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From the Ballot Box to City Hall: The Displacement of Criminal Violence in Mexican Municipal Politics

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Abstract

A significant part of the literature on Mexico holds that criminal violence filters local competition: it intimidates aspiring candidates and thins the field of contenders. Yet in 2024, in the most violent election of the country's recent history, municipal ballots reached election day full. This article seeks to explain that paradox. It argues that organized crime did not withdraw from municipal politics but changed arenas, from contesting elections to constraining those who govern. Drawing on a panel of 13,035 regular municipal elections held between 2006 and 2024 and a harmonized, event-level series of violence against mayors, I estimate fixed-effects models that compare violence before and after the registration of candidacies. Lethal violence does not reduce electoral supply, and attacks on sitting authorities trim it by about 5%. Since 2019, killings of mayors are no longer concentrated in the pre-electoral year, and mayors from the governor's party suffer fewer attacks than their opponents. Criminal violence now contests government more than it contests elections. Policies that concentrate protection on candidates and on the electoral calendar therefore address the least decisive arena of criminal governance.

Keywords: organized crime, criminal governance, municipal elections, violence against mayors, Mexico

1. Introduction

The municipality was the door through which political pluralism entered Mexico. Long before the opposition won a governorship or a majority in the Chamber of Deputies, the National Action Party (PAN) captured the city halls of Ciudad Juárez and Chihuahua in 1983, and in the years that followed municipal alternation became the first democratic apprenticeship of millions of citizens. That is why it is so troubling that, four decades later, the municipality has also become the point of entry for another force, one that seeks not votes but control of territory.

For much of the last two decades, the image that best captured the relationship between organized crime and elections in Mexico was that of a besieged contest. Candidates murdered in the middle of their campaigns, hopefuls who withdrew days before registration, municipalities where no party could find anyone willing to run: violence appeared as a filter that operated before election day and decided, at gunpoint, who could aspire to local power. The image was no journalistic exaggeration. The war among criminal organizations that followed the military deployment of December 2006 coincided with a wave of subnational alternation that broke the old arrangements of protection, and the cartels responded by attacking officials and candidates precisely when local power was changing hands (Trejo y Ley 2018, 2020, 2021). Democratization, which had promised to return to citizens the decision over who governs, thus lived alongside a force that sought to wrest it from them.

In 2024, however, that image ceased to describe what was happening at the polls. It was the most violent election in the country's recent history (Ley y Aparicio 2025), and yet municipal ballots reached election day with an average of almost six candidacies per race, a figure very close to that of 2018. Where violence struck hardest, parties kept fielding candidates; when one withdrew, another took the place. Formal competition survived the violence. This article starts from that paradox: if violence against local politics is growing, why does electoral supply not shrink?

I argue that criminal violence did not withdraw from municipal politics; it changed terrain. During the years of open war, contesting elections made sense, because outcomes were uncertain, local coalitions were reshuffled with every alternation, and control of city hall was won or lost at the polls. In today's Mexico, by contrast, electoral outcomes are more predictable, criminal economies depend on continuous control over the municipal administration, and state protection is concentrated on candidates during campaigns. Under these conditions, organized crime finds it more useful to constrain those who govern than to exclude those who compete. Violence, in short, moved from elections to government.

The argument rests on three pieces of evidence. Using a panel of 13,035 regular municipal elections held between 2006 and 2024, built from the official returns of the National Electoral Institute (INE) and the state electoral bodies, I first show that electoral supply holds. Homicides bear no relation to the number of options on the ballot, and violence aimed at local politics trims it, at most, at the margins. I then show that the timing changed: between 2013 and 2018, killings of mayors clustered in the year before the election, whereas since 2019 they have been spread across the term. Finally, I show that since 2019 mayors from the governor's party suffer fewer attacks, and those from the president's party more. The resilience of the ballot indicates that it is not the main target of violence; timing and protection suggest where that violence was redirected.

The article makes three contributions. First, it distinguishes two arenas of criminal politics, the electoral and the governmental, and explains why their center of gravity could shift from the first to the second while formal competition suffered only at the margins. Second, it brings order to findings that seemed contradictory, since the literature has found fewer candidates where there is violence (Ponce 2019), no effect of killings of mayors on competition (Lee 2026), and lower party fragmentation (Gómez 2026). Third, in the realm of public policy, it warns that responses focused on the electoral stage, from protection protocols for candidates to the "reasonable risk" screen approved in 2026 for the 2027 elections, may help keep the ballot full but leave unattended the arena of government, where violence appears to be concentrated.

Before going further, I should state what the article does not claim. I do not claim that violence has ceased to have electoral consequences, nor that Mexican elections are a sham; nor do I claim that the data allow me to identify alliances between parties and criminal organizations. My claim is narrower and, perhaps for that reason, more disquieting. The center of gravity of criminal politics has moved to a terrain where the instruments of electoral democracy have little reach.

The article is organized in six sections. After this introduction, I discuss the literature and set out the argument of the two arenas; I then reconstruct the Mexican setting; in the fourth

section I describe the data and the empirical strategy, in the fifth I present the results, and in the last I discuss what they mean for local democracy.

2. Criminal violence and local politics: two arenas

The idea that violence filters competition has its roots in the theory of candidate entry. If running for office is costly, and if those costs vary across aspirants, electoral supply is the outcome of a calculation that weighs both the prospects of victory and the risks of competing (Osborne y Slivinski 1996; Besley y Coate 1997; Cox 2004). The criminal threat raises those costs selectively. In the model of Dal Bó, Dal Bó and Di Tella (2006), the possibility of punishment—the lead of the choice between *plata o plomo*—discourages the entry of the most capable citizens and degrades the quality of the political class; in the limit, intimidation becomes a form of capture (Dal Bó y Di Tella 2003).

Comparative evidence confirmed much of that intuition. In Italy, mafia violence against local politicians intensifies around elections (Alesina et al. 2019) or immediately after them, especially when the municipal government changes hands (Daniele y Dipoppa 2017), and the presence of the mafia lowers the quality of those elected (Daniele 2019). In Colombia, paramilitary groups delivered votes to allied politicians, who in turn had few incentives to fight them (Acemoglu et al. 2013). In Mexico, Ponce (2019) found that the killing and threatening of politicians reduced the number of municipal candidates between 2006 and 2011, although the effect weakened where several criminal organizations competed with one another and each protected its own candidate.

This literature shares an assumption that is seldom discussed: that the election is the decisive moment in the relationship between crime and politics, and that violence serves to determine who runs or who wins. The most influential studies of Mexico explained violence against officials and candidates by the breakdown of the protection networks that alternation provoked and by partisan polarization across levels of government; in this reading, cartels attacked those left without the shelter of their party in the state or federal government, especially during local electoral cycles (Trejo y Ley 2018, 2021). Blume (2017) showed, moreover, that in some cases it was politicians themselves who turned to criminal organizations to get rid of their rivals. In all these readings, the election was the stage on which the conflict was settled. The assumption was not arbitrary, for it corresponded to a time when alternation was still a novelty and each election day could redistribute local power in unpredictable ways.

More recent studies, however, have begun to erode the idea that violence empties the ballot. Lee (2026) exploits the success or failure, nearly random, of attacks on mayors to compare completed assassinations with failed attempts, and finds no effects on the number of candidates, competitiveness, or turnout; the effects Lee does find concern government, in particular a drop of about 28% in municipal revenue over the following six years. Gómez Díaz (2026) observes that political violence reduced the fragmentation of the municipal party system between 2018 and 2024, which suggests that violence alters the structure of competition more than its volume. Alcocer and Erickson (2026), for their part, show that violence against officials is explained above all by the fragmentation of criminal organizations and the conflicts among them, and much less by the electoral calendar, partisan alignment, or government offensives.

Weller (2025) offers a different reading of the same relationship. Exploiting the staggered introduction of municipal reelection, the study finds that criminal groups disproportionately killed rival candidates where the incumbent mayor could run again, which favored incumbents' survival, and that those mayors were more prone to corruption. This finding shows that electoral violence can serve the ends of government, such as protecting a mayor with whom an arrangement already exists. Far from contradicting the argument of this article, it refines it. When violence enters the electoral arena, it often does so to preserve a governing arrangement, not to contest the outcome.

These works point in the same direction, even if none of them names it. Criminal violence against local politics is not, or has ceased to be, primarily a strategy to influence elections. The literature on criminal governance offers a clue as to what may have taken its place. Criminal organizations do not merely fight or evade the state; they also govern alongside it, regulate markets and behavior, and negotiate with formal authorities the terms of that coexistence (Arias 2017; Lessing 2021; Feldmann y Luna 2022). From this perspective, what matters to organized crime is not so much who wins the election as what the winner can do, or refrain from doing, once in office. What is missing is an explanation of why this logic would have displaced the electoral one in Mexico, and of the empirical traces such a displacement would leave.

There is, nonetheless, a simpler explanation for the persistence of supply, one unrelated to criminal strategy. Parties have strong institutional incentives to field candidates in every municipality—public funding, the preservation of their registration, statewide coalition agreements, gender parity rules—and they can replace candidates until a few weeks before election day. If this explanation sufficed, violence would not affect the number of options, but neither would it have any reason to change its timing or to discriminate among mayors according to their partisan alignment. Party incentives explain why the ballot fills up; they do not explain when, or against whom, violence is directed.

I propose to distinguish two arenas of criminal politics. In the *electoral arena*, organized crime seeks to influence who runs and who wins, and to that end it intimidates aspirants, finances candidates, and mobilizes or demobilizes voters. In the *arena of government*, it seeks to influence how power is exercised once the election has been decided: who heads the municipal police, who is awarded public works, how much is charged for permits, what is tolerated and what is prosecuted. The two arenas coexist, but their relative weight shifts with the costs and benefits each offers to criminal organizations.

Excluding candidates is a costly strategy. It requires intimidating several aspirants from different parties, in public, at the moment of greatest media attention and under the watch of electoral authorities. Its outcome is, moreover, uncertain, for the aspirant who withdraws can be replaced by another, and the winner may not honor what was agreed. Constraining those who govern, by contrast, allows criminal organizations to act on a known interlocutor, over a long period and away from the spotlight. The choice between one strategy and the other depends, I argue, on three conditions.

The first is the predictability of electoral outcomes. When competition is uncertain and city hall may change hands, contesting the election makes sense. When one party dominates the national scene and mayors can be reelected, the winner is easier to anticipate, and it

becomes more efficient to negotiate with that winner, or to pressure them, than to try to prevent their arrival.

The nature of criminal economies also matters. Organizations devoted to drug trafficking needed, above all, routes and protection at specific moments (Snyder y Duran-Martinez 2009). Those that live off extortion, protection rackets (*cobro de piso*), fuel theft, or the control of local markets need the permanent collaboration of the municipal administration or, at least, its indifference. For them, local government is not a trophy won every three years but a resource managed every day.

Finally, the distribution of state protection counts. Since 2021, the protection protocols of the INE and the federal government have been aimed at candidacies during the electoral process, not at sitting officials. If protection is concentrated on one target and at one moment, violence tends to shift toward the targets and moments left uncovered.

When these three conditions come together, organized crime does not need to empty the ballot. It is enough that whoever wins knows, from the first day, the limits of their government. The result is a form of capture that bypasses the ballot box and that I call *competition without consequence.* Voters choose among real options, but the winner's room for maneuver has been constrained in advance by the threat. The paradox is that electoral democracy appears to work precisely where democratic government works worst.

The argument allows that violence may have effects on supply, but holds that those effects have a ceiling. Pre-registration violence operates as a gradient of coercion, in which each attack raises the cost of running to the extent that it conveys a credible threat to those considering a candidacy. That this gradient produces marginal cuts rather than empty ballots can be explained by three reasons. An attack on a councillor or a municipal trustee (*síndico*) is a message aimed at the circle from which aspirants are drawn, and it barely resonates beyond the municipality; the killing of a mayor, by contrast, attracts national attention, federal forces, and party protection, so that its greater severity does not necessarily translate into greater deterrence. Parties, moreover, replace those who withdraw, and violence can change who runs without leaving voters without a choice. And if organized crime is more interested in constraining the winner than in excluding aspirants, it has no reason to escalate pre-electoral violence. The ceiling on the cut is thus a consequence of criminal strategy, not a limit on criminal capacity.

Three implications that can be tested against the data follow from the argument. If the ballot is not the main target, pre-registration violence may reduce the number of candidacies only at the margins, in proportion to the coercion it exerts on the local political class and without ever emptying the ballot, whereas post-registration violence should have no effect at all (H1). If the displacement occurred, lethal violence against mayors should have ceased to cluster in the year before elections and should instead be spread across the term (H2). And if what is at stake is the administration, a mayor's security should depend on ties to whoever controls the security resources in the territory, that is, the state government, rather than on membership in the party that governs the country (H3).

3. The setting: alternation, war, and a predominant party

The violence unleashed in Mexico from 2007 onward has a longer genealogy than the war that made it visible. The dismantling of the authoritarian state and the liberal reforms of the end of the century stripped the federal government of many of its instruments of

intervention, and the inability to build effective police forces led governments to turn again and again to the army without resolving the underlying weakness of the security system (Loaeza 2020). In that vacuum, municipalities, with small, poorly paid, and exposed police forces, found themselves on the front line of a conflict for which they had never been designed.

The military deployment of December 2006 inaugurated a period whose origins are still debated; the strategy of decapitating criminal organizations, their fragmentation, and the breakdown of arrangements of protection are among the most widely accepted explanations (Dell 2015; Pérez 2024). That period coincided with the consolidation of alternation in states and municipalities, so that criminal violence and electoral competition became intertwined for much of the 2010s (Trejo y Ley 2020). These were the years in which local elections became moments of risk, and in which the question of who would dare to run made sense in hundreds of municipalities.

Three institutional changes later altered the conditions under which crime and local politics interact. The 2014 constitutional reform on political and electoral matters allowed the consecutive reelection of municipal presidents; the first mayors to be reelected were in Coahuila in 2017, and in most states reelection applied from 2018 onward. That year's election turned Morena into a predominant party, which by 2024 governed 21 of the 32 states. And since 2021, the protection of candidates has been institutionalized through protocols coordinated between the INE and the federal security authorities. None of these changes was conceived with organized crime in mind, but their effects added up. By 2024, national competition was less uncertain, mayors could aspire to a second term, and the protection apparatus had been designed for campaigns.

Two reforms that close the period of study, and lend it topicality, must be added to these changes. The reform of April 2025 bans immediate reelection beginning with the 2030 elections, and that of June 2026 created within the INE a mechanism to identify aspirants with a "reasonable risk" of criminal ties. Both rest on the assumption that the problem is settled at the point of access to office, which is precisely the assumption this article puts to the test.

4. Data and empirical strategy

The unit of analysis is the municipal election. I build a panel of 13,129 elections held between 2006 and 2024 in the 2,057 municipalities that elect their authorities through the party system. The returns come from official sources: the tallies of the state electoral bodies for 2006–2014 and 2024, and the INE's electoral statistics system for 2015–2023; only 25 elections were completed with alternative sources, such as the CEDE-UAM transcriptions of local tallies and the compilation by Magar (2018). The analyses are restricted to the 13,035 regular elections.

The main dependent variable is the number of candidacies on the ballot, counting each coalition or joint candidacy as one. It is a measure of effective supply: it does not record those who withdrew before ballots were printed, but it does indicate whether parties managed to field someone. As complementary outcomes I use the margin of victory, the effective number of candidacies, turnout, alternation and, since 2018, the incumbent mayor's decision to seek reelection.

I distinguish three types of violence. *Diffuse violence* is measured with intentional homicides recorded in the death statistics of the National Institute of Statistics and Geography (INEGI), by date of occurrence. *Lethal violence against local politics* is measured with killings of sitting municipal officials—mayors, trustees, and councillors—which all sources record in a relatively homogeneous way. For 2000–2018 I use the dataset of Alcocer and Erickson (2026), in its version that includes only cases attributed to organized crime; I complete it with my own press-based compilation and, from July 2018 onward, with Data Cívica's *Votar entre balas* dataset (2026), and I remove cases duplicated by municipality and date. The series records between 16 and 42 killings of municipal officials per year between 2008 and 2018, and between 8 and 17 from 2019 onward. Finally, *violence against political actors broadly defined* includes any attack—assassination, attempted assassination, kidnapping, or assault—against aspirants, candidates, officials, and former officials. It is a more sensitive measure, but one more exposed to changes in source coverage. For this reason, the analyses of mayors use a harmonized series that reconciles, event by event, the datasets of Alcocer and Erickson (2026) and Lee (2026), my own compilation, and Data Cívica, and that since 2018 incorporates the records of the Armed Conflict Location & Event Data Project (ACLED), whose coding criteria did not change during the period (Raleigh et al. 2010); events recorded by more than one source in the same municipality within 30 days of each other are counted only once. The series comprises 357 attacks on sitting mayors between 2007 and 2024, 133 of them killings. For electoral supply in 2019–2024 I also compare Data Cívica with ACLED, and I distinguish attacks on aspirants and candidates from those aimed at sitting officials, because the former depend on the number of candidacies: the more candidates, the more potential targets. Online Resource 1 documents the coverage of each source and the degree of agreement among them.

The tests of timing and protection measure violence against sitting mayors, rather than against candidates, for reasons of theory, measurement, and design. The argument holds that the criminal dispute shifted toward municipal government, and the person who embodies that government is the mayor, not the aspirant. Attacks on mayors, moreover, have been recorded consistently since 2007 by several independent sources, whereas those against aspirants and candidates have been systematically documented only since 2018, which would make it impossible to compare the three presidential terms. And mayors form a fixed population, exposed throughout their term, which makes it possible to compute rates per mayor-year and to contrast the pre-electoral year with the others; candidates, by contrast, exist only during the weeks the campaign lasts. Attacks on aspirants and candidates do enter the analysis of supply, where distinguishing them from attacks on officials proves decisive.

The structure of the criminal market is measured with the dataset of Esberg (2026), which identifies the presence of criminal organizations by municipality and year between 2009 and 2020. I classify each municipality, in the year before the election, as having no criminal presence, as a monopoly (one group), or as contested (two or more groups).

The test of the persistence of supply requires measuring violence before the registration of candidacies closes. Registration dates vary by state and year, but Article 116 of the Constitution limits the length of local campaigns, which run from 60 to 90 days when a governor is elected and from 30 to 60 days otherwise, and which end three days before

election day. In every state, therefore, registration is approved at least 33 days before the election and never more than 93. With these limits I define three windows: before registration, from 365 to 121 days before election day; intermediate, from 120 to 31 days; and after registration, the last 30 days. Post-registration violence works as a placebo, since it should not affect a supply that has already been set.

I estimate Poisson models with municipality and state-by-electoral-cycle fixed effects, and standard errors clustered by municipality:

$$E[C_{mt}] = \exp\left(\beta_1 V_{mt}^{antes} + \beta_2 V_{mt}^{interm} + \beta_3 V_{mt}^{después} + \mathrm{X}_{mt}\gamma + \alpha_m + \delta_{st}\right)$$

where C_{mt} is the number of candidacies in municipality m in election t; V is the measure of violence in each window; α_m absorbs the permanent characteristics of the municipality; and δ_{st} absorbs everything common to the elections of a given state in a given year, such as the entry of new parties, statewide coalitions, concurrence with other elections, or changes in local legislation. The relevant comparison is that of a municipality with itself over time, relative to the other municipalities of its state in each election. In the extended specifications, the vector X_{mt} includes time-varying controls that could confound the relationship between violence and supply: the logarithm of population, the margin of victory in the previous election, the logarithm of federal revenue-sharing transfers (*participaciones*) per capita, dependence on transfers, the marginalization index, and whether the incumbent mayor could run for reelection. As additional tests I include the number of police officers per thousand inhabitants, the number of coalitions, criminal presence as recorded by the OCVED project, the number of criminal groups and splits identified by Esberg (2026), the rates of extortion, retail drug dealing, and kidnapping reported by the Executive Secretariat of the National Public Security System (from 2015), the trend in homicides between one election and the next, and the profile of the outgoing mayor, namely their sex and their partisan alignment with the governor and with the president.

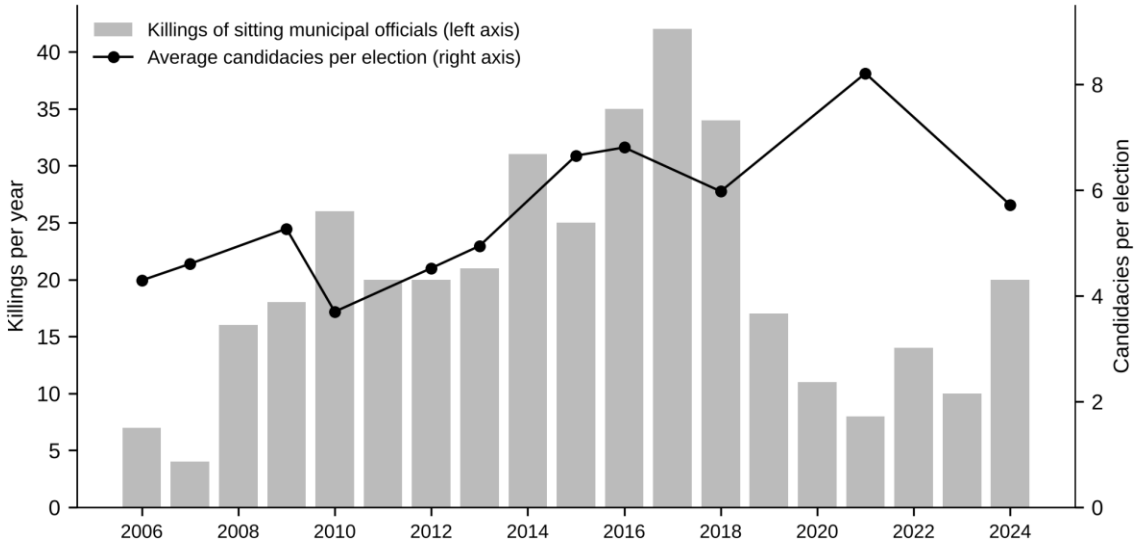
For the analysis of timing, I locate each killing of or attack on a mayor relative to the previous and the next election in the municipality, and I compare the share of cases that occurred in the pre-electoral year with the share that would be expected if events were distributed uniformly across the term. To test the change between periods formally, I regress the pre-electoral excess of each case—the difference between observed and expected—on period indicators, with standard errors clustered by state. For the analysis of protection, I build a panel of mayors by year (2007–2024) and estimate linear probability models of the sitting mayor being killed or attacked according to their partisan alignment with the president and with the governor. I define incumbency in neutral terms, as the party of the sitting president (the PAN between 2007 and 2012, the PRI between 2013 and 2018, and Morena between 2019 and 2024) and the party of the governor in each state and year. These models include state-by-year fixed effects, which compare mayors within the same state and year, and control for the municipal homicide rate; the extended specifications add population, marginalization, fiscal conditions, the margin by which the mayor won the election, extortion and retail drug-dealing rates, the year of the term, the mayor's sex, and whether the mayor is serving a second term. To prevent the results from depending on a

particular choice of controls, I estimate the central results with every possible combination of blocks of variables, in a specification curve (Simonsohn et al. 2020). Because killings of mayors are rare events, I also replicate the models with Poisson regressions, whose coefficients are read as rate ratios, and I compare mayors who won or lost by narrow margins in a regression discontinuity design, to address the fact that alignment with the governor is not assigned at random. Online Resource 1 reports the complete estimates of every test, and the data and code needed to reproduce them accompany the article. Data processing, the estimation code, and the English translation of the manuscript were prepared with the assistance of a large language model (Claude, Anthropic); I reviewed all code, estimates, and text, and I take full responsibility for them.

5. Results

Figure 1 summarizes the paradox that motivates the article. Killings of sitting municipal officials rose during the 2010s and peaked in 2017; from 2019 onward they fell, but remained at levels exceptional for a democracy. The average number of candidacies per election did not follow that trajectory: it increased with the entry of new parties in 2015 and, in the large concurrent cycles of 2018, 2021, and 2024, remained above five. Variations in supply respond to changes in the party system and in coalition rules more than to violence.

Fig. 1 Killings of sitting municipal officials and average candidacies per municipal election, 2006–2024



Source: author's elaboration with data from the INE, the state electoral bodies, Alcocer and Erickson (2026)*, and Data Cívica* (2026)

The impression left by the figure withstands the most demanding test of fixed effects, which compares each municipality with itself. Homicides that occurred before registration bear no relation to the number of candidacies (column 1). This is, moreover, a precise null, not the product of a lack of information: with a 95% confidence interval, doubling the

homicide rate before registration changes the number of candidacies by between −0.3% and +0.2%. Nor do homicides in the intermediate weeks or after registration have any effect.

Targeted lethal violence does not empty the ballot either. In the 66 elections in which a municipal official was killed in the year before registration, the number of candidacies was on average 3% lower than expected, a difference that is not statistically significant (column 2). The confidence interval rules out reductions larger than 7%, though not smaller effects, and the result does not change when municipal controls are added (−0.021; SE 0.023).

Table 1 Violence before and after registration and the number of candidacies on the ballot, 2006–2024

	(1) Homicides	(2) Killing of a municipal official	(3) Attack on political actors, long series	(4) Attack, Data Cívica (2019–2024)	(5) Attack, ACLED (2019–2024)	(6) Attack on officials, ACLED or Data Cívica (2019–2024)
Before registration (−365 to −121 days)	−0.001 (0.002)	−0.031 (0.023)	−0.036** (0.015)	−0.071*** (0.023)	0.053 (0.039)	−0.055* (0.029)
Intermediate window (−120 to −31 days)	−0.001 (0.001)	−0.048 (0.045)	−0.011 (0.015)	−0.011 (0.026)	0.035 (0.039)	−0.036 (0.037)
After registration (−30 to 0 days)	0.002 (0.001)	0.027 (0.043)	−0.001 (0.015)	0.077*** (0.024)	0.052 (0.032)	−0.022 (0.049)
Homicide rate before registration		Yes	Yes	Yes	Yes	Yes
Municipality and state-cycle fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Elections	13,004	13,004	13,004	3,604	3,604	3,604

Source: author's elaboration with data from the INE, the state electoral bodies, INEGI, Alcocer and Erickson (2026), Data Cívica (2026), and ACLED.

Note: Poisson coefficients; standard errors clustered by municipality in parentheses. In column 1, violence is measured as the logarithm of the annualized homicide rate per 100,000 inhabitants; in the other columns, as the occurrence of at least one event in the window. Column 3 uses the long series of attacks on aspirants, candidates, and officials, which since July 2018 relies on Data Cívica; columns 4 to 6 restrict the analysis to 2019–2024; columns 4 and 5 measure any attack recorded by Data Cívica or by ACLED, and column 6 attacks on sitting officials recorded by either source.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Non-lethal violence leaves a modest mark. With the long series of attacks, the ballot has on average 3.5% fewer candidacies when an attack occurs in the year before registration (column 3). Because that cut is concentrated in 2019–2024, when measurement relies on Data Cívica, I contrast it with ACLED. With Data Cívica, a pre-registration attack is associated with 7% fewer candidacies, but a post-registration attack, which can no longer affect ballots already set, with 8% more (column 4). The anomaly owes less to a bias in the source than to reverse causality. Post-registration attacks are aimed above all at candidates, and where there are more candidacies there are more targets; the number of candidacies does, in fact, predict those attacks ($p=0.03$). If only attacks on sitting officials are considered, whose number does not depend on that of candidacies, the placebo holds. A pre-registration attack is then associated with a ballot 5.4% smaller ($p=0.06$, with both sources), and a post-registration attack has no effect (column 6); with Data Cívica alone, the reduction reaches 8% ($p<0.01$). ACLED, which records fewer than half of those events

and agrees with Data Cívica in only 33 of 182 elections, shows no cut at all (+5.4%; p=0.17; column 5). Depending on the source, non-lethal violence may subtract about a third of a candidacy in a typical election of almost six. Its effect, moreover, is statistically indistinguishable from that of the killing of a municipal official (difference of −0.024; SE 0.037; p=0.51). Rather than one form of violence that reduces supply and another that leaves it untouched, what emerges is a single pattern of small cuts that never come close to emptying the ballot.

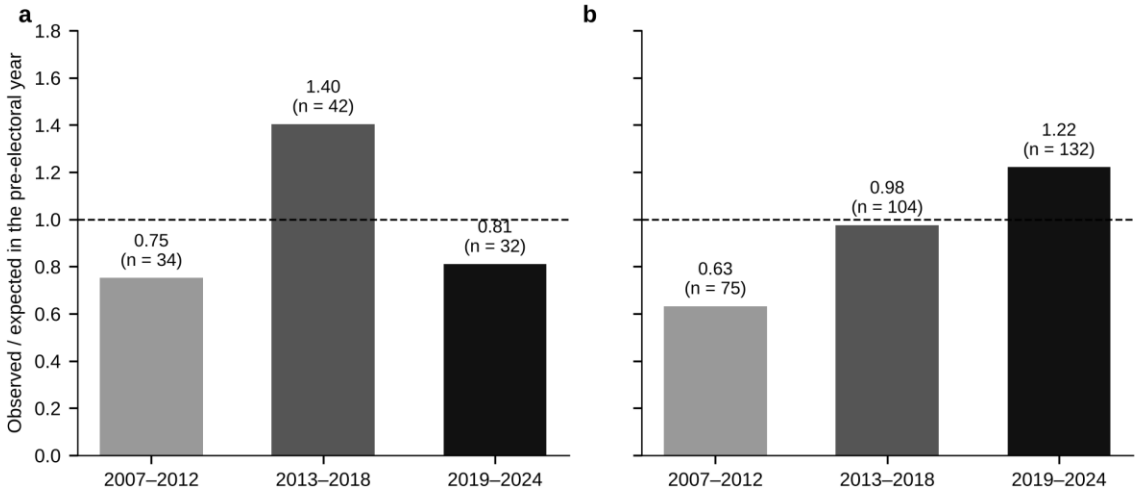
The results withstand a range of tests. When the size of the municipal police (from 2010), the number of coalitions, or the criminal presence recorded by the OCVED project (2006–2018) is included, the coefficients for lethal violence remain small and, at best, marginally significant; the largest, with police as a control, indicates a reduction of 4% (p=0.06). Nor does the pattern change with the structure of the criminal market. Using the classification of Esberg (2026), the effect of the killing of a municipal official is equally small and imprecise in contested municipalities (−0.041; SE 0.029), in municipalities under monopoly (−0.017; SE 0.115), and in those with no recorded criminal presence (−0.029; SE 0.029). It makes no difference, likewise, whether the incumbent mayor could run for reelection or whether the municipality has a high extortion rate. And violence does not alter the margin of victory, turnout, alternation, or the mayor's decision to seek a second term (Online Resource 1).

If violence had filtered supply during the years of open war and ceased to do so afterward, the effect should be larger in the earlier presidential terms. It is not. The killing of a municipal official before registration has a small and imprecise coefficient in all three periods (+0.042 in 2006–2012, −0.048 in 2013–2018, and −0.025 in 2019–2024, with 15, 42, and 9 cases), with no differences among them (p=0.41). With the long series of attacks, only the 2019–2024 coefficient is significant (−0.054), precisely when Data Cívica's coverage improves, and it does not differ from the earlier ones either (p=0.20). The persistence of supply is a constant of the whole period; what changed was not the effect of violence on the ballot but its timing and its targets. Nothing suggests, moreover, that the effect is hidden behind defensive coalitions, since pre-registration violence does not change the number of parties fielding candidates, eight on average, nor the number of coalitions, nor the effective number of candidacies. The only notable difference across municipalities runs against expectations. In 2019–2024, the cut associated with attacks on officials is concentrated where the previous election was won by a wide margin (interaction of −0.108; p=0.04), as if violence deterred the opposition more easily where the outcome seems decided in advance.

These results are consistent with what Lee (2026) found when comparing completed and failed attacks on mayors, and with the figures on withdrawals in 2024. There is no official count of withdrawals due to violence; parties reported 107 in the 2023–2024 electoral process, and the monitoring by Laboratorio Electoral (2024) counted more than a thousand among candidates and pre-candidates. Ballots were not emptied because parties replaced those who withdrew. A complementary analysis of the substitutions approved between 2017 and 2024 documents that mechanism. Municipalities attacked during the substitution period recorded 47% more replacements, and an attack occurring between four and twelve months before election day nearly doubled substitutions at the head of the mayoral slate. Violence expels people, but it does not eliminate options.

If violence no longer seeks to prevent competition, when does it occur? Figure 2 compares, for three periods, the share of killings and of attacks of any kind against mayors that took place in the year before the election with the share that would be expected if they were distributed uniformly across the term. A value above one indicates that violence clusters in the run-up to the election.

Fig. 2 Killings of and attacks on mayors in the pre-electoral year: ratio of observed to expected under a uniform distribution across the term, by period. **a** Killings. **b** Attacks of any kind. The dashed line marks a ratio of one; n is the number of events with a subsequent election in the panel



Source: author's elaboration with the harmonized series of violence against mayors (Alcocer y Erickson 2026; Lee 2026; Data Cívica 2026; Raleigh et al. 2010)

Between 2013 and 2018, killings of mayors in the pre-electoral year were 40% more frequent than expected (42 cases). This is the period the literature characterized as one of electoralized violence, in which the cycles of local competition set the calendar of attacks (Trejo y Ley 2021). Between 2019 and 2024, the ratio fell below expectations (0.81; 32 cases), and the pre-electoral excess of each killing is 18 percentage points lower than in the previous presidential term (SE 0.089; p=0.05, with standard errors clustered by state). The difference grows when the next election is imputed for events occurring after the last election day in the panel (ratio of 0.65; p=0.01), and it holds when events recorded only by ACLED, whose incorporation in 2018 coincides with the boundary between periods, are excluded (0.79 versus 1.49; p=0.06). Since the excess was also lower in 2007–2012 (0.75; p=0.08), everything indicates that the electoralization of lethal violence peaked during the presidency of Enrique Peña Nieto (Online Resource 1). Attacks of any kind tell a different story. Their pre-electoral concentration did not decline; it rose slightly, from 0.98 to 1.22 (132 cases; p=0.33), and was higher in 2022–2024 (1.31) than in 2019–2021 (1.18). The decoupling holds for lethal violence, the kind that decides who governs, and not for assaults. Recent cases are few, but the contrast is sharp: the most serious violence ceased to follow the electoral calendar, while lower-intensity intimidation has not abandoned it.

The composition of victims points in the same direction. In the Data Cívica dataset, sitting officials went from 27% of victims in the 2018 electoral cycle to 44% in 2021 and 73% in 2024, while aspirants and candidates fell from 59% to 47% and to 16%. This contrast must be read with caution, because in 2022 Data Cívica broadened its recording of assaults on municipal officials. Even so, between 2022 and 2025, a period of homogeneous coverage, the events recorded in 2024 exceed by about 30% the average of the years without concurrent municipal elections. The election adds violence, but it does not explain it; what predominates is a continuous flow.

The third implication of the argument concerns protection. If what is at stake is government, a mayor's security should depend on ties to whoever controls, in the mayor's territory, the resources of security and criminal justice, which in Mexico are largely in the hands of the states. Table 2 shows that this is the case for attacks, though only in recent years, and not for killings; before 2019, neither closeness to the president nor closeness to the governor offered clear protection.

Table 2 Partisan alignment and violence against sitting mayors
(per thousand mayor-years)

	2007–2012 (PAN presidency)	**2013–2018 (PRI presidency)**	**2019–2024 (Morena presidency)**	**2019–2024, with controls**	**2019–2024, all controls**	**2019–2024, municipality fixed effects**
A. Killings						
Mayor from the president's party	−0.3 (1.1)	1.2 (1.3)	2.9 (1.7)*	3.1 (1.8)*	2.6 (1.7)	2.5 (2.6)
Mayor from the governor's party	−0.1 (1.1)	−0.6 (1.2)	−1.5 (1.2)	−1.9 (1.3)	−1.5 (1.2)	−0.9 (1.7)
B. Attacks of any kind						
Mayor from the president's party	1.1 (1.8)	0.4 (1.8)	11.5 (3.4)***	8.8 (3.4)**	8.2 (3.4)**	2.8 (4.9)
Mayor from the governor's party	−0.3 (1.6)	−0.7 (1.9)	−6.1 (2.5)**	−7.4 (2.7)***	−7.0 (2.7)**	−5.2 (3.4)
Mayor-years	10,464	12,231	12,270	11,142	11,136	12,270

Source: author's elaboration with data from Alcocer and Erickson (2026), Lee (2026), Data Cívica (2026), ACLED, and Magar (2018).

Note: linear probability models with state-by-year fixed effects and a control for the homicide rate; standard errors clustered by municipality in parentheses. Coefficients multiplied by one thousand. The controls are the logarithm of population, the marginalization index, the logarithm of revenue-sharing transfers per capita, dependence on transfers, and the margin by which the mayor won the election; the "all controls" column adds extortion and retail drug-dealing rates, the year of the term, the mayor's sex, and whether the mayor is serving a second term. Killings and attacks come from the harmonized series of violence against mayors, which integrates Alcocer and Erickson (2026), Lee (2026), an original compilation, Data Cívica (2026), and, since 2018, ACLED. The means are 3.3 killings and 12.1 attacks per thousand mayor-years in 2019–2024.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

In none of the three presidential terms were mayors from the president's party less likely to be attacked or killed. In the most recent one the opposite occurred: they suffered 11.5 more attacks per thousand mayor-years than the others, against a mean of 12.1, and 8.2 more with all controls ($p=0.02$). Alignment with the governor has been associated since 2019 with about six fewer attacks ($p<0.05$), and with seven fewer when controls are added.

Against killings, protection is weaker; the coefficient has the expected sign but is small and imprecise (−1.5 against a mean of 3.3; p=0.22). A mayor allied with the governor faces fewer assaults than an opposition mayor in the same state and year, although the data do not allow us to claim that the mayor runs a lower risk of being killed. Poisson models provide the relative measure. The attack rate of mayors aligned with the governor is 34% lower (rate ratio of 0.66; p=0.05), and that of mayors from the president's party is twice as high (2.06; p<0.01).

The result on attacks does not depend on the most obvious differences among municipalities. The protective effect of alignment with the governor increases slightly when population, marginalization, fiscal conditions, or electoral competitiveness are controlled for, and it does not change when extortion, retail drug dealing, the year of the term, the mayor's sex, or a second term are added. With municipality fixed effects, the coefficient retains its sign and magnitude (−5.2), although it loses precision (p=0.12). The same does not hold for the excess of attacks against the president's party, which falls to 2.8 and ceases to be significant, suggesting that it partly reflects the municipalities that party governs. With ACLED as the only source, the pattern recurs with less precision, with 4.2 more attacks for the president's party (p=0.06) and 2.6 fewer for those aligned with the governor (p=0.11).

The most serious objection to these results is that the governor's party does not win municipalities at random, so that aligned mayors may govern different municipalities, for reasons that also affect their exposure to violence. To address it, I compare mayors who reached office by narrow margins, in a regression discontinuity design around the tie between the candidate of the governor's party and the strongest rival. With a bandwidth of 15 percentage points, aligned mayors who narrowly won suffered in 2019–2024 about 16 fewer attacks per thousand mayor-years than those who narrowly lost (−15.9; SE 6.6; p=0.02; −13.3 with state-by-year fixed effects). With narrower bandwidths, the estimate retains its sign and magnitude but loses precision (−15.3 with 10 points, p=0.06; −19.9 with 5 points, p=0.12), and the symmetric distribution of cases around the threshold rules out manipulation of the margin. There is no significant discontinuity for killings, nor for attacks in 2007–2018, when only marginal protection against killings appears (p=0.07). Close elections confirm the pattern of Table 2, although events are few and precision is limited (Online Resource 1).

Protection through alignment with the governor is not the same in every state. In states governed by Morena, the gap in attacks between aligned and non-aligned mayors is larger; the additional protection amounts to 18.4 fewer attacks per thousand mayor-years (p=0.02, with the basic controls), with no significant difference for killings (3.5; p=0.40). In those states, attacks tend to fall on mayors who oppose the governor, although the evidence rests on few states and few years.

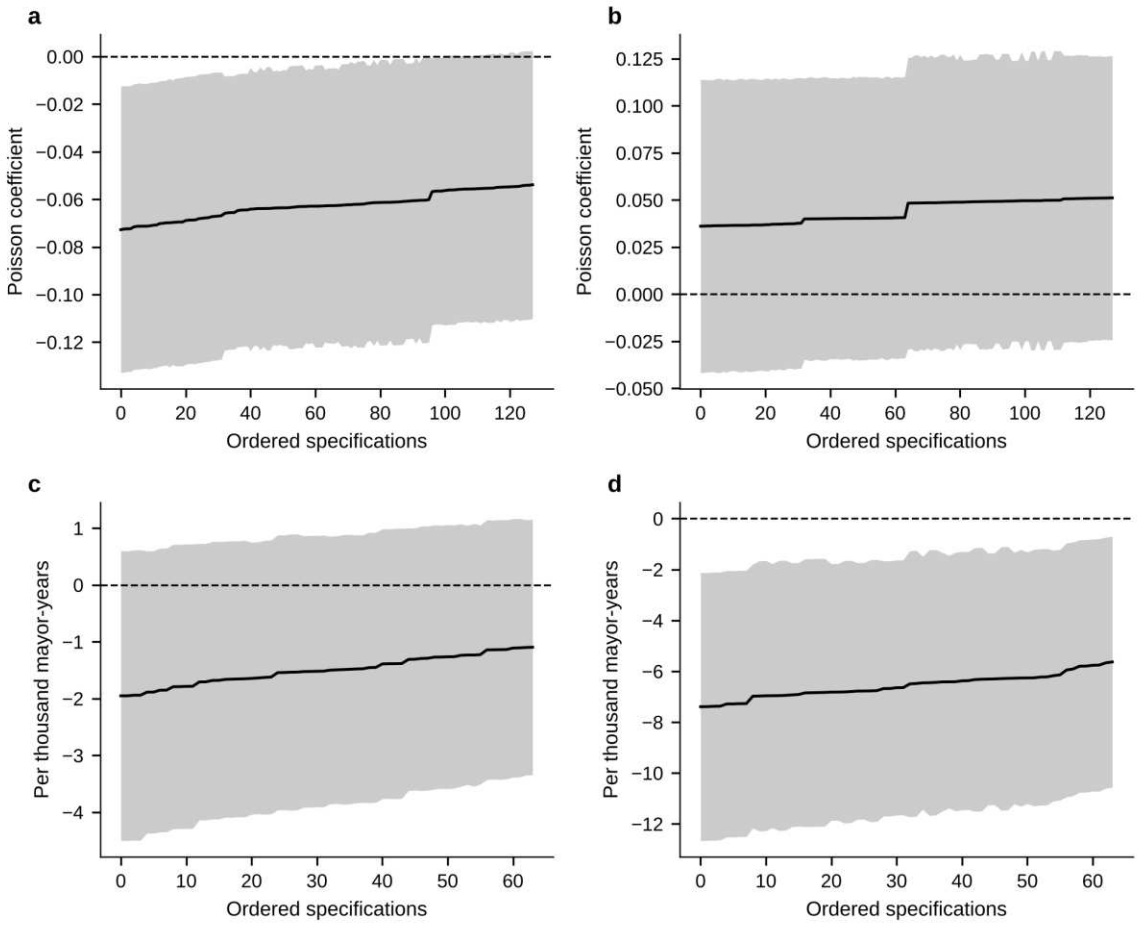
This finding admits several readings that my data cannot disentangle. It may reflect preferential state protection for allied mayors, whether through the coordination of security forces or through arrangements between authorities and criminal groups. It may express the logic of polarization that Trejo and Ley (2021) identified a decade ago, in which cartels attack those who lack partisan protection at the higher level of government. Or it may be due, in part, to the characteristics of the states Morena won between 2021 and 2024. What the data do show is that protection against attacks is decided at the state level and not at the

national level, and that there is no sign of preferential protection for the president's party in any of the three presidential terms. Protection, like violence, has become a matter of government.

If violence operates on government, its imprint should appear in municipal administration. To explore this, I relate attacks on the mayor to tax revenue per capita as recorded by INEGI in its statistics on state and municipal public finances, in a panel of municipalities by year (2007–2024) with municipality and state-by-year fixed effects. In the year of an attack, revenue is about 7% lower (−0.072; SE 0.028; p=0.01; 27,511 municipality-years), and the drop persists in the following year (−0.064; p=0.03), while total spending barely changes. The drop also appears after failed attacks (7% lower in the following year; p<0.01) and in the year of a killing (11%; p=0.03). These results should not, however, be read as a causal effect: revenue is already 6% lower in the year before the attack (p=0.05), and the association is sharper in 2008–2018 than in 2019–2024 (Online Resource 1). It seems, rather, that attacks on mayors and the deterioration of municipal finances are part of the same episode of criminal pressure on local government, like the one Lee (2026) documents for completed assassinations, although here the drop does not require the attack to succeed, nor is it peculiar to the recent period.

Any analysis of this kind rests on decisions that another researcher might have made differently: which controls to include, which years to consider, which sources to privilege. To learn how much my results depend on those decisions, I estimate the central results—the effect of pre-registration attacks, both against officials, with both sources, and of any kind, with ACLED, and protection through alignment with the governor—with every possible combination of blocks of controls: 128 specifications for each measure of supply, combining demographic, fiscal, political, and criminal variables, market crimes, the trend in violence, and police size, and 64 for each measure of violence against mayors. Figure 3 orders the coefficients from smallest to largest.

Fig. 3 Specification curve of the central results. **a** Electoral supply: attack on officials before registration, ACLED or Data Cívica (Poisson coefficient). **b** Electoral supply: any attack before registration, ACLED (Poisson coefficient). **c** Killings: mayor aligned with the governor (per thousand mayor-years). **d** Attacks: mayor aligned with the governor (per thousand mayor-years)



Source: author's elaboration. Coefficients and 95% confidence intervals under every combination of blocks of controls, 2019–2024

The curve confirms the foregoing. In all 128 specifications, a pre-registration attack on officials is associated with a ballot between 5.3% and 7.0% smaller, always significant at the 10% level and at the 5% level in 79% of them; with ACLED as the only source, the coefficient is always positive and never significant. In the 64 specifications for each measure, alignment with the governor is associated with between 5.6 and 7.4 fewer attacks per thousand mayor-years, always significant at the 5% level, but not with fewer killings; membership in the president's party is associated with more attacks in every specification (between 8.1 and 11.5). No result depends on a fortunate specification; what separates them is the source, in the case of supply, and the type of violence, in the case of protection.

6. Conclusions

The relationship between organized crime and local politics in Mexico has changed profoundly, if not always visibly. As has often happened in the country's political history, the continuity of forms has concealed the transformation of substance. During the 2010s, and above all between 2013 and 2018, criminal violence was organized around elections. Killings of mayors clustered in the run-up to election day, when control of city halls was at stake, and protection against that violence did not clearly depend on closeness to any level

of government. In the most recent period, violence has not declined, but it has changed terrain. Ballots remain full, killings of mayors are spread across the term, protection against attacks depends on alignment with the state government, and attacks on mayors accompany the deterioration of municipal revenue. Organized crime no longer needs to decide who runs; it is enough to constrain whoever governs.

This transformation unsettles the way in which the health of local democracy is usually assessed. Several options on the ballot, alternation, and competitiveness were, for decades, the indicators with which Mexican democratization was celebrated. In the setting I describe, those indicators can coexist with a form of capture that bypasses the ballot box, in which voters choose among real options but what they choose has a room for maneuver constrained in advance. This is what I call competition without consequence, and its effect is paradoxical: the election preserves its form precisely when it loses part of its meaning.

The transformation also has implications for public policy. The institutional response to political violence has concentrated on the electoral stage, with protection protocols for candidates, the monitoring of campaigns and, since 2026, a screen to identify aspirants at risk of criminal ties. My results do not suggest that these measures are useless—assaults still follow the electoral calendar, and these measures may partly explain why the ballot remains full—but rather that they act on the stage at which violence least alters outcomes. The protection of sitting officials, the professionalization of municipal police forces, and the oversight of the decisions most exposed to extortion—public works, permits, security appointments—appear to be more relevant terrains. And if protection against attacks depends on alignment with the governor, the risk that security mechanisms become instruments of partisan discipline deserves the attention of those who design reforms.

The argument has limits that should be acknowledged. The temporal decoupling rests on few cases and holds only for killings; protection through alignment with the governor holds against attacks, not against killings, and admits explanations that require qualitative evidence to be distinguished. The cut in supply associated with attacks on officials appears in Data Cívica but not in ACLED, and the relationship between attacks and revenue begins before the attack. Finally, I lack systematic information on candidacies withdrawn before ballots were printed, and a municipal measure of criminal presence after 2020.

The conditions I identify—more predictable elections, criminal economies that depend on local government, and state protection concentrated on the electoral process—may not be unique to Mexico. The literature on criminal governance documents local governments that coexist with organizations more interested in administering than in competing (Arias 2017; Feldmann y Luna 2022); whether violence in those cases also shifts from elections to government is an open question for comparative research. In the next municipal elections, the success of the process will be measured, as always, by the integrity of registration, the calm of election day, and the acceptance of results. These are necessary measures, but if my argument is correct they will not suffice to know whether the winners will be able to govern. For Mexican democracy, which for three decades measured its progress by the cleanliness of its elections, the lesson is a sober one: the ballot box is no longer the only place where it is decided who rules.

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