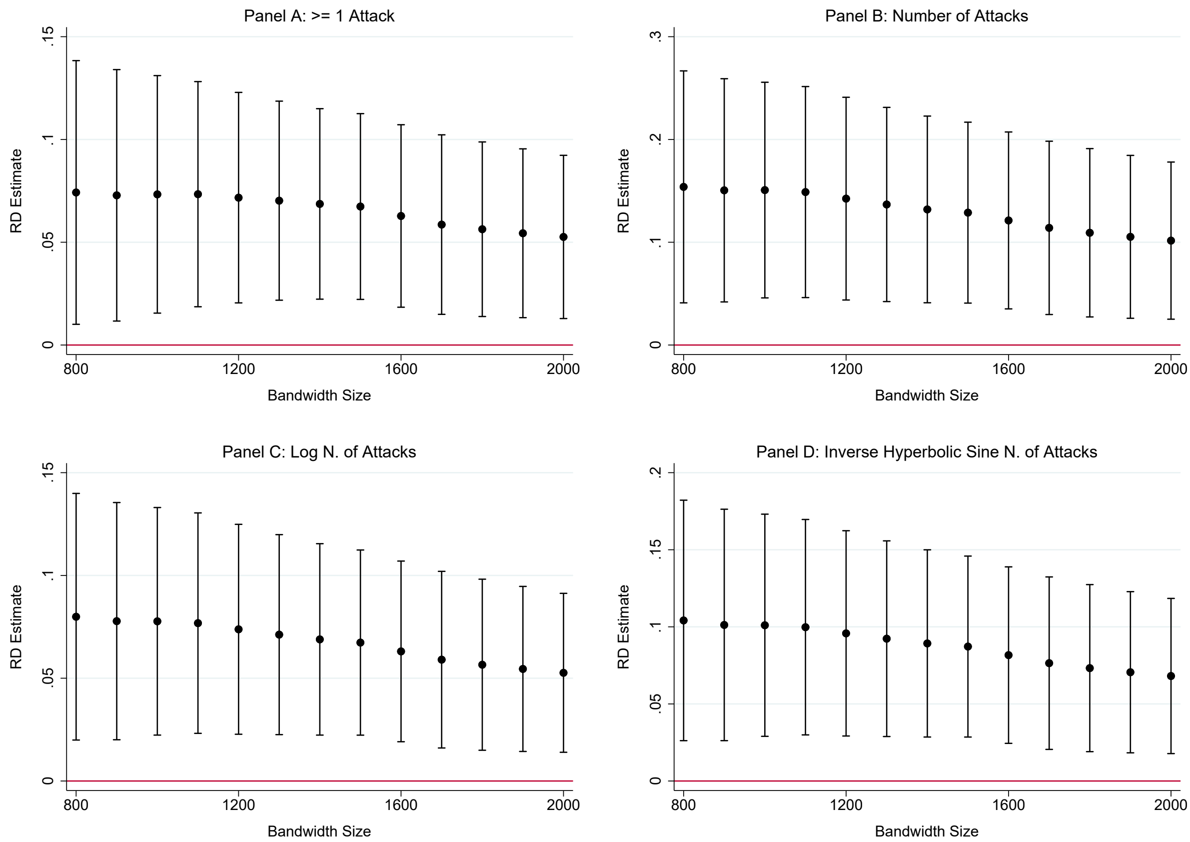
Notes: In each panel, the curves represent kernel approximations of the density, fitted separately on each side of the cutoff, with the relative 95% confidence intervals. In Panel A, each dot represents the density of the 2011 census population for the corresponding bin. Given the discrete nature of the running variable, the threshold is set to -0.5 to avoid asymmetry bias, as recommended in Eggers et al. (2018). The estimated log difference in height at the threshold is -.209, with a standard error of .177. In Panel B, the height of each bar represents the density of the 2011 census population for the corresponding bin. The t-statistic for the null hypothesis of no difference in density across the cutoff is -1.24. The corresponding p-value is .217.

Figure A6: Balance of Population and Wage Level in 2001 Census



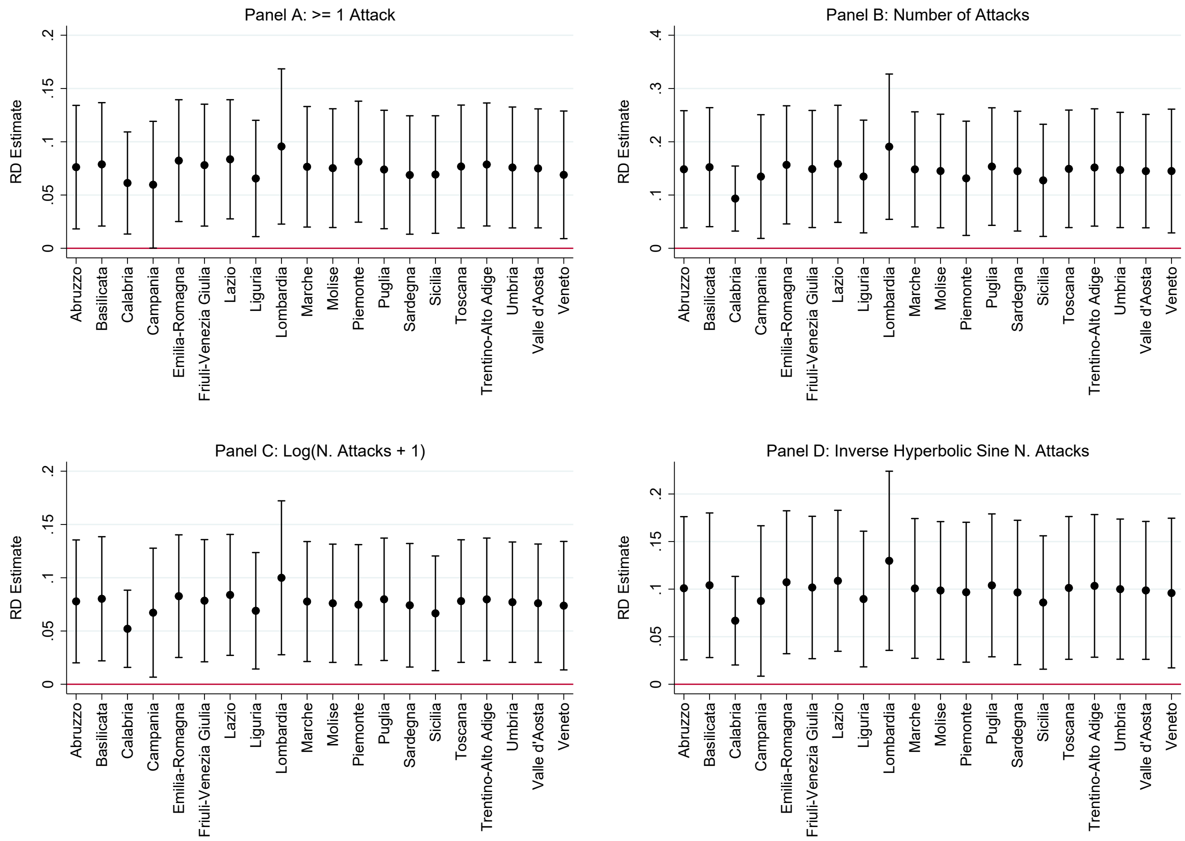
Notes: Each dot represents one RDD estimate from fitting Equation 1 with a first-order polynomial, using the bandwidth of the size indicated on the horizontal axis on each side of the 5,000 residents cutoff. Vertical bars are 95% confidence intervals, based on robust bias-corrected standard errors clustered at the province level. The black estimate refers to the bandwidth of 1,400, i.e., the round number closest to the one chosen by the data-driven algorithm of (Calonico et al., 2014) in column (1) of Table 2.

Figure A7: Robustness Test, Choice of Bandwidth Value



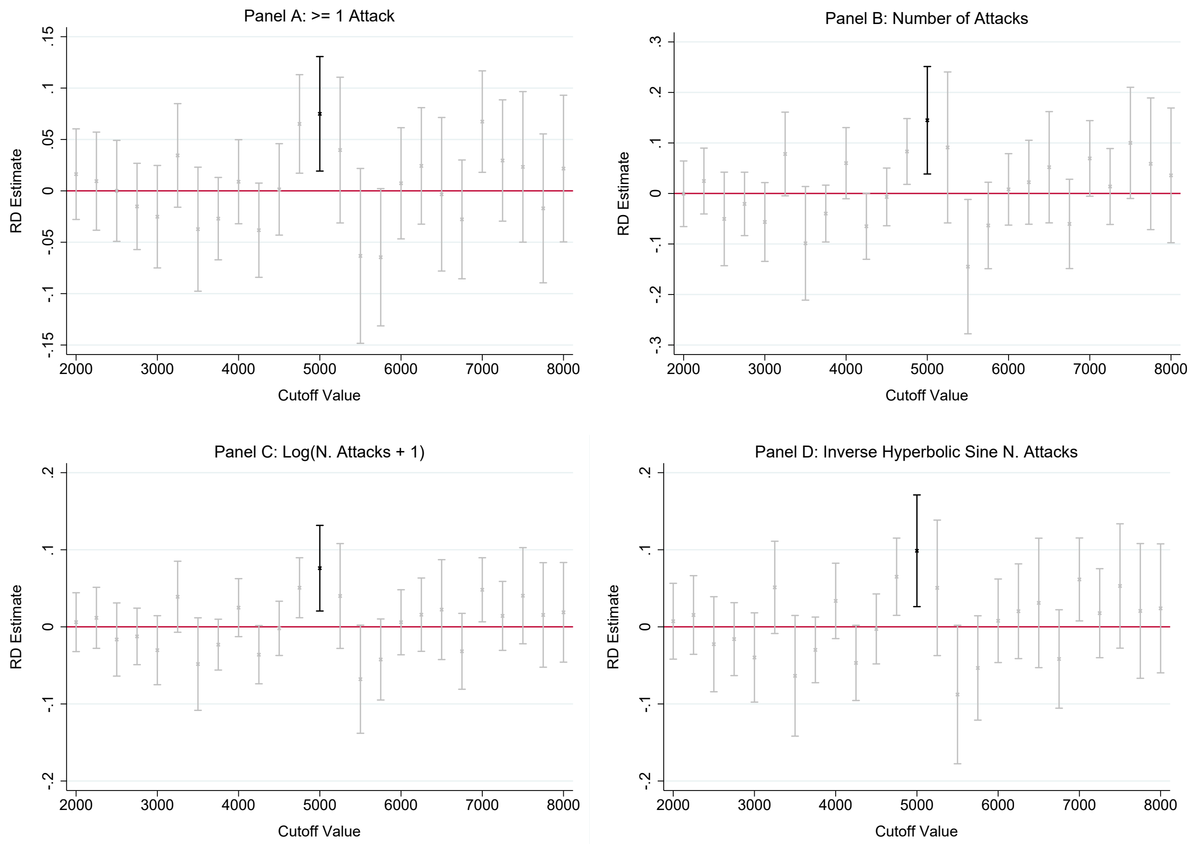
Notes: Each dot represents one RDD estimate from fitting Equation (1) with a first-order polynomial, using the bandwidth of the size indicated on the horizontal axis on each side of the 5,000 residents cutoff. Vertical bars are 95% confidence intervals, based on conventional standard errors clustered at the province level.

Figure A8: Jackknife Tests, by Region



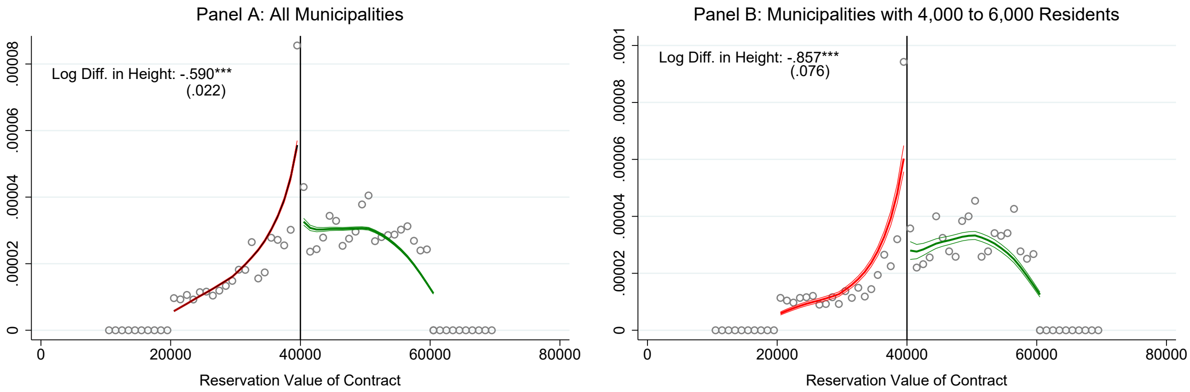
Notes: Each dot represents one RDD estimate from fitting Equation (1) with a first-order polynomial, excluding all municipalities within the region indicated on the horizontal axis. Vertical bars are 95% confidence intervals, based on robust bias-corrected standard errors clustered at the province level.

Figure A9: Placebo Test, Alternative Cutoffs of Running Variable



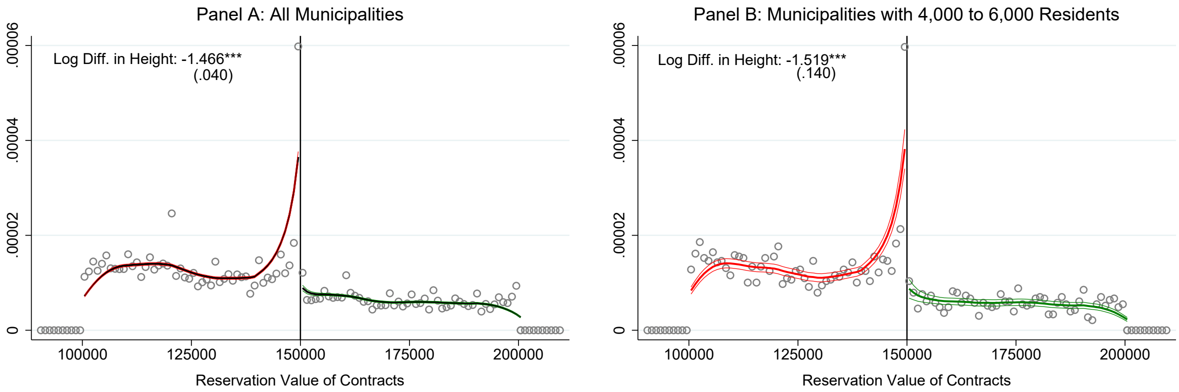
Notes: Each cross represents one RDD estimate from fitting Equation (1) without controls employing a first-order polynomial, using the cutoffs for the running variable (population as of 2011 Census) indicated on the horizontal axis. Vertical bars are 95% confidence intervals, based on bias-corrected standard errors clustered at the province level. The black estimate in the middle refers to the real cutoff of 5,000 inhabitants.

Figure A10: McCrary Tests of Reservation Value Bunching Below €40,000,
Contracts for Public Works, Full and Effective RDD Sample



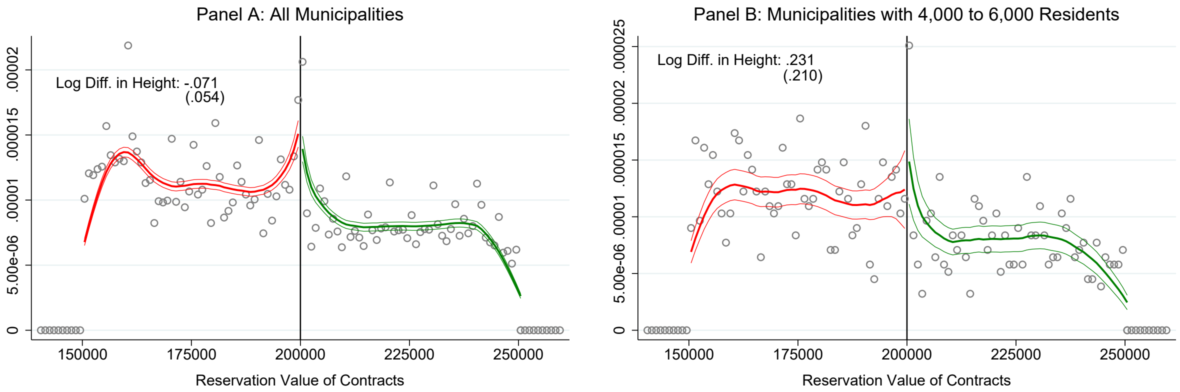
Notes: In both panels, densities refer only to contracts for public works drafted between 2013 and 2020. Number of contracts: 194,926. Number of unique municipalities between 4,000 and 6,000 residents: 467. Number of unique municipalities between 5,000 and 6,000 residents: 335.

Figure A11: McCrary Tests of Reservation Value Bunching Below €150,000, Contracts for Public Works, Full and Effective RDD Sample



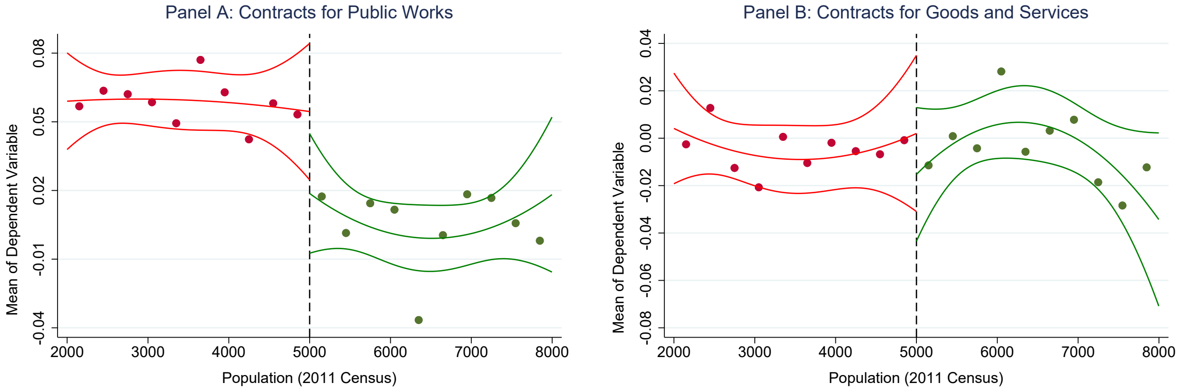
Notes: In both panels, densities refer only to contracts for public works drafted between 2013 and 2020. Number of contracts: 194,926. Number of unique municipalities between 4,000 and 6,000 residents: 467. Number of unique municipalities between 5,000 and 6,000 residents: 335.

Figure A12: McCrary Tests of Reservation Value Bunching Below €200,000, Contracts for Public Works, Full and Effective RDD Sample



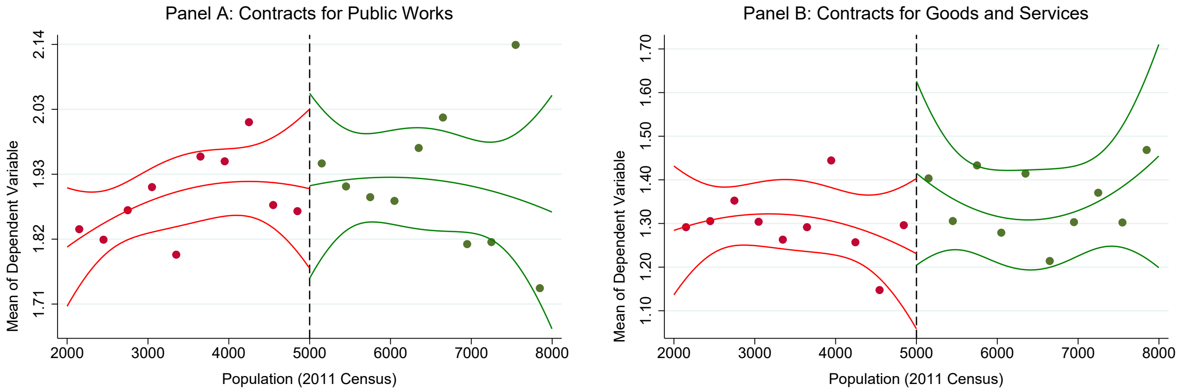
Notes: In both panels, densities refer only to contracts for public works drafted between 2013 and 2020. Number of contracts: 194,926. Number of unique municipalities between 4,000 and 6,000 residents: 467. Number of unique municipalities between 5,000 and 6,000 residents: 335.

Figure A13: Wages and Bunching of Contracts' Values, RDD Plots



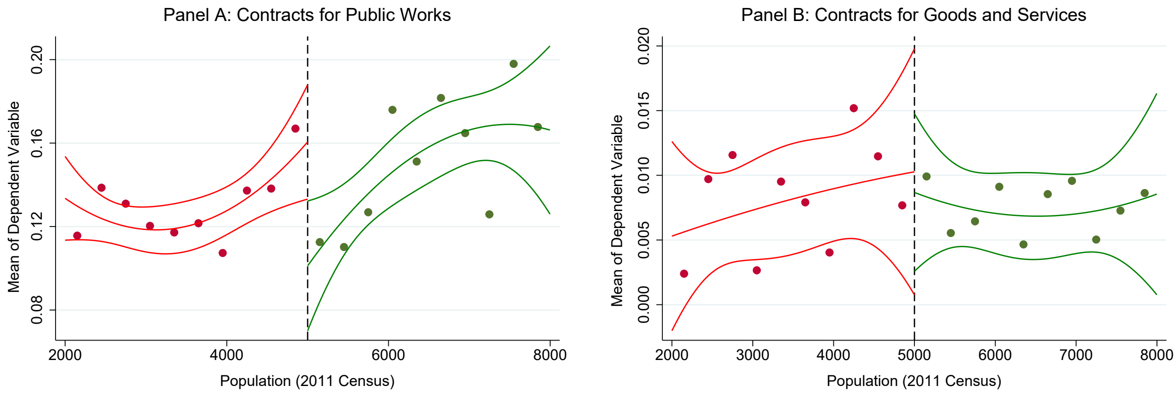
Notes: In both panels, each dot represents the average level of $Bunching_i$, as jointly defined by equations 2 and 3, for a given binned level of municipal residents.

Figure A14: Wages and Firms Invited to Negotiated Procedures, RDD Plots



Notes: In both panels, each dot represents the average log number of firms invited to bid in the negotiated procurement procedures of municipality i between 2013 and 2020, for a given binned level of municipal residents.

Figure A15: Wages and Subcontracting, RDD Plots



Notes: In both panels, each dot represents the average share of contracts between 2013 and 2020 that were subsequently subcontracted, for a given binned level of municipal residents.