Organized Crime, Local Politicians, and State Capacity

Seung-hun Lee*

Taipei School of Economics, National Tsing Hua University

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Abstract

This paper examines how the assassination of mayors affects local government capacity, leveraging quasi-random variation in the success of assassination attempts against Mexican mayors. Compared to municipalities with failed attempts, tax collection falls by 29% and public expenditures shift from essential services to construction investments in municipalities with successful assassinations. The evidence is most consistent with institutional disruption from the sudden loss of mayors, rather than the violent act itself. Effects attenuate when control municipalities also experience mayoral absence. In addition, municipalities with mayors killed 24-35 months into their term and represented by minority parties with weak political networks experience larger effects. In contrast, changes in security environments, municipal personnel composition, non-political violence, economic activity, demographics, and electoral dynamics do not account for the observed patterns. The results highlight how the loss of decision-makers in violent environments undermines local state capacity.

Keywords: State capacity, local government, mayors, organized crime, assassinations

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*Contact: seunghunlee0918@gmail.com.

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1 Introduction

Political violence, particularly the assassination of key officials by organized criminal groups, poses a serious threat to the development of local state capacity (Daniele 2019). It deters competent individuals from political careers, distorts electoral processes, and removes decision-makers responsible for essential bureaucratic functions such as managing public finances and recruiting personnel (Acemoglu et al. 2013; Finan et al. 2017). These functions, accounting for 24% of public expenditures and 35% of public employment globally, are increasingly pivotal to local development (OECD 2016; Bardhan 2002). While the political effects of assassinations are well-documented at the national level (Jones and Olken 2009), their impact on the bureaucratic and administrative capacity of local governments has received less attention.

This paper examines whether successful assassinations of leaders affect local governments' capacity to raise revenue and deliver public services. I construct an original municipality-level dataset that combines information on local public finances, personnel, organized criminal group presence, and political assassinations in Mexico. I identify the causal effect of losing mayors by comparing fiscal capacity in municipalities with successful assassinations to those where mayors remained in office after failed attempts. The findings show that successful assassinations weaken local tax collection and public service provision. The evidence is most consistent with institutional disruption following the sudden loss of mayors, rather than the violent act itself. Thus, assassinations disrupt the core bureaucratic functions of local government, extending beyond electoral consequences.

I examine this question in the context of Mexico, where political violence against local mayors is widespread. Mayors oversee personnel recruitment, provision of basic services, infrastructure, and tax collection (Dell 2015; Larreguy et al. 2020). They face persistent political violence, with ACLED reporting that Mexico has the highest number of attacks against local politicians globally.[1] At least 85 out of more than 15,000 mayors have been assassinated since 2000, making them at least 9 times more likely to be murdered than the general population (Calderón et al. (2019) and Figure 1).[2] These attacks are typically carried out by organized criminal groups seeking to influence local politics and extract resources from businesses, fiscal revenues, and construction projects (Grillo 2011; Trejo and

1. The statistics are obtained from the following online report: https://acleddata.com/2023/06/22/special-issue-violence-against-local-officials/ (Accessed on October 28th, 2023)

2. There are 2,471 municipalities in Mexico, including 16 boroughs in Mexico City. Each municipality has had 6-7 different mayors since 2000.

Ley 2021).[3] Evidence demonstrates that the assassination attempts are driven by criminal group presence and competition, rather than general violence (Rios 2012).

I isolate the effect of losing a mayor to a successful assassination by comparing municipalities with successful and failed assassination attempts. To do so, I construct a dataset combining information on assassination attempts, organized criminal group presence, municipal public finances, and personnel. Assassination attempts and their outcomes are obtained through text-scraping online newspaper articles. I estimate the treatment effects using various event-study specifications, comparing changes in the outcomes across municipalities where mayors were killed and those where mayors survived uninjured. By restricting the sample to locations with at least one assassination attempt, I mitigate the selection bias stemming from differences between attacked and non-attacked areas (Brodeur 2018; Jones and Olken 2009). The regression design addresses confounders such as political violence by making comparisons conditional on the occurrence of assassination attempts. Municipality and year fixed effects are included to account for time-invariant municipal characteristics and common temporal shocks. Thus, the treatment effects are identified by comparing changes in local state capacity among municipalities with successful and failed assassination attempts.

The first set of results studies the effects of successful assassinations on the capacity of local governments to raise revenues and deliver public services. Municipalities where mayors are assassinated experience a sustained decline in revenue collection. Total tax revenues decrease by approximately 29% over the 6 years following assassinations. Tax revenue per capita falls at a similar rate. Intergovernmental grants that partially depend on local tax revenues also fall by approximately 11%, while revenues from non-tax sources remain unaffected. These estimates are robust to different choices of event-study estimators. Overall, affected municipalities experience a sustained erosion of capacity to generate revenues and provide essential services.

Furthermore, public expenditures shift away from essential services to investment in construction. The share of public investment expenditure on construction projects rises by 5.5 percentage points, or 17.8% increase in volume. This comes at the expense of funding for core local government functions, poverty reduction, and economic development. Spending on basic operations and allowances to municipal institutions providing essential services declines by 1.4 and 1.9 percentage

3. For news sources on organized criminal groups exploiting local governments for revenues and construction projects, see https://www.economist.com/the-americas/2023/05/11/mexicos-gangs-are-becoming-criminal-conglomerates and https://www.nytimes.com/2016/01/17/opinion/sunday/why-cartels-are-killing-mexicos-mayors.html (accessed on September 5th, 2023)

points, respectively, corresponding to 40% and 42% reductions in volumes. These findings indicate that the capacity to provide public services deteriorates, consistent with documented evidence that corrupt or captured local governments distort spending toward construction projects with high rent-seeking potential (InSight Crime 2013, 2024; Grillo 2011; Liu and Mikesell 2014; Mauro 1998).[4,5]

In the next part of the paper, I assess the plausibility of various channels that may explain the results. I first re-estimate the outcomes using alternative control groups that also experience different degrees of mayoral absence following violence or non-violent events. The estimated effects attenuate when municipalities with successful assassinations are compared with control groups that resemble them more in terms of the absence of mayors. These findings align more closely with institutional disruptions following the sudden loss of mayors, rather than the violent act itself. I complement this interpretation with two additional pieces of evidence. The effects of successful assassinations are the largest when mayors are killed 24-35 months into their term – when they are more embedded in the organization of municipal affairs before entering the electoral cycle. In addition, effects are larger for municipalities with mayors from minority parties that possess weaker political networks and support from upper levels of government.

Then, I test the strength of other potential channels. Adjustments in the composition and task allocation of the municipal personnel materialize gradually and thus are unlikely to account for immediate impacts on local fiscal capacity. Changes in the organized criminal group presence, municipal expenditure in public security, and deployment of federal police forces are also unlikely to explain the main results. Lastly, I assess the strength of channels relevant to non-political violence, economic activities, demographics, and electoral dynamics. As these factors may independently affect the tax base regardless of assassinations, testing them is necessary to isolate the role of mayoral absence. I find no statistically significant changes in crime rates, nightlight intensities, individual-level economic activities, population composition, outmigration, electoral competitiveness, and voter participation. Thus, these alternative channels are unlikely to drive the findings.

Overall, the results show that successful assassinations of mayors undermine local state capacity beyond political consequences. The capacity to collect taxes and allocate public resources declines. Evidence is most consistent with institutional disruption following the sudden loss of mayors as a central factor, not necessarily the violent act of assassination itself. Other factors—such as ad-

4. Liu and Mikesell (2014) and Mauro (1998) find that corrupt politicians distort the allocation of government resources towards sectors with high rent-seeking potential, including construction, over social welfare.

5. In my dataset, the funds are aggregated at the municipal level without specifying the recipient of government funds.

justments in municipal personnel, changes in security environment and expenditures, non-political violence, economic activity, demographics, or electoral dynamics—play at best a limited role. These results highlight how the loss of local politicians to organized criminal violence weakens the effectiveness of local public organizations. More broadly, the results demonstrate that successful political violence can erode non-political dimensions of state capacity. Finally, the study highlights the institutional vulnerabilities faced by decentralized governments in violent contexts.

This research contributes to three strands of literature. First, it speaks to the literature on the formation of the capacity of local governments. Origins of state capacity at the national level have been widely studied across many disciplines (Acemoglu 2005; Besley and Persson 2009, 2010; Finan et al. 2017; Tilly 1985). Recent works analyze the effectiveness of subnational public institutions (Dal Bó et al. 2013; Fenizia and Saggio 2022; Marx et al. 2024). A growing literature examines how exogenous shocks and monitoring mechanisms shape the capabilities of local politicians (Daniele and Dipoppa 2017; Daniele 2019; De Feo and De Luca 2017; Larreguy et al. 2020; Vannutelli 2022). However, most of these works focus on electoral and political outcomes, with less attention to the bureaucratic capacity of local governments. I address this gap by using novel municipality-level data on public finances, personnel, and political violence to examine how violent disruptions to local leadership weaken tax collection, public good provision, and the structure of the municipal workforce.

Second, this paper adds to the literature on the developmental costs of political violence. The economic consequences of violence are well-documented (Brodeur 2018; Dell 2015; Pinotti 2015; Sviatschi 2022; Velásquez 2020). A burgeoning body of research exploits political violence at the national or regional level to study cases where formal authorities are being contested by non-state actors (Alesina et al. 2019; Acemoglu et al. 2013; Blattman and Miguel 2010; Blattman et al. 2024; Dal Bó and Di Tella 2003; Dal Bó et al. 2006; Sánchez de la Sierra 2020). However, political violence that takes place at the most local levels of administration, where institutions remain most vulnerable, receives less attention. I advance this literature by focusing on direct attacks against local politicians and distinguishing political violence from broader crime trends using detailed data on criminal group presence. In doing so, I highlight that the consequences of political violence extend beyond electoral dimensions by degrading core bureaucratic functions.

Last, this paper contributes to research on how decision-makers shape organizational performance. Existing studies exploit unexpected transitions in national leadership (Blakeslee 2018; Iqbal and Zorn 2008; Jones and Olken 2005, 2009; Rommel and Schaudt 2020). Similar approaches have been applied to investigate the role of decision-makers on firm performance (Becker and Hvide 2022; Bennedsen et al. 2020; Fahlenbrach et al. 2017; Jaravel et al. 2018). These studies typically examine aggregate outcomes such as macroeconomic growth, firm profits, and institutional policy choices. Recent works focus more on the performance of bureaucratic personnel in local institutions using leadership turnovers and field experiments (Akhtari et al. 2022; Bazzi et al. 2025; Best et al. 2023). However, most of these studies examine contexts without violence directed at political figures. I expand this literature by leveraging detailed data at the local government level and the sudden involuntary loss of local leaders due to political violence. I provide novel evidence on how overt violence targeting decision-makers affects local state capacity in fragile environments, underscoring how core administrative functions are impaired.

The rest of the paper proceeds as follows. Section 2 provides an overview of the role of the municipal government and the political violence in Mexico. Section 3 describes the data and descriptive statistics. I explain the empirical strategy in Section 4. Section 5 reports key findings on the effects of losing leaders to successful assassinations on local fiscal capacity. In Section 6, I weigh the plausibility of various potential channels that may affect the outcomes. Section 7 concludes.

2 Background: Municipal governments and political violence in Mexico

Municipal governments in Mexico offer a compelling context to examine how the loss of leaders to successful assassinations affects the local state capacity. Mayors have authority over tax collection, public goods provision, and personnel decisions. Since the mid-2000s, they have become increasingly vulnerable to assassinations at the hands of organized criminal groups trying to capture local resources vital to their operations. Data show that mayors in municipalities with a high presence of organized crime are more likely to be targeted and killed, regardless of broader non-political violence. This section provides an overview of municipal governments and organized crime in Mexico.

2.1 The authority and characteristics of municipal governments

Mayors serve as heads of municipal governments for a limited term. There are 2,471 municipalities in 32 states, including the 16 boroughs in Mexico City. Each mayor serves a 3-year term and has been eligible for reelection since 2018.[6] Mayors are elected with the vice mayor (*alcalde suplente*), one or two attorney generals (*sindicos*), and several councilors (*regidors*) as running mates.

When a mayor is unable to finish the term due to reasons including assassinations, a replacement is appointed to serve the remainder of the term. This individual is typically the vice mayor and is not selected through a make-up election (Esparza and Mancera 2018). The next formal mayor is chosen in the following regular election cycle. The replacement is often tasked with filling an unexpected governance vacuum, sometimes without adequate institutional support (Rios 2012; Trejo and Ley 2021). The other elected members of the municipal government retain their positions, according to Article 115 of the Constitution of Mexico.

Municipalities in Mexico finance their operations through local property taxes and intergovernmental transfers. Municipal governments gained fiscal autonomy in the middle of the 1990s, allowing them to collect property taxes independently (Careaga and Weingast 2003; Larreguy et al. 2020).[7] However, grants from the central government still account for a significant share of the municipal government revenue (Careaga and Weingast 2003). These consist of earmarked (*aportaciones*) and non-earmarked (*participaciones*) transfers (INEGI 2016). The latter is partly determined by the taxes collected at the municipal level and takes up about one-third of the municipal revenues (Timmons and Broid 2013; INEGI 2016).[8] Further details are in Appendix A.1.

Municipalities spend heavily on personnel payments, public investments, provision of public services, and transfers to internal institutions responsible for health and education (INEGI 2016). These expenditures typically cover water supply, waste management, infrastructure projects, and local health and education initiatives (Larreguy et al. 2020). These are mostly financed by local taxes and central government grants (Chong et al. 2015). As a result, reductions in such revenue streams can constrain the delivery of public services, which I verify in Section 5 (Careaga and Weingast 2003).

The personnel of the municipal government play a crucial role in financing and executing these operations. The heads of key municipal institutions are designated by mayors (Dell 2015; Grillo

6. Before 2018, mayors could not seek reelection (Larreguy et al. 2020). This ban was lifted as a result of the electoral reform in 2014, but only came into practice in 2018 due to the timing of election cycles (Enríquez 2022).

7. Property taxes are overseen by municipal governments, also clarified in Article 115 of the Mexican Constitution. Other forms of taxation, such as income taxes, are levied by the federal or state government.

8. The non-earmarked portion of the funds from the higher levels of government is a function of the municipal tax collection, economic growth, and previous *participaciones* (Timmons and Broid 2013). Part of the rationale for incorporating tax collection into the intergovernmental transfers is to incentivize the subnational governments to internalize local economic prosperity and to allow them to retain a higher share of revenues raised from growth (Weingast 2009). Further details, including the sample formula for these funds, will be included in the Appendix A.1.

2011). Mayors also have the final say in hiring and retention of bureaucrats, who comprise 21% of public sector jobs in Mexico and manage public service delivery, security, finances, and economic development (Dal Bó et al. 2013; INEGI 2022). The absence of mayors following assassinations can disrupt the recruitment and retention of workers, possibly undermining local government capacity. I examine this mechanism empirically in Section 6.

2.2 Organized criminals and the attacks on local officials

Organized criminal groups in Mexico were not always in conflict with local politicians. Until the 1990s, violence against local politicians was less prevalent because criminal groups benefited more from cooperating with corrupt officials than from targeting them (Lessing 2015; Grillo 2011). Organized criminal groups engaged in inter-cartel wars to win control over key trade routes, particularly along the U.S. border (Dell 2015; Trejo and Ley 2021). They bribed corrupt local government officials for cooperation in securing these routes and gaining an advantage over rival groups (Grillo 2011).

This dynamic has been shifting in the mid-2000s, when the federal government launched the "War on Drugs" and intensified efforts to curb drug trafficking and to dismantle major criminal organizations (Grillo 2011).[9] The federal crackdown led to high-profile arrests but also unintentionally resulted in fragmentation of large cartels into smaller ones (Magaloni et al. 2020). With narrower trafficking opportunities amidst heightening tensions, the fragmented groups sought alternative revenue sources and began targeting local officials who oversee access to valuable local resources (Trejo and Ley 2019).

As a result, an increasing number of local politicians are attacked by organized criminal groups, as seen in Figure 1. Organized criminal groups turned to alternative revenue sources such as ransoms, extortions, local fiscal funds, and construction projects as drug trafficking became difficult to sustain (Grillo 2011). Investigative reporting and previous studies further highlight that organized criminal groups have increasingly targeted municipal officials – mayors in particular – to influence local governance and gain leverage over public resources such as tax registries, public works, and infrastructure projects (InSight Crime 2013, 2024; Lessing 2015; Trejo and Ley 2019). In other cases, criminals attack mayors to influence the electoral process to facilitate access to this information (En-

9. The "War on Drugs", declared by President Felipe Calderón, involved the deployment of the federal military throughout Mexico's most contested regions. These strategies involved targeting the 'kingpins' of major criminal organizations (Magaloni et al. 2020). Despite some success, such as taking down the Beltrán-Leyva organization, others such as La Familia expanded their influence by retaliating against local politicians (Trejo and Ley 2019, 2021)

ríquez 2022; Magaloni et al. 2020).

This qualitative evidence corresponds with the descriptive results from the data. Mayors are the most vulnerable at the beginning and the end of their terms, coinciding with the election cycle (Appendix Figure B1). As attacks around elections facilitate involvement by illegitimate groups, this evidence aligns with the political motives of organized criminal groups seeking local resources (Enríquez 2022). In addition, mayors in locations with multiple criminal groups are retaliated for siding with rival groups or not cooperating at all (Lessing 2015). I explore this in the next section.

2.3 Which municipalities are more vulnerable?

This section studies whether the assassinations of mayors are more closely linked to criminal group presence rather than non-political violence. The effects of assassinations may simply reflect broad insecurity rather than targeted political violence if these events occur more in municipalities with high non-political crime rates. To attribute the effects of successful assassinations to political violence by organized criminal groups, the incidence of assassinations should correlate with criminal group presence but not with non-political violence.

I construct a municipality-by-year panel of criminal group presence using Coscia and Rios (2012), Osorio and Beltran (2020), and ACLED, which capture the presence of major criminal groups and unidentified organizations.[10] Then, I use the following descriptive regression.

$$y_{mt} = \alpha + \beta_{\text{OCG}}\text{OCG}_{mt} + \beta_{\text{hom}}\text{Homicide}_{mt} + \phi X_{mt} + \gamma_m + \delta_t + \epsilon_{mt} \tag{1}$$

y_{mt} is the dummy variable for assassinations. OCG_{mt} refers to the measures of organized criminal presence. I construct measures using both identified and unidentified criminal groups. Unidentified cases, where the affiliation of the criminal group is unknown, provide an upper bound of criminal group presence, while using only identified groups gives a lower bound. Homicide_{mt} is the homicide rate proxying for non-political violence from the National Institute of Statistics and Geography (INEGI). X_{mt} is the set of municipal-level demographic and socioeconomic characteristics. I include municipality (γ_m) and year fixed effects (δ_t). The error is clustered at the municipal level. Further explanations of the data are found in Section 3.

10. Coscia and Rios (2012) captures the presence of major criminal groups - such as but not limited to La Familia Michoacan, Sinaloa Cartel, Beltran Leyva Cartel. Other datasets identify both the local affiliates and their parent groups. For consistency across datasets, I aggregate the subsidiary groups to the higher level.

The results in Table 1 show that assassinations are correlated with the presence of criminal groups but not with non-political violence.[11] When considering all of Mexico, the presence of an additional criminal group is associated with a 0.2%-0.3% increase in the likelihood of assassinations. A new criminal group is associated with a 0.3 - 0.4 percentage point increase in assassinations. These relationships remain constant when the sample is narrowed to municipalities with assassination attempts. Homicide rates show no significant relationship with assassinations. All regressions include municipality and year fixed effects. The results also align with previous evidence that mayors are attacked for siding with rival groups or not cooperating (Lessing 2015).

3 Data

I construct a novel municipality-level panel dataset combining information on assassination attempts against mayors with municipal state capacity indicators. Assassination attempts are identified using text-scraped data from online newspapers, which are matched to fiscal, personnel, demographic, economic, and crime data from multiple sources. This dataset allows me to leverage variation in the outcome of assassination attempts and measure local government effectiveness across municipalities over time. I provide a detailed explanation of the steps for constructing the dataset.

3.1 Assassination attempts against mayors: Sources and collection procedure

I use two types of sources for assassination attempts against mayors. First, I collect newspaper articles documenting attacks against mayors from online archives such as *Newsbank* and *ProQuest*. The articles are originally published by both local and nationwide news outlets. Second, I complement these with existing event databases such as Global Database of Events, Language, and Tone (GDELT), Data Cívica, Armed Conflict Location and Event Data (ACLED), and reports by Esparza and Mancera (2018) and Magar (2018). Using independent databases alongside local and nationwide news outlets reduces reliance on any single media market. It mitigates concerns about potential underreporting of failed assassination attempts, particularly in contexts where local news outlets may be discouraged from reporting such events due to fear of retaliation.[12]

11. Conclusions are similar if I use the incidence of attacks on mayors for an outcome variable instead of assassinations. The results are in Appendix Table B1.

12. These event databases aggregate reports from news outlets and independent investigations. This reduces dependency on local news outlets, which may face intimidation from criminal groups (Grillo 2011). Though no single source provides

To collect information from the online archives, I generate a script that searches for articles mentioning keywords relevant to assassination attempts. Then, I extract the date of the attacks, the name and party affiliation of the victimized mayor, the municipality, and the outcome of the assassination attempt. The outcomes are categorized into assassinated, injured, and unharmed based on the language describing the status of the mayor. I also track the name of the publisher to ensure that a broad range of sources are used.[13] Further technical explanations will be included in Appendix A.2..[14]

The rationale for categorizing outcomes into successful, injured, and unharmed is as follows. A successful attack results in the death of a mayor within a few days. As mayors can no longer serve, these cases constitute the treatment group. Failed attacks include incidents in which the mayor is directly targeted — whether in transit, at home, or at the workplace — but survives. These are further disaggregated into unharmed and injured cases. Mayors who are not physically hurt and are either not present at the site of the attack or return to work immediately are categorized as unharmed. Because these incidents do not lead to disruption of mayoral presence, I use them as a control group. Cases in which mayors are injured — hospitalized or requiring medical attention — are excluded from the main analysis. This is because injured mayors do not cleanly fit into either treatment or control groups due to their temporary absence. However, the subsequent findings are robust to alternative classifications of failed attempts, as shown in Section 6. I explain the rationale for this design in Section 4.

Other forms of violence against mayors such as kidnappings that do not result in death, attacks on family members, and death threats are excluded from the main analysis. These acts may seek to coerce or intimidate rather than permanently remove mayors from office. Attacks occurring at public events and municipal properties are also excluded, since whether the mayor is the intended target or not is unclear in many such cases.[15] Excluding these cases ensures that the treatment captures variation in the outcome of the attempts to permanently eliminate sitting mayors.[16]

I extract a total of 163 assassination attempts from these sources between 2002 and 2021. Of these,

exhaustive information, triangulation across independent reporting channels reduces the likelihood that underreporting is systematically correlated with local criminal influence.

13. I include articles from nationwide sources such as *El Universal, La Jornada* and *Reforma* but also regional newspapers.

14. While many articles state the name of the culpable organized criminal groups, not all of them specify the identity of the attacker. This leaves me with the group affiliation of the attacker as the only proxy for the capacity of the attackers. I later show in Panel B of Table 2 that these are not different across treated and control municipalities. This suggests that given the available data, the differences in the capabilities of OCGs are unlikely to explain the results.

15. Re-estimating the main results with these cases included yields quantitatively similar results, as reported in Appendix figure E1.

16. Most results are robust to including these municipalities in the control group, as shown in Appendix Figure E2

85 were successful attempts. I record 69 failed attempts, further divided into 25 cases involving injuries and 44 in which mayors are unharmed. These events occurred in 138 municipalities.[17] Figure 2 shows the geographic and temporal distribution of these events. The full lists of mayors targeted by assassination attempts are in Appendix A.3. (Tables A1 - A3)

3.2 Data on municipal fiscal effectiveness and local government personnel

To measure fiscal capacity and the composition of the municipal personnel, I combine several datasets from INEGI. I use the annual panel of municipal fiscal revenues and expenditures (EFIPEM[18]) to track the local fiscal capacity. For personnel, I use the biennial census of municipal governments (CNGMD) and quarterly National Survey of Occupation and Employment (ENOE).[19]

The EFIPEM dataset includes various categories of revenues and expenditures. Revenues from taxes, intergovernmental transfers, service fees, and fines are included. I primarily use tax revenues as a measure of fiscal capacity, consistent with the literature on state capacity (Besley and Persson 2009, 2010). The data contains public expenditure on general services, construction investments, and transfers to municipal institutions. I use these to trace how the provision of various services and the scope of local government activities are affected. I use data from 1995 onward, when local governments gained more fiscal authority (Larreguy et al. 2020).[20] Detailed explanations are in Appendix A.4. Summary statistics appear in Table A4.[21]

The data on municipal personnel come from two sources. The CNGMD provides data on the total size and composition of the municipal workers by age group and task category, such as public service, security, and finance. These data are collected biennially by INEGI since 2010, resulting in a shorter timeframe than the fiscal data. The ENOE dataset provides nationally representative sectoral earnings data that is used to establish a conceptual framework for analyzing how successful assassinations affect worker retentions in Section 6. Summary statistics are provided in Tables A4.

17. There are also no less than 23 failed kidnapping attempts, 69 incidences of family members attacked, and 50 threatening messages directed at municipality presidents. These are excluded from the regression but included in Figure 1.

18. Estadística de Finanzas Públicas Estatales y Municipales

19. CNGMD and ENOE stand for *Censo Nacional de Gobiernos Municipales y Demarcaciones Territoriales de la Ciudad de México* and *Encuesta Nacional de Ocupación y Empleo*.

20. The raw data for EFIPEM dates as far back as 1989. Results are robust if all available EPIFEM data are used.

21. Summary statistics that include data from all municipalities in Mexico are in Appendix Table A5

3.3 Data sources for outcome variables used in falsification tests

I obtain variables that may confound the estimated effects of assassinations to validate potential mechanisms. These include spending on public security, deployment of *federal* police forces, non-political crimes, economic activity, demographics, and electoral environments. If municipalities with assassinations experience differential changes in these variables, it may indicate that the estimated effects are attributable to broader factors beyond the loss of mayors. In that case, the true magnitude of the effects of assassinations may be biased. To address this, I test whether these potential confounders covary with successful assassinations.

I compile the data from these sources. The data on public security expenditure also comes from EFIPEM. The data on the presence of federal police forces are obtained from the replication package of Flores-Macías and Zarkin (2024).[22] The municipal statistics on criminal activities are from INEGI and the Executive Secretariat of the National Public Security System (SESNSP). For economic activities, I use the nightlight data from DMSP and VIIRS as well as individual-level economic indicators from ENOE (Donaldson and Storeygard 2016; Henderson et al. 2012).[23]

Data on population dynamics are from the Mexican Census, WorldPop, and the Institute of Mexicans Abroad (IME). WorldPop provides an annual estimate of the population based on satellite images and complements the intercensal gaps in the Mexican Census. The two data correlate strongly for years in which they overlap (Appendix Figure A2). Internal outmigration is measured using the Mexican Census and is defined as the municipality-year share of individuals who lived in a given municipality five years earlier and reside in a different municipality within Mexico at the time of the census. I also use the number of consular ID Cards (MCAS) issued to Mexicans residing in the United States from IME data to measure outmigrants to the United States.[24]

To examine changes to the electoral environment, I draw on municipal election data from Calderón-Hernández et al. (2025) and Magar (2018) and state electoral commissions.[25] I use the number of candidates to capture the supply of candidates. I analyze effects on electoral competitiveness using vote shares by party, electoral outcomes for incumbent candidates. and voter participation rates.

22. According to Flores-Macías and Zarkin (2024), the original data is obtained through the right-to-information request. The permission was granted only for periods 2000-16. Thus, the results of the regression with this outcome should be interpreted with caution.

23. DMSP is available up until 2013 and is discontinued after. VIIRS data is only available from the year 2012. I generate a harmonized measure of nightlight data with a procedure detailed in Appendix Figure A1 in Appendix Section A.5.

24. MCAS and IME each stand for *Matrica Consular de Alta Seguridad* and *Instituto de los Mexicanos en el Exterior*.

25. As municipal elections are administered by state-level bodies, the availability and format of data vary across states.

3.4 Data sources for covariates

To address omitted variable bias, I include covariates for criminal group presence, general criminal activity, and key demographic and geographic characteristics in the robustness check. The data on organized criminal groups are the same as those used in Section 2: Coscia and Rios (2012) for periods before 2000, Osorio and Beltran (2020) for 2000-2018, and ACLED for 2019 onward. I include municipality-level homicide rates from INEGI to account for general criminal activities and other factors associated with lack of state presence (Dal Bó et al. 2013).

For demographic and geographic characteristics, I use the Mexican Census data on the average years of schooling and the share of the Indigenous population at the municipal level. These variables proxy for structural marginalization and underdevelopment, which are often linked to limited state capacity (Dal Bó et al. 2013). Further details are in Appendix A.4.[26][27]

4 Empirical strategy

I compare municipalities with successful assassinations to those with failed attempts that did not injure the mayors using event-study specifications. The treatment effect is identified by comparing changes in local government capacity indicators across the treated and control groups over time. This design addresses selection bias and controls for confounding factors such as the presence of political violence. I discuss the construction of the analysis sample and the main specification in this section.

4.1 Constitution of the treatment and control group municipalities

To isolate the effects of losing mayors to successful assassinations, I construct a counterfactual of the municipalities that lost their mayors to assassination with those whose mayors were unharmed after the attacks. The former group of municipalities is the treatment group while the latter is the control group (*near-miss*). I leave out municipalities whose mayors were injured since their temporary absence blurs the distinction between treatment and near-miss groups. I report that my findings are robust to including such cases in the control group in Section 6.

26. These census variables are available every five years. I fill gaps by linear interpolation. Results are robust to excluding these covariates.

27. While attacks may depend on mayors' political or educational backgrounds, such data are only available for select municipalities after the 2010s.

More importantly, municipalities without any assassination attempts against mayors are excluded.[28] As organized criminal groups select targets based on strategic considerations, including non-targeted municipalities could introduce selection bias to treatment (Dell 2015; Enríquez 2022). This creates imbalances in unobserved and observed characteristics across municipalities and contaminates the treatment effect estimates. By making comparisons conditional on mayors being targeted, the design nets out confounders and ensures that the treated and near-miss groups are similar in their exposure to political violence and other pre-treatment characteristics (Brodeur 2018; Jones and Olken 2009). This allows identification of the effects of losing mayors to successful assassination attempts while ensuring balance in pre-treatment characteristics.

To improve balance further, I implement the following measures. First, I drop four municipalities ranked in the top 3% of the population distribution. These municipalities have disproportionately large budgets and crime levels. This leaves me with 82 municipalities in the treated and 31 in the near-miss group, although retaining these municipalities does not alter the results significantly. Then, I conduct a balance test with the remaining municipalities in Table 2 using observable pre-treatment characteristics one year before assassinations (the year of assassinations for political affiliations).[29] Overall, these municipalities are mostly comparable except for the average year of schooling, affiliation to one of the political parties, and existence of *Usos y Costumbres*.[30] This shows that the success and the failure of assassination attempts are plausibly random within this sample.[31]

For municipalities with multiple assassination attempts, I retain the first successful event. This follows a general event study setup where treatment status is nondecreasing over time (Sun and Abraham 2021; Callaway and Sant'Anna 2021). If all such attempts have failed, they belong in the near-miss group. There could be concerns that these cases may act as outliers that skew the estimation results. I find that results are robust to excluding these municipalities (Appendix Figure E3).

28. To the extent that failed assassination attempts also affect outcomes in near-miss municipalities, the estimated treatment effects may represent lower bounds on the full effect of losing a mayor. The attenuation pattern documented in Section 6.1, where effects shrink as the control group increasingly resembles the treated group in terms of mayoral absence, is consistent with this interpretation.

29. The rationale for setting the timing differently for political affiliations is that for some municipalities, the political party of the mayor may differ between the year of assassinations and the year before. This ensures that the attacked mayor's party affiliation is accurately reflected.The balancedness of party affiliations is not affected when adjusting timing of comparisons to one year prior to attacks, as shown in Table A7.

30. Usos y Costumbres refers to election of local leaders through non-partisan community meetings that take place in many indigenous communities in Mexico (Díaz-Cayeros et al. 2014).

31. In Appendix Tables A6 and A8, I conduct a balance check across treated, near-miss, and municipalities where mayors are injured. Even in this setting, there are no noticeable differences across observable characteristics at the municipal level.

4.2 Model specifications: Measuring the effects of assassinations over time

To estimate the dynamic treatment effects, I employ an event-study regression that leverages the temporal and geographical variation of assassination attempts. The model includes time-relative-to-treatment indicators, municipality and year fixed effects, and observable municipal characteristics. The estimating equation is as follows.

$$y_{mt} = \alpha + \sum_{\substack{h=-6 \\ h\neq -1}}^{6} \tau_h I[t - \text{assassination} = h]_{mt} + \tau_{7^+} I[t - \text{assassination} \geq 7]_{mt} + \beta X_{mt} + \gamma_m + \delta_t + \varepsilon_{mt} \tag{2}$$

y_{mt} is the outcome variable of interest for municipality m and time t. I use annual municipal expenditures and revenues for fiscal outcomes and the biennial share of various groups of municipal workers for personnel outcomes. For regression testing alternative mechanisms, I use nightlights, population, crime statistics, and electoral outcomes at the municipal level. Municipality fixed effects (γ_m) control for time-invariant imbalances at the municipality level, while year fixed effects (δ_t) capture common temporal shocks. The baseline specification includes municipality and year fixed effects.[32] Standard errors are clustered at the municipality level.

The treatment indicators $I[t - \text{assassination} = h]_{mt}$ equal 1 if year t is h years since successful assassination in municipality m, and 0 otherwise. Near-miss municipalities are never assigned treatment τ_h captures the effect of assassinations on y_{mt} h years after successful assassinations. It is identified by comparing treatment municipalities h years since assassination against near-miss municipalities. I control for 6 leads and lags as this corresponds to two separate terms for mayoral positions before and after the event.[33] For normalization purposes, the year before the assassination ($h = -1$) is omitted (Borusyak et al. 2021). To isolate long-run effects, observations with $h \geq 7$ are grouped in to a single indicator $I[t - \text{assassination} \geq 7]_{mt}$ with coefficient τ_{7^+}. This follows a dynamic event-study setup as defined in Borusyak et al. (2021).

X_{mt} denotes a vector of observable municipality-level characteristics to mitigate omitted variable bias and to improve the precision of the treatment effect estimates. These include homicides per

32. While more demanding specifications such as state-by-year fixed effects are considered in robustness checks, they substantially reduce statistical precision given the limited number of treated clusters (115 municipalities across 23 states). Importantly, the results remain qualitatively unchanged when these fixed effects are included (Appendix Figures E4-E5).

33. The signs and estimators remain similar if I include different numbers of leads and lags.

100,000 persons, the log(total homicides), the log(number of criminal groups), the share of the Indigenous population, the average years of schooling of the municipal population, and the number of years since the most recent election (in levels and squared terms). For post-attempt years, covariates are held fixed at their final pre-treatment years to prevent bad control problems (Callaway and Sant'Anna 2021). While the balancedness between the treated and near-miss groups documented in Table 2 suggests that covariates are not strictly necessary for identification, I include them in several specifications to improve precision and absorb residual variation. I regress without covariates in the baseline specification while demonstrating that results remain consistent when covariates are included.

The identifying assumption is that treated and near-miss municipalities follow parallel trends in the absence of treatment. This assumption is violated if τ_h for pre-assassination periods ($h \leq -1$) are statistically different from zero. I test for pre-treatment outcome differences between the two groups by averaging the outcome variables across pre-assassination periods in Table 3. The results indicate no statistically significant differences, supporting the plausibility of the parallel trends assumption.

Because standard two-way fixed effects estimates may be biased in the presence of treatment effect heterogeneity across time and units (Baker et al. 2022; Sun and Abraham 2021), I also use three recently developed event study estimators: the stacked difference-in-difference estimator used in Cengiz et al. (2019), the imputation-based estimator developed by Gardner et al. (2024), and the 2×2 interaction-weighted estimator (Sun and Abraham 2021). The latter two maintain the same set of fixed effects structure as the baseline specification, while the stacked estimator additionally incorporates state-specific linear time trends to mitigate potential distortions arising from implicit weighting across event cohorts.[34] Similar to the baseline specification, covariates are not included.

To examine the robustness of the findings to specification choices, I conduct the following tests. First, I include outlier municipalities with large populations to check whether these observations influence the results. Second, I report estimates with covariates and confirm results remain similar. Third, I apply the Holm corrections for multiple hypothesis testing to account for the large number of outcomes tested. Fourth, I include state-specific linear trends and state-year fixed effects to account for differential state-level dynamics (Appendix Figures E4-E5). The findings are qualitatively consistent across all of these exercises.

34. The state-specific linear yearly trends address potential differences in estimation results that may arise due to different weights across subdatasets (Baker et al. 2022). Estimation results are largely similar even without linear trends.

5 Effects on local fiscal capacity

In this section, I empirically test whether local fiscal capacity declines following successful assassinations. Specifically, I examine the consequences of successful assassinations on the ability to raise revenues and allocate resources across various public services. Overall, findings indicate that affected local governments lose their capacity to maintain revenues and allocate public resources. The findings are robust across different specifications.

5.1 Negative effects on the municipal revenues

To assess whether tax collection deteriorates following successful assassinations, I examine changes in total and property tax revenues, both in levels and per capita terms. I also investigate whether the consequences also spill over to other revenue sources such as non-earmarked federal transfers (*fondos participaciones*[35]) and earmarked transfers (*aportaciones*[36]), revenues from public services (*derecho*) and legal fines (*aprovechamientos*). As non-earmarked transfers are partially determined by municipal tax revenues, I expect them to follow the changes in municipal tax collection. Other sources are largely independent of taxation, thereby not likely to respond following successful assassinations (Careaga and Weingast 2003; Timmons and Broid 2013).

Figure 3 presents event-study estimates and Columns (1) - (4) in Table 4 report the average treatment effects following successful assassinations over the six years on tax outcomes. The results indicate that the loss of mayors to assassination undermines the capacity to collect taxes. Revenues from all local taxes decline by approximately 29% relative to near-miss municipalities, with most estimates statistically significant at the 1% level. The effects are realized immediately following assassinations and persist for over six years. The fall in per capita tax revenue ranges from 67 to 105 pesos, or 18-28% relative to the control mean, albeit less precise. Property tax revenues decline, both in total (approximately 21%) and per capita terms (approximately 41 pesos).[37] Estimates are consistent across different specifications and Holm multiple hypothesis testing corrections.

35. Non-earmarked funds are comprised of General participation fund (FGP) and Municipal Development Fund (FFM). While equity across regions is the main objective, the latter also takes into account local taxation efforts (OECD 2016).

36. Earmarked funds are broken down into Municipal Fund for Social Infrastructure (FISM) and Funds for Municipal Development (FORTAMUN). Both are granted conditional on infrastructural and development projects within the municipalities while taking poverty levels and demographic factors into account (Larreguy et al. 2020).

37. 73% of municipal tax revenues are from property taxes (OECD 2016). However, property taxes account for just 2% of all taxes paid by individuals in Mexico (World Bank 2016). Furthermore, the share of own-source tax on total revenues is small and has high variation across small and large municipalities (World Bank 2016). Thus, changes in tax revenue for municipal governments are large, whereas per capita changes are small.

Other sources of revenue determined by local taxation are also affected, as reported in Figure 4 and Columns (5)-(8) of Table 4. Non-earmarked funds decline by 9.7% to 12.7% compared to near-miss municipalities, all significant at 5% or 1% level (Column (5) in Table 4). As this fund is partially proportional to municipal tax collection, this result is explained by a fall in municipal tax revenues. Other revenue sources are largely unaffected, as they are determined by demographic factors, demand for public services, and legal fines. Results are robust across different specifications and Holm multiple hypothesis testing corrections.

These results indicate that the loss of mayors to successful assassinations undermines local fiscal capacity to raise revenues relative to near-miss municipalities. Tax revenues decline, which spills over into other sources of revenue linked to taxation such as non-earmarked federal transfers. The findings are robust to alternative specifications and persist over time. These support the interpretation that the loss of leaders following assassinations weakens local fiscal capacity.

5.2 Diversion of government resources to select sectors

I examine how successful assassinations of mayors affect the composition and volume of local government expenditures. Specifically, I focus on public investments in infrastructure construction projects, general services expenditure, and transfers to municipal institutions providing educational and health services.[38] The part of general services expenditure examined here (henceforth non-infrastructural expenditures) includes spending on general administrative services such as rents, maintenance, purchases, and travel expenses incurred by municipal workers. Other categories, such as basic services and personnel expenditures, are reported in the Appendix (Appendix Figure B2). I report the outcomes log of expenditure amounts and their shares relative to total expenditures. The former captures changes in expenditure volume, while the latter reflects shifts in budget allocation across different expenditure categories.

Figure 5 reports event-study estimates on spending outcomes, with the 6-year post-assassination average reported in Table 5. Since assassinations, the share of public investment expenditure rises by 5 to 5.6 percentage points relative to near-miss municipalities, with most results significant at 1% level (Column (1) in Table 5). The volume of public investment expenditure rises by roughly 20%, although the log level estimates are less precise (Column (2) in Table 5). In contrast, both the share

38. Each is called *Inversión pública*, *servicios generales*, and *Transferencias, asignaciones, subsidios y otras ayudas* in EFIPEM.

and volume of non-infrastructural expenditure decline by 1.4-1.6 percentage points and around 40% relative to near-miss municipalities, respectively (columns (3) and (4) of Table 5). Similarly, transfers to municipal institutions fall by 1-2 percentage points in share and 42% in volume, respectively (columns (5) and (6) in Table 5). All results are robust to choices of specifications and Holm multiple hypothesis testing corrections.

These findings suggest a reallocation of resources toward sectors that may benefit criminal organizations, at the expense of essential services. In Mexico, prior studies and investigative journalism document how criminal groups influence municipal resource allocation and infrastructure projects (Calderón et al. 2019; InSight Crime 2013, 2024). These accounts emphasize the vulnerability of construction and public investment decisions as they involve large, discretionary outlays and procurement processes that can be more easily influenced through coercion or collusion. Similar patterns of resource diversion to construction following criminal infiltration are documented in other places, most notably in Italy (De Feo and De Luca 2017; Di Cataldo and Mastrorocco 2022). Meanwhile, declines in non-infrastructural spending and transfers to municipal institutions indicate that fewer resources are directed toward key services dedicated to poverty reduction, development, and welfare (Liu and Mikesell 2014; Mauro 1998). Overall, these findings highlight how successful assassinations undermine the local state capacity to fulfill fundamental public functions.

5.3 Summary of findings

Municipalities with successful assassinations of mayors fail to maintain their fiscal capacity. Affected local governments cannot sustain the level of tax collection and redirect expenditures to construction investments, often at the expense of essential services. These patterns indicate that successful assassinations undermine the capacity to finance and deliver public services pivotal for local development. In the following sections, I explore mechanisms that highlight the importance of the presence of local leadership in maintaining local fiscal capacity in light of political violence.

6 Discussion of mechanisms

This section evaluates the plausibility of the mechanisms behind the effects of successful assassinations. First, I test whether institutional disruptions and the absence of mayors, rather than the violent act itself, drive the results. Second, I investigate security responses and heterogeneity across political factors to see how the consequences of assassinations may be shaped. Third, I examine whether delayed personnel adjustments may represent one margin through which disruption in local leadership erodes local state capacity over time. Finally, I evaluate the plausibility of alternative explanations for the effects of successful assassinations.

6.1 Presence of mayors matters: Results using alternative control groups

To assess whether the absence of mayors – as opposed to the violent act itself – drives the results, I re-examine the findings using alternative control groups that also experience mayoral absence for different reasons. Specifically, I compare municipalities with successful assassinations to those in which mayors were absent due to injuries following failed attempts or due to nonviolent deaths such as health-related causes or accidents. If the absence of mayors and the ensuing institutional disruptions are the central drivers, then the effect sizes should attenuate as control municipalities increasingly resemble the treated municipalities in terms of absence of local leadership. Conversely, if the violent act itself is the primary driver, estimated effects should remain similar in magnitude under these alternative comparisons.

I conduct this exercise in two ways. First, I re-estimate Equation (2) with different sets of control groups. Specifically, I re-run Equation (2) using three alternative control groups: (1) all failed assassination attempts, (2) failed attacks resulting in injury, and (3) nonviolent deaths. The lists of mayors used in this exercise are found in Appendix Section A.3.[39]

Second, I run a triple-difference specification including all types of municipalities discussed above. I apply the following triple-difference specification.

$$\begin{aligned} y_{mt} = \alpha &+ \beta_1 \text{Post}_{mt} + \beta_2 \text{Injury}_m + \beta_3 \text{Death}_m + \beta_4 \text{Post}_{mt} \times \text{Injury}_m + \beta_5 \text{Post}_{mt} \times \text{Death}_m \\ &+ \beta_6 \text{Death}_m \times \text{Injury}_m + \beta_7 \text{Post}_{mt} \times \text{Injury}_m \times \text{Death}_m + \gamma X_{mt} + \delta_t + \gamma_m + \varepsilon_{mt} \end{aligned} \tag{3}$$

Post_{mt} equals 1 for years t following the attack or nonviolent death used to classify m, and zero otherwise. Injury_m equals 1 if a mayor in municipality m is injured or killed as a result of violence. Death_m equals 1 if a municipality loses a mayor to assassinations or nonviolent deaths at some point in the sample. The regression includes the same sets of covariates, as well as municipality fixed effects

39. 50 mayors have passed away due to COVID-19 in 2020-2021. I exclude them from this exercise due to a lack of post-event observations.

and year fixed effects. As Injury_m and Death_m are time-invariant at the municipality level, their main effects are absorbed by municipality fixed effects. Thus, identification comes from differential post-event changes across groups through interactions with Post_{mt}.

Among municipalities experiencing mayoral absence or attacks, the near-miss municipalities with uninjured mayors serve as the benchmark group. β_1 captures changes in fiscal capacity in near-miss groups relative to their own pre-event period. β_4 measures the additional effect of injuries to mayors compared to near-miss cases. β_5 captures the additional effect of nonviolent deaths relative to near-misses. Lastly, β_7 assesses additional effects of assassinations relative to municipalities with injuries or nonviolent deaths. It should be noted that since the current regression does not test for the balance of observable characteristics across all four types of municipalities, the findings should be taken as descriptive comparisons.

Consistent with the hypothesis, estimated effects attenuate as control groups increasingly experience mayoral absence across both event-study and triple difference designs (Figure 6 and Table 6). In particular, the estimated treatment effect in Table 6 is the largest when municipalities with successful assassinations are compared to those with failed attempts that did not injure the mayors. When compared with other control groups, the effect sizes decrease. This pattern is also evident in Figure 6. This is difficult to reconcile with a pure violence or generalized insecurity explanation: if the violent act itself were driving the results, comparisons with uninjured near-miss municipalities should yield effects of similar magnitude to those using other control groups.

Combined with the lack of make-up elections and the fact that the interim mayors remain in office until the next regular election, this pattern is consistent with the interpretation that the absence of elected mayors following the event plays an important role in declining local state capacity.[40]

6.2 Heterogeneity of the effects by political factors

In this section, I investigate whether the effects of successful assassinations vary with political factors. Specifically, I examine treatment effect heterogeneity along two dimensions: the timing of the assassination within the mayoral term and the party affiliation of the mayor. As longer-serving mayors are more likely to be integrated into the organization of municipal affairs, their sudden loss may generate greater institutional disruption and larger declines in local state capacity. Party affil-

40. Comparing injured (treated) to unharmed (control) mayors also yields a nonzero effect (Appendix Figure E6), suggesting the role of mayoral presence. However, the small sample (20 vs. 37 municipalities) limits statistical power.

iation of mayors shapes access to political networks and support from upper levels of government, which may influence how local governments navigate the aftermath following successful assassinations (Trejo and Ley 2019). Thus, the negative effects could be more pronounced in municipalities governed by mayors representing minority parties with weakly established networks.

The information on the timing of the assassination within the mayoral term and party affiliation is obtained from the electoral data described in Section 3. The timing of assassinations is divided into four tenure bins, each spanning 12 months of the mayoral term: 0-11 months, 12-23 months, 24-35 months, and 36 or more months since taking office. I categorize party affiliation into MORENA, PAN, PRD, PRI, and others. I use the following regression specification.

$$y_{mt} = \alpha + \tau_1 D_{mt} + \tau_2 D_{mt} \times I_{mt} + \pi_1 L_{mt} + \pi_2 L_{mt} \times I_{mt} + \beta X_{mt} + \delta_t + \gamma_m + \varepsilon_{mt} \tag{4}$$

D_{mt} equals 1 if municipality m has experienced a successful assassination within 6 years prior to year t, and zero otherwise. L_{mt} equals 1 for municipalities that experienced a successful assassination 7 or more years ago. I_{mt} denotes a set of indicator variables capturing heterogeneity by either mayoral tenure at the time of the assassination or party affiliation, excluding the benchmark group. Mayors serving less than 12 months and those from MORENA are used as benchmark groups for tenure and party affiliation, respectively. As before, the regression sample is restricted to municipalities that experienced any assassination attempts. All other variables retain their definitions from Section 4.

The results indicate that the negative effects of successful assassinations are larger in municipalities that lose mayors with longer tenure and in those governed by parties with weaker political networks. As reported in Table 7, declines in tax collection and shifts in expenditure toward public investment are larger in municipalities where mayors have served between 24-35 months in office at the time of assassination. The negative effects of successful assassinations are attenuated in municipalities governed by PAN, PRI, and PRD parties, while those represented by minority parties experience substantially larger impacts across most outcome variables (Table 8). The findings are robust to the exclusion of covariates. Taken together, these findings highlight that the magnitude of local fiscal disruptions following assassinations varies with the institutional embeddedness of mayors and, to a lesser extent, with the political parties. This further complements the interpretation that institutional disruption following the sudden absence of mayors plays a key role.

6.3 Adjustments in municipality personnel

In addition to the aforementioned channels, I test whether adjustments in municipal personnel following successful assassinations represent one margin through which leadership disruptions contribute to the erosion of local state capacity over time. The loss of mayors can disrupt internal organization, reduce the desirability of municipal employment – particularly for the more productive workers with better outside options – and lead to task reallocation toward public security (See Appendix Section C.1 for theoretical framework). I first investigate whether more productive workers, proxied by age and educational attainment, are more likely to leave following successful assassinations and calculate the cost of retaining them. Earnings data from the ENOE show that workers in their thirties and forties earn higher wages in the private sector (Appendix Figure C1). Second, I assess whether workers are reallocated from general public service delivery to public security tasks.

I estimate the changes in the share of workers in each age, education level, and task category as an outcome variable for Equation (2). The specification is adjusted to accommodate the biennial frequency and shorter time span of the data by reducing the number of lags and leads..[41,42] For workers across different age groups, the hypothetical cost of retention is calculated based on the labor supply elasticity from Dal Bó et al. (2013), with further details specified in Appendix Section C.2.

There are suggestive signs of a gradual decline in the share of younger and more productive workers in treated municipalities, as shown in Appendix Figures C2 - C3 and Appendix Tables C2-C6. Over time, the share of workers in their 30s and 40s drops by approximately 13 percentage points, corresponding to roughly 23% of the pre-assassination average. In contrast, there are minimal changes in the composition in terms of educational attainment. Notably, these personnel adjustments do not materialize immediately following successful assassinations, unlike the main fiscal capacity outcomes in Section 5. Thus, this gradual shift in worker composition is unlikely to explain the immediate impacts on local fiscal capacity. Instead, these adjustments suggest a possible erosion of the administrative capacity of the affected municipalities in the long run.

Treated municipalities also exhibit suggestive evidence of task reallocation from public service

41. The educational attainment data for municipal workers are only available from the 4th wave of the CNGMD, further limiting the statistical power.

42. I also re-estimate the main event study results in Section 5 to the post-2010 sample to match the availability of the CNGMD data. Point estimates for tax revenue and share of non-infrastrcutural services are generally consistent in sign with the full-sample results, though attenuated in magnitude and less precisely estimated (Appendix Figure E7)

toward public security efforts over time (Appendix Figure C4). The share of workers in public service tasks falls by roughly 6 percentage points, albeit less precise. While the share of *total municipal workers* in public security tasks remains unchanged, the proportion of *public security workers* engaged in operative duties increases by 15 percentage points. This suggests a shift from supporting roles to frontline responsibilities. These changes in worker composition across tasks may contribute to lower capacity for essential services in affected local governments, similar to Akhtari et al. (2022).

6.4 Security environment after assassinations

To assess whether the decline in local fiscal capacity reflects broader changes in the security responses rather than mayoral absence, this subsection examines changes in the presence of organized criminal groups, the expenditure on public security, and the deployment of federal military forces. I measure criminal presence with the log of the number of criminal groups and dummies for new and multiple criminal groups in the municipality. Public security spending is captured by the share of municipal expenditures devoted to security-related functions. Finally, I examine whether higher-level governments assume a more direct role in local security using a binary indicator for the presence of their operations, drawn from Flores-Macías and Zarkin (2024).

Across different outcomes, there is limited evidence of systematic differences in the broader security responses across the treated and near-miss municipalities following successful assassinations. Based on Appendix Figure D2 Table D2, criminal group presence increases temporarily in the year of assassinations but dissipates over subsequent years, and these patterns are robust across specifications and Holm adjustments.[43] The evidence on the difference in public security expenditure and federal security presence is also limited. Furthermore, these outcomes do not display changes of similar magnitude or timing to the results reported in Section 5. Although non-monetary security commitments may explain sustained fiscal effects (to be tested in the next section), the lack of differences in monetary resource allocations and criminal group presence reduces the plausibility of a persistent reallocation of resources toward security as an explanation for the fiscal effects.

Taken together, these patterns suggest that changes in the monetary public security responses and organized criminal group presence are unlikely to be the primary drivers of the main results. Although criminal presence increases temporarily, the absence of sustained differences in criminal

43. In addition, the average number of these groups in treated municipalities does not exceed those in near-miss municipalities in post-assassination periods (Appendix Table D1 and Figure D1)

activity or formal security expenditures makes it unlikely that a persistent expansion of security commitments is driving the long-run fiscal effects.

6.5 Ruling out alternative channels

This section evaluates alternative explanations for the effects of successful assassinations. I test whether economic activity, crime, demographics, and electoral environments confound the results. If treated municipalities have higher rates of non-political violence, the estimated impacts may merely reflect broader insecurity rather than the absence of mayors. Likewise, declines in economic activities and population could independently reduce tax collection and the supply of local government workers (Velásquez 2020). Shifts in electoral competitiveness and voter interest could affect the capability of incoming mayors. Thus, it is necessary to rule out these potential confounders.

I use the following outcomes for this exercise. Non-political violence is proxied by homicide rates (2005 - 2021) and robberies (2011 - 2021) per 100,000 people, with the former recalculated to exclude assassinated mayors. Economic activity is captured by the log of municipal nightlight intensity and individual-level outcomes from the ENOE survey. Demographic variables include population density, the log of the working-age population (1995 and onward), outmigration to the United States from each municipality (2008 - 2021), and internal outmigration to different regions in Mexico obtained from the Mexican Census (1995 and onward, every 5 years).[44] Last, I measure the electoral dynamics with the number of candidates, electoral outcomes of incumbents and runner-ups, margin of victory, participation rates, and vote shares of candidates from PAN, PRD, PRI, and MORENA parties.

The results in Appendix Figure D4- D6 and Appendix Tables D3 - D4 show that these indicators are largely similar across treated and near-miss municipalities following assassinations. Crime rates remain stable across the two types of municipalities. Nightlight intensities and outcomes from the ENOE survey do not show any significant changes. While population density estimates are negative, these estimates are imprecise and do not survive multiple hypothesis adjustments. Furthermore, patterns of outmigration to both the United States and other Mexican municipalities are largely unchanged. Finally, there are no detectable shifts in electoral competitiveness, participation rates, and

44. The total population includes those aged below 15 and above 65 who are less likely than those aged 15-64 to participate in the local economy. As such, I use this group in this exercise.

vote shares by parties following successful assassinations.[45] These findings are consistent across different specifications and multiple hypothesis testing corrections. Therefore, these findings suggest that broader changes in crime, economic activity, migration, or electoral dynamics are unlikely to account for the fiscal effects documented above.

6.6 Takeaway: Interpreting plausibility of different channels

The findings in this section reinforce the interpretation that the institutional disruption from the sudden absence of mayors plays a central role in the ensuing decline in local fiscal capacity, rather than the violent act itself. These effects are more pronounced for municipalities governed by mayors who are more embedded in internal organizations and affiliated with minority parties with weaker political networks. While some adjustments in municipal personnel composition and tasks are observed, these materialize gradually and do not align in timing with the fiscal declines. Lastly, changes in underlying security environments, non-political violence, economic activity, demographic shifts, and electoral dynamics are unlikely to account for the observed patterns.

7 Conclusion

This paper investigates how losing mayors to successful assassinations affects local governments' ability to conduct core bureaucratic functions. Using variation in the presence of mayors induced by the successful and failed assassination attempts, I find that municipalities with assassinated mayors face difficulties in sustaining their revenue stream and delivering public services. Various evidence points to the important role that institutional disruption following the sudden absence of mayors plays, rather than the violent act itself. Other channels — including changes in security expenditures, municipal personnel composition, non-political violence, demographics, economic activity, and electoral dynamics — play at best limited roles.

These findings highlight the broader institutional consequences of political violence and the critical role of local political leadership. Beyond electoral outcomes, political violence can stifle core bureaucratic functions such as tax collection and public goods provision (Daniele 2019; Jones and

45. While there are short-run fluctuations in individual party vote shares, the absence of long-run electoral shifts contrasts with settings where violence directed at the general population increases support for hardline parties in Israel and Colombia (Elster 2019; Morales 2021). In this context, assassinations target specific political figures rather than the general population, and levels of violence against the general population do not differ across treated and control groups.

Olken 2009). The results also suggest that organizational disruptions following the sudden vacuum in leadership play a central role in the observed outcomes. This complements recent work documenting the role of individuals in shaping the institutional effectiveness of public organizations (Akhtari et al. 2022; Best et al. 2023).

These insights have important policy implications. Assassinations not only remove political figures but also undermine the bureaucratic basis of the local government (Dal Bó and Di Tella 2003; Daniele and Dipoppa 2017). Such disruption poses a challenge to local development and calls for remedies that protect the continuity of local governance. This is particularly relevant for countries suffering from internal conflicts and infiltration by illegitimate actors (Blattman and Miguel 2010).

There are further avenues for research on this topic. Future research could explore how political violence interacts with decentralization. While granting local autonomy promises responsiveness and efficiency, my results highlight its vulnerabilities in violent environments with inadequate central oversight (Bardhan 2002). Advances in text analysis and geospatial data now enable more detailed measurement of local state capacity and the presence of illegitimate actors. These tools open the door for a more detailed study of institutional fragility and resilience.

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Tables

Table 1: Determinants of assassinations on mayors in a given year, since 1995

	All of Mexico (Coeff × 100)					Assassination and Near-miss				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Exclude unidentified groups										
log(# groups + 1)	0.208**			0.050	0.044	0.015			-0.005	-0.006
	(0.089)			(0.097)	(0.097)	(0.010)			(0.013)	(0.013)
I(New group)		0.336***		0.297**	0.331**		0.033**		0.037**	0.041**
		(0.120)		(0.140)	(0.140)		(0.013)		(0.017)	(0.018)
Homicide per million	0.006	0.005	0.006	0.005		0.001	0.000	0.001	0.000	
	(0.013)	(0.013)	(0.013)	(0.013)		(0.001)	(0.001)	(0.001)	(0.001)	
Panel B. Include unidentified groups										
log(# groups + 1)	0.329***			0.191**	0.196**	0.035***			0.012	0.013
	(0.075)			(0.075)	(0.074)	(0.009)			(0.011)	(0.011)
I(New group)		0.398***		0.296***	0.311***		0.049***		0.042***	0.044***
		(0.107)		(0.110)	(0.110)		(0.013)		(0.015)	(0.015)
Homicide per million	0.005	0.005	0.006	0.005		0.000	0.000	0.001	0.000	
	(0.013)	(0.013)	(0.013)	(0.013)		(0.001)	(0.001)	(0.001)	(0.002)	
N	59272	59272	59272	59272	60720	3153	3153	3153	3153	3165
Municipalities	2198	2198	2198	2441	2198	117	117	117	117	119
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$

The table shows the coefficient estimates from the regression of the incidence of assassinations on mayors on variables relevant to gang presence and crime at the municipality-year level. For the sample using all of Mexico, coefficients are multiplied by 100 for convenience. The homicide per million is recalculated by excluding cases of mayor assassinations. All regressions include municipality, year fixed effects, and controls. Control variables included are the average schooling of the municipal population, the share of the indigenous population, the log of the total population, and the year since the election (level and squared). log(# group + 1) is the log of the number of criminal groups in the municipality, adjusted by adding 1 to account for municipalities with no presence of organized criminal groups. New group refers to the dummy variable for the existence of a criminal organization that newly began its activities within the municipalities. Standard errors are clustered at the municipal level.

Table 2: Balance table for covariates

	(1) Near-miss			(2) Assassination			(2)-(1) Test for difference		
Variable	N	Mean	(SE)	N	Mean	(SE)	N	Difference	[p-value]
Panel A. Municipality level control variables									
Total homicides	31	11.6	(28.8)	81	6.21	(15.3)	112	-5.44	[0.317]
log(Total homicides)	31	1.2	(1.45)	81	1	(1.22)	112	-0.197	[0.501]
Homicides per 100k	31	12.8	(24.8)	81	34.6	(156)	112	21.8	[0.226]
Tenure at attack (mths)	32	19.7	(14.4)	82	20.4	(13.3)	114	0.671	[0.819]
Avg Schooling	31	7.8	(1.52)	81	6.41	(1.44)	112	-1.39***	[0.000]
Share of indigenous pop.	31	11.7	(21.7)	81	17.8	(25.8)	112	6.13	[0.205]
Pop density	32	222	(515)	82	198	(916)	114	-24.3	[0.858]
# identified crime groups	32	0.531	(1.02)	82	0.524	(1.03)	114	-0.007	[0.974]
log(# identified crime groups)	32	0.283	(0.491)	82	0.278	(0.484)	114	-0.005	[0.955]
I(New Group)	32	0.156	(0.369)	82	0.146	(0.356)	114	-0.010	[0.896]
# New groups	32	0.250	(0.672)	82	0.244	(0.730)	114	-0.006	[0.966]
Panel B. Organized criminal groups									
Beltran Leyva	32	0.031	(0.177)	82	0	(0)	114	-0.031	[0.316]
CJNG	32	0.063	(0.246)	82	0.037	(0.19)	114	-0.026	[0.590]
Huachicoleros	32	0.031	(0.177)	82	0.024	(0.155)	114	-0.007	[0.847]
Barbies	32	0	(0)	82	0.061	(0.241)	114	0.061	[0.024]
Familia	32	0.094	(0.296)	82	0.073	(0.262)	114	-0.021	[0.730]
Gulf Cartel	32	0.063	(0.246)	82	0.085	(0.281)	114	0.023	[0.668]
Juarez Cartel	32	0.031	(0.177)	82	0.024	(0.155)	114	-0.007	[0.847]
Sinaloa Cartel	32	0.063	(0.246)	82	0.073	(0.262)	114	0.011	[0.838]
Tijuana Cartel	32	0.031	(0.177)	82	0.037	(0.19)	114	0.005	[0.887]
Zetas	32	0.125	(0.336)	82	0.073	(0.262)	114	-0.052	[0.432]
Other Cartels	32	0	(0)	82	0.037	(0.19)	114	0.037*	[0.083]
Panel C. Political affiliation of mayors									
Partido Acción Nacional	32	0.125	(0.336)	82	0.171	(0.379)	114	0.046	[0.529]
Partido de la Revolucion Democrática	32	0.219	(0.42)	82	0.146	(0.356)	114	-0.072	[0.388]
Partido Revolucionario Institucional	32	0.344	(0.483)	82	0.390	(0.491)	114	0.047	[0.645]
Movimiento Regeneración Nacional	32	0.125	(0.336)	82	0.049	(0.217)	114	-0.076	[0.234]
Movimiento Ciudadano	32	0	(0)	82	0.061	(0.241)	114	0.061**	[0.024]
Partido Nueva Alianza	32	0.031	(0.177)	82	0	(0)	114	-0.031	[0.316]
Partido del Trabajo	32	0.063	(0.246)	82	0.024	(0.156)	114	-0.038	[0.414]
Partido Verde Ecologista de México	32	0.063	(0.246)	82	0.024	(0.156)	114	-0.038	[0.414]
Uso y Costumbres	32	0	(0)	82	0.110	(0.315)	114	0.110***	[0.002]

***<0.01, **<0.05, *<0.1
Variables in Panels A and B are based on the reported values from the year before the failed/successful assassinations. Party affiliations in Panel C are calculated based on the year of the failed attacks/successful assassinations. Robust standard errors are reported in parentheses, along with the p-value for the test of differences of group means in brackets.

Table 3: Pretrends for outcome variables

	Control mean	Pre-event difference	(SE)	[p-value]
Panel A. Fiscal capacity variables				
log(tax)	15.7	-0.111*	(0.063)	[0.080]
tax per capita	378	-37.7	(34.4)	[0.276]
log(property tax)	15.3	-0.051	(0.058)	[0.377]
property tax per capita	238	-17.6	(17.9)	[0.327]
log(nonearmarked funds)	17.3	-0.026	(0.033)	[0.439]
log(earmarked funds)	17.5	-0.016	(0.052)	[0.767]
log(service)	14	-0.210	(0.157)	[0.184]
log(legal)	15.4	0.033	(0.084)	[0.696]
log(public investments)	17.6	-0.056	(0.097)	[0.561]
% public investments	0.313	0.001	(0.014)	[0.947]
log(non-infra service)	16.2	-0.225	(0.174)	[0.199]
% non-infra service	0.081	-0.002	(0.005)	[0.682]
log(transfers)	15.9	-0.070	(0.090)	[0.439]
% transfers	0.077	-0.0003	(0.006)	[0.961]
Panel B. Mechanism outcome variables				
log(all criminal groups + 1)	0.617	0.022	(0.028)	[0.445]
New criminal groups	0.180	-0.005	(0.014)	[0.752]
I(Multiple criminal groups)	0.141	-0.008	(0.014)	[0.589]
% Security expenditures	0.009	-0.003	(0.003)	[0.303]
% Security personnel expenditures	0.008	-0.002	(0.003)	[0.374]
Federal police presence	0.451	-0.0004	(0.035)	[0.991]
% Personnel, ages 20-29	0.215	-0.008	(0.029)	[0.771]
% Personnel, ages 30-39	0.293	-0.002	(0.037)	[0.957]
% Personnel, ages 40-49	0.220	-0.009	(0.025)	[0.711]
% Personnel, ages 50-	0.156	0.049**	(0.023)	[0.036]
% Personnel, public service	1.070	-0.107	(0.334)	[0.748]
% Personnel, public security	0.211	-0.071	(0.050)	[0.161]
% Security Personnel, operative	0.787	0.011	(0.058)	[0.852]
% Security Personnel, admin	0.080	-0.013	(0.032)	[0.688]
Homicides per 100k, w/o mayors	11.90	-1.290	(1.440)	[0.372]
Robberies per 100k	249	-5.55	(17.90)	[0.757]
log(intensity)	2.090	-0.001	(0.011)	[0.956]
log(population, age 15-64)	10.20	-0.038	(0.031)	[0.219]
population density	213	-3.81	(12.80)	[0.767]
% outmigrated population	0.014	0.0001	(0.001)	[0.993]

***<0.01, **<0.05, *<0.1
The control mean is obtained by averaging the variables over the near-miss municipalities for the 6 years before the failed attempts. Pre-event difference is obtained by regressing the outcome variables using Equation (2) and taking averages of the time indicators up to 6 years before the successful assassinations. Standard errors are clustered at the municipality level and p-values from testing the statistical significance of the pre-event differences are reported in the brackets.

Table 4: Changes in municipal fiscal capacity, 6-year post-event window across specifications

	Taxes				Non-taxes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation	ln (tax)	tax pc	ln (prop.)	prop. pc	ln (non-mark)	ln (mark)	ln (serv.)	ln (legal)
TWFE, no covariates	-0.294***	-80.6	-0.213**	-41.0	-0.110**	-0.017	0.064	-0.101
	(0.105)	(54.0)	(0.104)	(30.5)	(0.053)	(0.080)	(0.127)	(0.224)
Holm p-value	[0.024]	[0.276]	[0.129]	[0.276]	[0.164]	[0.999]	[0.999]	[0.999]
N	2641	2632	2385	2376	2442	2215	2647	2517
Municipalities	115	115	115	115	115	115	115	115
TWFE, with covariates	-0.280***	-83.5	-0.188*	-39.1	-0.122**	0.017	0.086	-0.107
	(0.101)	(53.4)	(0.100)	(28.6)	(0.054)	(0.082)	(0.126)	(0.228)
Holm p-value	[0.028]	[0.242]	[0.192]	[0.242]	[0.104]	[0.999]	[0.999]	[0.999]
N	2593	2593	2338	2338	2403	2182	2599	2470
Municipalities	113	113	113	113	113	113	113	113
TWFE, with covariates	-0.272***	-105**	-0.181*	-51.8*	-0.127**	0.039	0.118	-0.073
(no omitted municipalities)	(0.098)	(51.7)	(0.096)	(28.0)	(0.053)	(0.078)	(0.123)	(0.220)
Holm p-value	[0.024]	[0.132]	[0.132]	[0.132]	[0.076]	[0.999]	[0.999]	[0.999]
N	2701	2701	2441	2441	2502	2274	2706	2578
Municipalities	117	117	117	117	117	117	117	117
Stacked DID, no covariates	-0.228**	-74.4*	-0.121	-39.0*	-0.096***	-0.045	-0.008	-0.248
	(0.098)	(44.2)	(0.095)	(22.4)	(0.037)	(0.049)	(0.116)	(0.163)
Holm p-value	[0.080]	[0.246]	[0.246]	[0.246]	[0.040]	[0.720]	[0.947]	[0.384]
N	22740	21870	20905	20135	16872	15397	22652	22212
Clusters	748	748	748	748	748	748	747	748
Gardner (2024), no covariates	-0.255***	-67.9*	-0.205**	-37.0*	-0.100**	-0.004	0.059	-0.035
	(0.085)	(37.5)	(0.083)	(21.6)	(0.040)	(0.060)	(0.106)	(0.189)
Holm p-value	[0.012]	[0.140]	[0.039]	[0.140]	[0.048]	[0.999]	[0.999]	[0.999]
N	2641	2632	2385	2376	2442	2215	2647	2517
Municipalities	115	115	115	115	115	115	115	115
Sun-Abraham (2021), no covariates	-0.283***	-78.6	-0.209**	-41.1	-0.112**	-0.017	0.075	-0.086
	(0.106)	(56.3)	(0.104)	(32.2)	(0.055)	(0.084)	(0.131)	(0.219)
Holm p-value	[0.032]	[0.324]	[0.135]	[0.324]	[0.164]	[0.999]	[0.999]	[0.999]
N	2641	2632	2385	2376	2442	2215	2647	2517
Municipalities	115	115	115	115	115	115	115	115
Control mean	15.771	369.454	15.461	243.94	17.711	17.688	15.459	14.243
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$ based on p-values not adjusted for multiple hypotheses testing.
The table reports the average of the 6-year post-assassination indicators in Equation (2). Each row contains results from different estimation methods. The outcome variables used in each regression are the log and per capita total tax revenue, log and per capita property tax, log(non-earmarked grants), log(earmarked grants), log(service revenues), and log(revenues from legal affairs). Control mean reports the average of the outcome variables for the near-miss municipalities one year before the assassination attempts. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression also includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are reported in parenthesis and clustered at the municipality level. Holm-adjusted p-values for multiple hypothesis testing are reported in brackets. Adjustment is performed separately for tax outcomes (four variables) and other revenue outcomes (four variables). Results are qualitatively similar if Bonferroni adjustments are applied.

Table 5: Changes in municipal expenditure post assassinations, log of expenditures

Estimation	(1) Investment (share)	(2) Investment (log)	(3) Non-infra (share)	(4) Non-infra. (log)	(5) Allowances (share)	(6) Allowances (log)
TWFE, no covariates	0.055***	0.266**	-0.014**	-0.410*	-0.019*	-0.425***
	(0.018)	(0.120)	(0.006)	(0.244)	(0.010)	(0.143)
Holm p-value	[0.042]	[0.219]	[0.042]	[0.064]	[0.283]	[0.036]
N	2694	2694	2555	2555	2701	2701
Municipalities	115	115	115	115	115	115
TWFE, with covariates	0.053***	0.218*	-0.016**	-0.489*	-0.010	-0.360**
	(0.018)	(0.120)	(0.006)	(0.249)	(0.009)	(0.145)
Holm p-value	[0.012]	[0.104]	[0.028]	[0.104]	[0.268]	[0.045]
N	2649	2649	2510	2510	2653	2653
Municipalities	113	113	113	113	113	113
TWFE, with covariates	0.056***	0.264**	-0.014**	-0.440*	-0.018*	-0.418**
(no omitted municipalities)	(0.018)	(0.119)	(0.006)	(0.243)	(0.010)	(0.142)
Holm p-value	[0.006]	[0.056]	[0.044]	[0.073]	[0.059]	[0.012]
N	2756	2756	2612	2612	2761	2761
Municipalities	117	117	117	117	117	117
Stacked DID, no covariates	0.053***	0.168*	-0.014**	-0.532**	-0.021***	-0.407***
	(0.013)	(0.095)	(0.006)	(0.234)	(0.006)	(0.093)
Holm p-value	[0.001]	[0.077]	[0.010]	[0.046]	[0.002]	[0.001]
N	22777	22777	20627	20627	22513	22513
Clusters	748	748	748	748	748	748
Gardner (2024), no covariates	0.053***	0.214*	-0.015**	-0.395**	-0.010	-0.309***
	(0.017)	(0.113)	(0.006)	(0.168)	(0.008)	(0.115)
Holm p-value	[0.006]	[0.059]	[0.016]	[0.038]	[0.202]	[0.021]
N	2694	2694	2555	2555	2701	2701
Municipalities	115	115	115	115	115	115
Sun-Abraham (2021), no covariates	0.050**	0.166	-0.016**	-0.534**	-0.010	-0.359**
	(0.020)	(0.138)	(0.007)	(0.247)	(0.009)	(0.145)
Holm p-value	[0.039]	[0.230]	[0.039]	[0.062]	[0.310]	[0.039]
N	2694	2694	2555	2555	2701	2701
Municipalities	115	115	115	115	115	115
Control mean	0.308	17.655	0.079	16.291	0.081	16.171
Municipality FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$ based on p-values not adjusted for multiple hypotheses testing.
The table reports the average of the 6-year post-assassination indicators in Equation (2). Each row contains results from different estimation methods. The outcome variables used in each regression are the shares and logs of investment in construction projets, general services expenditure not part of basic infrastructure spending, and allowances and transfers to municipal entities responsible for public service. Control mean reports the average of the outcome variables for the near-miss municipalities one year before the assassination attempts. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are reported in parenthesis and clustered at the municipality level.Holm-adjusted p-values for multiple hypothesis testing are reported in brackets. Adjustment is performed separately for share outcomes (three variables) and log outcomes (three variables). Results are qualitatively similar if Bonferroni adjustments are applied.

Table 6: Difference across assassinations, failed attempts, and nonviolent deaths

	(1) Tax (log)	(2) Tax (per capita)	(3) Invstment (log)	(4) Invstment (share)	(5) Non-infra. (log)	(6) Non-infra. (share)
Panel A. Triple difference-in-difference results						
Post	0.018	101.167*	-0.397***	-0.048**	0.720**	0.030***
	(0.133)	(59.435)	(0.145)	(0.021)	(0.308)	(0.009)
Post × Violent injury	-0.273	-154.069**	0.593**	0.088**	-1.134**	-0.022*
	(0.172)	(65.443)	(0.265)	(0.037)	(0.466)	(0.013)
Post × Death	-0.109	-107.201	0.219	0.029	-1.057**	-0.029*
	(0.187)	(65.216)	(0.188)	(0.027)	(0.417)	(0.015)
Post × Violent Injury × Death	0.086	141.984*	-0.292	-0.030	0.987*	0.018
	(0.236)	(73.848)	(0.312)	(0.044)	(0.573)	(0.018)
Panel B. Differences in the changes in outcome variables across different categories of municipalities						
Killed - Nonviolent death	-0.187	-12.09	0.301 *	0.058**	-0.147	-0.005
	(0.162)	(33.40)	(0.164)	(0.024)	(0.335)	(0.013)
Killed - Injury	-0.024	34.78	-0.073	0.001	-0.069	-0.011
	(0.143)	(34.58)	(0.250)	(0.035)	(0.392)	(0.010)
Killed - Unhurt	-0.296*	-119.29*	0.521***	0.087***	-1.203***	-0.033***
	(0.152)	(66.52)	(0.177)	(0.024)	(0.355)	(0.010)
Observations.	4911	3062	5066	5066	4484	4484
Municipalities	172	170	172	172	172	172
Municipality FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Covariates						

* $p < .10$, ** $p < .05$, *** $p < .01$

The table reports the triple difference-in-difference equation involving municipalities with successful assassinations, failed attempts that lead to injuries, failed attempts that did not lead to an injury, and non-violent deaths (diseases, accidents). Post is an indicator of an event (attacks or nonviolent deaths) taking place on or before that year. Violent injury indicates that a mayor in that municipality is hurt or killed as a result of violence and equals one if a municipality experiences assassinations or failed attacks that lead to injuries. Death equals 1 if a municipality loses a mayor to assassinations or nonviolent deaths at some point. Outcome variables are listed at the top of each column. Shares in columns (3) and (5) are measured relative to total expenditure. Panel A reports the coefficients and standard errors, clustered at the municipality level, from the triple difference-in-difference equation. Panel B reports the differences in changes in outcome variables following an event using linear combinations of the coefficients and their standard errors. The regression includes municipality fixed effects, year fixed effects. Covariates are not included. As indicators for violent injury and deaths are defined at the municipality level (not municipality-year), the fixed effects included in the regression absorbs these effects and thus are not included in the table.

Table 7: Heterogeneity by mayor's year in office at attack

	(1) Tax (log)	(2) Tax (per capita)	(3) Investment (share)	(4) Non-infra (share)	(5) Tax (log)	(6) Tax (per capita)	(7) Investment (share)	(8) Non-infra (share)
Panel A Difference-in-difference results								
Post	-0.320**	-14.573	0.060**	-0.015**	-0.286**	-7.808	0.058***	-0.015**
	(0.139)	(57.143)	(0.024)	(0.006)	(0.129)	(58.561)	(0.022)	(0.006)
Post × $I[12 \leq \text{tenure} < 24]$	0.326*	-55.300	-0.052	0.003	0.283	-64.194	-0.034	0.002
	(0.184)	(55.452)	(0.037)	(0.010)	(0.172)	(60.501)	(0.035)	(0.010)
Post × $I[24 \leq \text{tenure} < 36]$	-0.024	-85.697	-0.001	0.003	-0.060	-93.536*	0.010	0.002
	(0.196)	(52.563)	(0.030)	(0.010)	(0.182)	(54.757)	(0.028)	(0.011)
Post × $I[\text{tenure} \geq 36]$	0.401**	-75.852	-0.016	0.008	0.414*	-96.432	-0.035	0.011*
	(0.199)	(60.740)	(0.041)	(0.009)	(0.230)	(62.317)	(0.030)	(0.006)
Panel B Total effect of successful assassinations across different mayoral tenures								
$12 \leq \text{tenure} < 24$	0.006	-69.87*	0.008	-0.012	-0.003	-71.99*	0.024	-0.013
	(0.127)	(41.50)	(0.031)	(0.009)	(0.125)	(42.20)	(0.029)	(0.009)
$24 \leq \text{tenure} < 36$	-0.344**	-100.2***	0.058***	-0.012	-0.346**	-101.3***	0.068***	-0.013
	(0.135)	(36.70)	(0.022)	(0.0100)	(0.133)	(36.10)	(0.022)	(0.010)
$\text{tenure} \geq 36$	0.081	-90.43*	0.044	-0.007	0.127	-104.3*	0.023	-0.004
	(0.150)	(47.80)	(0.035)	(0.008)	(0.190)	(58.00)	(0.024)	(0.006)
N	2616	2607	2669	2531	2568	2568	2624	2486
Municipalities	114	114	114	114	112	112	112	112
Covariates					✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$

This table reports the estimates of heterogeneous effects of successful assassinations to the main outcomes presented in Section 5 by the length of the mayor's tenure at the time of assassination. The mayoral tenure bins are defined by internvals of 12 months, beginning from month zero. Mayors assassinated within 12 months since taking office are used as a benchmark group. Outcome variables are listed on the top of each columns. Columns (5) - (8) include the same sets of covariates used in Section 5. All specifications include municipality and year fixed effects. Panel B reports the total effect of successful assassinations across mayors with different tenure, obtained by adding Post variable and relevant interaction for each mayoral tenure bins. Standard errors for both panels are clustered at the municipality level.

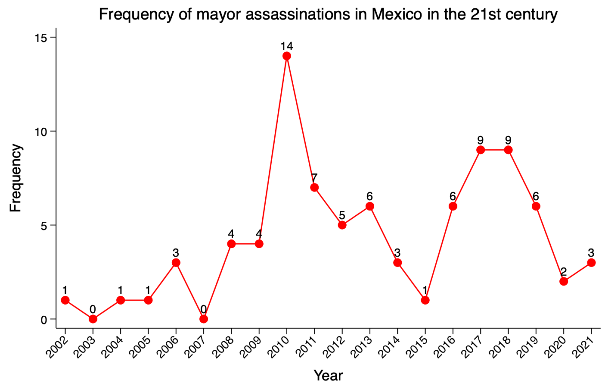
Table 8: Heterogeneity by mayor party affilaition

	(1) Tax (log)	(2) Tax (per capita)	(3) Investment (share)	(4) Non-infra (share)	(5) Tax (log)	(6) Tax (per capita)	(7) Investment (share)	(8) Non-infra (share)
Panel A Difference-in-difference results								
Post	-0.169*	-186.678***	0.063***	-0.042***	-0.044	-192.913***	0.047***	-0.035***
	(0.087)	(37.695)	(0.015)	(0.006)	(0.141)	(55.968)	(0.017)	(0.005)
Post × PAN	-0.025	102.452***	-0.045*	0.034***	-0.115	86.607*	-0.018	0.023***
	(0.160)	(28.547)	(0.027)	(0.006)	(0.214)	(44.803)	(0.032)	(0.007)
Post × PRD	0.048	125.027***	-0.088***	0.033***	0.028	91.740*	-0.070**	0.026***
	(0.217)	(34.180)	(0.030)	(0.005)	(0.210)	(48.677)	(0.034)	(0.007)
Post × PRI	0.027	157.454***	-0.019	0.033***	-0.118	175.573***	0.005	0.023***
	(0.091)	(36.685)	(0.018)	(0.006)	(0.158)	(52.005)	(0.021)	(0.007)
Post × Minority	-0.577	64.059***	0.079**	0.015	-0.675*	84.920**	0.080**	0.011
	(0.354)	(18.076)	(0.035)	(0.017)	(0.354)	(36.093)	(0.033)	(0.017)
Panel B F-test comparing effects on minority-party municipalities vs major parties								
Minority vs PAN	2.27	1.39	8.62	1.25	2.33	0.001	5.18	0.48
	[0.135]	[0.242]	[0.004]	[0.266]	[0.130]	[0.973]	[0.0247]	[0.490]
Minority vs PRD	2.50	2.88	14.7	1.14	3.59	0.02	11.2	0.85
	[0.117]	[0.093]	[0.0002]	[0.288]	[0.061]	[0.890]	[0.001]	[0.359]
Minority vs PRI	3.14	7.10	7.11	1.04	2.75	5.42	4.40	0.515
	[0.079]	[0.009]	[0.009]	[0.310]	[0.100]	[0.022]	[0.038]	[0.474]
N	2641	2632	2694	2555	2593	2593	2649	2510
Municipalities	115	115	115	115	113	113	113	113
Covariates					✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓

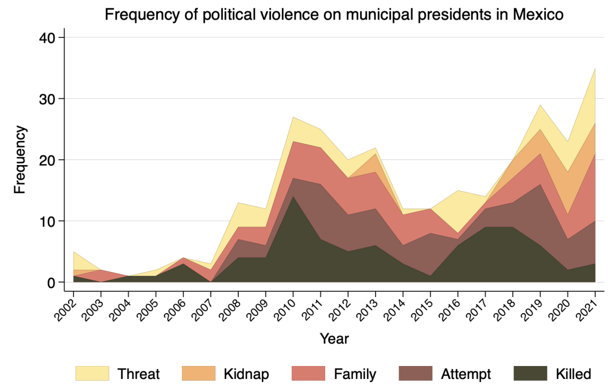
* $p < .10$, ** $p < .05$, *** $p < .01$
This table reports the estimates of heterogeneous effects of successful assassinations to the main outcomes presented in Section 5 by political parties of mayors. Parties analyzed include PAN, PRD, PRI, MORENA, and minority parties, with MORENA being the benchmark group. Outcome variables are listed on the top of each columns. Columns (5) - (8) include the same sets of covariates used in Section 5. All specifications include municipality and year fixed effects. Panel B reports the F-test results comparing effects of successful assassintions in municipalities governed by minority parties against PAN, PRD, PRI parties. Standard errors, in brackets for Panel A, are clustered at the municipality level. p-values for F-tests in Panel B are reported in square brackets.

Figures

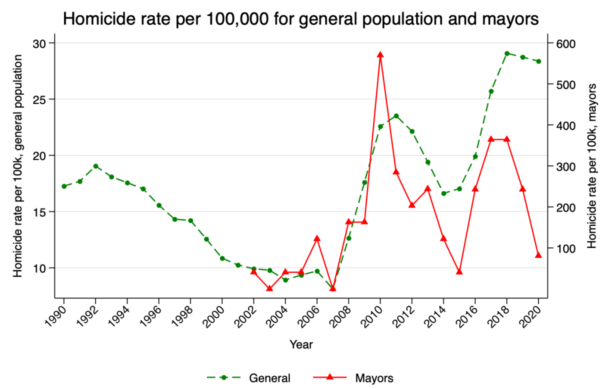
Figure 1: Assassination against mayors, in total numbers and murder rate



(a) Assassination against mayors, 2002-2021



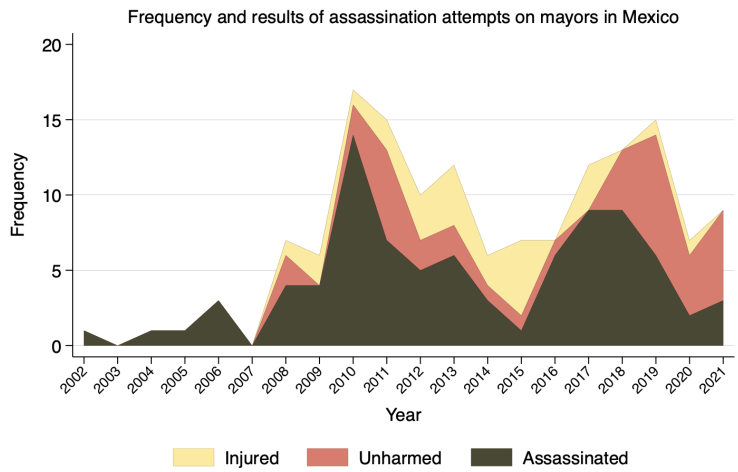
(b) Violence against mayors, 2002-2021



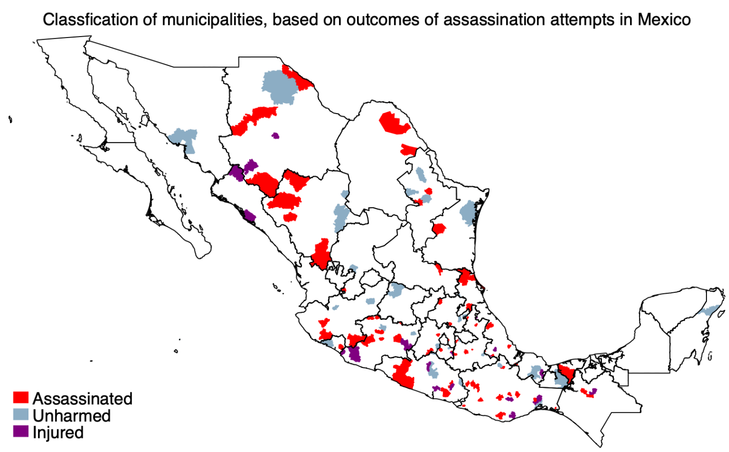
(c) Homicide rate per 100,000, general population and the mayors

Note: Figures above show the variation in the incidence of assassinations and murder rates across different years and municipalities. The figures in the top panel describe the number of assassinations against mayors from 2002-2021, based on the data collected by the author. The figures in the bottom panel present murder rates calculated as homicides per 100,000 people for mayors and all population. The numbers for the general population are represented by the left axis and the green dashed lines. The numbers are obtained from the World Development Indicators in the World Bank. The same for the mayors is displayed on the right axis and in a red solid line. This is calculated by dividing the annual number of mayors assassinated by the total number of municipalities and then multiplying by 100,000.

Figure 2: Temporal and Geographical variation in successful mayor assassinations vs near-misses



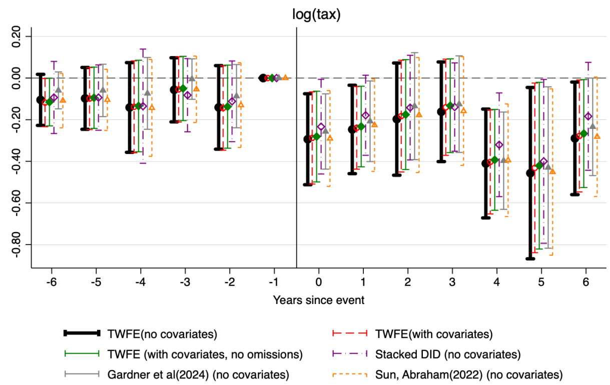
(a) Frequency of successful attacks and near-misses on mayors



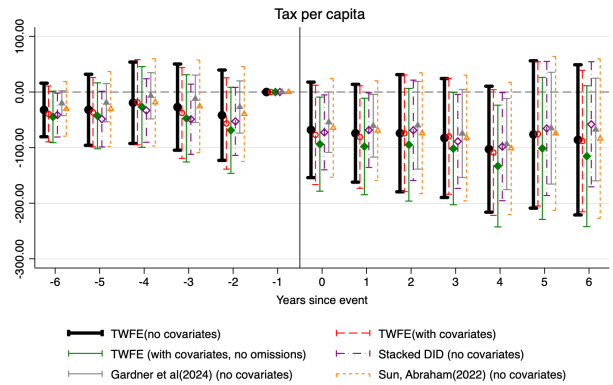
(b) Geographical distribution of the outcome of attacks on mayors

Note: Panel (a) shows the variation of the results of attacks against mayors across time. Categories include successful attacks resulting in the death of a a mayor (treatment), mayors who escaped unharmed (control), and those who were injured, but not killed. Panel (b) shows the results of these attacks at a geographical level. Municipalities in which both failed attacks and successful assassination has occurred is classified as a treatment group and appears as 'Assassinated' on the map. The data used for creating the figures are from various sources and the author's collection is based on the method described in Section 3. A full list of mayors who were victims of the attack and sources are in Appendix A.3.

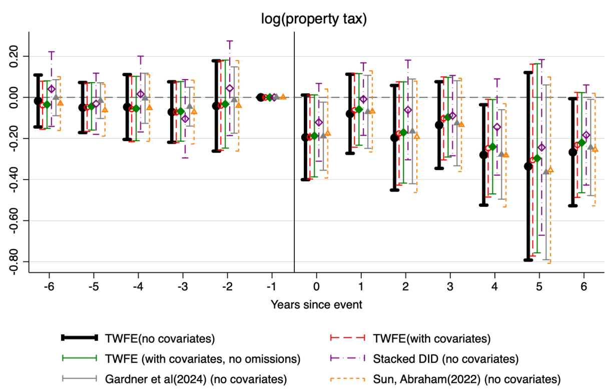
Figure 3: Decreases in tax revenues after assassinations



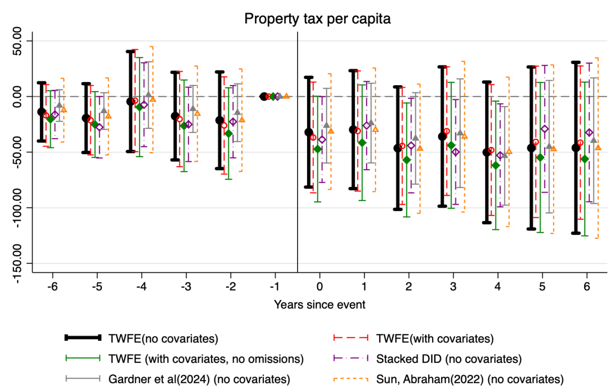
(a) log(Total tax revenue)



(b) Total tax revenue per capita



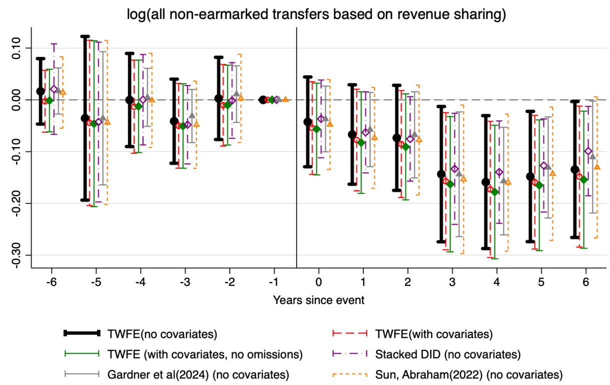
(c) log(Total property tax revenue)



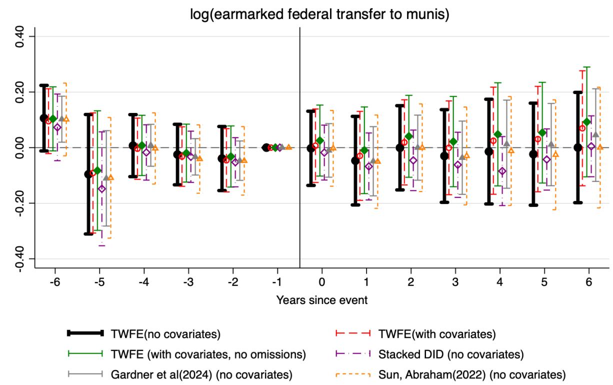
(d) Property tax revenue per capita

Note: The figures report the event study regression on the different measures of tax revenues.The outcome variables used in each regression are listed below each graph. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are clustered at the municipality level.

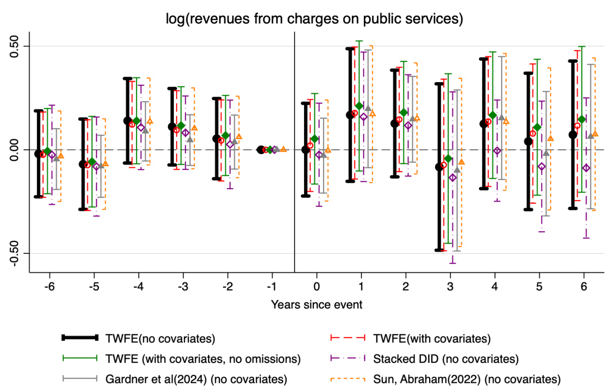
Figure 4: Changes in revenues from other sources for the municipalities



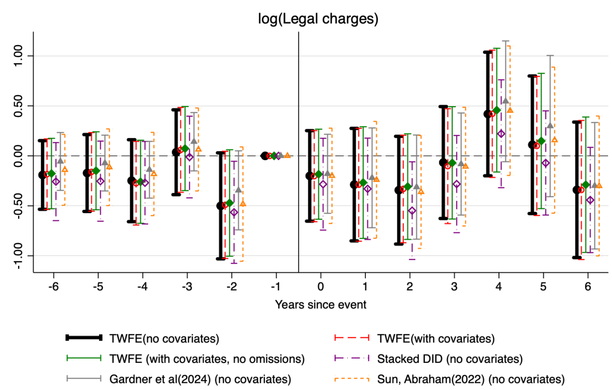
(a) Non-earmarked funds to municipalities



(b) Overall earmarked funds to municipalities



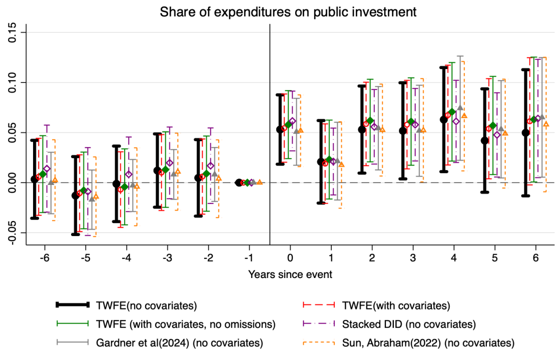
(c) Revenues from charge on public services



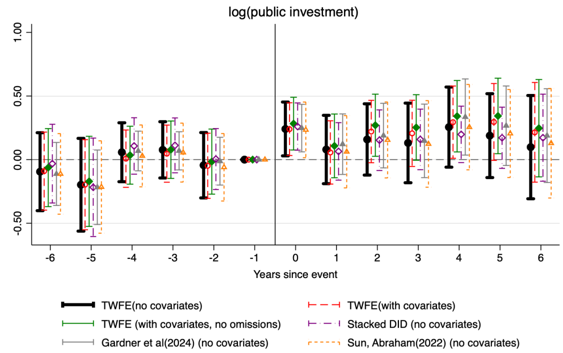
(d) Revenues from legal services

Note: The figures report the event study regression on the different sources of revenues for the municipal government. The outcome variables used in each regression are listed below each graph. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are clustered at the municipality level.

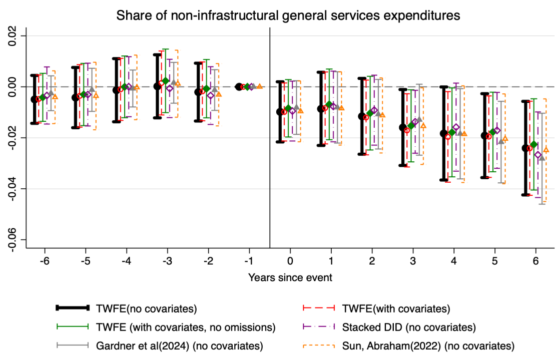
Figure 5: Share and volume of expenditures across different categories



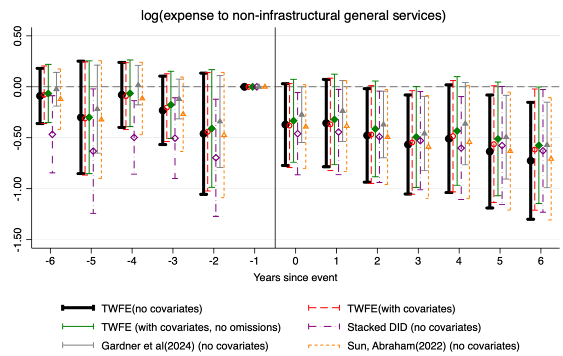
(a) Share of public investments on construction



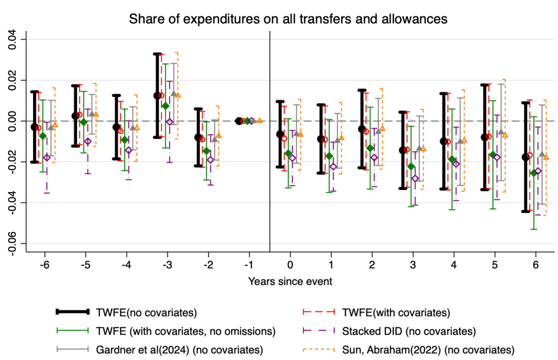
(b) log(Public investments on construction)



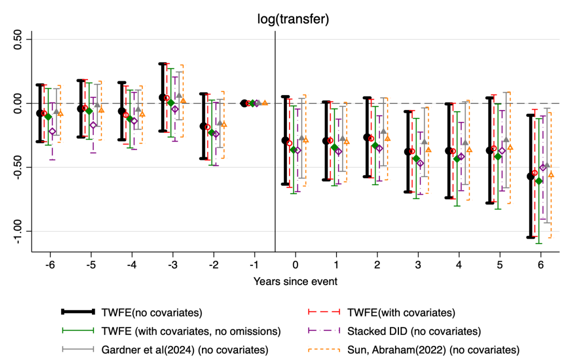
(c) Share of other general operations



(d) log(Other general operations)



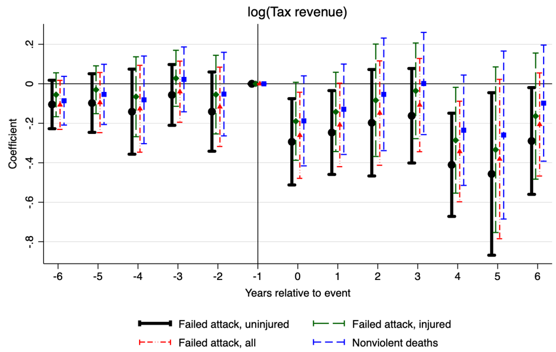
(e) Share of allowances to municipal entities



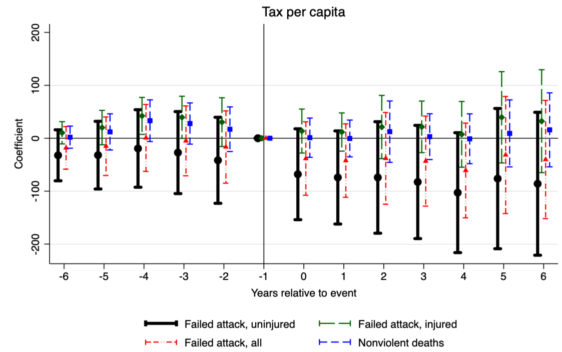
(f) log(Allowances to municipal entities)

Note: The figures report the event study regression on the different measures of expenditures of the municipal government. The outcome variables used in each regression are listed below each graph. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are clustered at the municipality level.

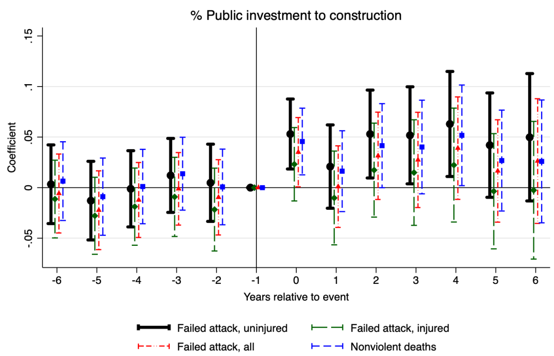
Figure 6: Regression using all other possible control groups



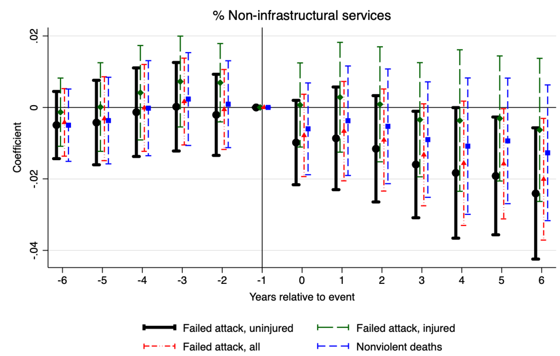
(a) log(tax revenue)



(b) tax per capita



(c) Share of total expenditure on construction



(d) Share of other general service expenditures

Note: The figures report the event study regression using Equation (2) but with different sets of control variables for some of the outcome variables used in Section 5. The outcome variables are specified as a caption for each picture. The control groups reported are 1) the same control group in the main results, 2) municipalities with all failed assassination attempts, with injured and unharmed mayors, 3) only the municipalities with failed attempts that injured the mayors, and 4) municipalities whose mayors passed away for nonviolent reasons. The treatment group, fixed effects, and covariates are identical to the ones used in Section 5. Standard errors are clustered at the municipality level.

Appendix A Further explanation on the background and the data

In this section, I will provide an additional explanation of the details of municipal finance in Mexico, the full procedure of collecting data on mayors who are victims of assassination attempts and the complete list, a further definition of key variables used in the research, and a detailed explanation on the composition of the nightlight dataset.

Subsection A.1 Additional details on municipal finance in Mexico

Municipalities in Mexico shoulder the work of providing key public goods to Mexico. The revenue required comes mainly from three sources - property taxes, non-earmarked funds (*particpaciones*) and earmarked funds (*aportaciones*). Property taxes are purely determined by the tax collection at the municipal level, but they take up only about 15-20% of the municipal revenues (INEGI 2016). Others are from the two funds from the federal government, with the design following the principles of fiscal federalism (Weingast 2009). Earmarked funds are designed to correct for equity, while non-earmarked funds include components that emphasize fiscal incentives and efficiency of subnational governments (Weingast 2009; World Bank 2016).

- **Property taxation**: Municipalities are responsible for collection and keeping records of property owners and values (World Bank 2016). This takes up 70% of the total tax revenues (World Bank 2016; INEGI 2016). However, tax rates are subject to approval from the state legislature (OECD 2016).

- **Non-earmarked funds**: These are composed of General Participation Funds (FGP) and Municipal Development Funds (FFM), as well as transfers from taxes received by the federal government (Timmons and Broid 2013). Each of these categories includes proportions determined by past receipt of the same funds, demographics, and tax revenues generated within municipalities (Timmons and Broid 2013). The state government receives these funds from the federal government, which are then passed on to the municipalities at the discretion of the state.

  The formula for FGP is as follows, according to SEGOB (2011). It is comprised of the fixed part derived from the nominal amount of transfers received from previous fiscal years and the variable part. The fixed prevents states and municipalities from suffering drastic adjustments to this transfer. The variable part provides additional funds to local governments that experience an increase in the statewide GDP per capita, an increase in local tax revenues, and a high level of local taxes. The three components are weighed as follows

  – 60% on increase in yearly statewide GDP per capita, weighted by population

  – 30% on three-year moving average increase in tax collection, weighted by population

  – 10% on the level of local tax collection, weighted by population

  The shocks to tax collection may affect the FGP amount through the changes in the variable part that determines the amount. Once it receives the transfers based on the above formula, the state legislature determines the amount of FGP to each municipality.

- **Earmarked funds**: These include Funds for infrastructural development (FISM) and Funds supporting municipal development (FORTAMUN). The former is conditioned primarily for infrastructural development while the other can be more general in purpose (SEGOB 2011). In both, the amount of funds primarily depends on population and poverty indices (SEGOB 2011; World Bank 2016)

Subsection A.2 Data collection procedure for identifying mayors who are attacked

The collection of the information on mayors who are the victims of successful and failed assassination attempts is based on a semi-automated program written in Python and primarily uses `selenium` package. The `selenium` package is a collection of codes that automate the human interaction with the web interface.[1] Actions that can be performed with this package include clicking links, typing designated phrases, and storing blocks of text. However, for getting through some security features such as two-way authentication, automation is complicated and needs human intervention. Thus, the program I have devised is semi-automated.

The workflow designed in the program is as follows. First, the program accesses the online newspaper archives (*Newsbank* and *ProQuest*) using log-in credentials provided by the school library.[2] In using the school login credentials, I follow the default security settings for the school and use two-way authentication. Then, The program types in key phrases on the search box and filters search

1. Alternatives to scraping texts include `scrapy` and `beautlfulsoup` packages. While they provide better performance in terms of speed, they are also likely to be subject to anti-scraping measures implemented by each website. Thus, I chose `selenium` as the primary package for this program.

2. Access to these online newspaper archives are mostly provided to libraries in many educational institutions in the US and other countries.

results based on newspaper source and date. Afterwards, the program collects the name of the publisher, date, title, and the full text of the article. Finally, I discard the unnecessary articles and categorize assassination attempts into successful and failed ones based on the texts in the article. This last step is not based on `selenium`, but done through reviewing the articles. The following diagram summarizes the process.

The key phrases used for the search are as follows

- Assassinated: *presidente municipal fue asesinado*, and *matan/asesinan/ejecutan a presidente municipal*
- Failed: *presidente municipal fue atacado/atentado* and *atentan/atacan a presidente municipal*
- Kidnapped: *presidente municipal fue secuestrado* and *secuestran a presidente municipal*
- Threats: *presidente municipal fue amenazado* and *amenazan/narcomensaje a presidente municipal*
- Famliy members targeted: Include the terms *esposo/esposa* (husband/wife), *hermano/hermana* (brother/sister), *hijo/hija* (son/daughter), *padre/madre* (father/mother), *primo/prima* (cousins), *tío/tía* (uncle/aunt), and *sobrino/sobrina* (nephews) to the key phrases used above
- Non-violent deaths: *presidente municipal fallecio/murio* and *fallece/muere presidente municipal*

Once the key phrases are entered, the program filters the articles based on the date of publication and source. Specifically, I select the dates up to Dec 31st, 2021 since I do not include cases from the year 2022 and onwards for the analysis due to the lack of data on key variables for this period. In addition, I limit the results to show just the newspaper articles, which rules out other types of sources stored in the online news archives such as books, and scholarly articles on the topic.

After filtering, the program collects information on the publisher, title, date, and text content of the article. The publishers used in this stage include *Reforma*, *El Universal*, *El Norte*, and *El Economista*, among others. The newspaper sources used to identify each case are contained in the list of mayors who are part of the study. Other information is used to identify whether the article is about attacks on mayors, as well as to pinpoint the date and location of the attacks.

Then, I discard the unrelated articles and categorize assassination attempts into successful and failed ones based on the information in the article text. Unrelated articles include all words in the key phrases but are not relevant to attacks on mayors, such as the article about a municipal president criticizing an assassination of other individuals. Based on the manual review and topic categorization using Latent Dirichlet Allocation, I narrow down the collection to relevant articles and determine the type of attacks carried out against a mayor. To distinguish between injured and unharmed mayors, I check for words such as *herido/lesionado/se translado al hopital* (injured) and *sale ileso/ilesa* (unharmed).[3]

Subsection A.3 List of mayors included in the study

The table below is a list of mayors who are included in the study. The list includes information on the names, municipalities, and political parties that they represented at the time of the attack, the date of the attack, and whether this was a successful or failed assassination attempt.

3. Any cases which mention that the mayor was not present at the attacks on the office/residence is categorized as unharmed. Also, I check for similar verbs for female mayors, with *o*'s in the end replaced with *a*'s.

Table A1: List of mayors who were assassinated

	Name	Municipality and state	Date	Sources
1	Jaime Valencia Santiago	San Agustín Loxicha-Oaxaca	2002/01/13	Imparcial Oaxaca, La Jornada, El Universal
2	Mario Sostenes Lozano Camacho	San Sebastián Tecomaxtlahuaca-Oaxaca	2004/07/14	Proceso, Wradio, El Universal
3	Fernando Chavez Lopez	Buenavista-Michoacan	2005/07/09	Esparza et al. (2018), El Universal, La Jornada
4	Neguib Tadeo Manriquez Madriaga	Ciudad Ixtepec-Oaxaca	2006/01/13	Esparza et al. (2018), El Universal, La Jornada
5	Raul Delgado Benavides	Cuautitlán de García Barragán-Jalisco	2006/07/15	Esparza et al. (2018), El Universal, Colima Noticias
6	Walter Herrera Ramirez	Huimanguillo-Tabasco	2006/11/15	Esparza et al. (2018), El Universal, El Heraldo de Tabasco
7	Juan Marcelo Ibarra Villa	Madero-Michoacan	2008/06/01	Esparza et al. (2018), El Universal, La Jornada
8	Manuel Angulo Torres	Topia-Durango	2008/06/03	Esparza et al. (2018), El Universal, Proceso
9	Homero Lorenzo Rios	Ayutla de los Libres-Guerrero	2008/09/25	Esparza et al. (2018), El Universal, La Jornada
10	Salvador Christopher Vergara Cruz	Ixtapan de la Sal-Edomex	2008/10/03	Esparza et al. (2018), El Universal, La Jornada
11	Claudio Reyes Nunez	Otáez-Durango	2009/02/04	Esparza et al. (2018), El Universal, La Jornada
12	Octavio Manuel Carrillo Castellanos	Vista Hermosa-Michoacan	2009/02/24	Esparza et al. (2018), El Universal, Vanguardia
13	Luis Carlos Ramirez Lopez	Ocampo-Durango	2009/06/01	Esparza et al. (2018), El Universal, Vanguardia
14	Hector Ariel Meixueiro Muñoz	Namiquipa-Chihuahua	2009/07/14	Esparza et al. (2018), El Universal, La Jornada
15	Ramon Mendivil Sotelo	Guadalupe y Calvo-Chihuahua	2010/02/17	Esparza et al. (2018), El Universal, Milenio
16	Manuel Estrada Escalante	Mezquital-Durango	2010/02/22	Esparza et al. (2018), El Universal, La Jornada
17	Vidal Olivera Cruz	San Lorenzo Albarradas-Oaxaca	2010/04/01	Esparza et al. (2018), Excelsior, AALMAC
18	Jose Santiago Agustin	Zapotitlán Tablas-Guerrero	2010/04/28	Esparza et al. (2018), El Universal, El Economista
19	Jesus Manuel Lara Rodriguez	Guadalupe-Chihuahua	2010/06/19	Esparza et al. (2018), El Universal, El Mañana
20	Oscar Venancio Martinez Rivera	San José del Progreso-Oaxaca	2010/06/20	Esparza et al. (2018), El Universal, La Jornada
21	Nicolas Garcia Ambrosio	Santo Domingo de Morelos-Oaxaca	2010/06/30	Esparza et al. (2018), El Universal, Expansion
22	Alfonso Pena Pena	Tepehuanes-Durango	2010/07/26	Esparza et al. (2018), El Universal, Expansion
23	Edelmiro Cavazos Leal	Santiago-Nuevo León	2010/08/18	Esparza et al. (2018), El Universal, LA Times
24	Marco Antonio Leal Garcia	Hidalgo-Tamaulipas	2010/08/30	Esparza et al. (2018), El Universal, LA Times
25	Alexander Lopez Garcia	El Naranjo-San Luis Potosí	2010/09/09	Esparza et al. (2018), El Universal, Expansion
26	Prisciliano Rodriguez Salinas	Doctor González-Nuevo León	2010/09/24	Esparza et al. (2018), El Universal, Vanguardia
27	Gustavo Sanchez Cervantes	Tancítaro-Michoacan	2010/09/27	Esparza et al. (2018), El Universal, Informador
28	Jaime Lozoya Avila	San Bernardo-Durango	2010/11/05	Esparza et al. (2018), El Universal, La Jornada
29	Saúl Vara Rivera	Zaragoza-Coahuila	2011/01/05	Esparza et al. (2018), El Universal, Excelsior
30	Abraham Ortiz Rosales	Temoac-Morelos	2011/01/10	Esparza et al. (2018), El Universal, Excelsior

31	Pedro Luis Jiminez Mata	Santiago Amoltepec-Oaxaca	2011/01/13	Esparza et al. (2018), El Universal, Excelsior
32	Saturnino Valdes Llanos	Tampico Alto-Veracruz	2011/02/23	Esparza et al. (2018), El Universal, Expansion
33	Fortino Cortes Sandoval	Benito Juárez-Zacatecas	2011/07/28	Esparza et al. (2018), El Universal, Vanguardia
34	Jose Eduviges Nava Altamirano	Zacualpan-Edomex	2011/08/19	Esparza et al. (2018), El Universal, Expansion
35	Ricardo Guzman Romero	La Piedad-Michoacan	2011/11/03	Esparza et al. (2018), El Universal, El Pais
36	Rafael Landa Fernandez	Atzalan-Veracruz	2012/04/18	El Universal, Alcalorpolitico, Vanguardia
37	Marisol Mora Cuevas	Tlacojalpan-Veracruz	2012/06/29	Esparza et al. (2018), El Universal, La Jornada
38	Pedro Filemon Luis Hernandez	San Miguel Tilquiápam-Oaxaca	2012/08/02	Esparza et al. (2018), El Universal, Libertad Oaxaca
39	Nadin Torralba Mejia	Técpan de Galeana-Guerrero	2012/08/05	Esparza et al. (2018), El Universal, Vanguardia
40	Himeldo Rayon de Jesus	San Juan Juquila Mixes-Oaxaca	2012/08/24	Esparza et al. (2018), El Universal, Diario Despertar de Oaxaca
41	Wilfrido Flores Villa	Nahuatzen-Michoacan	2013/02/04	El Universal, Justice in Mexico, La Jornada
42	Feliciano Martinez Bautista	San Juan Mixtepec Distrito 08-Oaxaca	2013/03/24	Esparza et al. (2018), El Universal, La Jornada
43	Jose Rene Garrido Rocha	San Salvador el Verde-Puebla	2013/04/21	Esparza et al. (2018), El Universal, Ell Siglio de Torreon
44	Celestino Felix Vazquez Luis	San Miguel Tilquiápam-Oaxaca	2013/06/04	Esparza et al. (2018), El Universal, Proceso
45	Geronimo Manuel Garcia Rosas	Aquila-Veracruz	2013/07/23	Esparza et al. (2018), El Universal, La Jornada
46	Ygnacio Lopez Mendoza	Santa Ana Maya-Michoacan	2013/11/07	El Pais, El Universal, Aristegui Noticias
47	Gustavo Garibay Garcia	Tanhuato-Michoacan	2014/03/22	Esparza et al. (2018), El Universal, Justice in Mexico
48	Teodulo Gea Dominguez	Pánuco-Veracruz	2014/07/14	Esparza et al. (2018), El Universal, Alcalorpolitico
49	Manuel Gomez Torres	Ayutla-Jalisco	2014/08/03	Esparza et al. (2018), El Universal, Expansion
50	Mario Sanchez Cuevas	San Miguel el Grande-Oaxaca	2015/10/07	Esparza et al. (2018), El Universal, Presencia
51	Gisela Mota Ocampo	Temixco-Morelos	2016/01/02	Esparza et al. (2018), El Universal, NY Times
52	Juan Antonio Mayen Saucedo	Jilotzingo-Edomex	2016/04/22	Esparza et al. (2018), Aristegui Noticias, Mexico News Daily
53	Domingo López González	Chamula-Chiapas	2016/07/23	Esparza et al. (2018), El Pais, El Financiero
54	Ambrosio Soto Duarte	Pungarabato-Guerrero	2016/07/24	Esparza et al. (2018), El Financiero, The Yucatan Times
55	Jose Santa Maria Zavala	Huehuetlán el Grande-Puebla	2016/08/01	Esparza et al. (2018), Expansion, El Economista
56	Jose Villanueva Rodriguez	Ocotlán de Morelos-Oaxaca	2016/12/17	Esparza et al. (2018), AALMAC, El Imparcial
57	Antolin Vidal Martinez	Tepexco-Puebla	2017/01/24	Esparza et al. (2018), La Jornada, El Mineral
58	Alejandro Hernandez Santos	San Bartolomé Loxicha-Oaxaca	2017/04/28	Esparza et al. (2018), Imagen del Golfo, Proceso
59	Stalin Sanchez Gonzalez	Paracho-Michoacan	2017/10/06	Esparza et al. (2018), El Financiero, El Universal
60	Manuel Hernandez Pasion	Huitzilan de Serdán-Puebla	2017/10/10	Esparza et al. (2018), Animal Politico, Cronica de Chihuahua
61	Crispin Gutierrez Moreno	Ixtlahuacán-Colima	2017/10/20	Esparza et al. (2018), La Jornada, El Universal Queretaro
62	Victor Manuel Espinoza Tolentino	Ixhuatlán de Madero-Veracruz	2017/11/25	Esparza et al. (2018), Noroeste, El Financiero
63	Jose Santos Hernandez	San Pedro el Alto-Oaxaca	2017/12/09	Esparza et al. (2018), Telesur TV, AALMAC
64	Sergio Antonio Zenteno Albores	Bochil-Chiapas	2017/12/18	Esparza et al. (2018), Zeta Tijiana, Sin Embargo

65	Arturo Gómez Pérez	Petatlán-Guerrero	2017/12/28	Esparza et al. (2018), Mexico News Daily, Noroeste
66	Jose Efrain Garcia Garcia	Tlanepantla-Puebla	2018/04/12	Esparza et al. (2018), El Pais, Noticieros Televisa
67	Juan Carlos Andrade Magana	Jilotlán de los Dolores-Jalisco	2018/04/15	Esparza et al. (2018), Telesur TV, La Jornada
68	Alejandro Gonzalez Ramos	Pacula-Hidalgo	2018/05/03	Esparza et al. (2018), Proceso, El Piñero
69	Abel Montufar Mendoza	Coyuca de Catalan-Guerrero	2018/05/08	Esparza et al. (2018), Aristegui Noticias, Alcaldes de Mexico
70	Alejandro Chavez Zavala	Taretan-Michoacan	2018/06/14	El Universal, NPR, Dallas News
71	Javier Urena Gonzalez	Buenavista-Michoacan	2018/06/27	ACLED, El Norte, Noroeste
72	Victor Jose Guadalupe Diaz Contreras	Tecalitlán-Jalisco	2018/07/02	ACLED, El Financiero, El Economista
73	Genaro Negrete Urbano	Naupan-Puebla	2018/08/06	ACLED, El Financiero, Milenio
74	Olga Gabriela Kobel Lara	Juárez-Coahuila	2018/12/16	ACLED, El Universal, Milenio
75	Alejandro Aparicio Santiago	Heroica Ciudad de Tlaxiao-Oaxaca	2019/01/01	ACLED, El Universal, Milenio
76	David Eduardo Otlica Aviles	Nahuatzen-Michoacan	2019/04/23	ACLED, Mexico News Daily, Milenio
77	Maricela Vallejo Orea	Mixtla de Altamirano-Veracruz	2019/04/24	ACLED, Infobae, El Universal
78	Carmela Parral Santos	San Jose Estancia Grande-Oaxaca	2019/08/17	ACLED, El Pais, Reporte Indigo
79	Francisco Tenorio Contreras	Valle de Chalco Solidaridad-Edomex	2019/10/29	ACLED, La Jornada, El Universal
80	Arturo Garcia Velazquez	San Felipe Jalapa de Díaz-Oaxaca	2019/12/23	ACLED, Milenio, La Jornada
81	Carlos Ignaio Beltran Bencomo	Temósachic-Chihuahua	2020/09/29	ACLED, Infobae, El Financiero
82	Florisel Rio Delfin	Jamapa-Veracruz	2020/11/11	ACLED, e-Veraceruz, Proceso
83	Leobardo Ramos Lazaro	Chahuites-Oaxaca	2021/02/04	ACLED, El Pais, El Economista
84	Alfredo Sevilla Cuevas	Casimiro Castillo-Jalisco	2021/03/11	ACLED, Infobae, 24horas
85	Manuel Aguilar Garcia	Zapotlán de Juárez-Hidalgo	2021/06/09	ACLED, La Jornada Hidalgo, Noroeste

Note: The above list includes mayors who were assassinated. 3 Municipalities were subject to multiple assassinations against their mayors (San Miguel Tilquiápam-Oaxaca in 2012 and 2013; Buenavista-Michoacan in 2005 and 2018; Nahuatzen-Michoacan in 2013 and 2018). Thus, there are 82 unique municipalities that experienced at least one assassination. Full link to the articles are stored in the separate data file.

Table A2: List of mayors subject to failed attacks

	Name	Municipality, State	Date	Time away	Sources
1	Antonio Pouchoulen Cardenas	Las Choapas-Veracruz	2008/03/29		Alcalor Politico, Wradio, Proceso
2	Jesus Fernando Garcia Hernandez	Navolato-Sinaloa	2008/11/05	✓	La Jornada, El Siglio de Torreon, El Universal
3	Luis Carlos Ramirez Lopez	Ocampo-Durango	2008/11/18		El Siglo de Torreon, Wradio, El Universal
4	Arturo Bonilla Morales	Tlacoapa-Guerrero	2009/10/14	✓	El Siglo de Torreon, El Universal,
5	Maria Santos Gorrostietta	Tiquicheo de Nicolás Romero-Michoacan	2009/10/15	✓	Insight Crime, El Universal, Expansion
6	Maria Santos Gorrostietta	Tiquicheo de Nicolás Romero-Michoacan	2010/01/23	✓	Insight Crime, El Universal, Expansion
7	Raul Mario Mireles Garza	Sabinas Hidalgo-Nuevo León	2010/10/11		Expansion, Wradio, El Economista
8	Jose Eligi Moreno Martinez	Cuencame-Durango	2010/10/20		Reforma, El Siglio de Durango,
9	Jaime Heliodoro Rodriguez Calderon	Garcia-Nuevo León	2011/02/25		Expansion, La Jornada, Proceso
10	Ricardo Solis Manriquez	Gran Morelos-Chihuahua	2011/03/23	✓	El Mañana, Reforma,
11	Jaime Heliodoro Rodriguez Calderon	García-Nuevo León	2011/03/29		Expansion, La Jornada, Proceso
12	Clara Luz Flores Carrales	General Escobedo-Nuevo León	2011/07/03		Expansion, La Jornada, El Economista
13	Eleazar Palacios Rojas	San Pedro Totolápam-Oaxaca	2011/07/08	✓	Quadratin Oaxaca, La Radio del Siglo XXI,
14	Julio Cesar Salmeron Salazar	Alcozauca-Guerrero	2011/08/04		Vanguardia, Informador,
15	Filiberto Martinez	Solidaridad-Quintana Roo	2011/09/14		Proceso, Noticaribe, EFE News
16	Alejandro Higuera Osuna	Mazatlan-Sinaloa	2011/11/08		Chicago Tribune, Wradio, El Universal
17	Miguel Hernandez Anaya	San Miguel el Alto-Jalisco	2011/12/18		Informador, Proceso,
18	Andres Cardenas Guerrero	Coahuayana-Michoacan	2012/03/09	✓	Arestegui Noticias, Quadratin Michoacan,
19	Francisco de Jesus Ayon Lopez	Guadalajara-Jalisco	2012/07/09		Informador, 24horas, El Economista
20	Francisco Omar Corza Gallegos	Vista Hermosa-Michoacan	2012/07/23		El Universal, Arestegui Noticias,
21	Alejandro Tejeda Lopez	Zacapu-Michoacan	2012/10/05		El Universal, Arestegui Noticias,
22	Gustavo Garibay Garcia	Tanhuato-Michoacan	2012/10/12	✓	El Pais, Excelsior, El Economista
23	Miguel Entzin Cruz	Pantelho-chiapas	2012/12/18	✓	Reforma, SDP Noticias, Proceso
24	Rocio Rebollo Mendoza	Gomez Palacio-Durango	2013/02/05		Vanguardia, El Siglo de Torreon, Excelsior
25	Feliciano Alvarez Mesino	Cuetzala del Progreso-Guerrero	2013/04/09	✓	Proceso, Diario,
26	Pedro Luis Jiminez Hernandez	Santiago Amoltepec-Oaxaca	2013/05/13	✓	Excelsior, La Jornada, Animal Politico
27	Cesar Miguel Penaloza Santana	Cocula-Guerrero	2013/06/06		La Silla Rota, Imagen Radio, Proceso
28	Pablo Rodriguez Santiago	San Miguel del Puerto-Oaxaca	2013/06/24	✓	Excelsior, Vanguardia, La Jornada
29	Feliciano Alvarez Mesino	Cuetzala del Progreso-Guerrero	2013/08/26	✓	Proceso, Diario,
30	Enrique Antonio Paul	Texistepec-Veracruz	2014/04/01	✓	El Universal, Reforma, El Economista

31	Elizabeth Gutierrez Paz	Juan R. Escudero-Guerrero	2014/05/19	✓	La Jornada, El Financiero, Notigodinez
32	Leopoldo Molina Corral	Guadalupe y Calvo-Chihuahua	2014/09/08		Milenio, Debate, Noroeste
33	Juan Raúl Acosta Salas	Choix-Sinaloa	2015/03/06	✓	The Guardian, Debate, Expansion
34	Leticia Salazar	Matamoros-Tamaulipas	2015/03/09		Expansion, Colima Noticias, Telesur TV
35	Miguel Antonio Castillo	Coahuitlan-Veracruz	2015/03/13	✓	Costa Veracruz, El Heraldo de Poza Rica, Marcha
36	Mario de la Garza Garza	San Fernando-Tamaulipas	2015/05/30		El Siglio de Torreon, Aristegui Noticias, Reforma
37	Miguel Angel Castro Rosas	Amatlan de los Reyes-Veracruz	2015/07/19	✓	Quadratin Veracruz, El Siglio de Torreon
38	Romualdo Fuentes Galicia	Jantetelco-Morelos	2015/08/13	✓	Zona Centro Noticias, El Financiero, Reforma
39	Jose Santa Maria Zavala	Huehuetlán el Grande-Puebla	2015/09/01	✓	Expansion, El Pais, El Universal
40	Víctor Eduardo Castañeda Luquín.	Ahualulco de Mercado-Jalisco	2016/03/01		Excelsior, La Vanguardia, Alcaldes de Mexico
41	Israel Varela Ordóñez	Batopilas-Chihuahua	2017/01/17	✓	La Jornada, AM, Sin Embargo
42	Oscar Toral Rios	Asuncion Ixtaltepec-Oaxaca	2017/06/01	✓	El Universal, Corta Mortraja, ABC Radio
43	Jose Misael Gonzalez	Coalcomán de Vázquez Pallares-Michoacan	2017/10/20	✓	El Universal, Reforma, Aristegui Noticias
44	Andres Valencia Rios	San Juan Evangelista-Veracruz	2018/01/08		ACLED, Enlace Veracruz, El Sol de Puebla
45	Jose Rafael Nunez Ramirez	San Martín Texmelucan-Puebla	2018/02/01		ACLED, Milenio, Angulo7
46	Hugo Garcia Rios	San José Tenango-Oaxaca	2018/04/28		La Silla Rota, Vanguardia, El Sol de Mexico
47	Pablo Higuera Fuentes	Eduardo Neri-Guerrero	2018/06/26		ACLED, El Universal, El Financiero
48	Antonio Ramirez Itehua	Astacinga-Veracruz	2019/02/04	✓	ACLED, El Universal, El Economista
49	Emilio Montero Perez	Juchitan de Zaragoza -Oaxaca	2019/03/09		El Imparcial, Noticieros Televisa, Debate
50	Ernesto Quintanilla Villareal	Cadereyta Jiménez-Nuevo León	2019/03/10		ACLED, El Universal, Linea Directa
51	Domingo Cordoba Martinez	Chapulco-Puebla	2019/06/04		ACLED, El Popular, Milenio
52	Felix Alberto Linares Gonzalez	Ocuilan-Edomex	2019/07/03		Debate, De Paso Yucatan, La Jornada
53	Griselda Martinez Martinez	Manzanillo-Colima	2019/07/27		ACLED, Infobae, El Universal
54	Benito Olvera Munoz	Acatlan-Hidalgo	2019/07/31		El Sol de Hidalgo, El Reportero, AM
55	Eduardo Maldonado Garcia	San Felipe-Guanajuato	2019/08/22		ACLED, Milenio, El Siglo de Durango
56	Sara Valle Dessens	Guaymas-Sonora	2019/10/10		ACLED, El Imparcial, La jornada
57	Fernando Vilchis Contreras	Ecatepec-Edomex	2019/11/05		El Sol de Mexico, Noticias CD
58	Juan de Dios Valle Camacho	Ahumada-Chihuahua	2020/03/04		El Sol de Mexico, Reforma, El Norte
59	Abraham Cruz Gomez	Chenalho-Chiapas	2020/07/07	✓	ACLED, Excelsior, La Verdad Noticias
60	Aldo Molina Santos	Tenango de Doria-Hidalgo	2020/09/04		ACLED, Milenio, Quadratin Hidalgo
61	Cuitlahuac Contrado Escamilla	Acayucan-Veracruz	2020/11/17		Data Civica, Milinio, Infobae
62	Ponciano Gomez Gomez	Chamula-Chiapas	2020/12/05		El Siglio Coahuila, Proceso, La Jornada
63	Sinforiano Armenta Garcia	Tepetongo-Zacatecas	2021/04/08		La Jornada
64	Jorge Alberto Quinto Zamorano	Hueyapan de Ocampo-Veracruz	2021/04/22		Data Civica, Diario de Xalapa, El Sol de Mexico

65	Sandra Velazquez Lara	Pilcaya-Guerrero	2021/08/11	ACLED, Milenio, La Jornada
66	Carlos Alberto Paredes Correa	Tuxpan-Michoacan	2021/10/07	ACLED, Proceso, El Sol de Morelia
67	Geminiano Hernandez	Chiconamel-Veracruz	2021/11/19	ACLED, Milenio, Avi Veracruz
68	Calixto Urbano Lagunas	Atlatlahucan-Morelos	2021/11/19	ACLED, Diario de Morelos
69	Sinforiano Armenta Garcia	Tepetongo-Zacatecas	2021/11/24	Proceso, Excelsior, El Norte

Note: The above list includes mayors who were subject to failed attacks. 4 Municipalities were subject to multiple failed attacks against their mayors (Tiquicheo de Nicolás Romero-Michocan in 2009 and 2010; García-Nuevo Leon 2011 Feb and March; Cuetzala del Progreso-Guerrero in 2013 Apr and Aug; Tepetongo-Zacatecas in 2021 Apr and Nov). In 7 of the municipalities listed here, a mayor was assassinated either before or after the failed attacks occurred (Ocampo-Durango in 2009; Vista Hermosa-Michoacan in 2009; Tanhuato-Michoacan in 2014; Santiago Amoltepec-Oaxaca in 2011; Guadalupe y Calvo-Chihuahua in 2010; Huehuetlán el Grande-Puebla in 2016; Chamula-Chiapas in 2016). Thus, 58 unique municipalities experienced at least one failed attack without experiencing successful mayor assassinations. These cases were separated into mayor spending time away from office due to being injured (*herido(a), lesionado(a), se translasdo(a) al hospital*) and returning due to being unharmed (*sale ileso(a)*). These cases were categorized based on expressions appearing in the articles mentioned in the source column. In one case, a mayor (Ricardo Solís Manríquez) was unharmed from attacks but had to spend time away due to injuries he suffered during the election. Full links to the articles are stored in a separate data file.

Table A3: List of mayors who passed away in a non-violent manner

	Name	Municipality, State	Date	Reason of death	Sources
1	Oscar Zúñiga Quiroz	Mier y Noriega-Nuevo León	2002/03/15	car accident	Magar (2018), Proceso, Vlex
2	Carlos Filemón Kuk y Can	Motul-Yucatan	2003/07/28	car accident	Magar (2018), Proceso
3	Cecilio Amador Cuauhtle	Contla de Juarez Cuamatzi-Tlaxcala	2004/02/14	car accident	Magar (2018), El Siglo de Torreon, Proceso
4	Pedro Rojas Pérez	Santa Cruz Quilehtla-Tlaxcala	2004/02/14	car accident	Magar (2018), Proceso, Vlex
5	Delia Garza Gutiérrez	San Fernando-Tamaulipas	2007/07/20	cancer	Magar (2018), La Jornada, Cimac Noticias
6	Miguel Ángel Nicolás Mata	San Pedro Totolapan-Oaxaca	2009/08/06	car accident	Magar (2018) Panorama del Pacifico
7	José Manuel Maldonado	Piedras Negras-Coahuila	2010/07/07	plane crash	Magar (2018), El Economista, Plano Informativo
8	Rogelio Pérez Arrambide	Pesquería-Nuevo León	2010/07/25	heart attack	Magar (2018), Vlex, Presencia
9	Ignacio Rodríguez Villa	Nahuatzen-Michoacan	2012/09/29	respiratory disease	Magar (2018), Quadratin Michocan, TVNotas
10	Salomón Domínguez Jiménez	San Juan Lajarcia-Oaxaca	2012/11/19	car accident	Magar (2018), Libertad Oaxaca, Quadratin Oaxaca
11	Félix San Juan Rebollar	San Baltazar Chichicapam-Oaxaca	2013/01/06	unspecified illness	Magar (2018), Quadratin Oaxaca,
12	Leobardo Díaz Estrada	Urique-Chihuahua	2013/02/07	car accident	Magar (2018), Vanguardia, La Jornada
13	Joel Cebada Bernal	Nogales-Veracruz	2013/04/14	kidney failure	Magar (2018), Alcalor Politico, Orizaba en Red
14	Ernesto Rodríguez Rodríguez	Juchipila-Zacatecas	2013/08/16	heart attack	Magar (2018), Zacatacas Online, Vanguadia
15	Filimón Carlos Robles Díaz	Tepetongo-Zacatecas	2013/09/30	suicide	Magar (2018), Zacatacas Online, La Jornada
16	Eliud Cervantes Ramírez	Catemaco-Veracruz	2013/11/02	heart attack	Magar (2018), El Economista, Quadratin Mexico
17	Juan Ángel Castañeda Lizardo	Sombrerete-Zacatecas	2014/02/10	car accident	Magar (2018), Milenio, La Jornada
18	Sadot Bello García	Copala-Guerrero	2015/06/19	respiratory disease	Magar (2018), Expansion, Excelsior
19	Jesús Alvarado Hernández	San Pedro Sochiapam-Oaxaca	2015/11/03	Car accident	Magar (2018), El Universal, Excelsior
20	Alfredo Vizcarra Díaz	Concordia-Sinaloa	2016/09/20	stroke	Magar (2018), Noroeste, Proceso
21	Martha Elvia Fernández Sánchez	Cuautitlán-Edomex	2017/03/05	cancer	Magar (2018), MVS Noticias, Infobae
22	Fernando Álvaro Gómez	Tianguistenco-Edomex	2017/03/25	heart attack	Magar (2018), Proceso, El Sol de Mexico
23	Aurelio Cortez Aguirre	Santa Maria la Asuncion-Oaxaca	2017/05/17	gastric ulcer	Magar (2018), Legislador43, Tvbus
24	Irma Camacho García	Temixco-Morelos	2017/07/19	unspecified illness	Magar (2018), Proceso, Sinembargo
25	Edgar Gil Yoguez	Venustiano Carranza-Michoacan	2017/08/26	heart attack	Magar (2018), Notivideo, Mi Morelia
26	Salvador Aguilar García	Cohetzala-Puebla	2018/01/29	car accident	Magar (2018), Contrastes de Puebla
27	Jorge Luis García Vera	Villanueva-Zacatecas	2018/08/11	car accident	Magar (2018), El Universal, El Sol de Zacatecas
28	Zótico Gómez Bautista	Santiago Tetepec-Oaxaca	2018/09/20	car accident	Magar (2018), Debate, Excelsior
29	Jesús Bernardo Torres García	Santiago Suchiquitongo-Oaxaca	2018/10/30	pneumonia	Magar (2018), El Pinero, Imparcial Oaxaca
30	Raymunda Che Pech	Kantunil-Yucatan	2019/10/06	fainted at home	Magar (2018), El Financiero, El Universal

31	Félix Alberto Linares	Ocuilan-Edomex	2020/01/04	plane accident	Magar (2018), El Economista, Infobae
32	Óscar Gurría Penagos	Tapachula-Chiapas	2020/02/20	heart attack	Magar (2018), El Sol de Mexico, Milenio
33	Armando Portuguez Fuentes	Tultepec-Edomex	2020/05/23	heart attack	Magar (2018), Infobae, Excelsior
34	Sergio Anguiano Meléndez	Coyotepec-Edomex	2020/06/08	covid	Magar (2018), El Financiero, El Economista
35	Javier Santiago Ruiz	Reyes Etla-Oaxaca	2020/06/15	covid	Magar (2018), El Economista, El Universal Oaxaca
36	Rigoberto González Pacheco	Bacoachi-Sonora	2020/06/16	covid	Magar (2018), El Economista, Reforma
37	José Humberto Arellano	Acaponeta-Nayarit	2020/06/17	covid	Magar (2018), El Economista, Infobae
38	Florencio San Germán Santiago	San Baltazar Chichicapam-Oaxaca	2020/06/28	covid	Magar (2018), La Razon, Central Municipal
39	Gerardo Tirso Acahua Apale	Coetzala-Veracruz	2020/06/28	covid	Magar (2018), El Economista, El Universal
40	Josué Antonio García Rodríguez	Vanegas-San Luis Potosí	2020/07/08	covid	Magar (2018), El Economista, El Sol de San Luis
41	Reyna Marlene de los Ángeles Catzín Cih	Maxcanú-Yucatan	2020/07/09	covid	Magar (2018), El Economista, El Universal
42	Faustino Carín Molina Castillo	Amaxac-Tlaxcala	2020/07/13	covid	Magar (2018), El Economista, La Jornada
43	Fernando Bautista Dávila	San Juan Bautista Tuxtepec-Oaxaca	2020/07/16	covid	Magar (2018), El Economista, El Universal Oaxaca
44	Irma Delia Bárcena Villa	Miahuatlan-Veracruz	2020/07/16	covid	Magar (2018), El Sol de Mexico, Imagen del Golfo
45	Rigoberto Javier Tun Salas	Samahil-Yucatan	2020/07/19	covid	Magar (2018), El Economista, El Universal
46	Artemio Ortiz Ricárdez	Tamazulapan del Espiritu Santo-Oaxaca	2020/08/05	covid	Magar (2018), El Economista, El Universal Oaxaca
47	Victoria Rasgado Perez	Moloacan-Veracruz	2020/08/09	covid	Magar (2018), El Economista, Milenio
48	Alfredo Juarez Diaz	Matias Romero-Oaxaca	2020/08/18	covid	Magar (2018), El Economista, Excelsior
49	Pedro Escárcega Pérez	Santiago Jocotepec-Oaxaca	2020/08/21	covid	Magar (2018), El Economista, Infobae
50	Miguel Ángel Antonio Vázquez	General Felipe Ángeles-Puebla	2020/08/24	covid	Magar (2018), El Economista, Milenio
51	Victorino Gómez Martínez	San Bartolomé Quialana-Oaxaca	2020/08/25	covid	Magar (2018), El Economista, Milenio
52	Simón Ursino Barzán	San Simón Zahuatlán-Oaxaca	2020/08/26	car accident	Magar (2018), SDP Noticias, Milenio
53	Tomás Primo Negrete	Tonanitla-Edomex	2020/08/30	covid	Magar (2018), El Economista, El Universal
54	Daniel Efren Hernández Hernández	San Miguel del Rio-Oaxaca	2020/09/13	covid	Magar (2018), El Economista, Quadratin Oaxaca
55	Pedro Modesto Santos	Santa Cruz Xitla-Oaxaca	2020/09/24	covid	Magar (2018), El Economista, Sopitas
56	Héctor Carrasco Márquez	Venustiano Carranza-Puebla	2020/10/03	covid	Magar (2018), El Economista, Milenio
57	Roberto Arriaga Colín	Ocampo-Michoacan	2020/10/05	covid	Magar (2018), El Economista, El Universal Oaxaca
58	Carlos Mario Ortiz Sánchez	Salvador Alvarado-Sinaloa	2020/10/07	covid	Magar (2018), El Economista, El Universal
59	Juan Manuel Rodríguez Rodríguez	Tulcingo del Valle-Puebla	2020/10/26	covid	Magar (2018), El Economista, Heraldo de Mexico
60	Carmen Prieto Mortera	Moloacan-Veracruz	2020/11/08	covid	Magar (2018), El Economista, Milenio
61	Rubén Díaz Espinoza	Santo Domingo-San Luis Potosí	2020/11/09	covid	El Sol de San Luis, Quadratin Queretaro
62	Jorge Luis Peña Peña	Los Aldamas -Nuevo León	2020/12/14	heart attack	Magar (2018), El Norte, Reforma
63	José Rosario Romero Lugo	Jaltenco-Edomex	2020/12/17	covid	Magar (2018), El Economista, El Universal
64	Juan José Losoya Ponce	San Francisco de los Romo-Aguascalientes	2021/01/05	heart attack	El Universal, El Sol de Centro, La Razon

65	Efraín Lázaro	San Juan Tamazola-Oaxaca	2021/01/23	covid	Magar (2018), El Universal, Reforma
66	José Yolando Jarquín Bustamante	Xitlapehua-Oaxaca	2021/01/25	covid	Magar (2018), Proceso, Milenio
67	Filogonia Adorno Aragon	San Bartolo Cohuecan-Puebla	2021/01/27	covid	El Economista, El Sol de Puebla, Milenio
68	María de Jesús Chávez	Tasquillo-Hidalgo	2021/01/30	covid	Magar (2018), Excelsior, La Silla Rota
69	Aparicio Reyes Rojas	Santos Reyes Tepejillo-Oaxaca	2021/01/30	covid	Magar (2018), Excelsior, Proceso
70	Leonilo Ruiz Martínez	Santa Catarina Loxicha-Oaxaca	2021/02/02	covid	Magar (2018), Quadratin Oaxaca, Milenio
71	Fernando Raymundo Valeriano Rodriguez	San Simon Zahuatlán-Oaxaca	2021/02/05	covid	Nvinoticias, La Silla Rota
72	Misael Lorenzo Morales	Atzacan-Veracruz	2021/02/08	covid	Magar (2018), Infobae, Milenio
73	Jan Cruz Idiaquez	San Francisco Sola de Vega-Oaxaca	2021/02/08	unspecified illness	La Silla Rota, El Universa Oaxaca
74	Patricia González	Villa Tezontepec-Hidalgo	2021/02/18	covid	Magar (2018), La Jornada, Excelsior
75	Juvenal Garcia Hernandez	San Sebastian Rio Hondo-Oaxaca	2021/02/19	covid	El Economista, El Universal, El Imparcial Oaxaca
76	Amado Vasquez	San Pedro Mixtepec - Distrito 26–Oaxaca	2021/02/22	covid	El Economista, El Universal Oaxaca
77	Filadelfo Vergara Tapia	Petlalcingo-Puebla	2021/02/23	covid	El Economista, Reforma, El Sol de Puebla
78	Nicolas Galindo Marquez	Jalpan-Puebla	2021/02/25	covid	El Economista, La Jornada de Oriente, Milenio
79	Hugo García Ríos	San Jose Tenango-Oaxaca	2021/02/28	covid	El Economista, SDP Noticias, El Universal Oaxaca
80	Baltazar Gaona Sánchez	Tarimbaro-Michoacan	2021/03/05	covid	El Economista, La Jornada, El Sol de Morelia
81	Leobardo Aguilar Flores	Soltepec-Puebla	2021/03/31	covid	El Economista, Milenio, La Jornada de Oriente
82	Rogelio Torres Ortega	Tepoztlan-Morelos	2021/04/13	covid	El Economista, Infobae, Milenio
83	Jose Dolores Jimenez Lopez	Santa Maria Nativitas-Oaxaca	2021/06/09	covid	El Economista, El Universal Oaxaca
84	Trinidad Perez Coria	Mazatepec-Morelos	2021/07/20	heart attack	Milenio, El Sol de Cuernacava, La Jornada
85	Evergisto Gamboa Diaz	Santiago Choapam-Oaxaca	2021/07/31	covid	El Norte, La Razon, Nvinoticias
86	Jorge Humberto Aguilar Perera	Kaua-Yucatan	2021/08/10	covid	Grillo de Yucatan, Diario de Yucatan
87	Carlos Manuel Calvo Martinez	Jiquipilas-Chiapas	2021/09/08	covid	La Jornada, Vanguardia Veracruz, Excelsior
88	Antonio Francisco Perez	Hermenegildo Galeana-Puebla	2021/09/15	covid	Municipios Puebla, Angulo7, El Sol de Puebla
89	Abel Sanchez Campos	San Antonino Castillo Velasco-Oaxaca	2021/12/28	natural	Meganoticias, El Universal Oaxaca

Note: The above list includes mayors who were subject to non-violent deaths. 3 municipalities experienced multiple non-violent deaths of their mayors (Moloacan-Veracruz in Aug and Nov of 2020; San Baltazar Chichicapam-Oaxaca in 2013 and 2020; San Simon Zahuatlán-Oaxaca in 2020 and 2021). In 2 municipalities, a mayor was also assassinated (Nahuatzen-Michoacan in 2013; Temixco-Morelos in 2016). Thus, 84 unique municipalities experienced non-violent deaths of the mayors without assassinations.

Subsection A.4 Definition of key variables from other datasets

A.4.1 Fiscal indicators: Revenues to municipal government

Following are the definitions of the fiscal variables used in the research. The definition and the categorization come from the INEGI's database (INEGI 2016).

- Tax revenues (*impuestos*): These are revenue that is paid by legal and natural persons under the relevant taxation law. At the municipal level, the following taxes are collected
  - Property taxes (*impuesto predial*)
  - Land tax revenues (*impuestos al patrimonio*): Summation of property taxes and sale tax on real estate. In some cases, this is translated as wealth tax
  - Other taxes include additional taxes on education (*impuestos adicionales para educación*) and public works (*impuestos adicionales para obras de públicas*)
- Non-earmarked funds from the federal government (*participaciones*): These are funds and resources given to the municipal governments, with no conditions specifically defined. The funds in this category depend both on demographic traits and local revenue-generating activities (SEGOB 2011)
  - General Participation Funds (*Fondo General de Participaciones*): This is also shared with the state governments, who must also share some of the amount they receive from this fund to municipalities according to the Financial Coordination Law (*Ley de Coordinación fiscal*)
  - Municipal Development Funds (*Fondo de Fomento Municipal*): There are more components determined by taxation in this category in general. This fund is exclusively destined to the municipalities and not the states (SEGOB 2011)
  - Other categories include transfers based on taxes collected at the federal or state level, such as vehicle taxes, gasoline taxes, and payroll taxes
- Earmarked funds from the federal government (*aportaciones*): These are funds and resources given to the municipal governments, with conditions on where these funds could be spent according to the Financial Coordination Law

- Municipal Fund for Social Infrastructure (*Fondo de Aportaciones para la Infraestructura Social Municipal*): Conditioned for the public projects and infrastructure development that benefits municipal population
- Funds for Municipal Development (*Fondo de Aportaciones para el Fortalecimiento de los Municipios*): Conditioned for supporting municipal treasuries and other requirements of the municipalities, such as public security. Generally, the conditions on this fund are weaker than those of FISM (SEGOB 2011).

- Revenues from provision of public service (*derechos*): These are contributions to the municipal revenue through receipt of fees from servicing a public goods and services. The following are included
  - Registration services (*registro civil, registro público de la propiedad y del comercio*)
  - Certification and recording services (*certificaciones y constancias diversas*)
  - Licenses (*icencias al comercio ambulante, licensias de construcción*)
  - Water (*agua potable*)
  - Services related to urban development (*Servicios de desarrollo urbano y obras públicas*)
- Revenues from legal functions (*aprovechamientos*): Income received from public law functions.
  - Surcharges for interest payments (*recargos*), Fines (*multas*), Penalties for late payments of fees (*Rezagos*)

A.4.2 Fiscal indicators: Municipal government expenditures

Like the revenue variables, the definition and categorizations are from the INEGI (2016)

- Total payments to personnel (*Servicios personales*): Expenses towards the remuneration of personnel at the service of public entities. This includes wages, bonuses, and social security benefits.
  - General remunerations (*remuneraciones al personal*)
  - Others: Additional pay (*Remuneraciones adicionales y especiales*), Social security quotas (*cuotas de seguridad social y seguros*)

- Expenditures on general services (*Servicios generales*): Expenses designed to cover the costs of the services provided by the municipal government
  - Basic services (*servicios basico*): Includes expenses to water, electricity, telephone, and internet services
  - Those that are counted as other general expenditures include leases (*arrendamientos*), financial services (*servicios financieros, bancarios, y comerciales*), expenses on maintenance services including waste management (*servicios de instalación, reparación, mantenimiento y conservación*) and travel expenses for municipal personnel (*servicios de translado y viáticos*)
- Public investment (*Inversíon pública*): Expenses on public projects and contracts on works related to municipal development and infrastructure.
  - Includes construction of residential and nonresidential buildings, schools, hospitals, and energy infrastructures on public and private domains
- Transfers and allowances to municipal institutions (*Transferencia, Asignaciones, subsidios y otra ayuda*): Allowances destined directly or indirectly to various entities to support economic and social policy, following the strategies for development and maintenance of the performance of the recipient entities
  - Transfers and allowances to internal public organizations (*ransferencias internas y asignaciones al sector público*)
  - Subsidies to private entities (*Subsidios*)
  - Social assistance to individuals (*Ayudas*)

A.4.3 Variables on municipal personnel

- Committees mentioned in the Census of Municipal Governments: Among many others, the primary ones are treasury, internal control, public security, social development, and economic development. Other minor ones include committees for culture, municipal presidents, and others. (The categorization has changed in the 6th wave of the Census of Municipal Governments, published in 2021)

A.4.4 Further definition of the control variables used in the main specification

- Number of organized criminal groups: Calculated based on the number of organized criminal groups appearing in Coscia and Rios (2012) and Osorio and Beltran (2020) and ACLED. While Osorio and Beltran (2020) and ACLED also identifies subdivision of the major organized criminal groups, this is not the case for Coscia and Rios (2012). Thus, I use the number of major organized criminal groups and not their subdivisions for consistency.

- Homicide indicators: The total count of homicides is generated from the homicide records in INEGI, accessible with this link https://www.inegi.org.mx/sistemas/olap/proyectos/bd/continuas/mortalidad/defuncioneshom.asp?s=est. As for the homicide rate per 100,000 people, this is generated by dividing this with population measure

- Average level of schooling: Calculated based on response to year of schooling questions from the Mexican Census, with intercensal years calculated based on interpolation

- Share of indigenous population: Calculated based on response to year of schooling questions from the Mexican Census and population from census and WorldPop, with intercensal years calculated based on interpolation[4]

- Years since election: Number of calendar years passed since the most recent election

- Resource endowment: Amount of gold, silver, iron, copper, and zinc extracted in each municipality measured in tons. Data from 2000 and after uses a Mining-metallurgical industry survey from INEGI. Earlier data are from the mineral yearbook of the Council of Mineral Resources.

Subsection A.5 Creation of harmonized nightlight measures from DMSP and VIIRS

The two sources of the nightlight data primarily available for research purposes are the Defense Meteorological Satellite Program (DMSP) and Visible and Infrared Imaging Suite (VIIRS).[5] DMSP is available from 1992 to 2013, with multiple different satellites (F10, F12, F14, F15, F16, F18) covering different time periods.[6] F10 satellite was operated from 1992-1994. F12 covers 1994-1999. F14 is

4. Results for homicides rates and share of population are robust to using either the Census of the combination of Census and WorldPop as population measures

5. Both datasets can be downloaded from the website for the Payne Institute for Public Policy under the Colorado School of Mines: https://eogdata.mines.edu/products/dmsp/ (DMSP) and https://eogdata.mines.edu/products/vnl/ (VIIRS)

6. As individual satellites were degrading in quality of measurements over time, multiple satellites were employed to make up for the shortcomings. (Yuan et al. 2022)

available from 1997-2003. F15 is used from 2000-2007. F16 runs from 2004-2009. For 2010-2013, F18 is used. VIIRS, on the other hand, is available publicly from 2012 and onwards, using a single satellite. The timeframe of this research spans from 1995 and 2021. With no single dataset having a time coverage that spans this period on its own, it is necessary to combine the two datasets to utilize the nightlight variables

However, two other differences complicate the combination of the two datasets. First, each pixel in the two datasets is measured in different geographic units. Each pixel of nightlight intensities in DMSP is measured in a 1km-by-1km unit, whereas the same for VIIRS is 500m-by-500m. Thus, I need to match the pixel units by aggregating the observations in the VIIRS to match the same unit of distance in DMSP.

More importantly, the measure of light intensity used in the two datasets is different. In DMSP, nightlight intensity is measured using 'digital numbers' (DNs), which is an arbitrary unit generated with a 6-bit quantization radiometric resolution over the nightlights (Yuan et al. 2022). The range for the DNs is 0 to 63, with extremely bright (dark) nightlights being topcoded (bottomcoded). For VIIRS, the nightlight intensities are measured in terms of the actual radiance and capture a wider range of nightlight intensities than DMSP. Furthermore, 1 value of DNs in DMSP can correspond to multiple values of nightlight intensities in the VIIRS dataset (Li et al. 2022; Yuan et al. 2022). Therefore, I create a unified light intensity measure by translating the VIIRS nightlight intensities to the corresponding DMSP DN values.

I take the following steps to create a combined dataset with an identical geographic pixel unit and consistent light intensity measure, based on the methods suggested by Li et al. (2022) and Yuan et al. (2022). I first create consistent nightlight intensity measures across all the different satellites in the DMSP sample. For years with multiple satellites, I averaged the different intensity values to represent the nightlight for each pixel. Then I generate a regression with the DN of each year t for each pixel i as an outcome variable, with the constant, DN, and DN-squared of the base year (2010) for the same pixel as an input (Yuan et al. 2022).[7]

$$DN_{i,t} = \beta_0 + \beta_1 DN_{i.2010} + \beta_2 DN^2_{i.2010} + u_i \tag{A1}$$

7. Base year of 2010 is suggested by Yuan et al. (2022) on the basis that the DN values for that year had the highest total and thus, a sufficient variation to be used as a reference year.

After the regression, I generate the fitted nightlight values for each year by fitting the estimated coefficients $\hat{\beta}_0$, $\hat{\beta}_1$ and $\hat{\beta}_2$ in the following manner

$$\widehat{DN}_{i,t} = \hat{\beta}_0 + \hat{\beta}_1 DN_{i,t} + \hat{\beta}_2 DN_{i,t}^2 \tag{A2}$$

I apply this to all for $t \leq 2013$. This generates a consistent nightlight measure for all DMSP samples.

Then, I generate a DMSP-like measure for the VIIRS data. For this, I use the two years for which both DMSP and VIIRS are available as references - 2012 and 2013. I start by aggregating the pixels in VIIRS resolution from the 500m-by-500m level to the 1km-by-1km level by taking averages across the 4 pixels making up the 1km-by-1km space. I denote the newly aggregated pixel values as $x_{i,t}$ for year t at point i. Then, I take the inverse hyperbolic sine on the aggregated pixel values to optimize the fitting procedure (Li et al. 2022).[8] Then, I fit this measure with the nonlinear regression using the following sigmoid function to follow the idea that the DMSP is bottom-coded and top-coded.[9] This step generates the DMSP-like nightlight values in DNs for all the VIIRS samples.

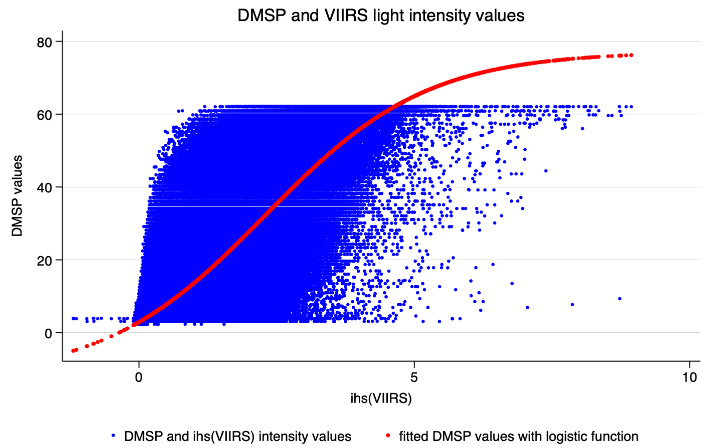
$$DN_{i,t} = \gamma_0 + \frac{\gamma_1}{1 + \exp(-\gamma_2(ihs(x_{i,t}) - \gamma_3))} + e_i \quad (2014 \leq t \leq 2021) \tag{A3}$$

The resulting nightlight measures are summarized by Figure A1. The top panel reports the degree of fit between the DMSP and VIIRS nightlight intensities. The bottom panel shows the nightlight intensity measures across different satellites in the two datasets. The blue and red line represents the DMSP nightlight intensity values that fit across different satellites in the DMSP sample and the generated DMSP values for the VIIRS dataset. For the research, these two lines were used as the nightlight intensity measures.

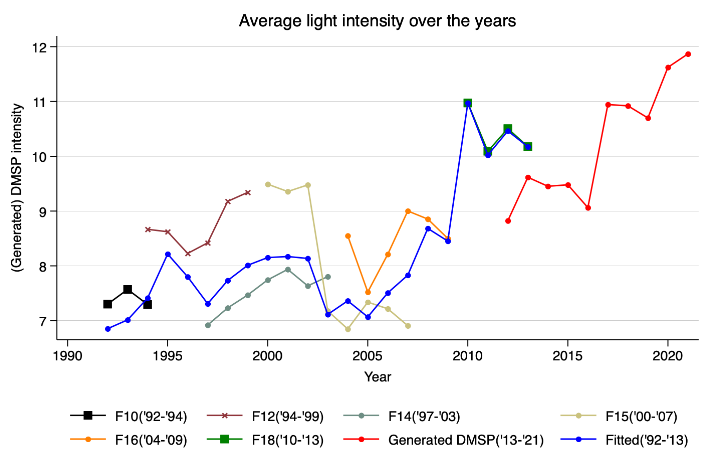
8. This step is carried out to smooth out the coarse values that are calculated as a result of aggregating from 500m-by-500m level to the 1km-by-1km level. Further technical details are found in Li et al. (2022).

9. For this, I use the `nl` command in Stata with `log4` option, which fits the outcome and independent variables with a logistic function

Figure A1: Harmonizing nightlight intensity variables across DMSP and VIIRS



(a) Fitting VIIRS and DMSP for 2013 and 2013



(b) Measure of nightlight intensity variables in the two datasets

Note: The top panel describes the fit between DMSP and VIIRS nightlights matched with the logistic function in Equation (A3). The bottom panel maps out the nightlight values for all satellites in the data as well as the fitted DMSP values for all the DMSP datasets (in blue) and VIIRS dataset (in red).

Subsection A.6 Full summary statistics and balance tables

Table A4 provides summary statistics for the whole sample in the survey. Table1 A5 breaks down the summary statistics for key variables in the text by near-miss, assassination, and the rest of Mexico.

Table A4: Summary statistics for outcome variables at municipality-year level

Variable (unit)	N	Mean	St. dev.	10th pct.	Median	90th pct.
Panel A. Outcome variables for municipal government revenues						
Total income (th. Pesos)	2,867	201,221	674,966	4,462	46,033	365,208
Tax revenues (th. Pesos)	2,749	27,774	130,620	36	1,027	37,709
Tax per capita (Pesos)	2,740	165	388	3	49	349
Property Tax (th. Pesos)	2,488	17,609	85,108	35	721	25,016
Property Tax per capita (Pesos)	2,479	102	215	3	37	224
Non-earmarked Fund (th.Pesos)	2,541	65,761	230,756	3,280	16,627	104,933
Earmarked Fund (th. Pesos)	2,307	58,115	130,252	4,122	20,241	128,190
Usage Fee (th. Pesos)	2,754	13,016	63,695	38	1,037	17,181
Legal Service (th. Pesos)	2,625	6,072	27,879	10	340	9,296
Panel B. Outcome variables for municipal government expenditures						
Total expenditure (th. Pesos)	2,867	201,222	674,966	4,462	46,033	365,208
Personnel expenditure (th. Pesos)	2,858	69,677	280,919	962	11,225	110,376
Public Investment (th. Pesos)	2,801	42,410	92,226	517	14,077	98,426
Basic Infrastructure (th. Pesos)	2,657	8,441	27,620	118	1,631	14,282
Other General Services (th. Pesos)	2,657	22,995	94,658	323	2,887	28,632
Transfer/allowance (th. Pesos)	2,809	22,609	107,404	242	2,830	24,925
Internal transfers (th. Pesos)	2,281	14,127	70,042	125	1,672	17,007
Panel C. Outcome variables for municipal workers						
Total (Persons)	699	604	1,582	35	198	1,163
20s (Persons)	699	103	228	2	37	199
30s (Persons)	699	164	429	3	57	311
40s (Persons)	699	149	473	2	40	263
$\geq$50s (Persons)	698	133	447	0	23	231
Panel D. Outcome variables for alternative mechanisms						
Fitted nightlights (DNs)	3,213	8.65	10.5	2.92	5.33	17.1
Total Outmigration (Persons)	1,666	611	1,845	24	251	1,078
Total population (Persons)	3,203	73,809	216,142	4,473	19,509	121,831
Population age 15-64 (Persons)	3,086	46,010	143,373	2,305	10,605	74,483
# Organized Criminal (Groups)	3,213	0.402	1.06	0	0	1
Total homicides (Cases)	3,159	12.9	53.5	0	1	21
Homicide per 100k (Rate)	3,159	17.4	43.7	0	5.55	44.4
Robbery (Cases)	1,150	669	3,478	1	27	963
% Security expenditure	2,867	0.010	0.025	0	0	0.043
% Security personnel expenditure	2,858	0.009	0.024	0	0	0.040
Federal police presence	2,023	0.250	0.433	0	0	1

The table lists the summary statistics for the variables in Section 3 at the municipal level. The statistics presented here are mean, standard deviation, 10th percentile, median, and 90th percetile. For the units, “th. Pesos " refers to a thousand Pesos. While most outcomes are counted from 1995-2021, some outcomes in Panel C and D are available at a shorter time frame.

Table A5: Summary statistics for outcome variables, per category of municipalities

	(1) Near-miss			(2) Assassination			(3) Rest of Mexico		
Variable (unit)	N	Mean	St. dev	N	Mean	St. dev	N	Mean	St. dev
Panel A. Outcome variables for municipal government revenues									
Total income (th. Pesos)	939	468,047	1,119,396	1,928	71,268	126,669	55,617	109,605	368,459
Tax (th. Pesos)	926	74,679	216,647	1,823	3,948	14,770	52,324	13,828	86,654
Tax per capita (Pesos)	922	278	528	1,818	107	276	51,305	108	267
Property tax (th. Pesos)	854	46,750	140,390	1,634	2,379	7,742	45,739	8,424	48,876
Property tax per capita (pesos)	850	167	302	1,629	68	139	44,875	68	144
Nonearmarked fund (th. Pesos)	843	149,626	382,926	1,698	24,126	41,433	48,659	37,383	114,899
Earmarked fund (th. Pesos)	770	109,325	204,544	1,537	32,460	50,504	43,991	35,214	79,165
Service Revenue (th. Pesos)	916	15,402	45,786	1,715	1,121	2,719	48,659	37,383	114,899
Legal functions (th. Pesos)	910	34,344	106,963	1,838	2,387	6,430	48,211	3,443	22,690
Panel B. Outcome variables for municipal government expenditures									
Total expenditure (th. Pesos)	939	468,047	1,119,396	1,928	71,268	126,669	55,617	109,605	368,459
Personnel expenditure (th. Pesos)	936	168,959	470,849	1,922	21,327	48,155	55,322	36,727	144,366
Public Investment (th. Pesos)	919	76,296	141,247	1,882	25,864	45,711	54,174	27,711	74,661
Basic Infrastructure (th. Pesos)	879	20,074	45,337	1,778	2,690	4,960	50,892	5,004	18,782
Other general service (th. Pesos)	879	59,021	157,961	1,778	5,095	9,732	50,892	11,178	50,235
Transfer/allowances (th. Pesos)	925	58,023	181,406	1,884	5,222	11,638	54,283	10,671	48,633
Internal transfers (th. Pesos)	777	35,343	116,705	1,504	3,167	7,554	44,743	7,130	33,917
Panel C. Outcome variables for municipal workers									
Total (persons)	218	1,365	2,609	481	259	425	13,770	398	1,000
20s (persons)	218	221	367	481	49.8	76.3	13,770	71.4	159
30s (persons)	218	361	709	481	74.9	119	13,770	111	269
40s (persons)	218	342	799	481	60.8	111	13,770	99.4	276
≥50s (persons)	218	322	753	480	47.2	101	13,628	95.3	333
Panel D. Outcome variables for alternative mechanisms									
Fitted nightlights (DNs)	999	12	14.6	2,214	7.14	7.55	63,126	9.03	10.7
Total outmigration (persons)	518	1,221	3,160	1,148	336	444	32,889	335	986
Total population (persons)	994	176,660	361,706	2,209	27,528	44,781	61,869	44,432	127,924
Population aged 15-64 (persons)	958	112,423	240,826	2,128	16,112	28,960	59,824	27,498	84,410
# Criminal groups (groups)	999	0.527	1.31	2,214	0.346	0.923	63,180	0.224	0.778
Total homicides (cases)	972	28.8	92	2,187	5.88	14.9	56,727	8.59	75.9
Homicide per 100k (rate)	972	11.1	17.6	2,187	20.3	50.9	56,619	10.9	36.6
Robbery (cases)	382	1,788	5,851	768	113	424	21,173	351	1,560
% Security expenditure	939	0.016	0.031	1,928	0.007	0.021	55,617	0.008	0.023
% Security personnel expenditure	936	0.014	0.029	1,922	0.006	0.020	55,322	0.008	0.022
Federal police presence	629	0.294	0.456	1,394	0.230	.0421	39,752	0.161	0.368

The table lists the summary statistics for the variables in Section 3 at the municipal level, broken down into three categories. The categories are defined depending on whether there were assassinations that failed to kill and injure a mayor (Column (1)), those that killed a mayor (Column (2)), and the rest of Mexico (Column(3)). The number of municipality-year observations, mean, and standard deviation are presented. For the units, "th. Pesos" refers to a thousand Pesos. While most outcomes are counted from 1995-2021, some outcomes in Panel C and D are available at a shorter time frame.

Table A6: Balance table for covariates, including municipalities with injured mayors

Variable	(1) Near-miss Mean	(2) Injured Mean	(3) Treated Mean	(2)-(1) Injured vs Near-miss Difference	[p-value]	(3)-(1) Treated vs Near-miss Difference	[p-value]	(3)-(2) Treated - Injured Difference	[p-value]
Panel A. Municipality level control variables									
Total homicides	11.6	5.3	6.21	-6.35	[0.249]	-5.44	[0.317]	0.91	[0.698]
log(Total homicides)	1.2	1.22	1	0.022	[0.951]	-0.197	[0.501]	-0.219	[0.446]
Homicides per 100k	12.8	29.7	34.6	16.8	[0.115]	21.8	[0.226]	4.95	[0.803]
Tenure at attack (mths)	19.7	12.4	20.4	-7.27**	[0.038]	0.671	[0.819]	7.94***	[0.004]
Avg Schooling	7.8	6.02	6.41	-1.78***	[0.001]	-1.39***	[0.001]	0.39	[0.232]
Share of indigenous pop.	11.7	22.4	17.8	10.7	[0.170]	6.13	[0.205]	-4.61	[0.523]
Pop density	222	80.3	198	-142	[0.136]	-24.3	[0.858]	118	[0.259]
Total identified crime groups	0.531	0.300	0.524	-0.231	[0.272]	-0.007	[0.974]	0.224	[0.149]
log(crime groups), known	0.283	0.208	0.278	-0.075	[0.510]	-0.006	[0.955]	0.070	[0.439]
I(New Group)	0.156	0.100	0.146	-0.056	[0.555]	-0.010	[0.896]	0.046	[0.556]
Number of New groups	0.250	0.100	0.244	-0.150	[0.280]	-0.006	[0.966]	0.144	[0.176]
Panel B. Organized criminal groups									
Beltran Leyva	0.031	0.050	0	0.019	[0.751]	-0.031	[0.316]	-0.05	[0.312]
CJNG	0.063	0	0.037	-0.063	[0.158]	-0.026	[0.590]	0.037*	[0.084]
Huachicoleros	0.031	0	0.024	-0.031	[0.324]	-0.007	[0.847]	0.024	[0.159]
Barbies	0	0.050	0.061	0.050	[0.319]	0.061**	[0.024]	0.011	[0.845]
Familia	0.094	0.100	0.073	0.006	[0.943]	-0.021	[0.730]	-0.027	[0.717]
Gulf Cartel	0.063	0	0.085	-0.063	[0.158]	-0.023	[0.668]	0.085***	[0.007]
Juarez Cartel	0.031	0	0.024	-0.031	[0.324]	-0.007	[0.847]	0.024	[0.159]
Sinaloa Cartel	0.063	0	0.073	-0.063	[0.158]	-.011	[0.838]	0.073**	[0.013]
Tijuana Cartel	0.031	0	0.037	-0.031	[0.324]	0.005	[0.887]	0.037*	[0.084]
Zetas	0.125	0	0.073	-0.125**	[0.041]	-0.052	[0.432]	0.073**	[0.013]
Other Cartels	0	0.100	0.037	0.100	[0.150]	0.0367*	[0.083]	-0.063	[0.373]
Panel C. Political affiliation of mayors									
PAN	0.156	0.100	0.171	-0.057	[0.555]	0.015	[0.851]	0.071	[0.377]
PRD	0.219	0.150	0.159	-0.069	[0.536]	-0.060	[0.476]	0.009	[0.925]
PRI	0.344	0.400	0.390	0.056	[0.691]	0.047	[0.645]	-0.010	[0.937]
MORENA	0.094	0	0.049	-0.094*	[0.081]	-0.045	[0.434]	0.049**	[0.045]
MC	0	0	0.061	0	-	0.061**	[0.024]	0.061**	[0.024]
PNA	0.031	0.050	0	0.019	[0.751]	-0.031	[0.316]	-0.050	[0.312]
PT	0.063	0	0.024	-0.063	[0.158]	-0.038	[0.414]	0.024	[0.159]
PVEM	0.063	0.150	0.024	0.088	[0.348]	-0.038	[0.414]	-0.130	[0.131]
Uso y Costumbres	0	0.100	0.110	0.100	[0.150]	0.11***	[0.002]	0.010	[0.898]

***<0.01, **<0.05, *<0.1
Variables in Panels A and B are based on the reported values from the year before the failed/successful assassinations. Party affiliations in Panel C are calculated based on the year of the failed attacks/successful assassinations. Test for pairwise differences have been obtained by regressing municipalities in each of the two groups on separate regressions. Robust standard errors are reported in parentheses, along with the p-value for the test of differences of group means in brackets.

Table A7: Balance table for party affiliations in the previous year

	(1) Near-miss			(2) Assassination			(2)-(1) Test for difference		
Variable	N	Mean	(SE)	N	Mean	(SE)	N	Difference	[p-value]
PAN	32	0.281	(0.457)	82	0.171	(0.379)	114	-0.111	[0.224]
PRD	32	0.125	(0.336)	82	0.134	(0.343)	114	0.009	[0.896]
PRI	32	0.438	(0.504)	82	0.488	(0.503)	114	0.050	[0.631]
MORENA	32	0	-	82	0.012	(0.110)	114	0.012	[0.321]
MC	32	0	-	82	0.012	(0.110)	114	0.012	[0.321]
PNA	32	0	-	82	0	-	114	0	-
PT	32	0.031	(0.177)	82	0.012	(0.110)	114	-0.019	[0.569]
PVEM	32	0.094	(0.296)	82	0.012	(0.110)	114	-0.082	[0.130]
Uso y Costumbres	32	0	-	82	0.012	(0.110)	114	0.012	[0.321]

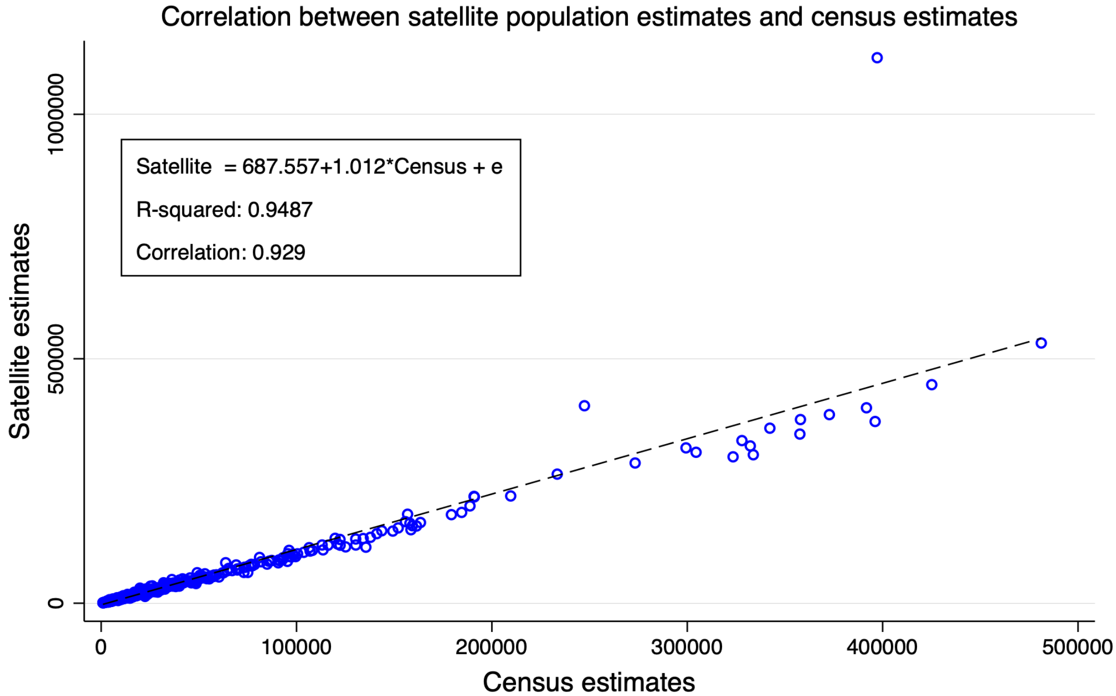
***<0.01, **<0.05, *<0.1
Party affiliations are calculated based on the year prior to the failed attacks/successful assassinations. Robust standard errors are reported in parentheses, along with the p-value for the test of differences of group means in brackets.

Table A8: Balance table for party affiliation in previous year, including municipalities with injured mayors

Variable	(1) Near-miss Mean	(2) Injured Mean	(3) Treated Mean	(2)-(1) Injured vs Near-miss Difference	[p-value]	(3)-(1) Treated vs Near-miss Difference	[p-value]	(3)-(2) Treated - Injured Difference	[p-value]
PAN	0.281	0.250	0.171	-0.031	[0.808]	-0.111	[0.224]	-0.079	[0.458]
PRD	0.125	0.300	0.134	0.175	[0.152]	0.009	[0.896]	-0.166	[0.136]
PRI	0.438	0.250	0.488	-0.188	[0.165]	0.050	[0.631]	0.238^{**}	[0.037]
MORENA	0	0	0.012	0	-	0.012	[0.321]	0.012	[0.322]
MC	0	0	0.012	0	-	0.012	[0.321]	0.012	[0.322]
PNA	0	0.050	0	0.050	[0.319]	0	-	-0.050	[0.312]
PT	0.031	0	0.012	-0.031	[0.324]	-0.019	[0.569]	0.012	[0.322]
PVEM	0.094	0.050	0.012	-0.044	[0.548]	-0.082	[0.130]	-0.038	[0.458]
Uso y Costumbres	0	0	0.012	0	-	0.012	[0.321]	0.012	[0.322]

***<0.01, **<0.05, *<0.1
Party affiliations are calculated based on the year prior to the failed attacks/successful assassinations. Test for pairwise differences have been obtained by regressing municipalities in each of the two groups on separate regressions. Robust standard errors are reported in parentheses, along with the p-value for the test of differences of group means in brackets.

Figure A2: Correlating Mexican Census and WorldPop estimates



Note: The Figure displays the correlates between WorldPop estimates (vertical axis) and Census counts (horizontal axis) for years in which both values are available - every year that ends with 5 or 0. Blue dots represent the values from sources. Black dotted line plots linear regression between the WorldPop estimates and Census counts. The regression results are reported in the box on the top left, with slope estimate being statistically significant at 1% level. R-squared from that regression and raw correlates between two data sources are also displayed in the box.

Appendix B Supplementary results and statistics for Sections 2 and 5

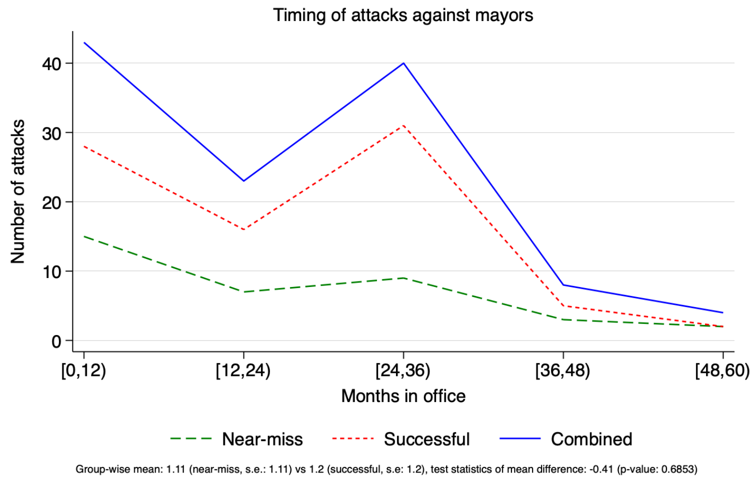
Table B1: Incidence of attacks on mayors in a given year, since 1995

	All of Mexico (Coeff × 100)					Assassination and Near-miss				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Exclude unidentified groups										
log(# groups + 1)	0.291**			0.180	0.175	0.027**			0.014	0.014
	(0.120)			(0.150)	(0.140)	(0.011)			(0.017)	(0.017)
I(New group)		0.347**		0.210	0.246		0.036**		0.025	0.029
		(0.160)		(0.200)	(0.200)		(0.018)		(0.025)	(0.025)
Homicide per million	0.011	0.011	0.011	0.011		0.001	0.001	0.001	0.001	
	(0.013)	(0.013)	(0.014)	(0.013)		(0.001)	(0.001)	(0.001)	(0.001)	
Panel B. Include unidentified groups										
log(# groups + 1)	0.494***			0.360***	0.368***	0.053***			0.033**	0.034***
	(0.100)			(0.110)	(0.110)	(0.011)			(0.013)	(0.013)
I(New group)		0.481***		0.287*	0.303**		0.055***		0.037*	0.039*
		(0.139)		(0.150)	(0.150)		(0.017)		(0.021)	(0.021)
Homicide per million	0.010	0.011	0.011	0.010		0.001	0.001	0.001	0.001	
	(0.013)	(0.014)	(0.014)	(0.013)		(0.001)	(0.001)	(0.001)	(0.001)	
N	59272	59272	59272	59272	60720	3153	3153	3153	3153	3165
Municipalities	2198	2198	2198	2198	2441	117	117	117	117	119
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

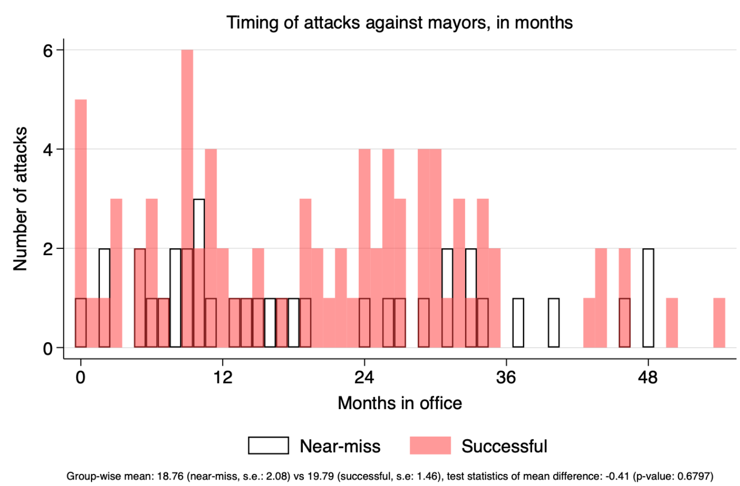
* $p < .10$, ** $p < .05$, *** $p < .01$

The table shows the coefficient estimates from the regression of the incidence of attacks on mayors on variables relevant to gang presence and crime at the municipality-year level. For the sample using all of Mexico, coefficients are multiplied by 100 for convenience. Homicides per million is recalculated to exclude cases of mayor assassinations. All regressions include municipality, year fixed effects, and controls. Control variables included are the average schooling of the municipal population, the share of the indigenous population, the log of the total population, and the year since the election (level and squared). log(# group + 1) is the log of the number of criminal groups in the municipality, adjusted by adding 1 to account for municipalities with no presence of organized criminal groups. New group refers to the dummy variable for the existence of a criminal organization that newly began its activities within the municipalities. Standard errors are clustered at the municipal level.

Figure B1: Timing of the attacks on mayors



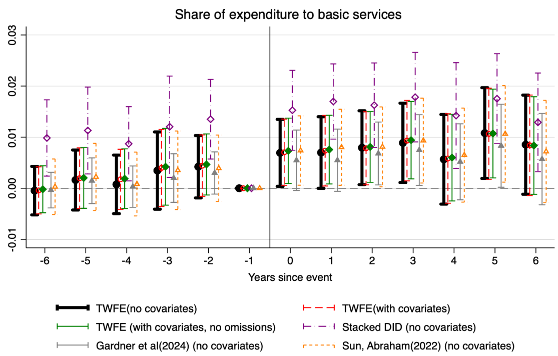
(a) Timing of attack, in terms of year in office



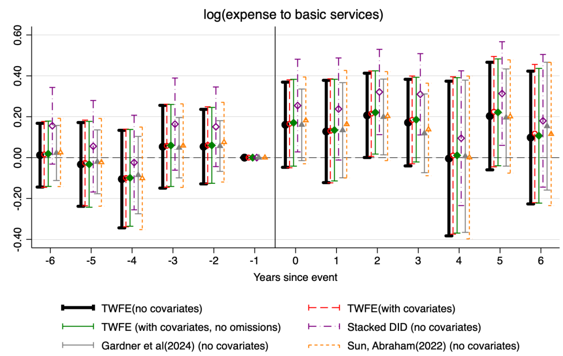
(b) Timing of attacks, in terms of months in office

Note: The graphs in this figure trace the timing of attacks that target mayors in terms of year and months in office for both cases where the assassination attempt succeeded and failed. Panel (a) traces the number of assassination attempts in terms of years while Panel (b) does so for each month in office. The notes in each paragraph show the t-test result of the difference in group-wise means. In both cases, there are no meaningful differences in the timing of the attacks against the mayors across cases where the assassinations were successful or not. The sources of the data used are based on the data collected by the authors, among others. A detailed explanation of the data is found in Section 3.

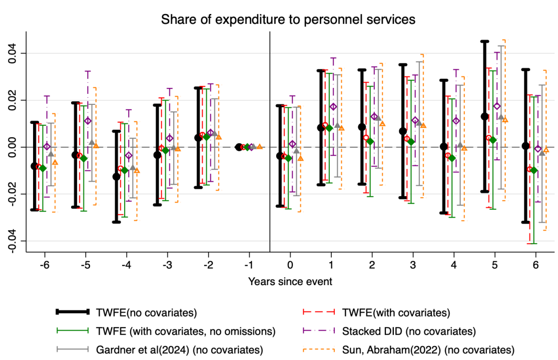
Figure B2: Shares of various expenditures across different categories



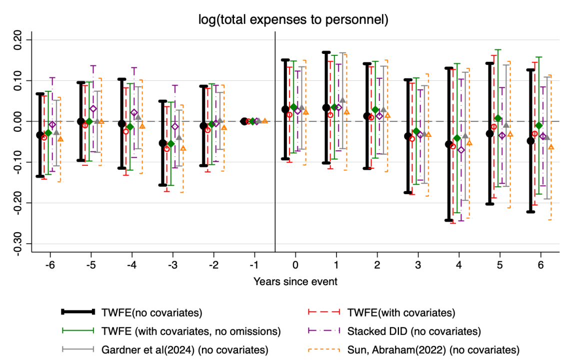
(a) Share of basic infrastructure expenses



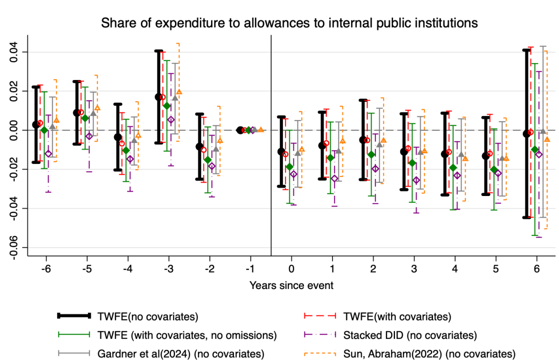
(b) log(Basic infrastructure expenses)



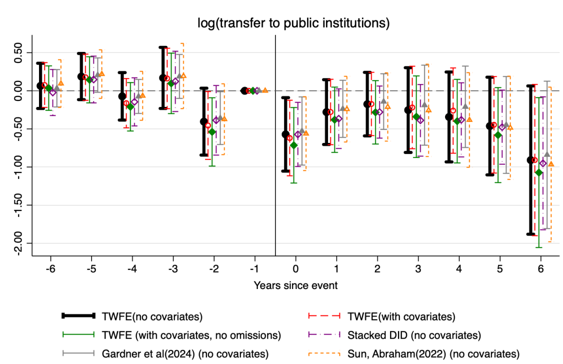
(c) Share of personnel compensation



(d) log(Personnel compensation)



(e) Allowances to internal institutions



(f) log(Allowances to internal instituitions)

Note: The figures report the event study regression on the different measures of expenditures of the municipal government. The outcome variables used in each regression are listed below each graph. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Standard errors are clustered at the municipality level.

Appendix C Framework and supplementary results for municipality personnel adjustments

In this section, I provide a theoretical framework and the derivation of the key conditions. I first derive the first-order conditions for the demand for public sector labor and the socially optimal allocation of workers across tax collection and public goods provision. Then, I show the comparative statics involving changes in productivity, value of public goods, and amenities for working in the public sector.

Subsection C.1 Setup of the framework

Consider an economy consisting of a continuum of individuals, normalized to 1, and a local government. Individuals choose between working at the local government or taking an outside option. The local government collects taxes and provides public goods using labor as an input to maximize social utility. Successful assassinations are modeled as exogenous shocks that reduce the attractiveness and productivity of working at the local government, thereby affecting the supply and allocation of labor. The framework helps rationalize empirical patterns observed in the previous section and generates testable hypotheses on the personnel dynamics of local governments.

Individuals choose the public sector if the utility it provides outweighs that of outside options. Utility from the public sector is the sum of the wage w and nonpecuniary amenity π.[1] v represents gains from outside options, drawn from a known distribution $f(v)$ with cumulative distribution $F(v)$. Individuals work for the public sector if $w + \pi \geq v$, and otherwise select the outside option. Thus, the supply of local government labor is $L = F(w + \pi)$, which is increasing in w and π, and decreasing in v.

The local government collects taxes and provides public goods to maximize social utility. Individuals derive utility from private consumption X and public good G, weighted by $\alpha > 0$, giving individual utility $\alpha G + X$ subject to $X \leq Y - T$.[2] Public sector workers earn a wage of w, while those on outside employment earn $E[v|v > w + \pi]$ on average. Local government pays each public worker

1. π can be interpreted as the pro-social sentiment that motivates individuals to serve in the government sector, as in Dal Bó et al. (2013). It can also represent nonpecuniary amenities provided by the government or work environments, such as a sense of security and work-related routines.

2. The choice of lump sum tax follows from the observation that local governments primarily levy property taxes. Income taxes are collected by state or national governments in many countries (Weingast 2009). Furthermore, this setup is sufficient to capture the idea that tax collection depends on the amount of labor allocated.

out of taxes T and other revenues R.[3] Aggregating over all individuals, the social welfare function and the government budget constraint are given by

$$\alpha G + F(w+\pi)w + (1 - F(w+\pi))E[v|v > w+\pi] - T \tag{C1}$$

$$R + T \geq wF(w+\pi) \tag{C2}$$

Local government capacity in this context is defined by its ability to produce public goods and collect taxes, each relying on allocated labor and task-specific productivity. Let $j \in \{T, G\}$ denote tax collection and public goods provision, respectively. Each task has a production function consisting of labor L_j and productivity A_j. Production functions are denoted as $t(\cdot)$ for tax collection and $g(\cdot)$ for the provision of public goods, respectively, with both increasing and concave in labor. Labor is fully allocated to either one of the two tasks.[4] The production function and allocation constraints are written as

$$T = A_T t(L_T), \;\; G = A_G g(L_G) \;\; (t' > 0, \; t'' < 0, \; g' > 0, \; g'' < 0) \tag{C3}$$

$$L_T + L_G = L \tag{C4}$$

The local government allocates labor to maximize social welfare (C1) subject to constraints (C2)-(C4). Without external shocks, the local government equates marginal costs of taxation with the marginal benefit of public goods provision (as shown in Section C.1.1 below). Successful assassination disrupts this allocation through shocks to productivity and amenities, thereby affecting available labor supply (L) and allocation across tasks (L_T and L_G).[5] Although public sector wages are inflexible in practice, I relax this condition for theoretical tractability. This setup allows for the derivation of the following comparative statics and facilitates a hypothetical exercise in which I estimate the

3. In this framework, R captures the grants from the central government. This is exogenously given in the current setup for analytical convenience. However, this amount is determined by the central government based on the revenues generated within the municipalities for places complying with fiscal federalism (Weingast 2009).

4. This is to ensure that every worker who prefers to work in the public sector gets assigned. Allowing non-assignment implies that there are unemployed workers in the model, which is the situation not addressed in this research.

5. Unsuccessful attempts may also reduce π, as municipal workers are exposed to political violence regardless of the outcome. However, the magnitude of the amenity reduction is likely smaller for failed attempts, where institutional routines and leadership remain intact. Successful assassinations produce a larger reduction in π by additionally requiring workers to adapt to new leadership and reorganized institutional routines, compounded by a negative productivity shock ($\Delta A < 0$). To the extent that the control group experiences a partial reduction in π, the estimated treatment effects in Section 5 may represent lower bounds on the full effect of losing a mayor.

counterfactual wage increase required to retain workers following successful assassinations.

Proposition 1. The effects of successful assassination on local state capacity

1. A negative productivity shock ($\Delta A_T(A_G) < 0$) decreases L_T (L_G), leading to a fall in T (G). If wages are flexible, w decreases due to decreased labor demand.

2. A negative amenity shock ($\Delta\pi < 0$) decreases labor supply, lowering both L_T and L_G downwards. This decreases T and G. If wages are flexible, w increases due to contracting supply.

Proof: Section C.1.2 below.

The framework yields three important insights. First, it explains how tax revenues and public goods provision may decline following successful assassinations. Second, it predicts that younger and more productive workers with better outside options are more likely to depart, given their lower relative utility from local government employment. Third, the framework motivates a counterfactual exercise in which I estimate the hypothetical wage increase required to retain workers in the aftermath of assassinations. I test these predictions by estimating the departure rates and retention costs of various types of workers in the next section.

C.1.1 Deriving the first order conditions

The problem of finding the allocation of labor across tax collection and public goods provision that maximizes social utility follows two steps. First, the local government determines the total amount of public labor that minimizes the cost of operations. In turn, wages w, which are assumed to be equal for both types of public workers, are determined. Then, the government maximizes the summation of individual utilities by optimally allocating workers across tax collection and public goods provision.

5. Unsuccessful attempts may also reduce π, as municipal workers are exposed to political violence regardless of the outcome. However, the magnitude of the amenity reduction is likely smaller for failed attempts, where institutional routines and leadership remain intact. Successful assassinations produce a larger reduction in π by additionally requiring workers to adapt to new leadership and reorganized institutional routines, compounded by a negative productivity shock ($\Delta A < 0$). To the extent that the control group experiences a partial reduction in π, the estimated treatment effects in Section 5 may represent lower bounds on the full effect of losing a mayor.

Cost minimization of the government: Here, the local government selects the total available labor for the public sector that minimizes its costs given its production function. In turn, this is where the wage w is determined. I use L to denote the total public sector labor, equivalent to $F(w+\pi)$. I assume that the wages across the tax collectors and the public goods providers are equal. Given this, the objective function and the production function are to minimize total expenditure on workers subject to the production function and labor allocation rule. This is written as

$$\min_{L} wL \text{ s.t. } T = A_T t(L_T).\ \ G = A_G g(L_G)$$

Here, the public sector is allocated to either one of L_T or L_G, so $L = L_G + L_T$. With this, the Lagrangian can be written as

$$wL + \lambda_T[T - A_T t(L - L_G)] + \lambda_G[G - A_G g(L - L_T)]$$

where λ_T and λ_G refer to the value of taxation and public goods to the government. Solving the first-order conditions with respect to L yields

- $[L]$: $w - \lambda_T A_T t'(L - L_G) - \lambda_G A_G g'(L - L_T) = 0$
- Complementary slackness: $\lambda_T[T - A_T t(L - L_G)] = 0, \lambda_G[G - A_G g(L - L_T)] = 0,\ \lambda_T, \lambda_G \geq 0$

Rearranging $[L]$ condition yields

$$w = \lambda_T A_T t'(L - L_G) + \lambda_G A_G g'(L - L_T)$$

In other words, public sector labor and wages are selected to satisfy the condition where the wage is equal to the weighted sum of marginal productivities across tax collection and public goods provision.

Allocating public labor to maximize social utility: Maximizing (C1) subject to (C2)-(C4), the Lagrangian can be written as

$$\max_{\{L_T, L_G\}} \alpha A_G g(L_G) + [F(w+\pi)w + (1 - F(w+\pi))E[v|v > w+\pi]] - A_T t(l_T) + \lambda[R + A_T t(L_T) - wF(w+\pi)]$$

Taking first-order conditions with respect to L_T and L_G yields

- $[L_T]$: $(\lambda - 1)A_T t'(L_T) - \alpha A_G g'(L_G) = 0$

- $[L_G]$: $\alpha A_G g'(L_G) - (\lambda - 1)A_T t'(L_T) = 0$

- Complementary slackness: $\lambda[R + A_T t(L_T) - wF(w + \pi)] = 0$ with $\lambda \geq 0$

Combining the two first-order conditions yields

$$\alpha A_G g'(L_G) = (\lambda - 1)A_T t'(L_T)$$

Here, α is the value of the public good to the society. λ is the value of taxation, with 1 subtracted to reflect that tax collection comes at a cost to private good consumption. This implies that the L_G and L_T are selected to equate the value of marginal productivity of public goods and taxation from the societal point of view. In addition, for a nonzero amount of tax collection, the condition implies that $\lambda > 1$.

C.1.2 Comparative Statics

Now I incorporate the assassination into the framework by addressing how the allocation of labor, tax collection, and public goods provision respond to the changes in the key parameters. Assassinations can negatively affect tax collection and public goods provision by introducing various inefficiencies in these operations. This is captured by the decrease in productivity A_T and A_G. In addition, assassinations can increase fear of exposure to political violence among the workers, decreasing the amenity π. The comparative statics of these changes yield the results stated in Proposition 1. The proofs are as follows.

Proof for part 1. To analyze how changes in A_T affect L_T and w, I start by applying the total derivatives to the two first-order conditions derived above.

$$w - \lambda_T A_T t'(L_T) - \lambda_G A_G g'(L - L_T) = 0$$
$$\alpha A_G g'(L - L_T) - (\lambda - 1)A_T t'(L_T) = 0$$

where I write L_G in terms of L_T by using the allocation restraint $L = L_T + L_G$. Taking total derivatives with respect to changes in A_T yields

$$\frac{dw}{dA_T} - \lambda_T A_T t''(L_T)\frac{dL_T}{dA_T} + \lambda_G A_G g''(L-L_T)\frac{dL_T}{dA_T} = \lambda_T t'(L_T)$$
$$-\alpha A_G g''(L_T)\frac{dL_T}{dA_T} - (\lambda-1)A_T t''(L_T)\frac{dL_T}{dA_T} = (\lambda-1)t'(L_T)$$

In matrix form, this can be written as

$$\underbrace{\begin{bmatrix} 1 & -\lambda_T A_T t''(L_T) + \lambda_G A_G g''(L-L_T) \\ 0 & -\alpha A_G g''(L-L_T) - (\lambda-1)A_T t''(L_T) \end{bmatrix}}_{=X} \begin{bmatrix} \frac{dw}{dA_T} \\ \frac{dL_T}{dA_T} \end{bmatrix} = \begin{bmatrix} \lambda_T t'(L_T) \\ (\lambda-1)t'(L_T) \end{bmatrix}$$

From here, I invoke the implicit function theorem to get the solutions for $\frac{dw}{dA_T}$ and $\frac{dL_T}{dA_T}$. Obtaining the inverse function of X, I solve

$$\begin{bmatrix} \frac{dw}{dA_T} \\ \frac{dL_T}{dA_T} \end{bmatrix} = \frac{1}{\det(X)} \begin{bmatrix} -\alpha A_G g''(L-L_T) - (\lambda-1)A_T t''(L_T) & \lambda_T A_T t''(L_T) - \lambda_G A_G g''(L-L_T) \\ 0 & 1 \end{bmatrix} \begin{bmatrix} \lambda_T t'(L_T) \\ (\lambda-1)t'(L_T) \end{bmatrix}$$

where $\det(X) = -(\alpha A_G g''(L-L_T) + (\lambda-1)A_T t''(L_T)) > 0$ ($t''(\cdot) < 0, g''(\cdot) < 0$). From these, we can obtain

$$\frac{dL_T}{dA_T} = \frac{(\lambda-1)t'(L_T)}{\det(X)} > 0$$
$$\frac{dw}{dA_T} = \frac{A_G(-g''(L-L_T))t'(L_T)[\alpha\lambda_T + (\lambda-1)\lambda_G]}{\det(X)} > 0$$

since $\lambda > 1, \alpha > 0$ for nonzero taxation and public goods and the complementary slackness conditions implies $\lambda_T \geq 0$, $\lambda_G \geq 0$. Thus, changes in A_T shift L_T and w in the same direction, implying that negative shocks to A_T after successful assassination decrease L_T and w. Consequentially, tax collection decreases relative to the pre-assassination equilibrium (marked with asterisk)

$$T = A_T t(L_T) < A_T{}^* t(L_T{}^*) = T^*$$

Similar logic can be applied to identifying changes in L_G and w in response to exogenous changes in A_G. Writing the total derivatives with respect to A_G for the first order conditions in matrix yields

$$\underbrace{\begin{bmatrix} 1 & \lambda_T A_T t''(L-L_G) - \lambda_G A_G g''(L_G) \\ 0 & \alpha A_G g''(L_G) + (\lambda-1)A_T t''(L-L_G) \end{bmatrix}}_{=W} \begin{bmatrix} \frac{dw}{dA_G} \\ \frac{dL_G}{dA_G} \end{bmatrix} = \begin{bmatrix} \lambda_G g'(L_G) \\ -\alpha g'(L_G) \end{bmatrix}$$

Invoking the implicit function theorem, I can write

$$\begin{bmatrix} \frac{dw}{dA_G} \\ \frac{dL_G}{dA_G} \end{bmatrix} = \frac{1}{\det(W)} \begin{bmatrix} \alpha A_G g''(L_G) + (\lambda-1)A_T t''(L-L_G) & -\lambda_T A_T t''(L-L_G) + \lambda_G A_G g''(L_G) \\ 0 & 1 \end{bmatrix} \begin{bmatrix} \lambda_G g'(L_G) \\ -\alpha g'(L_G) \end{bmatrix}$$

with $\det(W) = \alpha A_G g''(L_G) + (\lambda-1)A_T t''(L-L_G) < 0$. Given these,

$$\begin{aligned} \frac{dL_G}{dA_G} &= \frac{-\alpha g'(L_G)}{\det(W)} > 0 \\ \frac{dw}{dA_G} &= \frac{A_T t''(L-L_G) g'(L_G)[\alpha\lambda_T + (\lambda-1)\lambda_G]}{\det(W)} > 0 \end{aligned}$$

With changes in A_G shifting L_G and w in the same direction, negative shocks to A_G from successful assassinations decrease wages and L_G. As a result, public goods are under-provided compared to pre-assassination equilibrium (marked with asterisk)

$$G = A_G g(L_G) < {A_G}^* g({L_G}^*) = G^*$$

■

Proof for part 2. π enters the framework through the labor supply of the public sector. Specifically

$$L = F(w+\pi) = \Pr(v \leq w+\pi) = \int_{-\infty}^{w+\pi} f(v)dv$$

To differentiate this with respect to π, I use the fundamental theorem of calculus.

$$\begin{aligned} \frac{d}{d\pi}\int_{-\infty}^{w+\pi} f(v)dv &= \frac{d}{d\pi}\left[F(w+\pi) - F(-\infty)\right] \\ &= \frac{d}{d\pi}\left[F(w+\pi)\right] \\ &= f(w+\pi) > 0 \end{aligned}$$

This implies that public sector labor supply changes in the same direction as π. Thus, decreases in π due to successful assassinations decrease the labor supply.

To see how this changes the allocation of labor across L_T and L_G, I return to the first-order conditions from the social utility maximization problem

$$\alpha A_G g'(L_G) = (\lambda - 1) A_T t'(L_T)$$

By taking total derivatives with respect to π, I obtain

$$\alpha A_G g''(L_G) \frac{dL_G}{d\pi} - (\lambda - 1) A_T t''(L_T) \frac{dL_T}{d\pi} = 0$$

which can be written as

$$\frac{dL_G/d\pi}{dL_T/d\pi} = \frac{(\lambda - 1) A_T t''(L_T)}{\alpha A_G g''(L_G)} > 0$$

The last inequality is justified by the fact that $t''(\cdot) < 0, g''(\cdot) < 0$ from the concavity of the production functions and that $\alpha > 0, \lambda > 1$, a condition imposed for nonzero production of public goods and tax collection. This rules out the case where L_T and L_G changes in the opposite direction with respect to π without any productivity changes. Thus, in the case of a successful assassination that drives the public sector labor supply downward, both L_T and L_G face downward pressure.

With fewer L_T and L_G compared to the pre-assassination equilibrium (denoted with an asterisk), the total tax collected and the public goods supplied decrease.

$$T = A_T t(L_T) < A_T t(L_T^*) = T^*$$
$$G = A_G g(L_G) < A_G g(L_G^*) = G^*$$

As for wages, I return to the first-order condition on the cost minimization problem.

$$w = \lambda_T A_T t'(L_T) + \lambda_G A_G g'(L_G)$$

Taking total derivatives with respect to π yields

$$\frac{dw}{d\pi} = \lambda_T A_T t''(L_T) \frac{dL_T}{d\pi} + \lambda_G A_G g''(L_G) \frac{dL_G}{d\pi} < 0$$

where the last inequality comes from the fact that $\frac{dL_j}{d\pi} > 0$ for $j \in \{T, G\}$, $t''(\cdot) < 0$, $g''(\cdot) < 0$, and $\lambda_T \geq 0$, $\lambda_G \geq 0$ from the complementary slackness conditions in the first order conditions. Thus, w and π move in opposite directions, implying that a decrease of π from successful assassinations induces upward pressure on w. ■

Effectively, changes in A_T and A_G act similarly to labor demand shock, whereas changes to π mimics labor supply shock.

Subsection C.2 Calculating the hypothetical retention cost of young workers

Based on the insights above, I test whether treated municipalities lose productive workers and estimate the wage increases required to retain them. Among others, I use age as a proxy for worker productivity, given its correlation with potential labor market earnings in the Mexican private sector. To support this approach, I construct wage profiles by age using the individual-level earnings data from ENOE. I use both hourly wages and monthly earnings to capture the relative returns to outside options v across different age groups.[6]

I take the following two approaches to highlight the relationship between earnings and age. First, I compute average earnings by age group and find that individuals in their 30s and 40s have the highest earnings (Panels (a) and (b) in Figure C1). Second, I estimate the following regression to recover age-earnings profiles net of unobserved heterogeneity across industries, municipalities, and periods:

$$y_{imjt} = \alpha + \sum_{G} \beta_G I[i \in G] + \phi_j + \gamma_m + \delta_t + \epsilon_{imjt} \quad (G \in \{\text{20s, 30s, 40s, 50s, 60s, 70s}\}) \tag{C5}$$

$I[i \in G]$ indicates that individual i belongs in age group G. Fixed effects for industries (ϕ_j), time (δ_t), and municipalities (γ_m) are included. The β_G coefficients capture average earnings differentials across age groups, holding sectoral, geographical, and temporal factors constant. The estimates show that those in their 30s and 40s are at the peak of the earnings profile (Panels (c) and (d) in Figure C1), supporting the use of age for proxies of higher productivity and stronger outside options.

Then, I examine whether workers in their 30s and 40s are more likely to leave following successful assassinations. The outcome variables are age-specific share of municipal workers - 20s, 30s, 40s, and

6. The educational attainment data for municipal workers are only available from the 4th wave of the CNGMD, limiting the statistical power. Age, on the other hand, is consistently available in this dataset.

50s or above.[7] I modify Equation (2) to accommodate biennial frequency and shorter timeframe of data availability by reducing lags and leads. Other features of Equation (2) are preserved.

Figure C2 presents the baseline two-way fixed effects estimates for worker departures, with post-assassination averages reported in Panel A of Table C2. The share of workers in their 30s drops by 8.7 percentage points, a 27.6% drop from the pre-assassination average. Similarly, the combined share of workers in their 30s and 40s falls by 13.3 percentage points. These findings are similar across alternative specifications (Figure C2 and Appendix Tables C3-C6), confirming the higher likelihood of departure following successful assassinations for younger and more productive workers.

The retention cost is defined as the (hypothetical) increase in wages required to retain municipal workers following the amenity shock from successful assassinations. Drawing from the labor supply framework discussed earlier, I assume that outside options v remain constant. Since v reflects nation-wide averages, they are less likely to be directly affected by localized political violence. Using total derivatives, I model the trade-off in wages and amenities as follows:

$$\frac{dw}{d\pi} = \frac{-\frac{\partial L(w,\pi)}{\partial \pi}}{\frac{\partial L(w,\pi)}{\partial w}} \tag{C6}$$

I use my estimates on the departure of workers along with labor supply elasticity from Dal Bó et al. (2013) to compute retention costs. In Equation (C6), the numerator captures the reduction in labor following successful assassinations, as estimated in Panel A of Table C2. The denominator represents the elasticity of labor supply with respect to wages. For this, I use the benchmark estimate of 2.15 from a field experiment with Mexican municipal workers conducted by Dal Bó et al. (2013).[8]

The calculated retention costs for workers in their 30s are approximately 11% (Panel B of Table C2), and remain robust across alternative specifications (Appendix Tables C3-C6). Though hypothetical, these estimates help quantify the cost of political violence and the organizational disruptions that arise when a replacement mayor assumes office. In addition, retention costs are higher for workers in their 30s compared to other age groups, reflecting greater outside options and productivity. Overall, the findings suggest that the weakening local state capacity is driven in part by changes in personnel following successful assassinations.

7. The first two waves of CNGMD do not include distinct categories for the 60s and 70s

8. The municipalities studied in Dal Bó et al. (2013) and mine differ. Using a different indicator of violence, Dal Bó et al. (2013) finds that labor supply elasticity could be lower in violent municipalities. Thus, the wage cost estimates presented here may be a lower bound of the true cost.

Subsection C.3 Supplementary results

Table C1: Hypothetical wage costs of retaining departing workers by age group, TWFE w/o covariates

	(1) 20s	(2) 30s	(3) 40s	(4) 50s	(5) 20-30s	(6) 30-40s
Panel A. Change in proportion of workers by age						
Change in share	-0.016	-0.069^{**}	-0.035	0.021	-0.086^{**}	-0.105^{**}
	(0.032)	(0.034)	(0.033)	(0.029)	(0.042)	(0.045)
Pre-event share (1=100%)	0.220	0.314	0.252	0.214	0.534	0.566
% change in size due to π (1=100%)	-0.073	-0.220	-0.139	0.098	-0.161	-0.186
Panel B. Wage-amenity tradeoff with Dal Bó et al. (2013) elasticity estimate (2.15)						
Trade-off rate	-0.034	-0.102	-0.065	0.046	-0.075	-0.086
N	663	663	663	663	663	663
Municipalities	115	115	115	115	115	115
Municipality FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Covariates						

* $p < .10$, ** $p < .05$, *** $p < .01$
This table reports the estimates of the rate of increase in wages required to retain different types of municipal workers. The first row in Panel A reports the point estimates and the standard errors of the average post-assassination treatment effects for the proportion of each age group within municipal governments specified in the header of each column. Results are obtained using two-way fixed effects without covariates analogous to Equation (2). Standard errors are clustered at the municipal level and reported in parentheses. The second row is obtained from taking the average of the proportion of these workers one period before the assassination attempt took place. Numbers in the third row are obtained by dividing the point estimates in the first row by the same in the second row. This represents the change in the number of workers in each category before and after the assassination attempts. In Panel B, the wage-amenity trade-off rate is calculated by dividing the percent change in size of workers obtained from Panel A with changes in labor supply with respect to wages from Dal Bó et al. (2013), 2.15. This represents the increase in wages needed to keep workers employed. Given that this cost arises from a decrease in amenities due to assassinations and the fear of political violence that follows it, it quantifies the cost of political violence to the local government.

Table C2: Hypothetical wage costs of retaining departing workers by age group, TWFE with covariates

	(1) 20s	(2) 30s	(3) 40s	(4) 50s	(5) 20-30s	(6) 30-40s
Panel A. Change in proportion of workers by age						
Change in share	−0.013	−0.085**	−0.045	0.039	−0.098**	−0.130***
	(0.033)	(0.036)	(0.036)	(0.033)	(0.046)	(0.046)
Pre-event share (1=100%)	0.220	0.314	0.252	0.214	0.534	0.566
% change in size due to π (1=100%)	−0.059	−0.271	−0.178	0.182	−0.184	−0.230
Panel B. Wage-amenity tradeoff with Dal Bó et al. (2013) elasticity estimate (2.15)						
Trade-off rate	−0.028	−0.126	−0.083	0.085	−0.085	−0.107
N	652	652	652	652	652	652
Municipalities	113	113	113	113	113	113
Municipality FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$
This table reports the estimates of the rate of increase in wages required to retain different types of municipal workers. The first row in Panel A reports the point estimates and the standard errors of the average post-assassination treatment effects for the proportion of each age group within municipal governments specified in the header of each column. Results are obtained using two-way fixed effects and covariates analogous to Equation (2). Standard errors are clustered at the municipal level and reported in parentheses. The second row is obtained from taking the average of the proportion of these workers one period before the assassination attempt took place. Numbers in the third row are obtained by dividing the point estimates in the first row by the same in the second row. This represents the change in the number of workers in each category before and after the assassination attempts. In Panel B, the wage-amenity trade-off rate is calculated by dividing the percent change in size of workers obtained from Panel A with changes in labor supply with respect to wages from Dal Bó et al. (2013), 2.15. This represents the increase in wages needed to keep workers employed. Given that this cost arises from a decrease in amenities due to assassinations and the fear of political violence that follows it, it quantifies the cost of political violence to the local government.

Table C3: Hypothetical wage costs of retaining departing workers by age group, TWFE with covariates and outlier municipalities

	(1) 20s	(2) 30s	(3) 40s	(4) 50s	(5) 20-30s	(6) 30-40s
Panel A. Change in proportion of workers by age						
Change in share	-0.015	-0.078^{**}	-0.041	0.043	-0.092^{**}	-0.118^{***}
	(0.033)	(0.036)	(0.035)	(0.032)	(0.045)	(0.046)
Pre-event share (1=100%)	0.220	0.314	0.252	0.214	0.534	0.566
% change in size due to π (1=100%)	-0.068	-0.249	-0.163	0.201	-0.172	-0.209
Panel B. Wage-amenity tradeoff with Dal Bó et al. (2013) elasticity estimate (2.15)						
Trade-off rate	-0.032	-0.116	-0.076	0.093	-0.080	-0.097
N	676	676	676	676	676	676
Municipalities	117	117	117	117	117	117
Municipality FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$

This table reports the estimates of the rate of increase in wages required to retain different types of municipal workers. The first row in Panel A reports the point estimates and the standard errors of the average post-assassination treatment effects for the proportion of each age group within municipal governments specified in the header of each column. Results are obtained using two-way fixed effects and covariates analogous to Equation (2), without dropping outlier municipalities. Standard errors are clustered at the municipal level and reported in parentheses. The second row is obtained from taking the average of the proportion of these workers one period before the assassination attempt took place. Numbers in the third row are obtained by dividing the point estimates in the first row by the same in the second row. This represents the change in the number of workers in each category before and after the assassination attempts. In Panel B, the wage-amenity trade-off rate is calculated by dividing the percent change in size of workers obtained from Panel A with changes in labor supply with respect to wages from Dal Bó et al. (2013), 2.15. This represents the increase in wages needed to keep workers employed. Given that this cost arises from a decrease in amenities due to assassinations and the fear of political violence that follows it, it quantifies the cost of political violence to the local government.

Table C4: Hypothetical wage costs of retaining departing workers by age group, Stacked DID

	(1) 20s	(2) 30s	(3) 40s	(4) 50s	(5) 20-30s	(6) 30-40s
Panel A. Change in proportion of workers by age						
Change in share	−0.043	−0.063**	−0.009	0.039	−0.106***	−0.071*
	(0.030)	(0.032)	(0.030)	(0.024)	(0.040)	(0.038)
Pre-event share (1=100%)	0.220	0.314	0.252	0.214	0.534	0.566
% change in size due to π (1=100%)	−0.195	−0.201	−0.036	0.182	−0.199	−0.126
Panel B. Wage-amenity tradeoff with Dal Bó et al. (2013) elasticity estimate (2.15)						
Trade-off rate	−0.091	−0.093	−0.017	0.085	−0.092	−0.058
N	4342	4342	4342	4342	4342	4342
Municipality FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Covariates						

* $p < .10$, ** $p < .05$, *** $p < .01$
This table reports the estimates of the rate of increase in wages required to retain different types of municipal workers. The first row in Panel A reports the point estimates and the standard errors of the average post-assassination treatment effects for the proportion of each age group within municipal governments specified in the header of each column. Results are obtained using stacked DID without covariates analogous to Equation (2). Standard errors are clustered at the municipal level and reported in parentheses. The second row is obtained from taking the average of the proportion of these workers one period before the assassination attempt took place. Numbers in the third row are obtained by dividing the point estimates in the first row by the same in the second row. This represents the change in the number of workers in each category before and after the assassination attempts. In Panel B, the wage-amenity trade-off rate is calculated by dividing the percent change in size of workers obtained from Panel A with changes in labor supply with respect to wages from Dal Bó et al. (2013), 2.15. This represents the increase in wages needed to keep workers employed. Given that this cost arises from a decrease in amenities due to assassinations and the fear of political violence that follows it, it quantifies the cost of political violence to the local government.

Table C5: Hypothetical wage costs of retaining departing workers by age group, Gardner (2024)

	(1) 20s	(2) 30s	(3) 40s	(4) 50s	(5) 20-30s	(6) 30-40s
Panel A. Change in proportion of workers by age						
Change in share	−0.014	−0.042***	−0.072***	0.025*	−0.057***	−0.114***
	(0.017)	(0.016)	(0.017)	(0.013)	(0.022)	(0.021)
Pre-event share (1=100%)	0.220	0.314	0.252	0.214	0.534	0.566
% change in size due to π (1=100%)	−0.064	−0.134	−0.286	0.117	−0.107	−0.202
Panel B. Wage-amenity tradeoff with Dal Bó et al. (2013) elasticity estimate (2.15)						
Trade-off rate	−0.030	−0.062	−0.133	0.054	−0.050	−0.094
N	663	663	663	663	663	663
Municipalities	115	115	115	115	115	115
Municipality FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Covariates						

* $p < .10$, ** $p < .05$, *** $p < .01$

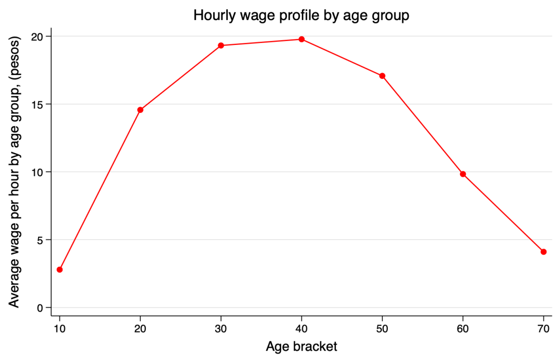
This table reports the estimates of the rate of increase in wages required to retain different types of municipal workers. The first row in Panel A reports the point estimates and the standard errors of the average post-assassination treatment effects for the proportion of each age group within municipal governments specified in the header of each column. Results are obtained using Gardner (2024) estimates without covariates analogous to Equation (2). Standard errors are clustered at the municipal level and reported in parentheses. The second row is obtained from taking the average of the proportion of these workers one period before the assassination attempt took place. Numbers in the third row are obtained by dividing the point estimates in the first row by the same in the second row. This represents the change in the number of workers in each category before and after the assassination attempts. In Panel B, the wage-amenity trade-off rate is calculated by dividing the percent change in size of workers obtained from Panel A with changes in labor supply with respect to wages from Dal Bó et al. (2013), 2.15. This represents the increase in wages needed to keep workers employed. Given that this cost arises from a decrease in amenities due to assassinations and the fear of political violence that follows it, it quantifies the cost of political violence to the local government.

Table C6: Hypothetical wage costs of retaining departing workers by age group, Sun-Abraham (2021)

	(1) 20s	(2) 30s	(3) 40s	(4) 50s	(5) 20-30s	(6) 30-40s
Panel A. Change in proportion of workers by age						
Change in share	0.013	−0.018	−0.020	-0.004	−0.004	−0.037
	(0.028)	(0.048)	(0.028)	(0.031)	(0.045)	(0.046)
Pre-event share (1=100%)	0.220	0.314	0.252	0.214	0.534	0.566
% change in size due to π (1=100%)	0.059	−0.057	−0.079	−0.019	−0.007	−0.065
Panel B. Wage-amenity tradeoff with Dal Bó et al. (2013) elasticity estimate (2.15)						
Trade-off rate	0.028	−0.027	−0.037	−0.009	−0.003	−0.030
N	663	663	663	663	663	663
Municipalities	115	115	115	115	115	115
Municipality FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Covariates						

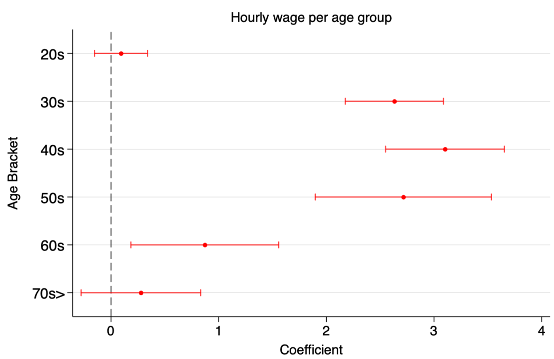
* $p < .10$, ** $p < .05$, *** $p < .01$
This table reports the estimates of the rate of increase in wages required to retain different types of municipal workers. The first row in Panel A reports the point estimates and the standard errors of the average post-assassination treatment effects for the proportion of each age group within municipal governments specified in the header of each column. Results are obtained using Sun and Abraham (2021) estimates without covariates analogous to Equation (2). Standard errors are clustered at the municipal level and reported in parentheses. The second row is obtained from taking the average of the proportion of these workers one period before the assassination attempt took place. Numbers in the third row are obtained by dividing the point estimates in the first row by the same in the second row. This represents the change in the number of workers in each category before and after the assassination attempts. In Panel B, the wage-amenity trade-off rate is calculated by dividing the percent change in size of workers obtained from Panel A with changes in labor supply with respect to wages from Dal Bó et al. (2013), 2.15. This represents the increase in wages needed to keep workers employed. Given that this cost arises from a decrease in amenities due to assassinations and the fear of political violence that follows it, it quantifies the cost of political violence to the local government.

Figure C1: Outside opportunities peak for those in 30s and 40s

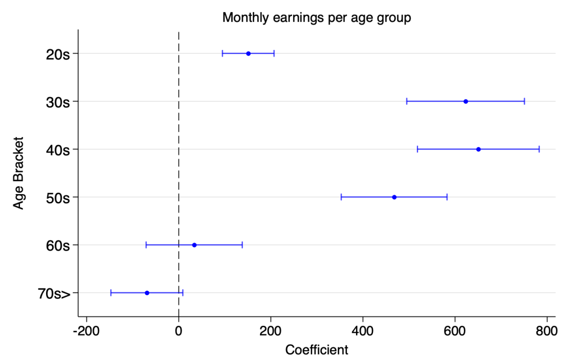


(a) Average hourly wage per age group

(b) Average monthly earnings per wage group



(c) Age premium net of industry, time, municipality FE for hourly wage

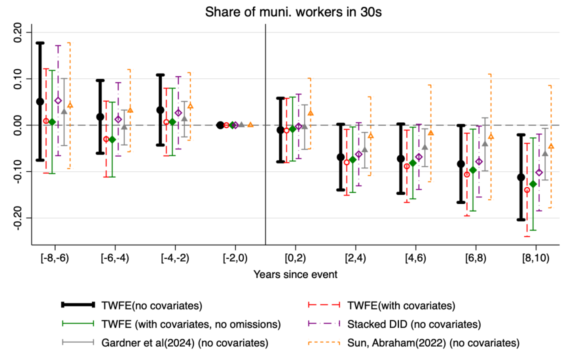


(d) Age premium net of industry, time, municipality FE for monthly earnings

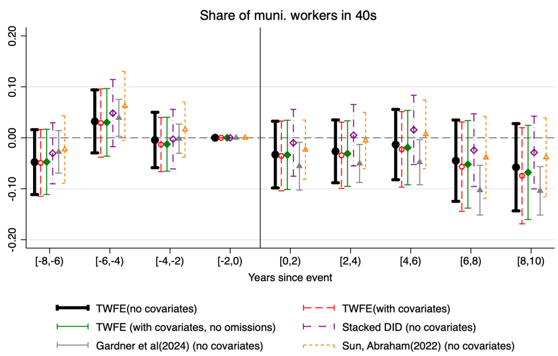
Note: The figure depicts the summary statistics for labor earnings by each age group, sourced from the National Survey on Occupation and Employment (ENOE) from INEGI. Panels (a) and (b) report the average hourly wage and monthly earnings per age group whose municipality of residence is included in the same group of municipalities in the regressions. Panels (c) and (d) report the regression coefficients for the dummies in the age group from the regression that uses each labor earnings as an outcome and includes fixed effects for industry, year, quarter of survey, and municipality. Respondents in their 10s were used as a benchmark group. The figures in Panels (c) and (d) also include a 95% confidence interval with standard errors clustered at the municipal level.

Figure C2: Changes in the size and age composition of municipal workers

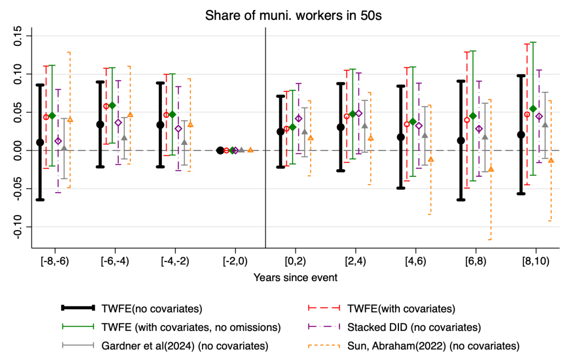
(a) Share of municipality workers in 20s



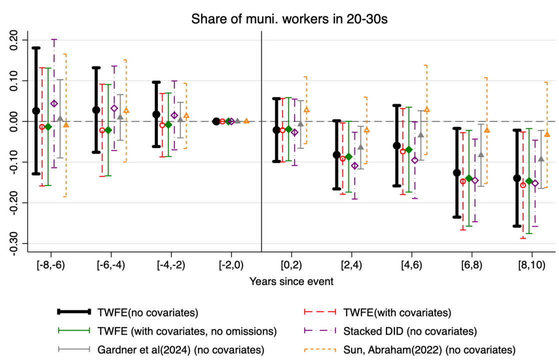
(b) Share of municipality workers in 30s



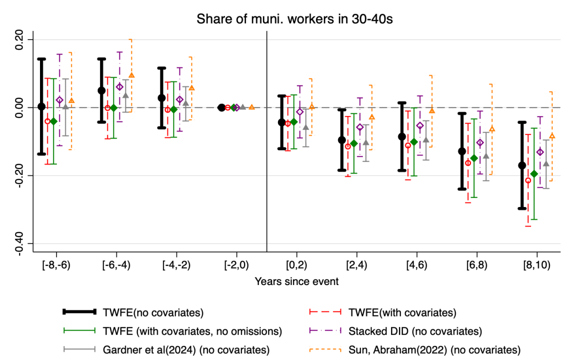
(c) Share of municipality workers in 40s



(d) Share of municipality workers in 50s



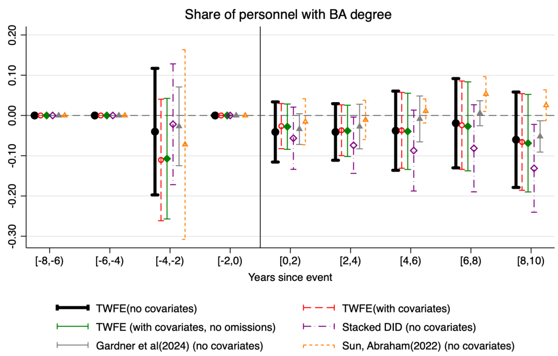
(e) Share of municipality workers in 20s-30s



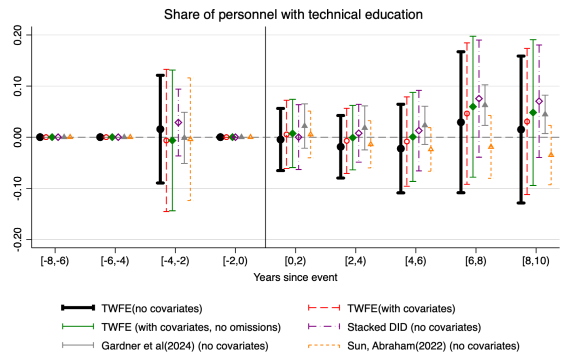
(f) Share of municipality workers in 30s-40s

Note: The figures report the event study regression on the composition of workers by age group. The outcome variables are calculated relative to the total number of municipal workers. All regressions include a binned indicator for municipalities experiencing assassinations beyond the event timing window, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are clustered at the municipality level.

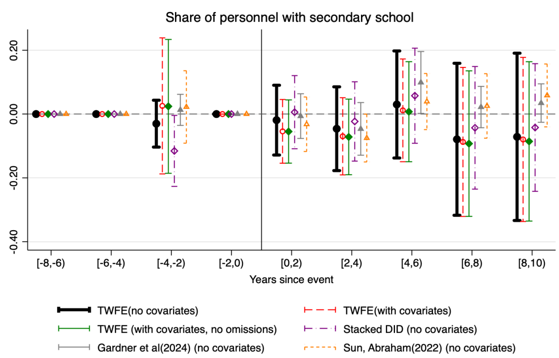
Figure C3: Allocation of municipal workers by highest educational attainment



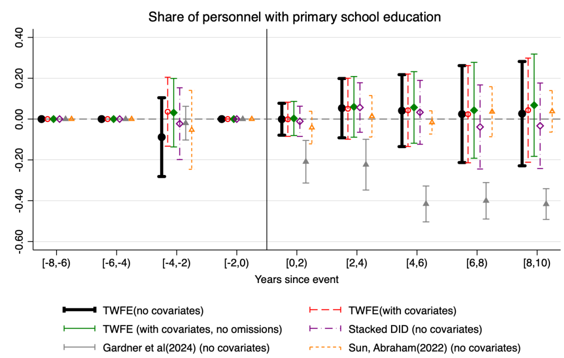
(a) %Municipal workers with BA degree



(b) %Municipal workers with 2-year college



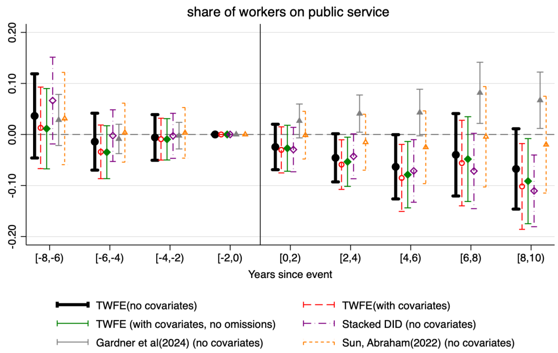
(c) %Municipal workers with secondary education



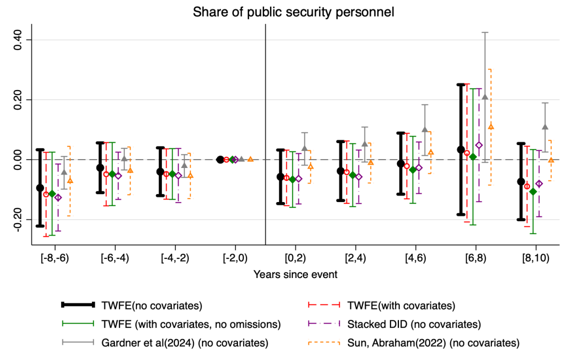
(d) %Municipal workers with primary education

Note: The figures report the event study regression on the composition of workers by the highest educational attainment. The outcome variables are share of each category of education calculated relative to the total number of municipal workers. All regressions include a binned indicator for municipalities experiencing assassinations beyond the event timing window, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are clustered at the municipality level.

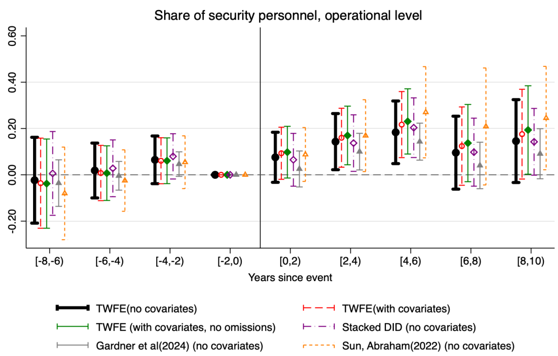
Figure C4: Allocation of municipal workers by type of work



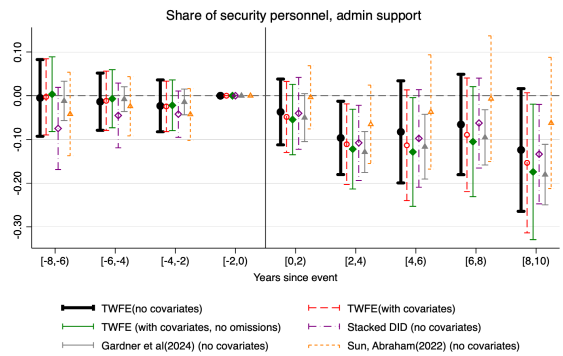
(a) %Municipal workers in public service



(b) %Municipal workers in public security



(c) %Public security workers in operative tasks



(d) %Public security workers in admin duties

Note: The figures report the event study regression on the composition of workers by the type of duties they conduct. The outcome variables for Panels (a) and (b) are calculated relative to the total number of municipal workers. The outcome variable for panels (c) and (d) are measured relative to the total number of workers on public security duties, including, but not limited to the municipal police and relevant committee members. All regressions include a binned indicator for municipalities experiencing assassinations beyond the event timing window, municipality fixed effects, and year fixed effects. Stacked DID regression includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are clustered at the municipality level.

Appendix D Supplementary regression results for Section 6

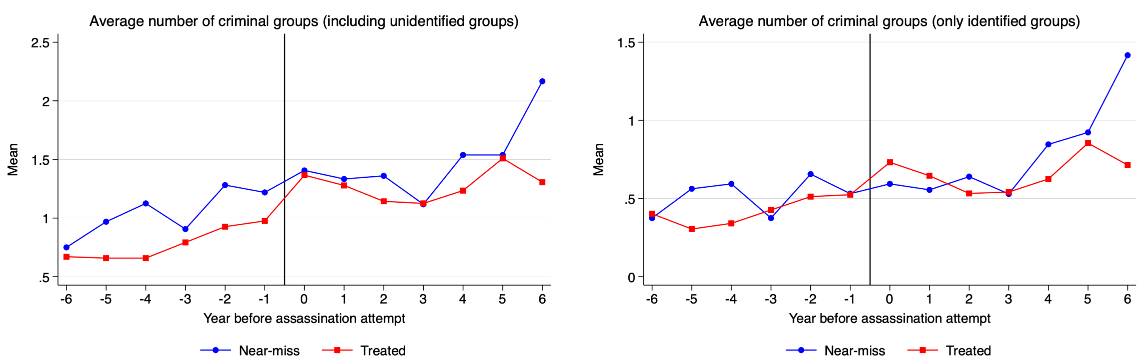
Subsection D.1 Results on security environments

Table D1: Distribution of organized criminal groups across treated and near-miss municipalities

	Near-miss		Treated	
	Pre-attempt	Post-attempt	Pre-attempt	Post-attempt
W/ unidentified groups	0.393	1.421	0.266	1.315
	(0.847)	(1.380)	(0.760)	(1.457)
Only identified groups	0.181	0.601	0.144	0.685
	(0.616)	(1.270)	(0.571)	(1.269)

Note: The table reports the mean and standard deviation of the number of organized criminal groups across near-miss and treated municipalities before and after assassination attempts. The first row includes criminal groups that did appear in the organized criminal group datasets used in the text but has not been identified. In this case, unknown criminal groups are considered as a unique group different from identified groups. The second row only includes criminal groups that has been identified. Standard deviations are reported in parentheses.

Figure D1: Trends in criminal group presence over time

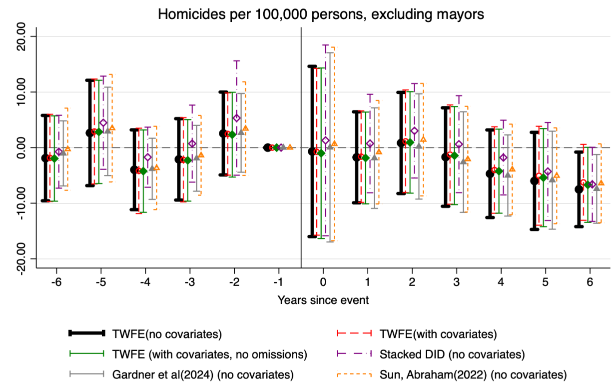


(a) W/ unidentified groups

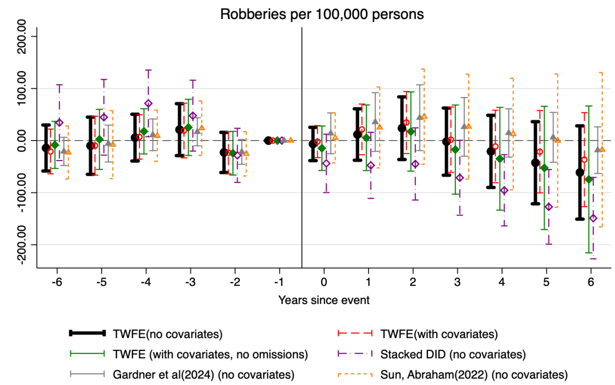
(b) Only with identified groups

Note: The figures report the changes in the mean of the number of criminal groups 6 years before and after assassination attempts. The choice of timeframe matches the timing indicators in the event study regression results. Red lines indicate averages for the treated municipalities, while blue ones represent those for near-miss municipalities. The graph on the left also includes cases where the criminal group is not identified, while the one on the right limits the calculation to those that have been identified.

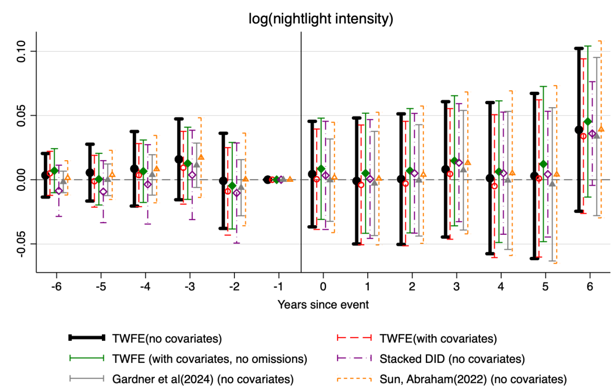
Figure D2: Organized criminal group presence and security environments



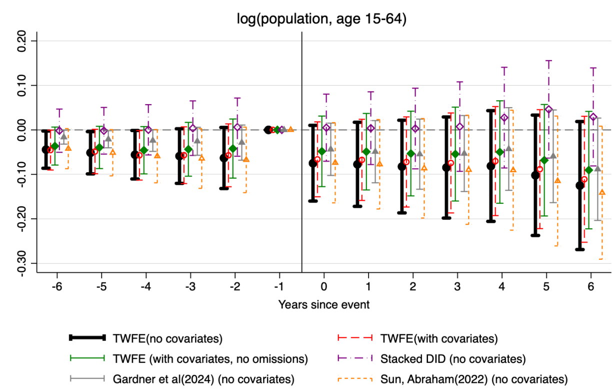
(a) log(number of criminal groups +1)



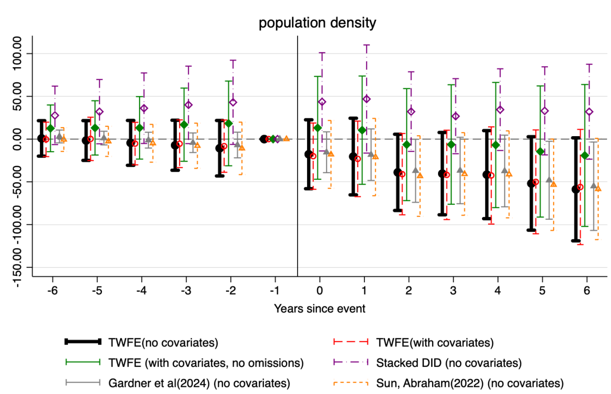
(b) Indicator for entry of new criminals



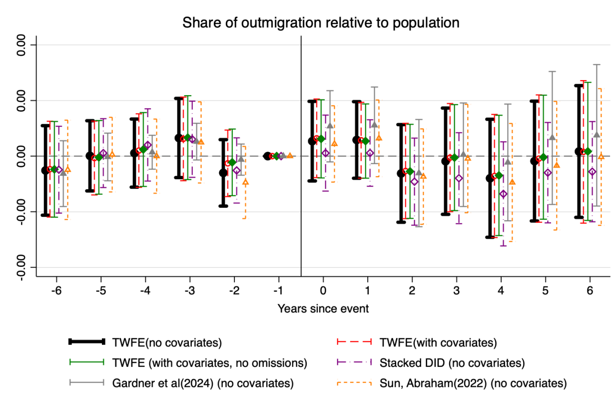
(c) Indicator for multiple criminal group presence



(d) Share of all public security spending



(e) Share of spending to security personnel



(f) Federal police presence

Note: The figures report the event study regression on the different measures of gang presence. The outcome variables used in each regression are specified in the sub-caption for each figure. Specifications used for each plot and 95% confidence intervals are listed in the bottom of each graph. Each regression includes fixed effects for years and municipalities, but no covariates. Standard errors are clustered at the municipality level.

Table D2: Organized Criminal Group Presence and Security Environment Post-assassinations

	Criminal group presence			Security environment		
	(1)	(2)	(3)	(4)	(5)	(6)
Estimation	ln (OCG+1)	I[New OCG]	I[Many OCG]	% Security	% Security personnel	Federal Police
TWFE, no covariates	0.101*	0.063*	0.059*	0.001	0.002	0.054
	(0.060)	(0.032)	(0.035)	(0.003)	(0.003)	(0.068)
Holm p-value	[0.182]	[0.153]	[0.182]	[0.999]	[0.999]	[0.999]
N	3105	3105	3105	2759	2750	1955
Municipalities	115	115	115	115	115	115
TWFE, with covariates	0.115*	0.071**	0.065*	0.001	0.001	0.043
	(0.059)	(0.032)	(0.035)	(0.003)	(0.003)	(0.065)
Holm p-value	[0.104]	[0.087]	[0.104]	[0.999]	[0.999]	[0.999]
N	3053	3053	3053	2711	2702	1921
Municipalities	115	115	115	113	113	113
TWFE, with covariates	0.111*	0.069**	.060*	0.002	0.003	0.027
(no omitted municipalities)	(0.057)	(0.032)	(0.035)	(0.003)	(0.003)	(0.064)
Holm p-value	[0.108]	[0.093]	[0.108]	[0.999]	[0.999]	[0.999]
N	3161	3161	3161	2819	2810	1989
Municipalities	119	119	119	117	117	117
Stacked DID, no covariates	0.114**	0.087***	0.080***	0.002	0.003	0.036
	(0.048)	(0.026)	(0.030)	(0.002)	(0.002)	(0.029)
Holm p-value	[0.018]	[0.003]	[0.016]	[0.564]	[0.564]	[0.564]
N	23936	24684	24684	23184	23105	12716
Clusters	748	748	748	748	748	748
Gardner (2024), no covariates	0.071	0.057**	0.053	0.003	0.003	0.040
	(0.053)	(0.027)	(0.032)	(0.003)	(0.003)	(0.057)
Holm p-value	[0.210]	[0.111]	[0.210]	[0.873]	[0.873]	[0.873]
N	3105	3105	3105	2759	2750	1955
Municipalities	115	115	115	115	115	115
Sun-Abraham (2021), no covariates	0.094	0.065**	0.060*	0.001	0.002	0.055
	(0.059)	(0.031)	(0.036)	(0.003)	(0.003)	(0.066)
Holm p-value	[0.333]	[0.114]	[0.291]	[0.999]	[0.999]	[0.999]
N	3105	3105	3105	2759	2750	1955
Municipalities	115	115	115	115	115	115
Control mean	0.664	0.156	0.156	0.004	0.002	0.846
Municipality FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$ based on p-values not adjusted for multiple hypotheses testing.
The table reports the average of the 6-year post-assassination indicators in Equation (2). Each row contains results from different estimation methods. The outcome variables used in each regression are the log of number of organized criminal groups plus one, indicator for new or multiple criminal group presence, share of security expenditures, share of expenditures of public security personnel, and indicator for presence of federal police operations. Control mean reports the average of the outcome variables for the near-miss municipalities one year before the assassination attempts. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression also includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are reported in parenthesis and clustered at the municipality level. Holm-adjusted p-values for multiple hypothesis testing are reported in brackets. Adjustment is performed separately for organized criminal presence (three variables) and security environment (three variables). Results are qualitatively similar if Bonferroni adjustments are applied.

Subsection D.2 Results from the individual level outcomes on ENOE surveys

In this section, we introduce some suggestive and descriptive evidence that supports the finding that differences in economic activities are not the alternative mechanisms behind the main findings in the paper. For this purpose, we use the individual-level responses from the quarterly ENOE surveys such as earnings, working hours, employment status, and sector of employment matched with the municipality of residence. Thus, the following equation that leverages variation at an individual-municipality-time period level is used.

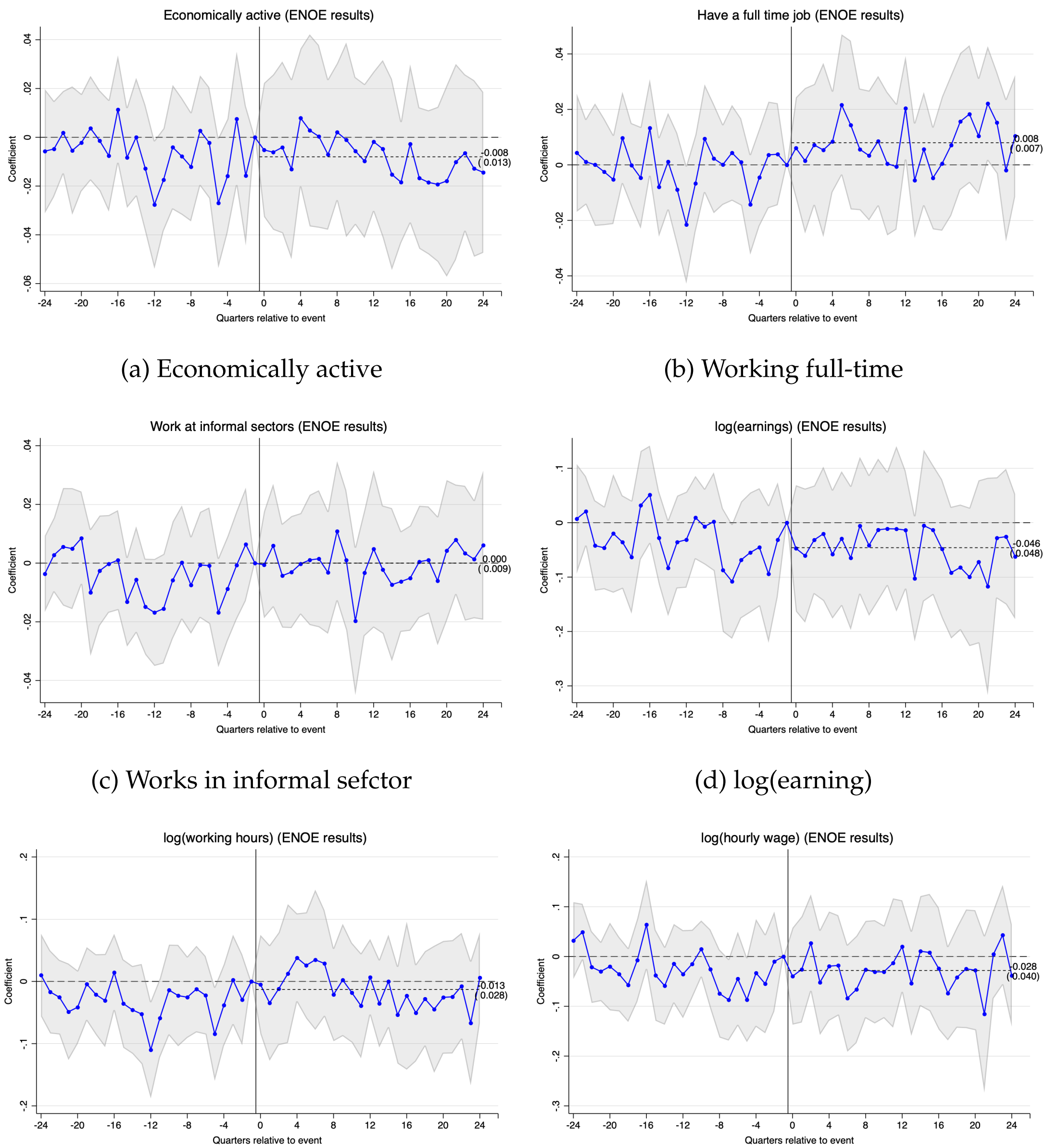
$$y_{imt} = \alpha + \sum_{\substack{h=-24 \\ h\neq -1}}^{24} \tau_h I[t - \text{assassination} = h]_{mt} + \tau_{25^+} I[t - \text{assassination} \geq 25]_{mt} + \gamma_m + \delta_t + \varepsilon_{imt}$$

y_{imt} are individual-levle outcomes of interest. Treatment indicators now have 24 windows to measure the dynamic effects 24 quarters (6 calendar years, as in the main text) before and after the event. Treatment indicators equal 1 if individual i resides in municipality m that experienced successful assassinations at quarter t . The regression includes municipality fixed effects (γ_m) and quarter fixed effects (δ_t). Standard errors are clustered at the municipality level.

It should be noted that the results presented in this section should be interpreted as descriptive and suggestive evidence, not causal. As this is a repeated survey of different individuals at the same municipalities over time, individual-level fixed effects cannot be included. Therefore, the regression does not take unobservable characteristics that may affect outcomes into account. Rather, the results are intended to provide descriptive trends to various economic activity indicators following successful assassination and complement the findings using nightlight data in Section 6.

In Figure D3, we report the outcomes on being economically active, working full-time, working in an informal sector, log(weekly earnings), log(working hours per week), and log(hourly wage). There are no significant changes in these outcomes following a successful assassination attempt. These suggest that the economic activities are not different among municipalities with successful assassinations relative to those experiencing near-miss events. Thus, the finding that there are minimal changes in economic activities stated in Section 6 still stands.

Figure D3: Changes in economic activity measured through individual ENOE survey results



(a) Economically active

(b) Working full-time

(c) Works in informal sefctor

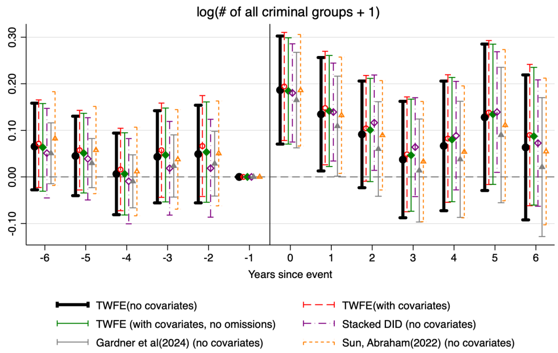
(d) log(earning)

(e) log(working hour)

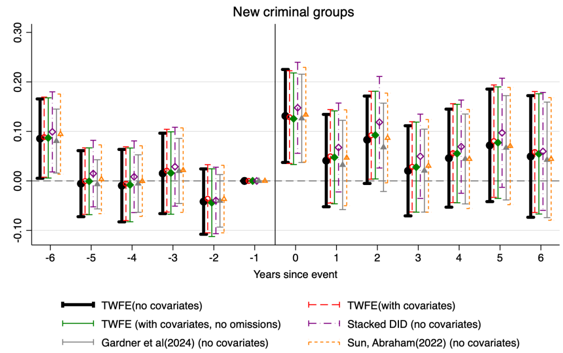
(f) log(hourly wage)

Note: The figures report the event study regression using the equation mentioned in Appendix Section D.2. The outcome variables are specified as a caption for each picture. Regression includes fixed effects at the municipality and year-quarter level. The average effects of the assassinations 24 quarters after the event and their standard errors are also reported at the right-hand side in each figure. Standard errors are clustered at the municipality level.

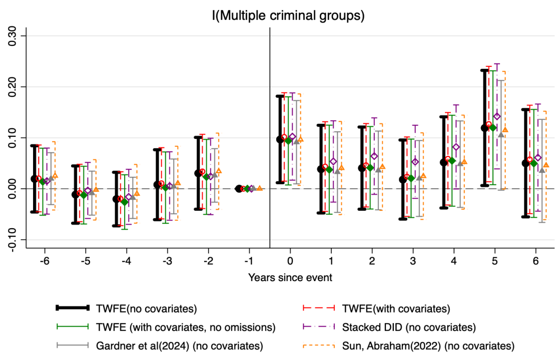
Figure D4: Testing for alternative mechanisms



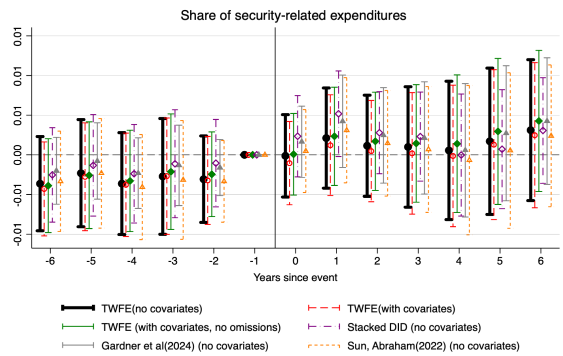
(a) Homicide rates per 100,000 people



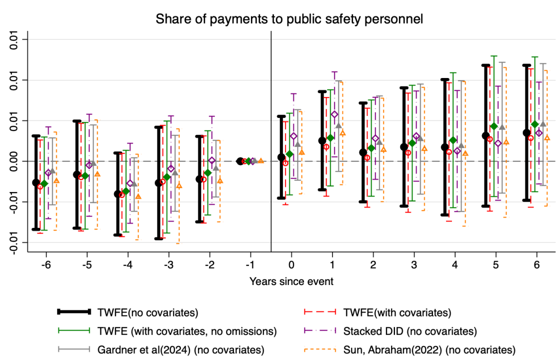
(b) Robberies per 100,000 people



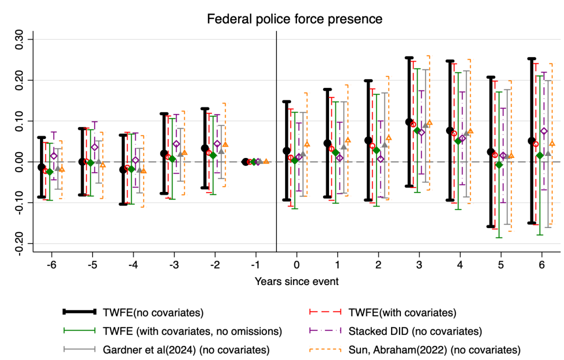
(c) log(nightlights intensity)



(d) log(municipal population aged 15-64)



(e) Population density



(f) Share of municipal outmigrants to the US

Note: The figures report the event study regression results to test for the existence of alternative mechanisms. The measures of homicides in Panel (a) are recalculated by omitting the assassination of a mayor. Panel (b) dates from 2011. Nightlight variables in panel (c) are sourced from harmonizing the nightlight intensities from DMSP (1995-2013) and VIIRS (2014-2021), as in Appendix Section A.5. The working age population in panel (d) and population density in panel (e) are from the WorldPop (2000 and after) and the Mexican Census (pre-2000). Outmigration data in panel (f) is from the Institute of Mexicans Abroad (IME) and dates from 2008. The regression setup and fixed effects are the same as Equation (2). Standard errors are clustered at the municipality level.

Subsection D.3 Effects on internal migration to different municipalities

Table D3: Changes in internal migration

	(1) log(internal migrants)	(2) % internal migrants	(3) log(internal migrants)	(4) % internal migrants
Post	0.026	0.006	0.079	0.020
	(0.150)	(0.020)	(0.159)	(0.021)
N	537	537	527	527
Municipalities	115	115	113	113
Control mean	5.230	0.112	5.230	0.112
Covariates			✓	✓
Municipality FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓

* $p < .10$, ** $p < .05$, *** $p < .01$
This table reports the estimates of the patterns in the internal migration to different municipalities, obtained from the Mexican Census. Columns (1) and (3) report the log of the number of internal migrants since the previous censuis, or in the last five years. Columns (2) and (4) presents the share of the municipal population in the last census (5 years ago) that have moved to a different municipality afterwards. Columns (3) and (4) include the same set of covariates used in Section 5. Municipality and time fixed effects are included in all specifications. Standard errors are clustered at the municipal level and reported in parentheses.

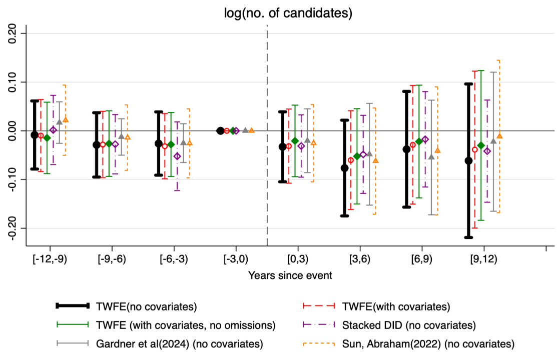
Table D4: Testing for alternative mechanisms

	Economics and demographics				Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Estimation	ln (nightlight)	ln (pop, 15-64)	Pop density	% Outmigrant	Robbery, 100k	Homicide, 100k
TWFE, no covariates	0.008	-0.090	-38.50	0.000	-13.90	-3.070
	(0.025)	(0.057)	(23.50)	(0.001)	(28.30)	(3.160)
Holm p-value	[0.999]	[0.416]	[0.416]	[0.999]	[0.666]	[0.666]
N	3105	2974	2415	1495	1104	3051
Municipalities	115	115	115	115	115	113
TWFE, with covariates	0.004	-0.078	-39.20	0.000	-1.900	-2.550
	(0.024)	(0.056)	(25.70)	(0.001)	(28.20)	(3.310)
Holm p-value	[0.999]	[0.516]	[0.516]	[0.999]	[0.946]	[0.888]
N	3045	2924	2373	1469	1084	3045
Municipalities	113	113	113	113	113	113
TWFE, with covariates	0.014	-0.059	-4.260	0.000	-24.40	-2.810
(no omitted municipalities)	(0.023)	(0.052)	(34.70)	(0.001)	(42.50)	(3.300)
Holm p-value	[0.999]	[0.999]	[0.999]	[0.999]	[0.792]	[0.792]
N	3153	3028	2457	1521	1128	3153
Municipalities	117	117	117	117	117	117
Stacked DID, no covariates	0.010	0.017	35.60	-0.001	-82.70***	-1.000
	(0.020)	(0.047)	(25.10)	(0.001)	(29.40)	(3.480)
Holm p-value	[0.999]	[0.999]	[0.628]	[0.999]	[0.010]	[0.774]
N	22440	22633	15708	9724	7625	23328
Clusters	748	748	748	748	748	729
Gardner (2024), no covariates	0.005	-0.056	-35.40**	0.000	17.50	-3.220
	(0.021)	(0.043)	(18.00)	(0.001)	(22.00)	(3.460)
Holm p-value	[0.999]	[0.576]	[0.192]	[0.999]	[0.706]	[0.706]
N	3105	2974	2415	1495	1104	3051
Municipalities	115	115	115	115	115	113
Sun-Abraham (2021), no covariates	0.010	-0.096	-39.40*	0.000	14.10	-2.320
	(0.025)	(0.059)	(23.40)	(0.001)	(47.40)	(3.280)
Holm p-value	[0.999]	[0.368]	[0.368]	[0.999]	[0.960]	[0.960]
N	3105	2974	2415	1495	1104	3051
Municipalities	115	115	115	115	115	113
Control mean	2.190	10.20	222	0.012	278	12.80
Municipality FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

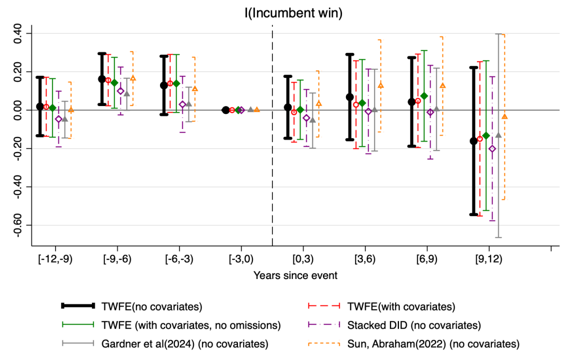
* $p < .10$, ** $p < .05$, *** $p < .01$ based on p-values not adjusted for multiple hypotheses testing.
The table reports the average of the 6-year post-assassination indicators in Equation (2). Each row contains results from different estimation methods. The outcome variables used in each regression are the log of nightlights, log of municipal population aged 15-64, population density, share of outmigrants to the United States, robbery per 100,000 persons, and homicides per 100,000 persons recalculated to exclude assassinations of mayors. Control mean reports the average of the outcome variables for the near-miss municipalities one year before the assassination attempts. All regressions include a binned indicator for municipalities experiencing assassinations 7 or more years ago, municipality fixed effects, and year fixed effects. Stacked DID regression also includes state-specific yearly linear trends to account for different weights across yearly subdatasets used to create the estimator. Two-way fixed effect regressions with covariates include controls for log(number of criminal organizations + 1), homicide rates, log(total homicides + 1), average years of schooling for the municipal population, the share of the indigenous population, and years since the most recent election (level and squared) fixed at the final pre-assassination attempt year. Other estimators do not include covariates. Standard errors are reported in parenthesis and clustered at the municipality level. Holm-adjusted p-values for multiple hypothesis testing are reported in brackets. Adjustment is performed separately for economic and demographic outcomes (four variables) and crime outcomes (two variables). Results are qualitatively similar if Bonferroni adjustments are applied.

Subsection D.4 Effects on electoral environment post-assassinations

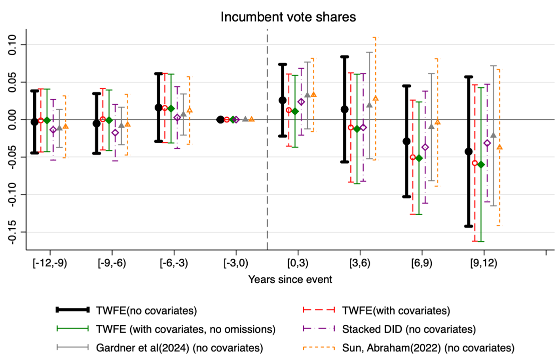
Figure D5: Effects on electoral outcomes



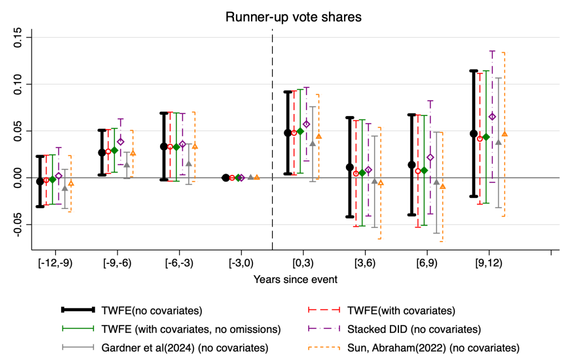
(a) log(number of mayoral candidates)



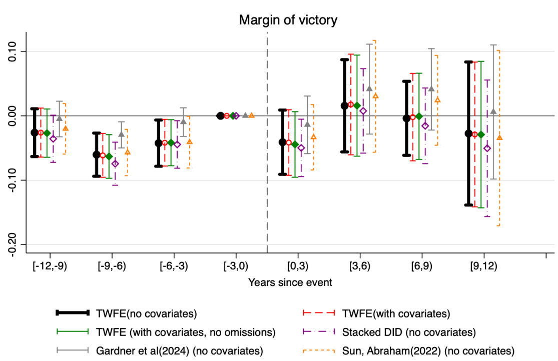
(b) Victory by incumbent party



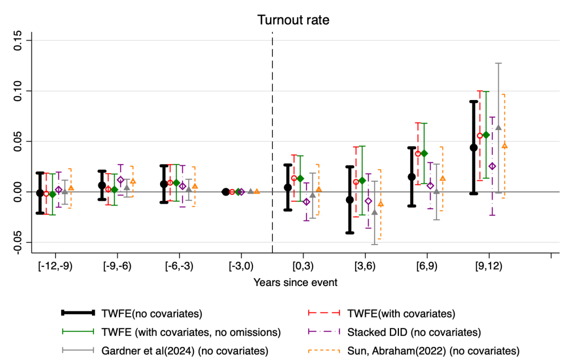
(c) Incumbent party vote share



(d) Runner up vote share



(e) Victory margin (by percentage points)

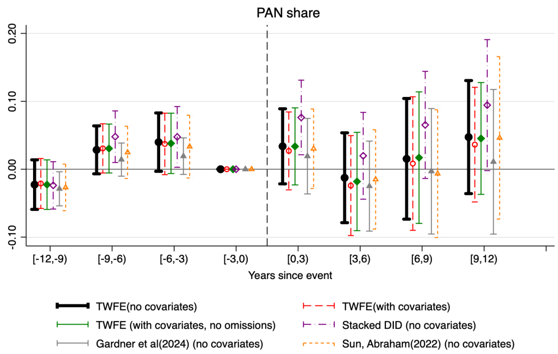


(f) Participation rate

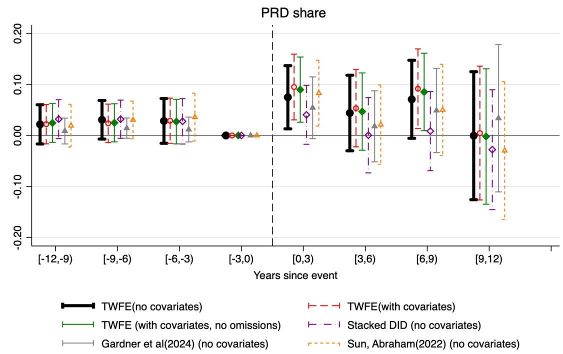
Note: The figures report the event study regression on electoral outcomes using Equation (2) but with different binning of time indicators, as elections occur once every three years. Regression uses the same control variables and fixed effects as in Section 5. Standard errors are clustered at the municipality level.

Subsection D.5   Effects on electoral outcomes by party post-assassinations

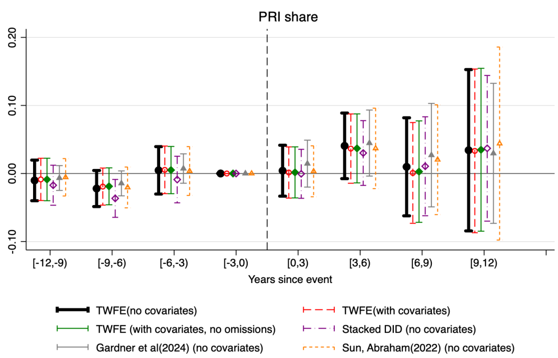
Figure D6: Effects on electoral outcomes



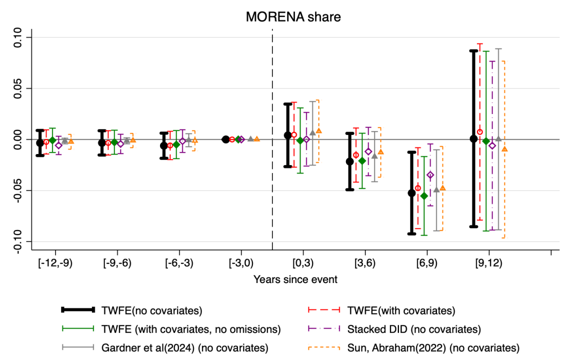
(a) PAN



(b) PRD



(c) PRI

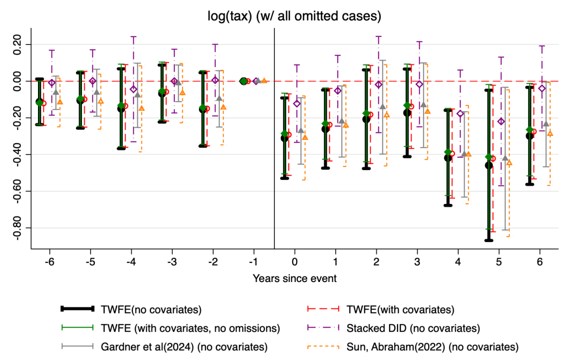


(d) MORENA

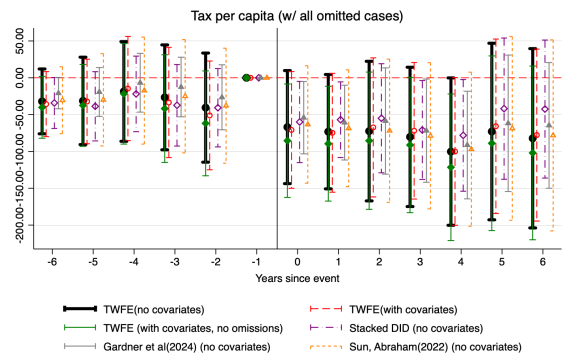
Note: The figures report the event study regression on electoral outcomes by party using Equation (2) but with different binning of time indicators, as elections occur once every three years. Regression uses the same control variables and fixed effects as in Section 5. Standard errors are clustered at the municipality level.

Appendix E Additional robustness tests

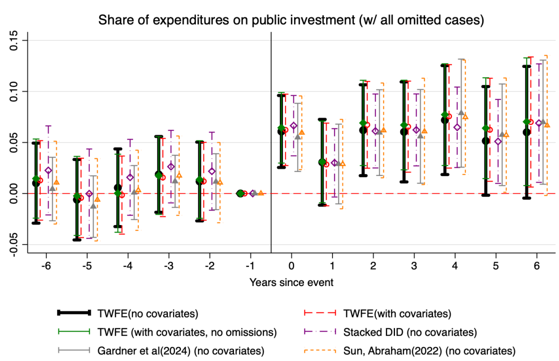
Figure E1: Regression results including attacks at public events and municipal properties



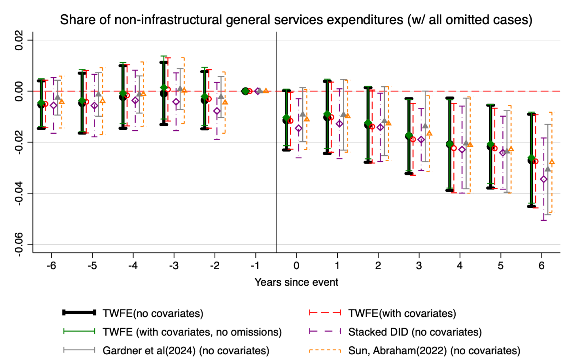
(a) log(tax revenues)



(b) Per capita tax revenues



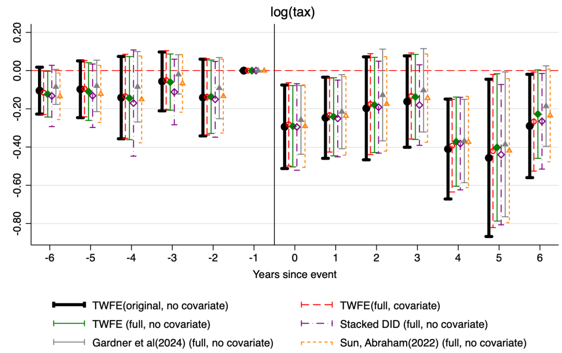
(c) Share of public investment



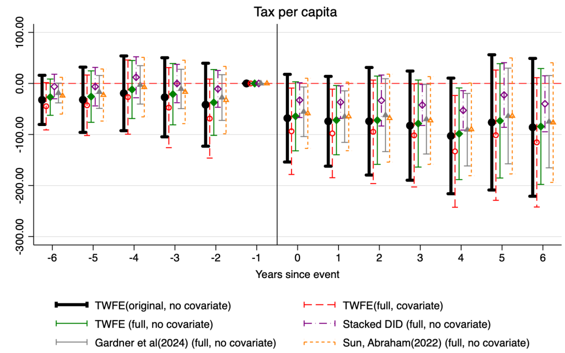
(d) Share of non-infra services

Note: The figures report the event study regression which compares municipalities with successful assassinations to near-miss municipalities. Unlike the main regressions reported in Section 5, this regression includes all cases of failed attacks that includes attacks at public events and municipal properties that were omitted for unclear intention of the attacker to actually kill the mayor or cause fear to general public. The outcome variables used in each regression are listed below each graph. Covariate and fixed effects setup are identical to results reported in Section 5. Standard errors are clustered at the municipality level.

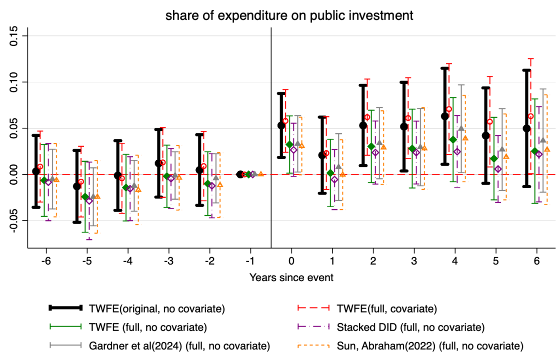
Figure E2: Regression with kidnappings, attacks on family members, and threats in control group



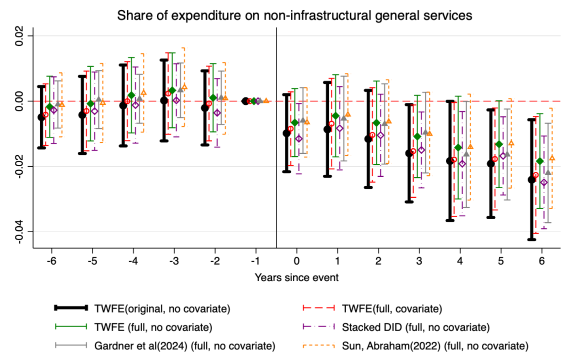
(a) log(tax revenues)



(b) Per capita tax revenues



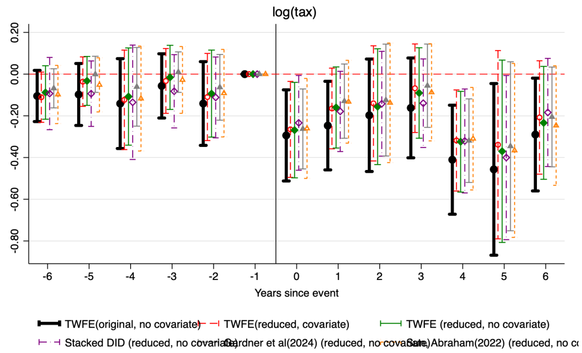
(c) Share of public investment



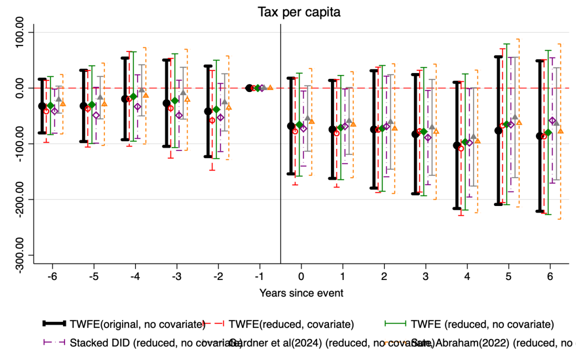
(d) Share of non-infra services

Note: The figures report the event study regression including places where mayors were kidnapped, threatened, or had family members attacks. " Full" refers to the these expanded set of municipalities. The baseline results with the main sample are replicated in green line for reference. The outcome variables used in each regression are listed below each graph. All covariates and fixed effect setups are identical to the main text. Standard errors are clustered at the municipality level.

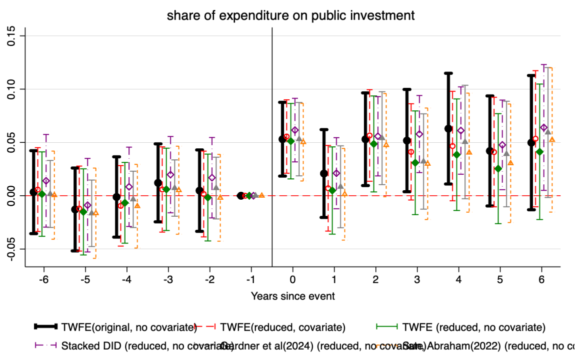
Figure E3: Regression without municipalities experiencing multiple attacks against mayors



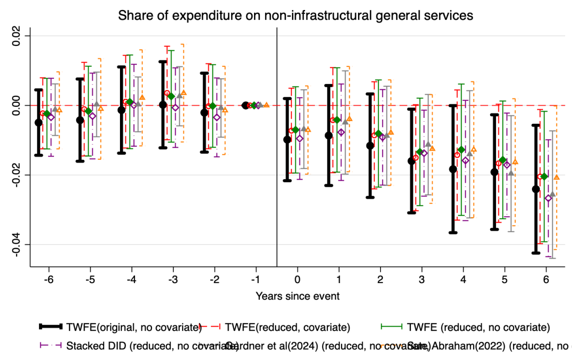
(a) log(tax revenues)



(b) Per capita tax revenues



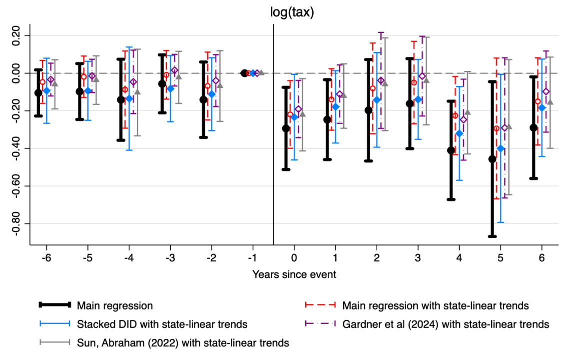
(c) Share of public investment



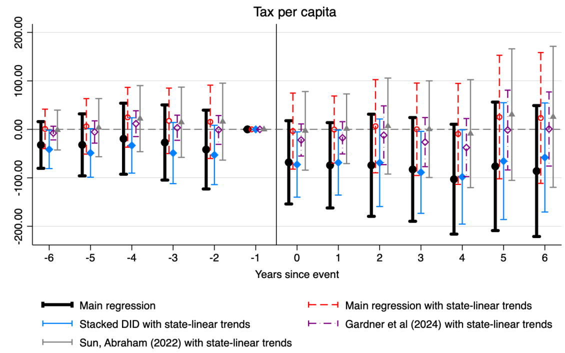
(d) Share of non-infra services

Note: The figures report the event study regression excluding places where multiple attacks against mayors were reported. " Reduced" refers to the restricted sample were municipalities with multiple attacks are omitted. The baseline results with the main sample are replicated in green line for reference. The outcome variables used in each regression are listed below each graph. All covariates and fixed effect setups are identical to the main text. Standard errors are clustered at the municipality level.

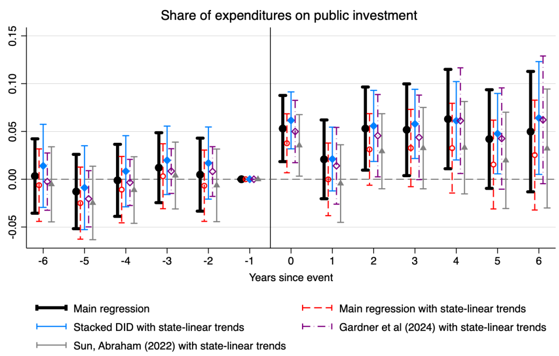
Figure E4: Alternative fixed effects setting: State-specific linear trends



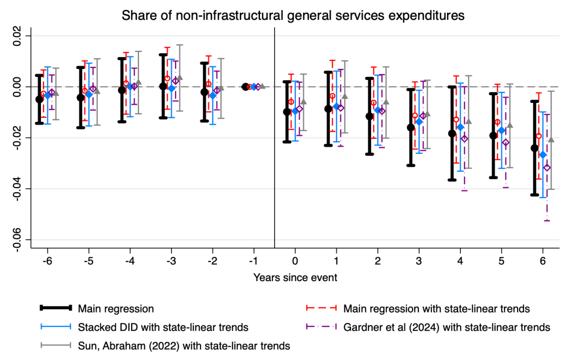
(a) log(tax revenues)



(b) Per capita tax revenues



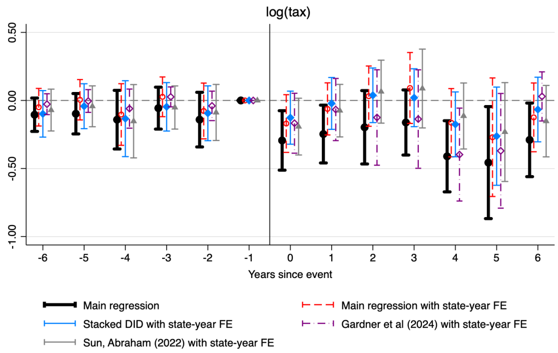
(c) Share of public investment



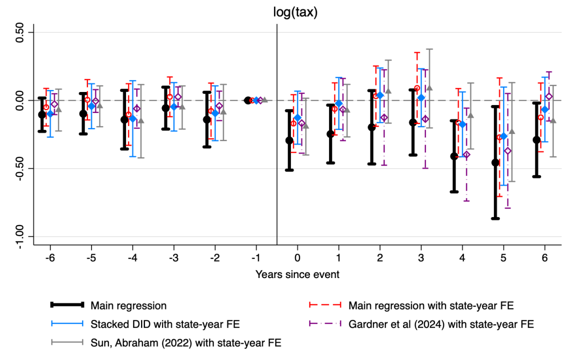
(d) Share of non-infra services

Note: The figures report the event study regression which compares municipalities with successful assassinations to near-miss municipalities. The baseline results with the main sample that uses two-way fixed effects are replicated in green line for reference. Other lines represent results that also includes state-specific linear trends. The outcome variables used in each regression are listed below each graph. The covariates are not included in the regression. Standard errors are clustered at the municipality level.

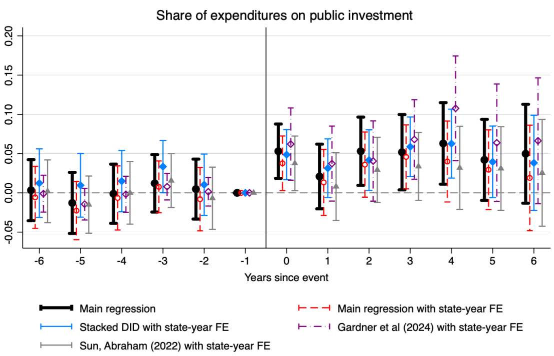
Figure E5: Alternative fixed effects setting: State-year fixed effects



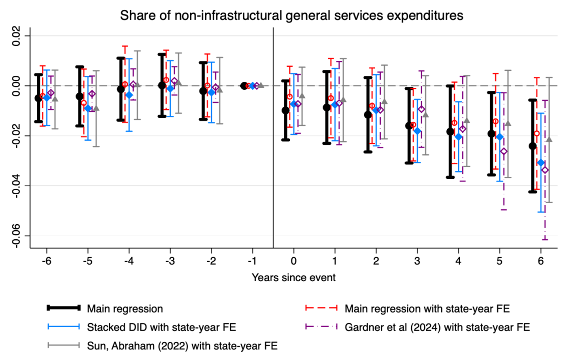
(a) log(tax revenues)



(b) Per capita tax revenues



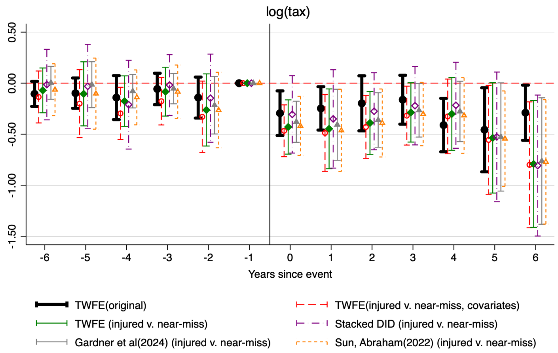
(c) Share of public investment



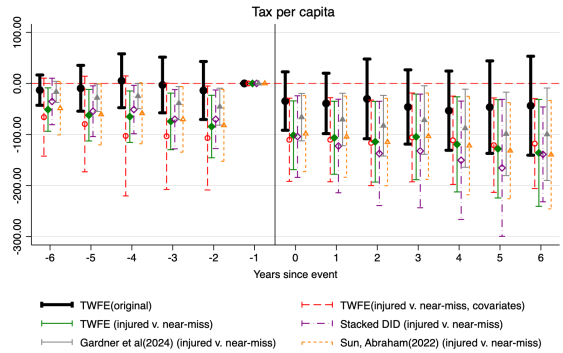
(d) Share of non-infra services

Note: The figures report the event study regression which compares municipalities with successful assassinations to near-miss municipalities. The baseline results with the main sample that uses two-way fixed effects are replicated in green line for reference. Other lines represent results that also includes state-year fixed effects. The outcome variables used in each regression are listed below each graph. The covariates are not included in the regression. Standard errors are clustered at the municipality level.

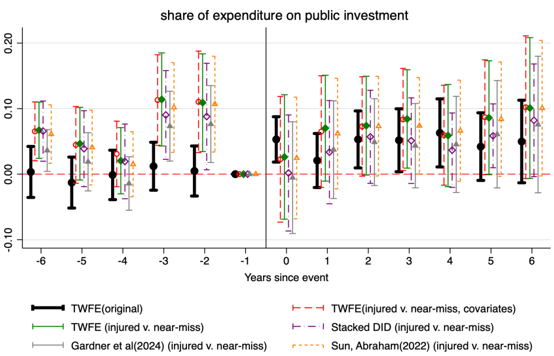
Figure E6: Comparing injured vs unharmed mayors



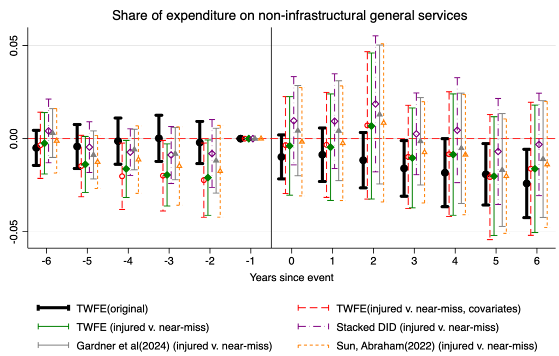
(a) log(tax revenues)



(b) Per capita tax revenues



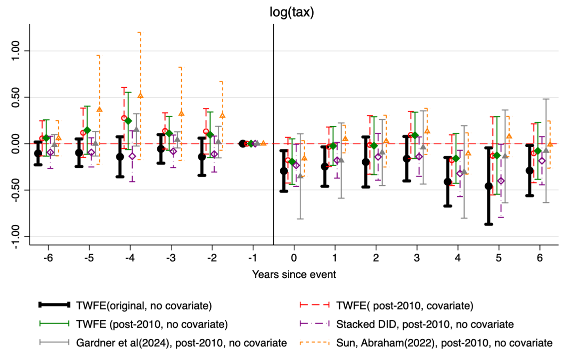
(c) Share of public investment



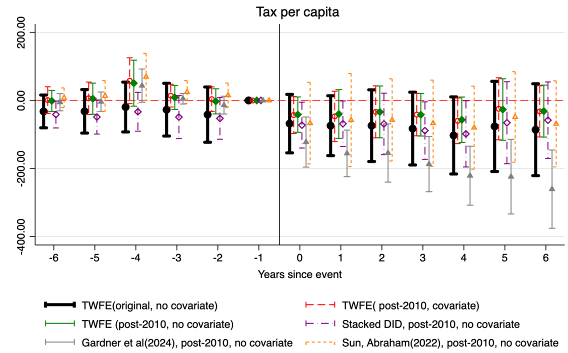
(d) Share of non-infra services

Note: The figures report the event study regression which compares municipalities with injured mayors to unharmed mayors. . The baseline results with the main sample are replicated in green line for reference. The outcome variables used in each regression are listed below each graph. All covariates and fixed effect setups are identical to the main text. Standard errors are clustered at the municipality level.

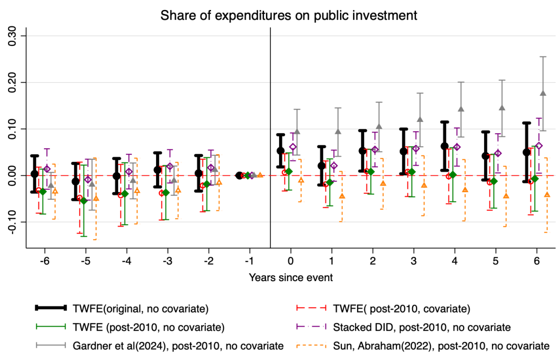
Figure E7: Regression results on post-2010 sample



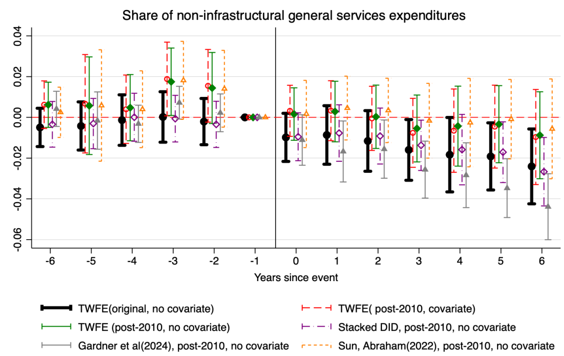
(a) log(tax revenues)



(b) Per capita tax revenues



(c) Share of public investment



(d) Share of non-infra services

Note: The figures report the event study regression which compares municipalities with successful assassinations to near-miss municipalities. Unlike the main regressions reported in Section 5, this regression uses only the post-2010 samples to match the availability of the CNGMD data. The outcome variables used in each regression are listed below each graph. Covariate and fixed effects setup are identical to results reported in Section 5. Standard errors are clustered at the municipality level.

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