POLITICAL INFORMATION CYCLES: WHEN DO VOTERS SANCTION INCUMBENT PARTIES FOR HIGH HOMICIDE RATES?*

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Do voters sanction incumbent parties for their performance in office? I argue that how governments are held to account depends upon *when* voters consume information about the relevant incumbent party. If news consumption follows electoral cycles, short-term performance indicators in the news prior to elections may powerfully shape voting behavior. In the context of local homicides and Mexican municipal elections, I test this theory's central implications using three distinct sources of plausibly exogenous variation. First, I show that voters indeed consume more news before local elections, and that homicides before such elections increase the salience of public security and reduce confidence in the mayor. Second, electoral returns confirm that pre-election homicide shocks substantially decrease the incumbent party's vote share and re-election probability. However, such sanctioning is limited to mayoral elections, and is barely impacted by longer-term homicide rates. Finally, the punishment of homicide shocks relies on, and increases with, access to local broadcast media stations. These effects are only pronounced among less-informed voters, who principally engage with politics around elections. The findings demonstrate the importance of when voters consume news, and contribute to explaining the mixed electoral accountability often observed outside consolidated democracies and in federal systems.

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1 Introduction

Electoral accountability rests upon informed voters electing competent representatives. Given the difficulty of directly observing candidate competence, performance on salient issues like the economy, public security, or corruption represent key signals of an incumbent party's continuing suitability for office (Fearon 1999; Ferejohn 1986). However, particularly in developing contexts, voters are often poorly informed about politics and current affairs (e.g. Keefer 2007; Pande 2011), or do not receive relevant information pertaining to their incumbent party (Snyder and Strömberg 2010).[1] This lack of politically-relevant information may explain why voters often do not hold incumbents to account on available performance metrics, or struggle to assign responsibility across layers of government.[2] While many scholars have argued that access to media facilitates electoral accountability (see Ashworth 2012; Pande 2011),[3] the availability of media coverage does not imply that voters actually *consume* politically-relevant news.

I instead argue that how voters hold incumbent parties accountable for their performance in office reflects the *timing* of their news consumption. In particular, if voters primarily consume news before elections—when they are more likely to actively seek out politically-relevant information (Hamilton 2004; Marshall 2016), and media outlets are more likely to supply such information (Prior 2007)—electoral sanctioning will reflect salient indicators of incumbent performance reported in the media at this time. Consequently, incumbent performance indicators only impact election outcomes if they are covered in the media when voters consume news before elections. I expect the effect of such *political information cycles* on vote choice to be most pronounced among poorly-informed voters, whose weak prior beliefs over the incumbent's continuing suitability for office are most responsive to available performance indicators (Zaller 1992).

This paper provides evidence for the central elements of this theory in the context of examining how Mexican voters hold different levels of government to account for local violent crime.

[1] Ill-informed electorates pose similar dilemmas in established democracies (Bartels 2008; Delli Carpini and Keeter 1996), but the risks of electing incompetent politicians are likely greater under weak democratic institutions.

[2] Evidence that voters, especially in consolidating democracies, punish or reward politicians for economic performance (Remmer 1991; Roberts and Wibbels 1999; Singer and Carlin 2013), levels of public security (Kronick 2014; Vivanco et al. 2015), or malfeasance in office (Arias et al. 2017; Chong et al. 2015; de Figueiredo, Hidalgo and Kasahara 2013; Ferraz and Finan 2008; Larreguy, Marshall and Snyder 2017) is mixed. The developed country literature similarly highlights mixed or conditional effects (see Anderson 2007; Golden 2010; Healy and Malhotra 2013).

[3] Well-identified studies show that news or advertising content affect voting behavior (e.g. Enikolopov, Petrova and Zhuravskaya 2011; DellaVigna and Kaplan 2007; Gerber et al. 2011; Larreguy, Marshall and Snyder 2016; Snyder and Strömberg 2010), while others emphasize that such information must have a credible source (Alt, Lassen and Marshall 2016; Chiang and Knight 2011; Lenz and Ladd 2009) and may require a minimum level of sophistication to process complex information (Alt, Lassen and Marshall 2016; Gomez and Wilson 2006). Experimental studies primarily focus on providing information just before elections, and rarely mimic typical newscasts.

Following significant democratizing and decentralizing reforms in the 1990s, Mexican elections have become relatively competitive at the national and sub-national levels and responsibility for administering key public services such as policing is shared across municipal, state, and federal governments. Public security is a major concern among Mexican voters, as in many other Latin American nations, and thus local homicides represent a salient incumbent performance metric. However, since voters are largely poorly informed about politics and a significant portion of the electorate lacks strong partisan ties (Chong et al. 2015; Greene 2011; Lawson and McCann 2005; McCann and Lawson 2003), there is scope for pre-election crime reports to influence voting behavior. Although incumbents cannot seek re-election, voters hold the incumbent's party responsible in Mexico's party-centric system (Chong et al. 2015; Larreguy, Marshall and Snyder 2017).

Leveraging plausibly exogenous variation in temporal proximity to local elections, municipal-level homicide shocks just before elections, and access to local broadcast media stations, I test the political information cycles argument at the individual and electoral precinct levels. First, to identify the effect of upcoming local elections on news consumption and voter beliefs, I exploit the irregular timing of survey waves and staggered electoral cycles across Mexican states to isolate variation in the proximity of municipal and state legislative elections (see Eifert, Miguel and Posner 2010). Rather than separate supply and demand explanations for information consumption, I focus on consumption cycles *in equilibrium*.[4] I first show that voters indeed consume more political news through television and radio just before local elections, and also demonstrate greater political knowledge. While better informed voters increase the intensity of their news consumption, less informed voters effectively only consume political news during elections campaigns.

Second, using electoral returns between 1999 and 2013, I show that pre-election homicide shocks substantially harm the municipal incumbent party's electoral performance. To identify the effects of homicide spikes before elections—when voters consume most news—I exploit idiosyncratic month-to-month volatility in municipal homicide counts. Specifically, I compare "shocked" municipal elections that experienced more homicides in the two months preceding the election to "control" elections from within the same municipality that experienced more homicides in the two months after the election.[5] Such homicide shocks are consistent with random sample variability and are well balanced over 101 covariates, while I find no evidence that drug trafficking organizations (DTOs) or politicians manipulate homicide rates around elections. Despite being uncorrelated with broader homicide levels and trends, and thus representing weak indicators of long-term performance, pre-election homicide shocks reduce the incumbent party's vote share by

[4] Marshall (2016) seeks to tease apart explanations for increased consumption.

[5] I include only municipalities experiencing at least one homicide over the two months before and after an election.

2.2 percentage points and its probability of winning by 12 percentage points.

Consistent with poorly-informed voters relying on the pre-election signals available when they actually consume news, voters do not hold incumbents accountable for longer-run homicide rates. Using a difference-in-differences design, I find no evidence that homicide rates over the prior year or electoral cycle—arguably more desirable indicators of incumbent performance (Healy and Lenz 2014), but which may not be observed by voters predominantly consuming information just before elections—affect incumbent electoral performance. Furthermore, my analysis of concurrent state and federal elections, and comparisons between municipalities with and without their own police force, indicate that the parties of incumbent mayors, rather than higher levels of government, are held to account for local homicide shocks (see also Vivanco et al. 2015). This finding chimes with research in the U.S. also highlighting that mayoral electoral performance is negatively correlated with local homicides (Arnold and Carnes 2012), but which does not illuminate the role of information cycles.

Third, I link the individual and aggregate-level findings by showing that *local* broadcast media play a crucial role in the transmission of these effects. Local broadcasters report politically-relevant news that voters may not be able to access otherwise, and local crime receives substantial media coverage (e.g. Trelles and Carreras 2012). Using detailed media coverage maps for every radio and television station in the country, I exploit within-neighboring precinct variation in coverage to identify the effects of broadcast media. I find that, on average, each additional local media station based within a precinct's own municipality reduces the incumbent's vote share by 0.22 percentage points, and is driven principally by television. However, the 19% of precincts covered by least local media stations do not significantly punish homicide shocks, while an additional non-local station does not affect voter sanctioning.

Turning to the mechanisms, I find that such results reflect voters updating in a Bayesian fashion from a weak signal of incumbent performance. First, suggesting that voters internalize the news in the media just before elections, pre-election homicides both increase concern about public security and reduce confidence in the municipal incumbent by around 10 percentage points. In contrast with the "recency bias" literature (see Healy and Lenz 2014), homicide shocks not occurring before elections—when voters consume most news—do not affect voter beliefs. Second, illustrating voters' capacity to integrate new information with their prior beliefs, sanctioning of homicide shocks is greatest where a different incumbent party at the previous election did not oversee a pre-election homicide shock or the same incumbent party experiences consecutive shocks. In contrast with Banerjee et al. (2011) and Kayser and Peress (2012), I find that that benchmarking is primarily temporal rather than spatial in this context. Third, changes in beliefs and electoral

sanctioning are concentrated among less educated voters, who consume less news outside election campaigns. This suggests that primarily voters with weaker prior beliefs about the incumbent party's performance on violent crime update from pre-election homicide shocks. Fourth, I show that although media coverage is closely associated with the occurrence of homicides, this association is no greater before elections. In conjunction with the fact that some voters only consume news before elections, this suggests that political information cycles reflect increased demand for politically-relevant news before elections more than increased supply.

My finding that political information cycles induce voters to heavily weight recent performance indicators in ways that can substantially alter electoral outcomes makes several theoretical and empirical contributions. First, it shows that, in addition to voters requiring access to performance indicators,[6] such indicators are only likely to meaningfully alter voting behavior when their coverage in the media coincides with voters actually consuming news. Political information cycles may thus explain the mixed evidence of electoral accountability across different performance metrics and release dates (e.g. Achen and Bartels 2004*b*; Brollo 2009; Chang, Golden and Hill 2010; Roberts and Wibbels 1999). My findings similarly rationalize studies showing that politicians release bad news when voters are distracted by other events (e.g. Durante and Zhuravskaya forthcoming; Eisensee and Strömberg 2007).

Second, in contrast with theories resting on myopic voters (e.g. Downs 1957; Nordhaus 1975) or short memories (e.g. Zaller 1992), my argument implies that the common finding that voters respond positively to economic performance just before elections could instead reflect a poorly-informed electorate updating—potentially with overconfidence (Ortoleva and Snowberg 2015)—from the only information they possess at the time. In this respect, my results micro-found and extend Healy and Lenz's (2014) conclusions from U.S. survey experiments across many elections in a major developing democracy. However, the sophisticated updating and differential punishment across levels of government exhibited by Mexican voters suggests that such behavior cannot be accounted for by voters blindly responding to events beyond the incumbent's control (see Achen and Bartels 2004*a*; Healy, Malhotra and Mo 2010).

Third, I contribute evidence that voters attempt to hold governments accountable for public security. Alongside the economy, public security represents the main valence issue in many developing contexts, and particularly in Latin America.[7] By illustrating how the timing of news

[6] See e.g. Banerjee et al. (2011), Casey (2015), Chang, Golden and Hill (2010), Ferraz and Finan (2008), Healy and Lenz (2014), Larreguy, Marshall and Snyder (2017), and Snyder and Strömberg (2010).

[7] While existing studies have suggested that high levels of violence can affect incumbent electoral performance (e.g. Berrebi and Klor 2006; Cummins 2009; Kronick 2014; Ley 2014), incumbent evaluations (García-Ponce, Wantchekon and Zeitzoff 2014), or increase informed participation among victims (e.g. Bateson 2012; Bellows and Miguel 2009), this study explains why responses to violence primarily relate to recent shocks, rather than standard

consumption affects sanctioning, my results help explain why Mexican scholars have struggled to verify the widely-held belief that voters respond to local crime (e.g. Vivanco et al. 2015).

Fourth, in a complex federal system where shared control of policing reduces "clarity of responsibility" (Powell and Whitten 1993), I show that Mexican voters are able to assign responsibility across layers of government, at least in their own minds, for local homicides. In contrast with previous studies suggesting that voters hold national-level politicians accountable for major policy outcomes (e.g. Rodden and Wibbels 2011), or interpret crime as a threat against voting for parties opposing organized crime (Alesina, Piccolo and Pinotti 2016), I find that voters perceive municipal rather than state or federal incumbents to be responsible for local violent crime.

The paper proceeds as follows. Section 2 analyzes how political information cycles affect electoral accountability. Section 3 describes the Mexican context used to test my argument. The empirical analysis then identifies the three central components of my theory: sections 4-6 respectively examine information consumption and voter beliefs, incumbent electoral peformance, and the moderating role of media coverage. Section 8 discusses the normative and policy implications.

2 Theoretical argument

Electoral accountability is underpinned by retrospective (Ferejohn 1986) or prospective voting (Fearon 1999) models premised on the assumption that voters re-elect incumbent parties based on relevant publicly available information. In light of the mixed evidence that governments are held accountable for their performance in office (see e.g. Anderson 2007; Ashworth 2012; Healy and Malhotra 2013; Pande 2011), scholars have pointed to the importance of voter *access* to politically-relevant information in the media (e.g. Chang, Golden and Hill 2010; Larreguy, Marshall and Snyder 2017; Snyder and Strömberg 2010). However, such studies assume that voters actually *consume* the available information, and by focusing on performance metrics released before elections fail to capture how the timing of politically-relevant news may differentially impact voters.

I instead argue that the impact of news on voting behavior depends upon the timing of voter information consumption. In particular, I propose that news consumption follows a cycle where poorly-informed voters only seriously engage with politically-relevant news just before elections, and thus heavily weight the incumbent performance indicators in the news at this time when casting their ballot.

longer-term measures. Furthermore, by isolating plausibly exogenous variation in homicide shocks, the timing of news consumption, and access to local media, the results advance a literature that has predominantly correlated indicators of violence with electoral outcomes.

2.1 Political information cycles

There are various reasons to believe that consumption of politically-relevant news increases before elections. First, to the extent that politics is a pure consumption good (e.g. Hamilton 2004), electoral campaigns are likely to particularly interest voters. While coverage of political events out of election season is often more focused on specific policy issues, campaigns are specifically designed to appeal to voters. Second, Marshall (2016) shows that voters in social networks that collectively value knowledge about politics may strategically acquire more information to cultivate a reputation as politically sophisticated, especially around elections when political discussion is more common (see also Baker, Ames and Renno 2006). Third, voters may feel a civic duty to become informed around elections (Feddersen and Sandroni 2006). Fourth, voters without instrumental or intrinsic motivations may consume more information around elections simply because more air time is devoted to political news, advertising, and specific election programming. Even for uninterested voters, such information becomes difficult to avoid, especially where there are relatively few channels available (Prior 2007) or such information is recast as entertainment (Baum 2002), and performance indicators are increasingly placed in the context of appraising incumbent politicians and parties (Semetko and Valkenburg 2000).

Regardless of whether increased consumption is driven principally by voter demand for information or political and media supply of information, consumption of politically-relevant news is likely to reflect a political information cycle with a clear spike prior to elections. Such increased consumption is likely to cause many voters—especially in developing contexts where baseline political knowledge is relatively low (Pande 2011)—to only engage with the news before elections. Relative politically engaged voters are likely to to ratchet up their consumption. Since the timing of news consumption is the foundation of my argument, it is essential to first establish the existence of such political information cycles:

H1. *Voters start to consume, or consume relatively more, news just before elections.*

Akin to voters learning about candidate positions over an electoral campaign (e.g. Alvarez 2001; Druckman 2005; Hirano et al. 2015; Lenz 2009, 2012), increased information consumption is likely to translate into greater voter awareness of current affairs including salient valence issues.

2.2 Implications for electoral accountability

Political information cycles may impact voters' political beliefs by emphasizing politically-relevant information in the news when consumption is greatest. Events in the news may prime the salience

of valence issues, or induce voters to learn about the performance of incumbent politicians (Lenz 2012). For example, if there are many homicides in the news just before an election, voters may become more concerned about public security and negatively update their beliefs about the incumbent party's ability to address this issue. This requires that voters believe the signal to be informative about the party's competence. These issue priming and learning mechanisms imply:

H2a. *Voters care more about issues that are in the news just before elections.*

H2b. *Negative information about incumbent performance on salient issues just before elections reduces confidence in the incumbent party's competence for office.*

Especially at local elections, voters are poorly informed about incumbent performance in office.[8] There is thus good reason to believe that voters may substantially update their beliefs in line with the news that they consume before elections. With weak prior beliefs, voters may still update substantially when recent news is a weak signal of performance over the full electoral cycle, especially if they are overconfident in the signal's precision.

For such beliefs to translate into voting behavior, the issues in the news before elections must be sufficiently important to voters (e.g. Krosnick and Kinder 1990).[9] The model in the Appendix, which derives the hypotheses enumerated in this section, shows that the electoral impact of increasing the salience of an issue can be ambiguous, depending upon posterior beliefs about the relative competence of incumbent and challenger parties on that issue.[10] However, if the news substantially updates voters' posterior beliefs about the suitability of a candidate for office—as the literature examining voter learning about candidate positions suggests (Hirano et al. 2015; Lenz 2009)—the learning effect dominates and implies that:

H3. *Negative information about incumbent performance on salient issues just before elections reduces the incumbent party's vote share.*

The voting implications of news consumption also depend upon how voters attribute responsibility for performance across different levels of government. Particularly in federal systems where powers are shared or assigned according to non-transparent rules, disentangling such responsibility

[8]Even in the U.S., Healy and Lenz (2014) provide evidence that voters use recent economic performance over the last year to proxy for cumulative performance over a Presidential term.

[9]Rather than policy issues, voters may rely heavily on partisanship, ethnicity or social ties (e.g. Brader and Tucker 2012; Casey 2015; Chandra 2007).

[10]The model shows that bad news about the incumbent on a given issue induces voters to negatively update about the party's suitability for office, and that this negative learning effect is compounded by increasing the issue's salience if the incumbent already scored badly on the issue. If the incumbent scored well, the negative learning effect dominates the opposing priming effect when the increase in salience is small or news departs substantially from a voter's prior.

can be challenging (Anderson 2006; Powell and Whitten 1993). Although the federal government is often the primary target of sanctions (e.g. Rodden and Wibbels 2011), voters may also rely on visible local indicators of responsibility such as the frequency with which they engage with local as opposed state or federal police forces. Changes in voting behavior induced by recent news are then most pronounced when voters can clearly assign responsibility:

H4. *Negative information about incumbent performance on salient issues just before elections only reduces the vote share of incumbent parties deemed responsible for such performance.*

Exactly how voters *perceive* responsibility across layers of government on different issues is ultimately an empirical question.

However, the electoral impact of news reports before elections depends upon the availability and content of the news. Voters rely upon media sources such as television and radio to supply credible and *politically-relevant* news—news that pertains to a voter's incumbent party. Locally-based media are more likely to report information about relevant local incumbents (e.g. Snyder and Strömberg 2010). Furthermore, given that media sources may vary in the extent to which they distort or under-report certain types of news (e.g. Besley and Prat 2006; Gentzkow and Shapiro 2006; Mullainathan and Shleifer 2005), and market segmentation may mean that not all stations reach all types of voters (Barabas and Jerit 2009; Prat and Strömberg 2005), the ultimate impact of information about incumbent performance on voting behavior should increase with the number of local media stations as the likelihood that voters consume relevant news increases (Larreguy, Marshall and Snyder 2017). I thus hypothesize that:

H5. *Negative information about incumbent performance on salient issues just before elections only reduces the incumbent party's vote share if voters can access relevant information via the media, and is increasing in the level of such exposure.*

Moreover, events in the news before elections differentially impact different types of voters. Voters with weak priors, who may be consuming politically-relevant information for the first time in months or years, are most susceptible to news because their beliefs are most malleable (e.g. Hirano et al. 2015; Lawson 2004; Zaller 1992). Conversely, voters consuming news throughout the electoral cycle possess stronger prior beliefs over the issues they regard as important and how well different parties address such issues. They are also well-placed to distinguish informative from uninformative signals. These arguments are demonstrated formally in the Appendix, and receive empirical support from Da Silveira and De Mello (2011) and Larreguy, Marshall and Snyder (2016), who find that relatively uneducated voters in Brazil and Mexico respectively respond most

to political advertising. For incumbent performance indicators, I therefore hypothesize that:[11]

H6. *The consumption of negative information about incumbent performance on salient issues just before elections impacts most the beliefs and voting behavior of voters with weak priors.*

The empirical analysis will use education as a proxy for the strength of voter priors.

Nevertheless, even if voters primarily consume political information before elections, this does not entail that they entirely lack prior beliefs. Incumbent performance information available around the previous election may represent an important component of priors beliefs that voters can benchmark current indicators of incumbent party performance against. For example, if voters received a positive signal of incumbent performance on a given issue relating to a *previous* incumbent party's competence before the last election, a negative information in the new before the current election may cause voters to negatively update about the current incumbent party. Similarly, consecutive negative or positive signals about the same incumbent party may strengthen posterior beliefs. Conversely, voters should update less when two different parties experience the same signal, or the same party experiences different signals across time; these cases point to common shocks or high signal variance, and thus imply that voters should downweight the signal.

H7. *The impact of negative information about incumbent performance on the incumbent party's vote share is greater where voters previously received signals that opposition parties performed well or where such information compounds previous negative information about the incumbent party.*

To the extent that voters observe events in neighboring municipalities and believe that their governments would behave similarly in their own municipality, a similar logic may hold for spatial comparisons.

I now test these hypotheses in the context of local violent crime reported in the media—a prevalent performance indicator on a highly salient issue—before Mexican municipal elections.

3 Violent crime and political accountability in Mexico

Mexico was an archetypal competitive authoritarian regime until the late 1990s, but has since democratized significantly.[12] Three main political parties—the right-wing National Action Party

[11] The results follow most straight-forwardly from common priors. A sufficiently strong correlation between the position of a voter's prior distribution and education could reverse this result.

[12] The PRI had retained a stranglehold on power since 1929 by implementing populist policies, establishing strong clientelistic ties, effectively mobilizing voters, creating barriers to political entry, and manipulating electoral outcomes (e.g. Cornelius 1996; Greene 2007; Magaloni 2006).

(PAN), left-wing Party of the Democratic Revolution (PRD), and previously-hegemonic Institutional Revolutionary Party (PRI)—have competed for power in recent decades.[13] Despite growing partisan alignment, often induced by clientelistic exchanges, many Mexicans have yet to develop strong ties to specific political parties (Greene 2011; Lawson and McCann 2005).

Mexico's federal system is divided between three administrative and elected layers of government: approximately 2,500 municipalities, 31 states (excluding the Federal District of Mexico City), and the federal government. Constitutional reforms in the mid-1990s substantially increased mayoral autonomy over the provision of local public services (see Diaz-Cayeros, González and Rojas 2006), inducing municipal spending to rise to around 20% of total government spending.[14] The median municipality contains 13,000 people, although large cities with the exception of the Federal District—which is divided into delegations—represent singular municipal bodies.

Municipal mayors are typically elected every three years to non-renewable terms,[15] and enter office between three and seven months after election day. Municipal elections are almost always held in tandem with state legislative elections, although gubernatorial elections are often held separately. I refer to simultaneous municipal and state legislative elections as "local elections." Such local elections have become increasingly competitive, with fewer than 50% of municipal incumbent parties gaining re-election. Congressional elections to the House and Senate are held every three years, while the President is elected every six years. The majority of local elections do not coincide with federal elections.

Although incumbent mayors could not seek re-election, Mexico's party-centric system ensures that voters are likely to hold incumbent parties accountable for the actions of individual politicians. First, differences in candidate selection mechanisms across parties at the state level ensure that candidate choices differ across party but are highly correlated within parties (Langston 2003). Second, voters are substantially better informed about parties than individual politicians (Chong et al. 2015; Larreguy, Marshall and Snyder 2016); several examples are provided below. Ultimately, various studies show that voters frequently punish parties for an incumbent's performance in office (Arias et al. 2017; Chong et al. 2015; Larreguy, Marshall and Snyder 2017).

[13] After attaining legislative pluralities in the 1990s, the long-time conservative opposition PAN fully broke PRI control when Vicente Fox won the Presidency in 2000. In 2006, Felipe Calderón retained the Presidency for the PAN, narrowly defeating the PRD's Andrés Manuel López Obrador. After regaining legislative majorities, but never relinquishing regional control (Langston 2003), the PRI candidate Enrique Peña Nieto won the Presidency in 2012.

[14] This expansion of spending included policing, although most municipalities already had their own police forces. Presidents Fox and Calderón also increased federal transfers to municipalities for policing in the 2000s (Sabet 2010).

[15] From 2018, re-election will become possible for legislators, and mayors in most states.

3.1 Public security forces

Responsibility for public security is shared across levels of government, and like other federal systems such as the United States, mayors play an important role in fighting crime. Both state and federal laws can be used to prosecute criminals, and uniformed (preventive) and investigative police forces exist at both the federal level and in each of Mexico's 31 states and the Federal District of Mexico City. State and federal police, and increasingly the army, are responsible for investigating major crimes in different jurisdictions. State police investigate state crimes such as homicides, while federal officers focus on organized crime (Reames 2003). However, around three-quarters of municipalities also possess their own police force. Although such forces also support higher-level operations and supply information, their principal role is preventive: they primarily patrol the streets and maintain public order, address administrative issues, and respond first to criminal incidents (Reames 2003; Sabet 2010). Municipal police are by far most numerous, accounting for more than half of Mexican enforcement personnel (Sabet 2010). A large central police force controlled by the mayor of Mexico City covers all delegations within the Federal District.

In municipalities with their own police force, mayors can influence the medium-term incidence of crime. Mayors choose the local police chief and set local policies. PAN mayors have played a key role in supporting Calderón's crackdown on Mexico's DTOs (Dell 2015), while political alignment across neighboring municipalities has reduced rates of violent crime (Durante and Gutierrez 2015). The widely-publicized case of Iguala represents a particularly egregious example of political control of the local police, where the mayor and his wife exploited their links with law enforcement to cover up and possibly instigate the murder of 43 student protesters in 2014. However, as shown below, mayors are not able to manipulate short-term crime rates, at least with respect to homicides.

Given the number of municipal police offers and their "on-the-streets" presence, it is not surprising that municipal police are the foremost police force in the minds of voters. When asked which law enforcement authorities they can identify, the 2010 National Survey About Insecurity (ENSI) finds that while 75% of voters could identify municipal police, only 38% and 48% respectively recognized state and federal forces. Focus groups that I conducted in the field suggest that voters primarily blame local government for crime in their community.

3.2 Trends in violence

According to the United Nations Office on Drugs and Crime (UNODC), Mexico suffers one of the world's highest homicide rates. In 2012, 21.5 people per 100,000 were intentionally murdered

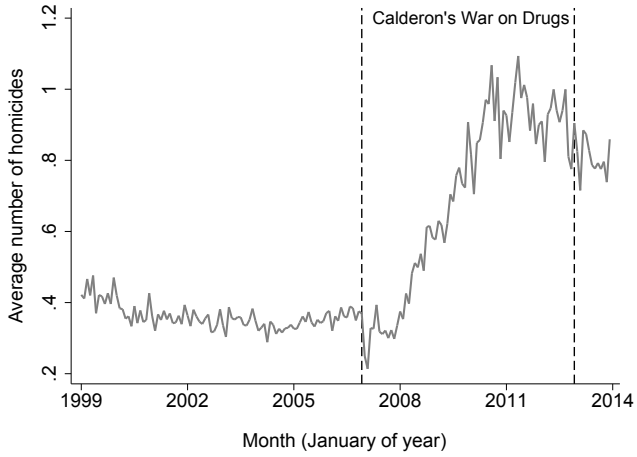


Figure 1: Trends in monthly homicides (as defined by INEGI) in the average Mexican municipality, 1999-2013

(UNODC 2013). This represents the 20th highest homicide rate in the world—slightly less than South Africa, Colombia and Brazil, and slightly more than Nigeria, Botswana and Panama.

Using data from the National Institute of Statistics, Geography, and Information (INEGI), Mexico's autonomous statistical agency, Figure 1 plots the number of homicides per month occurring in the average municipality between 1999 and 2013. INEGI defines an intentional homicide as an unnatural death, as determined by a coroner's report.[16] Month and location of death are also based on the coroner's report. The monthly homicide rate has been substantial throughout this period, but increased dramatically after President Calderón entered office in December 2006 and began Mexico's "War on Drugs" (see Dell 2015). Although these figures may understate the true homicide rate if unnatural causes cannot be detected (México Evalúa 2012), it is unlikely that the coroners reports are falsified because aggregated data is only released publicly several years after the event.[17] Encouragingly, 94% of homicides were registered in the same month that they occurred. The homicide clearance rate is only 20% (México Evalúa 2012), and drug-related homicides—which are regionally concentrated—represent 50% of homicides over this period (Heinle, Rodríguez Ferreira and Shirk 2014). However, many municipalities only rarely experience a homicide; in fact,

[16] Although there is no official measure of drug-related homicides enshrined in Mexican law, the federal government has sporadically released monthly data. This data suffers from various problems beyond its short time-series (see Heinle, Rodríguez Ferreira and Shirk 2014), while Dell's (2015) estimates show that total homicides measured by INEGI rose substantially more than drug-related homicides following Calderón's war on drugs. Furthermore, voter fears about public security are not only linked to organized crime, but also reflect equally-prevalent non-drug related homicides (of which only a tiny fraction occur within families). Table A11 shows broadly similar results for the small sample (December 2006-October 2011) when homicides were classified as drug-related by a committee composed of representatives from the ministries constituting the National Council of Public Security.

[17] Heinle, Rodríguez Ferreira and Shirk (2014) discuss Mexican homicide metrics in depth.

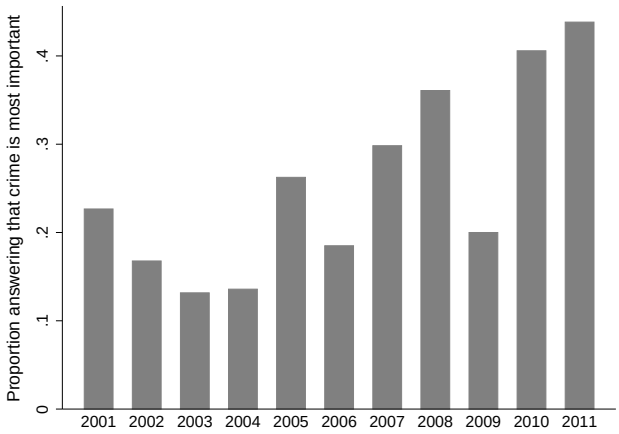


Figure 2: Voters listing crime as the most important problem that Mexico faces, 2001-2011

Notes: Data are from the 2001-2011 Latinobarómetro surveys. Crime includes concerns about crime/public security, drug trafficking, violence, or terrorism/political violence.

only one homicide occurs in the median municipality each year.

Unsurprisingly, voters are concerned about Mexico's high rates of violent crime. Like many other Latin American countries (Blanco 2013), Figure 2 shows that the number of Mexican Latinobarómetro respondents citing public security as the most important problem facing the country increased broadly in line with homicide rates. For most of the 2000s, public security registered as the most salient issue for voters ahead of the economy.

3.3 Media coverage and voter political knowledge

Like most developing countries, broadcast media is voters' main source of news. According to the 2009 Latinobarometer, 83% of Mexicans receive political information from television, 41% from radio, 30% from newspapers, and 41% from family, friends and colleagues. In 2008, around 10% listened to radio every day, while the average person watched 4 hours of television (Ibope/AGB México 2009). In contrast, only 21% have access to the internet at home, while 3G coverage only started expanding in 2011.[18]

The nature of media coverage thus plays a central role in determining whether voters voters receive politically-relevant information. Mexico contains 852 AM radio stations, 1,097 FM stations and 1,255 television stations, which generally cover relatively small geographic areas (see Figure 4 below). Most stations form part of broader regional and national radio or television networks—such as Grupo ACIR, Radiorama, Televisa, and TV Azteca—where affiliates share branding or are

[18]In 2010 and 2011, 60% of Latinobarómetro respondents reported that they never use the internet.

owned-and-operated subsidiaries (Larreguy, Marshall and Snyder 2017). Among radios, a 2016 survey shows that 83% of radios report news more than once a day, and typically report on municipal rather than national issues. For television stations, identical entertainment content is generally bought from or relayed by network providers. However, while national news is typically centrally provided, affiliates and regional subdivisions emitting from major cities within each state also provide significant local news content. Of the 52 distinct television channels (excluding Mexico City's 24-hour news channels) for which schedules were available in 2015, the average channel broadcasts 3.6 hours of news coverage each weekday (both before and after the June elections). Slightly less than half of this news is devoted to state or city-specific programming. Private radio and television outlets rely on advertising revenues to support themselves, and thus face strong incentives to tailor their programming to local audiences (Larreguy, Marshall and Snyder 2017). The pro-PRI coverage biases that characterized elections before the late 1990s have somewhat dissipated (e.g. Hallin 2000; Lawson 2002).

Homicides are not always reported in great depth, but are regularly covered in print (Ley 2014; Osorio 2015) and "omnipresent" in the local broadcast media (Trelles and Carreras 2012).[19] In a survey of all local radio stations in 2016, 96% report regularly covering local security stories, while Table 9 below demonstrates that newspaper reporting of violent crime indeed tracks the homicide rate. Based on the 2010 ENSI survey, 87% of respondents report learning about public security in the country and in their state from television news programs, while 34%, 29%, and 9% respectively report learning from radio news programs, periodicals or newspapers, and the internet.[20] Furthermore, 82% of voters believe that television news has the most important influence on public opinion.

Despite claiming reasonable levels of attention to political news, knowledge of public affairs is limited. I show below that only half of Mexicans can answer basic questions about politics. Although 80% of voters can correctly identify their municipality's incumbent party (Arias et al. 2017), Castañeda Sabido (2011) and Chong et al. (2015) similarly find that voters are generally unaware of mayoral responsibilities and performance in office. This lack of knowledge suggests that many voters may possess weak prior beliefs about their incumbent party's suitability for office, and may thus be particularly likely to internalize information when they actually follow the news.

[19]For example, see this local news report of an gangland-style murder in Monterrey. The same news station also reports on domestic murders, e.g. this case of a woman strangling her partner. News programs may continue to report on arrests and cases that go to trial, especially when the defendants are found guilty. For example, on 17th February 2015, W Radio reported here that two men were convicted of intentional homicide and sentenced to 27 years.

[20]Although social networks may represent a key source of information on more explicitly political issues (Baker, Ames and Renno 2006), comparatively few learn from work colleagues (5%) or family, friends and neighbors (13%).

4 The existence of political information cycles

The starting block of my argument is that voters consume substantially more political information just prior to elections. While some voters only start consuming politically-relevant information during election campaigns, others will consume more than before. This section identifies the effect of upcoming local elections, providing survey-level evidence for the existence of such political information cycles.

4.1 Data

To examine political information cycles, I use four cross-sectional waves of the National Survey of Political Culture and Civil Practices (ENCUP) conducted over several weeks in November 2001, February 2003, December 2005, and August 2012.[21] Each round draws stratified random samples of around 4,500 Mexican voters for face-to-face interviews from pre-selected electoral precincts within urban and rural strata defined by the electoral register.[22] The survey was commissioned by the Interior Ministry and designed to be broadly nationally representative, and focuses on the country's political culture rather than more contentious questions about elections.[23] Each survey wave was conducted in a different month of the year, and the irregularly-spaced waves do not correspond with the federal electoral cycle. The pooled sample thus includes up to 17,213 respondents across 539 municipalities (including delegations within the Federal District).

I measure political news consumption in two ways. First, I use the frequency with which voters watch or listen to the news, programs about politics, or programs about public affairs.[24] To understand the margins at which consumption is changing, I examine various consumption intensities: never, at some point, at least monthly, at least weekly, and daily. I also compute a 5-point scale (from 0 to 4). Second, I assess whether politically-relevant news consumption translates into greater political knowledge. I focus on topical political knowledge that voters are

[21] The 2008 wave does not provide a respondent's municipality and asks different questions about media consumption. The ENCUP surveys are preferred to the Latinobarómetro, which covers more years, but does not always ask about media consumption and especially political information, and does not contain municipal identifiers (only states). Nevertheless, the Latinobarómetro returns similar results. My identification strategy is not compatible with the rich Mexican Panel Surveys that have been conducted around the 2000, 2006 and 2012 presidential elections.

[22] In 2012, 5 broad strata were identified, and electoral precincts and then voters were randomly selected from within such strata to match the strata's rural-urban, gender, and age distribution. In 2005 and 2012, 10 voters were surveyed from each precinct according to specific directions (see the 2012 methodological manual here). Although such detailed sampling information is not available for the earlier surveys, the overall design is similar.

[23] The study was implemented by INEGI in 2001 and 2003, and private firms in 2005 and 2012. The specific objectives of the study, which does not address elections at all, are enumerated here.

[24] I focus on radio and television, which are by far the most prevalent sources of political information in Mexico.

more likely to encounter through the media (Barabas et al. 2014). Topical political knowledge is measured as the first (standardized) factor from a set of indicators coding correct responses to simple factual questions regarding recent political news.[25] A advantage of this measure is that voters cannot falsely inflate their knowledge; the average respondent answered around half the questions correctly. Because these consumption measures cannot distinguish changes in demand for information among voters from changes in the supply of political news provided by media outlets, this paper emphasizes *equilibrium* political information consumption.

4.2 Identification strategy

Using a similar design to Eifert, Miguel and Posner (2010), I exploit the timing of survey administration with respect to state-specific election cycles to identify the effects of an upcoming election. Operationally, I code an indicator for respondents facing an upcoming municipal election, and typically a simultaneous state legislative election, within five months of the survey. Although there is some state-level discretion over campaigning in such local elections, campaigns generally last around five months.[26] As noted above, the ENCUP surveys are administered irregularly—both in terms of the month in which the survey was conducted and the number of years between survey rounds. In conjunction with the fact that states historically follow different electoral cycles, varying both in the month and year of their elections (including over time within states, especially due to a recent constitutional reform),[27] whether the surveys were administered just before or after elections or whether an election was held recently at all effectively occurs by chance.

There is no evidence to suggest that these surveys, which do not explicitly address elections, were strategically timed. This is reinforced by the fact that 27 of Mexico's 32 states (including the Federal District) register an election in one of the survey years, of which 14 only hold an election in a survey year but after the survey occurred. Supporting the plausibility of these arguments, Table A2 shows that individuals surveyed prior to local elections are well-balanced over individual and municipal level characteristics: only 2 of 18 tests report a significant difference at the 10% level.

[25] In 2001, 2003, 2005 and 2012 respectively, respondents were asked 6, 3, 3 and 4 questions (see Appendix). Questions regarding basic (national and local) knowledge of political institutions were excluded.

[26] Political advertising slots are specifically allocated for these purposes five months prior to federal elections (Larreguy, Marshall and Snyder 2016). The results are robust to defining upcoming elections by any number of months between 1 and 12.

[27] See Table A1 in the Appendix for a full list of municipal elections by month. In Chiapas, Coahuila, Guerrero, Michoacán, Quintana Roo, Veracruz, and Yucatán, the typical 3-year cycle was adjusted over the sample period by switching to a 2 or 4-year term for a single electoral cycle. Moreover, following a constitutional amendment in 2007, states were subsequently mandated to hold local elections on the same day as federal elections when the state cycle coincides (Serra 2014). Consequently, states also changed the month of their elections. To reduce constant electoral competition, some states holding off-cycle elections also homogenized elections after the reform.

Table A5 shows that neither changes in upcoming local elections nor violence predict changes in whether a municipality is included in a given survey round.

To identify the effect of an upcoming local election, I first estimate the following regression for respondents i in municipality m at survey year t:

$$Y_{imt} = \beta \, Upcoming\ local\ election_{mt} + \mu_t + \varepsilon_{imt}, \tag{1}$$

where Y_{imt} is a measure of politically-relevant news consumption. Survey fixed effects, μ_t, capture common period effects that might arise from concurrent federal elections (in 2003 and 2012), presidential elections (in 2012), or national trends in political behavior. As a robustness check, I control flexibly for the only—but potentially important—imbalance on respondent education. Throughout, standard errors are clustered by municipality.

Although upcoming local elections are plausibly exogenous to the timing of the survey, I also implement a difference-in-differences design by including state fixed effects, λ_s. This design exploits only variation in the presence of state-level upcoming elections within states across surveys, and thus relies on the parallel trends assumption that states facing and not facing upcoming elections at the time of the survey follow similar trends in political news consumption. While this assumption is especially plausible in a context where all states repeatedly hold elections, this design entails an important efficiency cost. Because half the states in the dataset never experience an election within 5 months in advance of an election, there is no variation in the treatment for half the sample. In addition, the estimates only pertain to the 14 states experiencing at least one upcoming local election over the four survey waves.

4.3 Upcoming elections increase news consumption and political knowledge

The results in panel A-C of Table 1 demonstrate that local elections increases both the likelihood that a voter consumes any political information at all as well as the level of information consumed.[28] Column (1) of panel A shows that an upcoming local election significantly increases the probability that a voter listens or watches news at all by five percentage points. Although this shift could still reflect an increase in the supply of political news on television and the radio, only consuming news before elections is likely to represent a conscious acquisition choice by voters that had previously avoided the relatively extensive news coverage available outside election campaigns. Column (1) in panel B also reports a large increase in weekly news and political program consumption. The effect on the five-point scale in column (1) of panel C is also strongly positive.

[28] Comparable questions from 2001 were not available.

These findings suggest that virtually all types of voters, ranging from the least to most engaged, consume more political information prior to statewide elections.

Panel D shows that the cycle of increased information consumption prior to elections mirrors changes in political knowledge. Contrary to potential concerns about social desirability bias, column (1) demonstrates that voters facing an upcoming local election are more than a quarter of a standard deviation, or nine percentage points, more likely to correctly answer a question testing their political knowledge.[29] This increase indicates that voters actively engage by internalizing political information—a prerequisite for political information to influence electoral accountability.

Column (2)-(9) demonstrate the robustness of these findings at both the extensive and intensive margins of political news consumption. First, column (2) shows that the results are broadly similar when including fixed effects for the five educational categories. This reduces the sample because education was not elicited in 2005. Second, at notable efficiency cost, column (3) reports the difference-in-differences results including state fixed effects. Although this approach effectively removes observations from states that do not experience an upcoming election in any survey (more than half the sample), the results are surprisingly stable and remain statistically significant in spite of the reduced precision. Third, the results do not depend upon the particular definition of the upcoming election indicator. Columns (4)-(6) instead define an upcoming local election as a survey within two months before election day. Although the number of instances of upcoming elections decreases, the increased point estimates provide further confidence that voters indeed consume most political information before elections. Adopting a linear approach, columns (7)-(9) similarly confirm that voters exhibit greater news consumption and political knowledge as the number of months until the next local election decreases. In sum, the results provide strong evidence that political news consumption indeed tracks the electoral cycle.

5 Local violence and electoral accountability

Given that voters are significantly better informed just before an election, voters may punish incumbent parties for poor performance at the ballot box—even if such poor performance is a weak signal of performance over the entire election cycle. To test this, I exploit plausibly exogenous spikes in the occurrence of homicides before elections to identify the effect of short-term performance indicators coinciding with voter news consumption cycles on the incumbent party's electoral prospects.

[29] Like Hirano et al. (2015), this result shows that voters learn more about politics during electoral campaigns, even around lower-level elections. Unreported results interacting upcoming local elections with baseline covariates for both consumption and knowledge outcomes indicates that the same voters consuming more news also become more knowledgeable about politics.

Table 1: The effect of upcoming local elections on political news consumption and topical political knowledge

	Measure of political news consumption								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Outcome: Watch and listen to news and political programs ever									
Upcoming local election	0.049***	0.033*	0.006						
	(0.014)	(0.017)	(0.018)						
Upcoming local election				0.093***	0.079***	0.044*			
(two-month)				(0.015)	(0.015)	(0.023)			
Months until next election							-0.002***	-0.001*	-0.002***
							(0.001)	(0.001)	(0.001)
Observations	13,030	8,330	13,030	13,030	8,330	13,030	13,030	8,330	13,030
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.87	0.86	0.87	0.87	0.86	0.87	0.87	0.86	0.87
Treatment mean	0.19	0.24	0.19	0.02	0.03	0.02	17.53	18.88	17.53
Panel B: Outcome: Watch and listen to news and political programs weekly									
Upcoming local election	0.082***	0.053**	0.058*						
	(0.022)	(0.024)	(0.030)						
Upcoming local election				0.116***	0.095***	0.028			
(two-month)				(0.038)	(0.030)	(0.045)			
Months until next election							-0.005***	-0.005***	-0.004***
							(0.001)	(0.001)	(0.001)
Observations	13,030	8,330	13,030	13,030	8,330	13,030	13,030	8,330	13,030
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.63	0.63	0.63	0.63	0.63	0.63	0.63	0.63	0.63
Treatment mean	0.19	0.24	0.19	0.02	0.03	0.02	17.53	18.88	17.53
Panel C: Outcome: Watch and listen to news and political programs scale									
Upcoming local election	0.276***	0.174**	0.158						
	(0.075)	(0.079)	(0.098)						
Upcoming local election				0.416***	0.343***	0.132			
(two-month)				(0.122)	(0.093)	(0.145)			
Months until next election							-0.016***	-0.014***	-0.012***
							(0.002)	(0.003)	(0.003)
Observations	13,030	8,330	13,030	13,030	8,330	13,030	13,030	8,330	13,030
Outcome range	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}
Outcome mean	2.58	2.57	2.58	2.58	2.57	2.58	2.58	2.57	2.58
Treatment mean	0.19	0.24	0.19	0.02	0.03	0.02	17.53	18.88	17.53
Panel D: Outcome: Topical political knowledge									
Upcoming local election	0.312***	0.152***	0.119***						
	(0.051)	(0.036)	(0.044)						
Upcoming local election				0.218**	0.188**	0.002			
(two-month)				(0.102)	(0.080)	(0.080)			
Months until next election							-0.006***	-0.003**	-0.002
							(0.002)	(0.001)	(0.001)
Observations	17,213	12,513	17,213	17,213	12,513	17,213	17,213	12,513	17,213
Outcome range	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]
Outcome mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Treatment mean	0.16	0.18	0.16	0.03	0.04	0.03	18.58	19.88	18.58
Education fixed effects		✓			✓			✓	
State fixed effects			✓			✓			✓

Notes: All specifications include survey-year fixed effects, and are estimated using OLS. The outcomes in panels A-C were not collected in the 2001 survey, and education was not collected in 2005. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

I then contrast the effect of pre-election homicide shocks with longer-term homicide rates.

5.1 Data

To examine the electoral effects of violence in the run-up to elections I utilize two main sources of data. First, electoral returns for municipal, state and federal elections covering Mexico's c.67,000 electoral precincts were assembled from the Federal Electoral Institute (IFE),[30] state electoral institutes, and freedom of information requests. I focus on municipal elections between 1999 and 2013, for which data is widely available across all states; state and federal electoral returns are used to parse accountability channels below. Since municipal elections generally occur every three years, the full dataset contains around four elections per municipality. Second, I combine the electoral data with the INEGI's monthly municipal homicide data (described in section 3).

The empirical analysis focuses on two measures of incumbent electoral performance: a municipal-level indicator for whether the incumbent party won the election, and the change in the incumbent party's vote share at the precinct level.[31] Reflecting their persisting power at the state level (see Langston 2003), 53% of incumbent mayors in the sample are from the PRI; 29% are from the PAN and 12% are from the PRD. As noted above, voters generally sanction parties for the performance of their politicians in Mexico's party-centric system.[32] As in Brazil (Klašnja and Titiunik forthcoming), the average incumbent party experiences a 5.7 percentage point decline in their vote share, but still wins 54% of races. Turnout are typically around 60%. Municipalities without their own police forces, including the delegations of the Federal District, are excluded from the analysis.[33]

[30] IFE recently became the National Electoral Institute (INE).

[31] Similar results obtain for the incumbent vote share level as a robustness check, but I prefer the precision associated with changes in vote. Given that the analysis weights by the number of registered voters in each precinct and standard errors are clustered by municipality, the precinct-level vote share estimates are almost identical to municipal-level estimates. Because some precincts are missing within municipalities, a precinct-level analysis has slightly more power. The local media analysis exploits precinct-level variation.

[32] In the 32% of cases where the incumbent won as part of a coalition formed by several parties, and given such coalitions can vary across elections, if the coalition changes I define the incumbent as the party with the largest vote share at the following election. In general, coalitions are dominated by large parties; the three main parties represent more than 90% of incumbents. Table 2 shows that the results are robust to restricting attention to incumbents containing the three largest parties and single-party incumbents.

[33] Using the descriptions of municipal public security forces provided by the municipal governments in the 2000, 2002, 2004, 2011 and 2013 ENGM surveys, and imputing data for missing years, I exclude municipalities without a police force or relying on state, federal, community, private, or other security forces. Given the difficulty of assigning responsibility, the few municipalities with inter-municipal and civil association-run police are excluded. The Appendix describes the imputation procedure.

5.2 Identification strategy

While the national homicide rate has changed relatively smoothly over time, this masks considerable inter-month volatility within municipalities. At the height of the War on Drugs in 2010, the monthly homicide count in Ciudad Juárez, Chihuahua, oscillated dramatically, e.g. from 394 in September to 477 in October before falling to 242 in November and again rising to 309 in December. Conversely, the median municipality experiences no homicides in any given month, and only one homicide over a year. More generally, month-to-month fluctuations appear fairly random: since 1999, the average municipal homicide count registering a 0.002 month-on-month increase is dwarfed by its 1.7 homicide standard deviation. Moreover, the count is almost exactly as likely to increase as decrease—both in the run-up to elections and in the middle of a government's tenure.[34]

To identify the electoral effects of homicides occurring at the times when voters consume most politically-relevant news, I exploit *short-term deviations from trend* in the number of homicides around local elections.[35] In particular, I compare otherwise similar elections experiencing a short-term spike relative to the municipal homicide rate before the election to elections experiencing a short-term relative decrease. This strategy is preferred to a design examining changes in pre-election homicides rates across elections because it is hard to purge the correlation between such changes and the longer-run homicide trends that I seek to differentiate pre-election shocks from. Nevertheless, Table A12 shows broadly similar results using a difference-in-differences strategy to examine deviations between pre-election homicide rate and the homicide rate over the electoral cycle.

In particular, I define a municipality as "treated" by a pre-election homicide shock if, conditional on experiencing at least one homicide over the four months spanning the penultimate month before an election and the second month after an election, a municipality m experiences more homicides in the two months before an election in month τ than the two months after the

[34] Across municipalities since 1999, the monthly homicide change was positive in 11.86% of cases and negative in 11.79% of cases. In election months, these shares are 11.90% and 11.87%. In the month preceding an election, the shares are 11.87% and 11.86%. Most changes are zero because most municipalities do not experience a homicide. This ratio is identical when conditioning on a homicide in either the last month or the current month.

[35] A benefit of the unavailability of the number of homicides reported by broadcast media stations is that the empirical strategy is not susceptible to differential media reporting biases. Table A6 shows that delayed homicide registration, a potential indicator of strategic registration and missing homicides, is balanced over homicide shocks.

election:[36]

$$\textit{Homicide shock}_{m\tau} \equiv \begin{cases} 1 & \text{if } Homicides_{m\tau-2} + Homicides_{m\tau-1} > Homicides_{m\tau} + Homicides_{m\tau+1} \\ & \text{and } \sum_{\tau'=\tau-2}^{\tau+1} Homicides_{m\tau'} > 0, \\ 0 & \text{if } Homicides_{m\tau-2} + Homicides_{m\tau-1} \leq Homicides_{m\tau} + Homicides_{m\tau+1} \\ & \text{and } \sum_{\tau'=\tau-2}^{\tau+1} Homicides_{m\tau'} > 0, \\ . & \text{if } \sum_{\tau'=\tau-2}^{\tau+1} Homicides_{m\tau'} = 0. \end{cases} \quad (2)$$

Akin to Ferraz and Finan's (2008) comparison of Brazilian municipalities, where federal audit reports were randomly released just before and just after municipal elections, conditioning on the occurrence of a homicide around elections produces the relevant counterfactual by extracting homicide trends.[37] I use a two-month window to capture the most final months of the campaign and the post-election period before the winner enters office, while covering a sufficiently short span that month-to-month changes in the homicide count are plausibly random. This homicide shock equates to an increase of around three homicides per month in shocked municipalities relative to control municipalities. I show similar results when using one or three month bandwidths instead. After dropping the 30% of municipalities where no homicide is registered in either the two months before or after the election, Figure A1b in the Appendix highlights the final sample.

The key identifying assumption is that, within municipalities, the *timing* of homicides around elections is effectively random. Leveraging within-municipality variation controls for all time-invariant municipal-level features, and also reduces noise generated by differences in homicide histories among municipalities receiving the same treatment.[38] However, although month-to-month changes in homicide rates are highly idiosyncratic, this does not necessarily imply that short-term changes in homicide shocks across elections in a given municipality occurred by chance.

I first show that homicide shocks are uncorrelated with a wide variety of pre-treatment covariates and homicide pre-trends. First, Table A6 in the Appendix examines 101 time-varying and time-invariant covariates capturing demographic, socioeconomic, media coverage, and political

[36]Elections typically occur on the first Sunday of the month, so I define the treatment using the two months prior to the election month (i.e. $\tau-2$ and $\tau-1$). For the 9% of elections held on the 16th or later, I use months $\tau-1$ and τ. I show below that the results are robust to dropping elections not held during the first half of the month.

[37]These municipalities differ significantly from even municipalities experiencing only a single homicide over the 4-month window. Municipalities without a homicide are less developed, less politically competitive, and more violent.

[38]The robustness checks below show that a simple difference-in-means yields similar results.

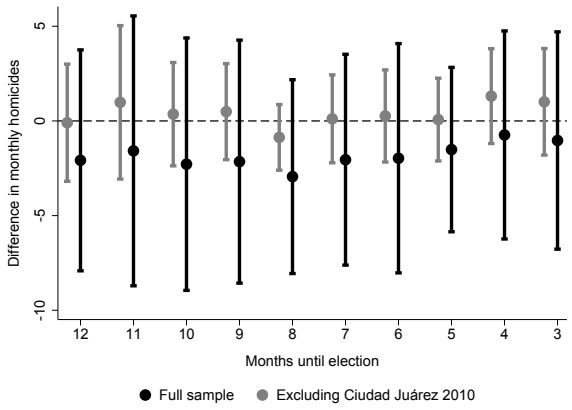
features of each municipality. Only five differences are statistically significant at the 10% level.[39] In particular, homicide shocks are equally distributed across political variables including the incumbent's previous vote share, the competitiveness of the previous election, and the incumbent's party identity, but also with respect to the number of media stations, the share that own a television or radio, and educational attainment. Second, Figure 3a examines whether municipalities receiving positive and negative shocks exhibit differential pre-trends in homicide rates or different underlying levels of violence. The figure shows that the difference in monthly homicides between treated and untreated precincts is both relatively constant over the 10 months preceding the period used to define homicide shocks and never statistically significant.[40] In fact, most of the difference reflects the extremely high homicide rate in Ciudad Juárez in 2010. The balance tests in Table A6 also confirm that the average number of homicides in the 12, 6 and 3 months preceding the period defining the treatment do not significantly vary with homicide shocks. Furthermore, I show below that a placebo shock defined 6 months prior to the election does not affect electoral outcomes, and that the results are robust to controlling for homicide levels. Third, consistent with sampling variability rather than strategic manipulation, Figure 3b shows that the distributions of homicides prior to the period defining a shock, two months before elections, and two months after elections are very similar.

I also find no evidence for the more specific concern that DTOs alter the number of homicides around elections to signal their preferred electoral outcome (e.g. Alesina, Piccolo and Pinotti 2016). Table A6 shows that homicide shocks are not significantly correlated with the number of drug-related homicides in the prior year over the 2006(Dec.)-2011 period when monthly data was made publicly available by the Mexican National Security System. Homicide shocks are also balanced across municipalities registering more than one drug-related homicide in any pre-election year over this period, and no more likely to occur in the 5% of precincts designated by the IFE as high-risk (typically locations with high DTO activity). In addition, I show below that the results are robust to removing municipalities with high levels of drug-related homicides and states with high DTO presence.

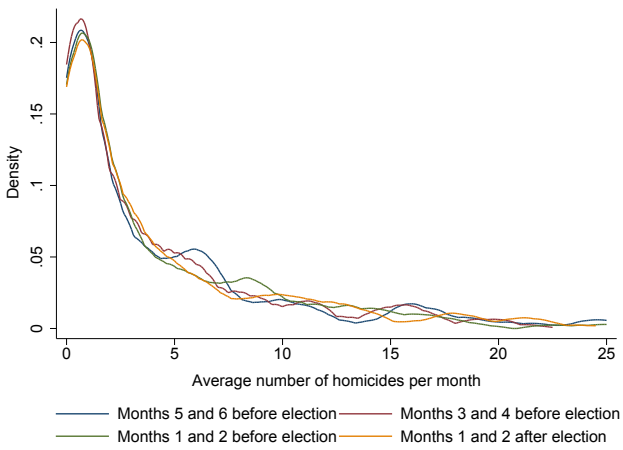
Nevertheless, the types of homicide could change without affecting overall levels. Gangland killings are typically concentrated among young and uneducated men, and are often committed using firearms or more gruesome methods—particularly if intended as signals. Using the International Classification of Diseases codes in INEGI's coroner reports, Table A6 also examines the causes of death and victim characteristics of homicides occurring in the two months before an

[39] Municipality fixed effects are excluded for the time-invariant 2010 Census variables. Table A7 shows analogous tests for precinct-level outcomes.

[40] Fewer homicides in treated municipalities would downwardly bias estimates if voters sanction greater violence.



(a) Difference in the number of homicides between municipalities with a positive and negative homicide shock, by month until the election (95% confidence intervals)



(b) Distribution of homicides by relation to election

Figure 3: Distribution of pre-election homicide shocks

Notes: Each bar in Figure 3a denotes the difference in the number of homicides in treated (positive shock) and control (negative shock) municipalities in the ten months prior to the period defining the homicide shock around the election. Each estimate is from a regression akin to the main estimating equation (3).

election. In particular, homicide shocks are well balanced with respect to the share of homicides caused by a firearm, hanging, or chemical substances. Of relatively frequent types of homicide, only cutting implements register a slight increase. Furthermore, there is no evidence that such homicides disproportionately afflict young, male, unmarried, or uneducated individuals that are most likely to be involved with organized crime.

A different potential concern is that homicide rates change around elections because effective governments can crack down on crime prior to win votes (e.g. Levitt 1997), or because election outcomes themselves impact post-election homicides rates. Although no expert on municipal politics that I interviewed believed that municipal governments crack down on local homicides, I examine this potential concern more systematically. First, proxies for state capacity—including municipality size and budget, police per voter, alignment with governors or the president, and neighbor homicide shocks—are uncorrelated with homicide shocks, while if anything mayors controlling a municipal police force are more likely to experience a shock. This indicates no differential ability to engage in such crackdowns. Second, using Osorio's (2015) newspaper-based measures of actual DTOs crackdowns, Table A6 demonstrates that homicide shocks are uncorrelated with violent enforcement, drug-related arrests, asset seizures, drug seizures and gun seizures in the two months before the election. Moreover, Table A8 finds no change in the number of security force employees

per voter in election years.[41] Third, although the new mayor does not enter office until more than two months after the election, Table A9 shows that neither pre-election political variables nor election outcome variables predict violence levels in the two months after an election.[42] The definition of homicide shocks including post-election homicides is thus unlikely to induce bias.

Ultimately, I estimate the effects of homicide shocks just prior to the election in precinct p of municipality m in election year t using regressions of the form:

$$Y_{pmt} = \beta Homicide\ shock_{mt} + \eta_m + \mu_t + \varepsilon_{pmt}, \tag{3}$$

where Y_{pmt} is either a municipal-level indicator for the incumbent party winning the election or precinct-level change in the incumbent party's vote share relative to the previous election. Municipality and year fixed effects are respectively denoted by η_m and μ_t. All observations are weighted by the number of registered voters.

5.3 Pre-election homicide shocks harm municipal incumbent parties

Table 2 shows that pre-election homicide shocks severely hinder the municipal incumbent party's electoral prospects. Column (1) of panel A demonstrates—for this study's main outcome—that such a homicide shock causes an 12 percentage point decline in the incumbent party's probability of being re-elected. This equates to a 22% reduction in the mean mayor's re-election probability. Column (1) of panel B reports that this decline reflects a 2.2 percentage point reduction in the incumbent party's vote share.[43] This equates to 0.45 percentage points for each additional homicide per month relative to the baseline levels in the treatment and control group.

These findings are robust to various checks. First, I show that the results are not sensitive to tests of the identifying assumptions. Column (2) of Table 2 first includes 32 time-varying municipal-level covariates to show that the results do not reflect imbalances on observables.[44] Column (3) shows that the results do not depend upon including municipality and year fixed effects. Given the baseline fixed effect structure, the greatest identification threat reflects time-varying unobservables.

[41] Although such data are only available from the National Census of Municipal Governments (ENGM) on an annual basis over five waves, I was able to impute 9,655 municipal-years.

[42] Dell (2015) finds that homicide rates increased where PAN mayors were elected during the Calderón administration, but such mayors do not enter office within two months of the election. More generally, post-election homicides are also uncorrelated with interactions between election outcomes and background covariates.

[43] Consistent with the model in Arias et al. (2017), unreported results indicate that the change in vote share reflects both an increase in turnout, but also a reduction in the total votes (as a share of registered voters) for the incumbent party.

[44] To preserve sample size, I exclude from the control set variables with significant missingness, namely homicide victim characteristics, threatened precincts (federal elections only), municipal spending, and child mortality.

Table 2: The effect of a pre-election homicide shock on municipal incumbent electoral outcomes

Panel A: Incumbent party win	Baseline spec.	Controls	No fixed effects	State-year effects	Incumbent party trends	Municipality-specific trends	Placebo 6 months earlier	Violence bin effects	Fewer than 25 homicides per month	Few drug homicides	Non-DTO states	One-month shock	Three-month shock	Four-month shock	First half of month	Main party incumbent	Single-party incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Homicide shock	-0.119**	-0.086*	-0.086**	-0.097**	-0.108**	-0.124		-0.126***	-0.107**	-0.119**	-0.108*				-0.116**	-0.120**	-0.136*
	(0.050)	(0.051)	(0.034)	(0.040)	(0.052)	(0.091)		(0.046)	(0.053)	(0.050)	(0.065)				(0.056)	(0.051)	(0.080)
Placebo homicide shock							-0.017										
							(0.071)										
Homicide shock (one month)												-0.047					
												(0.063)					
Homicide shock (three month)													-0.110**				
													(0.053)				
Homicide shock (four month)														-0.085*			
														(0.050)			
Observations	3,494	3,279	3,494	3,494	3,217	3,494	2,912	3,314	3,293	3,494	2,416	2,528	4,106	4,570	3,133	3,217	2,433
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.55	0.55	0.55	0.55	0.55	0.55	0.54	0.55	0.54	0.55	0.53	0.55	0.54	0.54	0.55	0.55	0.54
Homicide measure mean	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.43	0.42	0.41	0.48	0.43	0.42	0.42	0.42	0.40
Panel B: Change in incumbent party vote share	Baseline spec.	Controls	No fixed effects	State-year effects	Incumbent party trends	Municipality-specific trends	Placebo 6 months earlier	Violence bin effects	Fewer than 25 homicides per month	Few drug homicides	Non-DTO states	One-month shock	Three-month shock	Four-month shock	First half of month	Main party incumbent	Single-party incumbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Homicide shock	-0.022***	-0.018***	-0.017**	-0.018***	-0.015*	-0.030***		-0.026***	-0.023**	-0.022***	-0.025**				-0.019**	-0.022***	-0.033***
	(0.009)	(0.007)	(0.007)	(0.007)	(0.008)	(0.011)		(0.009)	(0.009)	(0.009)	(0.010)				(0.009)	(0.008)	(0.012)
Placebo homicide shock							-0.004										
							(0.012)										
Homicide shock (one month)												-0.015					
												(0.011)					
Homicide shock (three month)													-0.024***				
													(0.009)				
Homicide shock (four month)														-0.027***			
														(0.009)			
Observations	174,622	172,956	174,622	174,622	165,920	174,622	153,861	174,148	162,299	174,622	108,401	152,300	186,750	194,084	160,314	165,920	115,765
Outcome range	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.87]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]
Outcome mean	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.06	-0.05	-0.05	-0.06	-0.05	-0.05	-0.05
Outcome std. dev.	0.14	0.14	0.14	0.14	0.14	0.14	0.14		0.15	0.14	0.15	0.14	0.15	0.15	0.15	0.14	0.15
Homicide measure mean	0.42	0.42	0.42	0.42	0.42	0.47	0.42	0.42	0.43	0.42	0.41	0.47	0.43	0.42	0.41	0.42	0.40

Notes: Columns (2) and (3) respectively define homicide shocks over one- and three-month windows. Column (4) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (5) excludes municipalities averaging more than 25 homicides per month over the two months either before or after the election. Column (6) excludes municipalities that average more than one drug-related homicide a month over the 12 months before an election between 2006 and 2011. Column (7) excludes states with high-level of DTO activity (see footnote 46). Columns (8) includes the controls in Table A6, with the exception of the variables listed in footnote 44 with non-trivial missingness. Column (9) excludes municipality and year fixed effects. Column (10) includes state-year fixed effects. Column (9) includes only election where the PAN, PRD, or PRI are an incumbent. Column (11) includes party-specific incumbent trends. Column (12) includes municipality-specific trends. Column (13) includes only elections that take place on or before the 15th of the month. Column (14) include only incumbent containing the PAN, PRD, or PRI. Column (15) includes only observations where the incumbent is not a coalition. Column (16) uses a placebo homicide shock defined six months before the election. All specifications are estimated using OLS, and all observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

To address this, column (4) includes state-year fixed effects to fully control for election-specific state shocks. Column (5) and (6) further include incumbent-specific (for the three largest parties) and municipality-specific linear trends to ensure that the results are respectively not driven by differential trends across party-incumbent performance and municipality. The point estimate in panel A for municipality-specific trends ceases to be statistically significant, but actually increases in magnitude. Finally, I conduct a placebo test where a homicide shock is defined six months earlier according to equation (3). The results in column (7) show that such pre-campaign shocks, which could be indicative of pre-trends, do not affect incumbent electoral performance.

Second, the results are not driven by municipalities experiencing particular homicide levels or types of homicide. By including fixed effects for the total number of homicides over the four-month window (in 10-homicide bins), column (8) provides further evidence that the results are not driven by differing homicide levels.[45] Moreover, column (9) excludes municipalities experiencing more than 25 homicides per month to show that the results do not simply reflect a few large municipalities with many homicides. Although homicide shocks are uncorrelated with indicators of DTO presence, to further address the potential concern that the results are driven by strategic political violence in areas with high drug-related criminal activity, I also exclude the elections most vulnerable to strategic political violence by DTOs: column (10) excludes municipalities that average more than one drug-related murder a month over any pre-election year between 2006 and 2011 (when such data were collected), while column (11) excludes nine states with high DTO-related drug crime.[46] In both cases, the estimates do not substantially change.

Third, the results do not reflect particular definitions of homicide shocks or electoral outcomes. First, columns (12)-(14) show that homicide shocks defined by one-, three-, or four-month windows also reduce the incumbent's vote share by around 2.5 percentage points and the incumbent's probability of winning by 5-11 percentage points. The smaller one-month window estimates could reflect the smaller sample or weaker signals imparted by such shocks. Column (15) shows that the results are robust to removing elections that occurred in the second half of the month, which are more challenging to classify with respect to homicides occurring before the election. Column (16) restricts attention to PAN, PRD, and PRI incumbents, demonstrating that the results are not driven by the few mayors from smaller parties. Similarly, column (17) shows that the results are robust to restricting attention to single-party incumbents.

Together, these results robustly show that an inopportune temporary increase in violence can have substantial electoral implications for an incumbent when it coincides with the pre-election

[45] I obtain similar results using smaller bin sizes, including size 1, at the cost of absorbing large municipalities with such fixed effects.

[46] Baja California, Chihuahua, Durango, Guerrero, Michoacán, Nuevo León, Sinaloa, Sonora, and Tamaulipas.

period when voters are more likely to acquire political information.

5.4 Negligible effects of longer-run homicide rates

The political information cycles argument implies that voters are particularly likely to respond to homicides that occur just before elections. Longer-run homicide metrics may not affect responsibility attribution if enough voters are insufficiently politically-engaged at other times in the political cycle, even if voters (at least intend to) punish longer-term performance (see Healy and Lenz 2014; Healy and Malhotra 2013).

To examine whether homicide shocks before elections indeed induce greater incumbent sanctioning than long-run homicide rates, I use a difference-in-differences design to estimate the effects of homicides over the prior year and prior (three-year) electoral cycle on the incumbent party's electoral prospects. Specifically, I compare changes in support for incumbent parties in municipalities that experienced large increases in their longer-run homicide rate between elections to changes in support for incumbent parties in municipalities that did not using the following specification:

$$Y_{pmt} = \beta Average\ monthly\ homicide\ rate_{mt} + \eta_m + \mu_t + \varepsilon_{pmt}. \quad (4)$$

As a robustness check, I also include municipality-specific time trends to adjust for differential trends in incumbent support.

The results in Table 3 indicate that long-run homicide trends have had limited impact on electoral performance. The point estimates in columns (1), (2), (5), and (6) of panel A show that homicides over the year before the election are not significantly correlated with either the incumbent's probability of being re-elected or their vote share, regardless of the inclusion of municipality-specific time trends. In fact, the estimates in columns (5) and (6) suggest that 5 more homicides a month over the year before an election—almost half a standard deviation increase—only translates into a 0.25 percentage point decline in the municipal incumbent party's vote share. This effect is around ten times smaller than the five-homicide average differential between shocked and non-shocked municipalities. Columns (3), (4), (7), and (8) include a quadratic term, but also find little suggestion that any effect is non-linear. Panel B similarly shows that voters do not punish poor performance over the full electoral cycle. This difference mirrors the findings of Healy and Lenz (2014), who suggest that U.S. voters wish to punish performance over the full electoral cycle but rely on more recent information in its absence. The evidence thus suggests that longer-run homicide trends play a substantially smaller role than short-term homicide shocks in informing electoral accountability.

Table 3: The effect of long-run homicide rates on municipal incumbent electoral outcomes

Panel A: 12 months before election	Incumbent party win				Change in incumbent party vote share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average monthly homicide rate	0.0009	-0.0031	-0.0009	-0.0007	-0.0004	-0.0006	-0.0014	-0.0023
(12 months before election)	(0.0020)	(0.0028)	(0.0080)	(0.0114)	(0.0003)	(0.0006)	(0.0012)	(0.0024)
Average monthly homicide rate			0.0000	-0.0000			0.0000	0.0000
(12 months before election) squared			(0.0000)	(0.0000)			(0.0000)	(0.0000)
Observations	3,494	3,494	3,494	3,494	174,622	174,622	174,622	174,622
Outcome mean	0.47	0.47	0.47	0.47	-0.05	-0.05	-0.05	-0.05
Homicide rate mean	8.13	8.13	8.13	8.13	8.12	8.12	8.12	8.12
Homicide rate standard deviation	21.92	21.92	21.92	21.92	22.01	22.01	22.01	22.01
Municipality-specific time trends		✓		✓		✓		✓
Panel B: Since last election	Incumbent party win				Change in incumbent party vote share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average monthly homicide rate	0.0027	-0.0029	0.0039	0.0072	-0.0002	0.0000	-0.0004	0.0001
(3 years before election)	(0.0024)	(0.0058)	(0.0109)	(0.0215)	(0.0003)	(0.0008)	(0.0014)	(0.0033)
Average monthly homicide rate			-0.0000	-0.0001			0.0000	-0.0000
(3 years before election) squared			(0.0001)	(0.0001)			(0.0000)	(0.0000)
Observations	3,314	3,314	3,314	3,314	174,148	174,148	174,148	174,148
Outcome mean	0.47	0.47	0.47	0.47	-0.05	-0.05	-0.05	-0.05
Homicide rate mean	6.97	6.97	6.97	6.97	6.63	6.63	6.63	6.63
Homicide rate standard deviation	15.07	15.07	15.07	15.07	15.09	15.09	15.09	15.09
Municipality-specific time trends		✓		✓		✓		✓

Notes: All specifications are estimated using OLS, and include municipality and year fixed effects. All observations are weighted by the number of registered voters for the relevant electoral unit. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

5.5 Voters distinguish responsibility across levels of government

Although mayors play an important role in local public security, state and federal governments are also important players. The punishment of municipal mayors could then reflect broader punishment of the party controlling higher office. Conversely, if voters believe that mayors are primarily responsible for local crime (or that their actions are only weakly correlated with co-partisans at higher levels), they may update less about national parties following local homicide shocks. To disentangle these chains of accountability and assignment of responsibility, and thus assess hypothesis H4, I first examine how the effects of homicide shocks vary with the existence of a municipal police force, before comparing the electoral performance of parties at the municipal, state and federal levels using data from simultaneous municipal, state and federal elections, and then finally examining survey-level voter confidence in higher levels of government.

If voters believe that the mayor is responsible for local crime rates because they control the local police, homicide shocks should only be punished in municipalities with their own police force. In contrast with the preceding analysis which excluded municipalities without their own police force, I now include such municipalities (including delegations in the Federal District) to test whether possessing a police force influences electoral sanctioning. Panel A of Table 4 show that, consistent with voters recognizing that elevated homicides rates may be beyond the control of municipal mayors lacking local police forces, homicide shocks only significantly affect the electoral prospects of incumbent parties commanding a local police force. Columns (2) and (4) address the concern that the effect attributed to police forces reflects the lack of sanctioning in smaller and less developed municipalities by controlling for the interaction of a homicide shock with previous incumbent party vote share and the average monthly homicide rate over the last year. At least when they consume politically-relevant news, these results echo previous research suggesting that voters in developing contexts differentiate incumbent performance from external forces (Harding and Stasavage 2014; Kronick 2014).[47] Combined with the evidence above that voters learn from homicide shocks, this finding further suggests that voters are not indiscriminately punishing events like American football losses (Healy, Malhotra and Mo 2010).

Nevertheless, if the parties of municipal mayors, state deputies, state governors, or the president are correlated, the substantial electoral penalties found above could reflect punishment of other political actors. To test for such spillovers down layers of government, columns (1)-(3) of panel B in Table 4 respectively examine the effect of a homicide shock on the municipal vote share of the party of the state deputy, state governor, and the presidency. The results, however, suggest that if anything the governor and president's parties increase their vote share, and thus provide

[47] Table A14 provides mixed evidence that voters account for homicide shocks affecting neighboring municipalities.

Table 4: Effects of homicide shocks across levels of government with differing responsibilities

Panel A: Presence of municipal police force	Incumbent party win		Change in incumbent party vote share	
	(1)	(2)	(3)	(4)
Homicide shock	-0.122**	-0.095**	-0.023***	-0.024***
	(0.047)	(0.047)	(0.008)	(0.007)
No municipal police force	0.002	0.023	0.006	-0.006
	(0.126)	(0.125)	(0.016)	(0.014)
Homicide shock	0.236	0.234	0.066***	0.051**
× No municipal police force	(0.158)	(0.147)	(0.023)	(0.022)
Observations	3,825	3,788	200,976	200,976
Outcome range	{0,1}	{0,1}	[-0.96,0.89]	[-0.96,0.89]
Outcome mean	0.48	0.48	-0.04	-0.04
Homicide shock mean	0.41	0.41	0.42	0.42
No municipal police force mean	0.09	0.09	0.13	0.13
Test: Homicide shock + Homicide shock × No municipal police force = 0 (p value)	0.43	0.31	0.04	0.19
Interactive controls		✓		✓

Panel B: Sanctioning across levels of government	Change in municipal vote share of... ...state district party	...state governor's party	...president's party	Change in state vote share	Change in federal vote share incumbent	Change in state vote municipal incumbent	Change in federal vote municipal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Homicide shock	0.001	0.027*	0.032*	-0.005	-0.005	0.008	-0.017
	(0.009)	(0.014)	(0.018)	(0.009)	(0.015)	(0.016)	(0.017)
Observations	149,807	142,167	174,622	141,112	54,852	133,647	64,912
Outcome mean	0.00	0.03	0.04	-0.04	-0.05	0.45	0.39
Homicide shock mean	0.42	0.42	0.42	0.41	0.41	0.42	0.43

Notes: Specifications in panel A and B include municipal and year fixed effects, weight by the number of registered voters in the electoral precinct, and are estimated using OLS; the interactive controls are lagged incumbent party vote share and average monthly homicides over the last year (both standardized). Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

no support for punishment at higher levels of government spilling over to municipal elections.[48] Nevertheless, could still reserve punishment for state and federal politicians. However, columns (4) and (5) similarly indicate that a homicide shock reduces neither the vote share of state nor federal deputies in concurrent state and federal elections.

Although I find no evidence that state and federal incumbent parties are held responsible for homicide shocks by voters, punishment of the local party could instead spill up to the national level. However, when municipal and higher-level elections are held simultaneously, columns (6) and (7) of panel B show that the state and federal deputy vote shares of the municipal incumbent's

[48]Consistent with Table A15 in the Appendix, the positive effect at the federal level reflects increased support for Calderón's PAN government.

party are not affected by a homicide shock. Table A15 also shows that voters do not differentially punish different parties for homicide shocks.

6 The moderating effect of local media

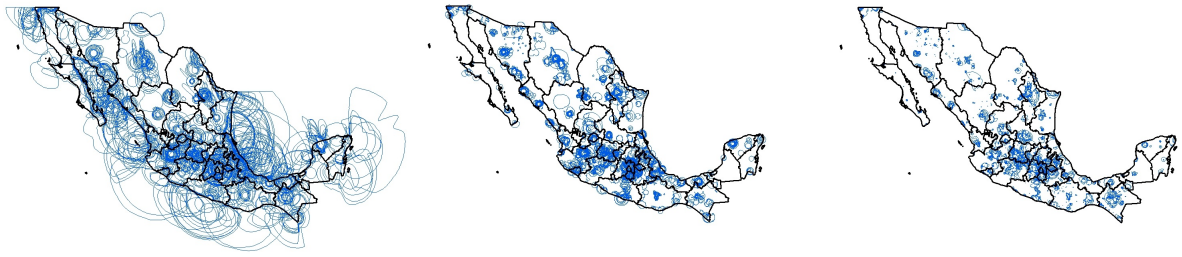
Bringing together the individual- and aggregate-level findings, I argue that the electoral sanctioning of local violence before elections depends upon access to a media environment covering local news. To demonstrate that the sanctioning of homicide shocks reflects such media coverage, I exploit precinct-level variation in media signal coverage to identify the effect of an additional local radio or television station when a pre-election homicide shock occurs.

6.1 Data

As part of Mexico's major media reform in 2007 (see Larreguy, Marshall and Snyder 2016; Serra 2012), the IFE required that all radio and television stations in the country submit detailed coverage and technological data. This included the location and power of their antennae and a map detailing their commercial quality coverage—the level of coverage that media stations may base advertising sales on and which the IFE deemed relevant for minimizing cross-state advertising spillovers. The signal inside the commercial quality coverage area is very strong, and should cover virtually all households, while signal quality declines quickly as the distance from the commercial quality coverage boundary increases.[49]

Figure 4 maps the commercial quality coverage of each type of media station. While FM radio, and especially television, stations have limited and primarily urban coverage, AM radio stations can travel considerable distances—particularly when aided by stretches of sea water with high electrical ground conductivity. Virtually all of the population is covered by at least one media station, but the number of outlets—both emitting from within a municipality and without—providing commercial quality signals to any given electoral precinct varies considerably. Furthermore, the extent to which a given electoral precinct is covered by a signal varies substantially: in some cases, a signal only covers a tiny fraction of a precinct, while in others the entire precinct is covered. I follow Larreguy, Marshall and Snyder (2017) by defining a precinct as covered by a given media

[49]The IFE defines the boundary of the coverage area using a 60 dBμ threshold for signal strength. Accordingly, to the U.S.-based National Communications and Information Administration, this "60 dBμ level is recognized as the area in which a reliable signal can be received using an ordinary radio receiver and antenna." Outside this area, a high-performance antenna is typically required to avoid receiving a weak signal. This is the threshold commonly used to determine a radio station's audience and sell advertising space commercially in the United States.



(a) AM radio stations (b) FM radio stations (c) Television stations

Figure 4: Media station commercial quality signal coverage areas

station only if at least 20% of voters fall within the commercial coverage boundary.[50]

6.2 Identification strategy

To identify the electoral effects of media stations covering local homicides, I leverage geographic variation in coverage (see also Larreguy, Marshall and Snyder 2017).[51] I compare *neighboring* electoral precincts within the same municipality that differ in the total number of *local* AM, FM and television stations—defined as media stations whose antennae are located within the electoral precinct's municipality—by which they are covered.[52] Since signal quality declines, but without disappearing entirely, across the commercial quality boundary, this strategy identifies the "intent to treat" effect of an increase in the probability of exposure to an additional local media station.[53] Figure 5 illustrates the identification strategy graphically, using the example of electoral precincts 1571 and 1583 in the municipality of Villa de Tututepec de Melchor Ocampo in Oaxaca. The identifying assumption is that these neighboring precincts differ only because precinct 1583 receives a commercial quality signal from a local television station that does not cover precinct 1571.

Operationally, for each electoral precinct in the country I identify the set of neighboring precincts from within the same municipality that differ in the number of local media stations that they are covered by. Each such grouping n is defined by a "treated" precinct and the set of neighboring

[50]The INEGI provides detailed Census population counts in 2010, for both rural localities and urban blocks. This data is used to identify the proportion of voters affected covered by a commercial quality signal.

[51]Ansolabehere, Snowberg and Snyder (2006), Enikolopov, Petrova and Zhuravskaya (2011), and Snyder and Strömberg (2010) use similar designs.

[52]Although data does not exist to adjust for news consumption "non-compliance," any effect would be larger among compliers that only receive news because they were exposed to an additional commercial quality local signal.

[53]Exposure to commercial quality coverage does not constitute a geographic regression discontinuity design because the treatment is not binary (some neighbors differ by more one media station) and where neighbors differ by more than one media station it is not clear how to coherently define the running variable.

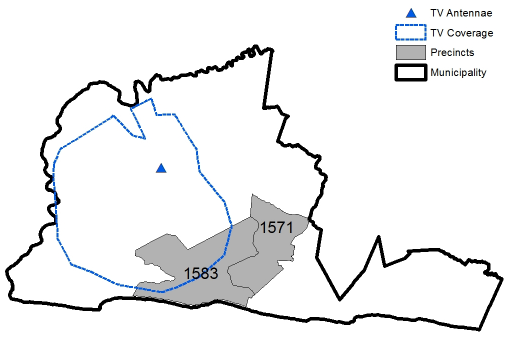


Figure 5: Identification strategy example

Notes: Both precincts are located in the municipality of Villa de Tututepec de Melchor Ocampo in the state of Oaxaca. Precinct 1583 is covered by the local television emitting from within the municipality, but precinct 1571 is not.

precincts receiving different local media exposure. Focusing on municipalities included in the homicide shock sample produced 2,689 neighboring groups, containing an average of 2.3 comparison units per election. The average precinct is covered by 6.3, 8.9, and 3.2 local AM, FM, and television stations respectively, while the total number of local media stations covering a precinct ranges from 0 to 40.[54]

There are good reasons to believe that, among neighboring electoral precincts, local media coverage at the commercial quality boundary is effectively random. First, neighboring precincts often differ in coverage because of physical characteristics such as water, geographic contours and large physical objects that aid or impede ground conductivity (in the case of AM radio) and "line of sight" radio waves (in the case of FM radio and television) between an antenna and precincts at the coverage boundary. Second, given that media stations lack the technology to differentiate media markets at fine-grained levels,[55] and voters that specifically locate according to the availability of local media are unlikely to choose to live close to a coverage boundary (preferring a location guaranteeing coverage), it is unlikely that coverage reflects strategic sorting by either media stations or voters. Finally, I restrict attention to neighboring precincts with an area of less than 2km^2. This removes larger precincts where media outlets could more plausibly target their signals, and prevents comparisons between urban and suburban, or suburban and rural, precincts that may differ in their

[54]Given that data from the Secretariat of Communications and Transportation show that the number of media stations has not changed since 2003, I continue to pool the years 1999 to 2013.

[55]The IFE technical data show that the power of signal transmitters are fairly discrete. The power output in watts for AM, FM and television stations is almost exclusively round thousands that are divisible by five.

electoral behavior. The final sample is shown in Figure A1c.

Combining within-neighbor variation in local media coverage with homicide shocks just before an election,[56] I estimate the following specification to identify the interaction between homicide shocks and local media coverage:

$$\begin{aligned} Y_{pmnt} &= \beta_1 Homicide\ shock_{mt} + \beta_2 Local\ media_{pt} + \\ &\quad \beta_3 \Big(Homicide\ shock_{mt} \times Local\ media_{pt} \Big) + \xi_n + \mu_t + \varepsilon_{pmnt}, \end{aligned} \tag{5}$$

where ξ_n is a fixed effect for each set of neighboring precincts that ensures I only exploit within-neighbor variation in local media.[57] To weight each neighboring group equally, precincts are weighted by electorate size divided by the number of control units per neighbor set in order.

To assess the plausibility of exogenous differences in local media coverage between neighboring precincts, I use equation (5) to examine balance across a wide range of demographic, socioeconomic, homicide, and political municipal and precinct characteristics. Table A10 shows that only nine of these 109 variables are significantly correlated with the number of local media stations at the 10% level. In particular, there are no significant differences in key indicators of rural-urban geography (such as precinct area, electorate size, or distance to the municipality head), the number of non-local media stations, homicide indicators, or previous electoral outcomes. The few significant differences are small in magnitude.

6.3 Local media increase punishment of homicide shocks

The estimates in Table 5 show that access to local media stations plays a critical role in supporting the electoral sanctioning of homicide shocks. The key finding in column (1) shows that homicide shocks are only punished when an electoral precinct is covered by sufficient local media stations. In the relatively rare case of precincts with fewer than eight local media stations (19% of the sample), the marginal effect plot in Figure 6 indicates that there is no significant effect of a homicide shock. Implying that local media are necessary for Mexican voters to punish the incumbent party for pre-election violence, the effect of a homicide shock in an electoral precinct with no local media stations is close to zero. However, for more than eight local media stations, the significant interaction coefficient kicks in and homicide shocks substantially reduce the vote share of the municipal incumbent party. Like Ferraz and Finan (2008) and Larreguy, Marshall and Snyder (2017), the significant positive coefficient on the lower-order local media term suggests that voters may re-

[56] Homicide shocks remain well-balanced across pre-treatment variables in this subsample.

[57] Since I use within-municipality neighbors, neighbor fixed effects incorporate municipality fixed effects.

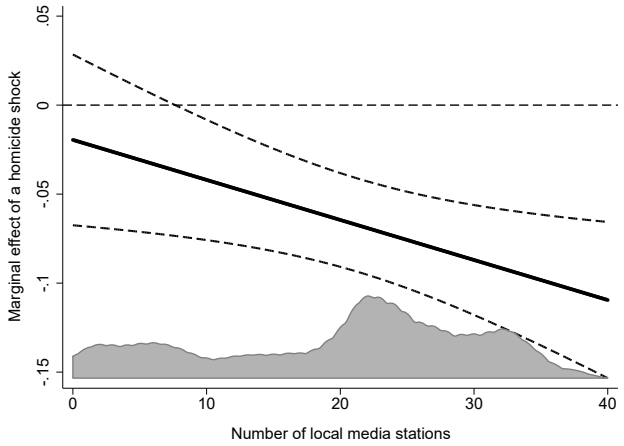


Figure 6: Electoral effect of a homicide shock, conditional on the number of local media stations (95% confidence interval)

Notes: Calculated using the estimates from column (1) of Table 5. The gray density plot above the x axis plots the distribution of the local media variable in the sample.

ward incumbents that do not experience a homicide shock before the election. In contrast, column (2) shows no significant relationship with homicides over the last year.

Columns (3)-(5) examine which types of local media contribute most to the electoral sanctioning and rewarding of incumbent parties. Since radio and television coverage is correlated, to identify the contribution of each—albeit in different samples—I exploit within-neighbor differences in the number of local AM, FM, and television stations separately. Given that television is by far the primary media source for Mexicans, it is not surprising to find that local television stations produce the largest effects. As column (5) demonstrates, each additional local television station reduces the vote share of an incumbent facing a homicide shock by 1.8 percentage points. The smaller effects of FM and especially AM radio are relatively similar in magnitude to the average effect reported in column (1), but are not statistically significant in these subsamples.

To assess the importance of local media stations, as opposed to media stations emitting from other municipalities, I use the same strategy to identify the effects of non-local media. Consistent with the findings of Larreguy, Marshall and Snyder (2017), column (6) shows that non-local media stations with weaker incentives to report homicides outside their own municipality do not affect how voters sanction their incumbents. This reiterates the role of receiving politically-relevant news in electoral sanctioning.

Table 5: Electoral effects of homicide rates, conditional on the presence of local and non-local media

	Change in incumbent party vote share					
	Media measure:					
	Local media (1)	Local media (2)	Local AM media (3)	Local FM media (4)	Local television (5)	Non-local media (6)
Homicide shock	-0.0195		-0.0045	-0.0620*	-0.0239	-0.0050
	(0.0245)		(0.0326)	(0.0331)	(0.0205)	(0.0182)
Media measure	0.0017*	-0.0001	0.0025	0.0009	0.0055	0.0000
	(0.0010)	(0.0003)	(0.0035)	(0.0018)	(0.0041)	(0.0006)
Homicide shock	-0.0022**		-0.0023	-0.0012	-0.0175***	-0.0006
× Media measure	(0.0010)		(0.0042)	(0.0026)	(0.0058)	(0.0004)
Average monthly homicide rate		-0.0639***				
(12 months before election)		(0.0165)				
Average monthly homicide rate		0.0004				
× Media measure		(0.0008)				
Observations	30,099	30,099	1,385	16,345	13,422	95,160
Outcome range	[-0.63,0.50]	[-0.63,0.50]	[-0.50,0.26]	[-0.63,0.41]	[-0.62,0.47]	[-0.75,0.73]
Outcome mean	-0.04	-0.04	-0.02	-0.03	-0.04	-0.05
Homicide shock mean	0.40		0.62	0.33	0.44	0.46
Average monthly homicide rate mean		10.39				
Media measure mean	18.40	18.40	3.40	10.81	2.92	35.20
Media measure std. dev.	10.80	10.80	4.20	5.18	1.64	23.21

Notes: All specifications include neighbor group and year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters in the electoral precinct divided by the number of comparison units within each neighbor group. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

7 Mechanisms

The preceding analysis provides clear evidence that voters consume most political information just before elections, and are highly responsive to homicides that occur at this time, but only when covered by local media stations likely to report such news. To understand the theoretical underpinnings of these findings, I now explore the mechanisms driving such political information cycles. In particular, I provide a variety of evidence suggesting that voters are updating their beliefs in a somewhat sophisticated manner, before showing that political information cycles are more likely driven by changes in voter demand for news than changes in media coverage around elections.

7.1 Voters behave as Bayesian with weak prior beliefs

To examine the extent to which the voter behavior underpinning political information cycles, I examine whether belief updating is consistent with voting behavior and how beliefs and behavior vary with the position and precision of priors beliefs.

7.1.1 Voters update their beliefs from homicide shocks

Given the salience of crime in Mexico, greater attention to news in the run-up to an election is likely to increase exposure to violent crime in the local media. However, exposure to information about homicides should only affect voting behavior to the extent that voters update their posterior beliefs about the salience of public security and/or the performance of their municipal institutions in addressing this key valence issue. To test whether these prerequisites for holding municipal incumbents to account hold, this subsection examines whether the content of the news during local election campaigns affects voter beliefs by extending equation (1) to allow the effect of upcoming elections to vary with municipal homicide experiences:

$$Y_{imt} = \beta_1 Upcoming\ local\ election_{mt} + \beta_2 X_{mt} + \beta_3 \Big(Upcoming\ local\ election_{mt} \times X_{mt} \Big) + \mu_t + \varepsilon_{imt}, \quad (6)$$

where X_{mt} is either a pre-survey homicide shock (defined analogously to pre-election homicide shocks),[58] or the monthly homicide rate over the one or three years prior to the survey.[59] Given

[58] Since the day of the survey varies, but homicide data is monthly, a homicide in the month of the survey could occur after the survey was carried out. I thus use an indicator for a homicide occurring in either the month of the survey or the prior month.

[59] Table A2 indicates that neither measure is significantly correlated with upcoming local elections.

that the former measure is more likely to be reported in the media, the political information cycles argument implies that only recent homicide shocks are likely to influence voter beliefs. The combination of plausibly exogenous variation in upcoming local elections and homicide shocks suggests a causal interpretation of the interaction effect β_3.[60] The longer-term measures are less likely to be covered directly in the news, although well-informed voters may have already internalized such information.

I use the ENCUP survey to measure the salience of violence and beliefs about the competence of the incumbent party.[61] To evaluate whether homicides around elections translate into concern about public security, I define an indicator for respondents citing crime and insecurity, drug trafficking, violence, or vandalism as the most important problem for their community to solve.[62] I also create an indicator of low confidence in the municipal mayor, defined by respondents ranking their confidence at 5 or below on a scale from 0 to 10 (for 2012) or expressing no, or almost no, confidence in the given institution (in 2001).[63]

The results in panel A of Table 6 demonstrate that homicides just before an election substantially increase fears regarding public security. The insignificant effect of the lower-order local election term, combined with the large positive interactions in columns (1) and (2), show that upcoming local elections only increase concerns about security among voters in municipalities experiencing a recent homicide shock.[64] Relative to the sample mean, a homicide shock increases security concerns by almost 50% during an election campaign.

The insignificant lower-order homicide shock term indicates that voters do not update about public security from short-run homicide spikes during periods of lower news consumption. This suggests that it is not the case that voters forget about homicides that happened earlier in the electoral cycle (Zaller 1992). This finding also suggests that voters are not initially frustrated but subsequently won back by incumbent policies designed to appeal to disgruntled voters (Brollo 2009). Rather, the lack of immediate response indicates that voters do not internalize homicide shocks outside of election campaigns.

It remains possible that voters that primarily consume political news before elections are ac-

[60] Reinforcing the identification strategy detailed above, Tables A3 and A4 show that short-run homicide shocks, and their interaction with upcoming local elections, are not systematically correlated with individual-level and municipal covariates.

[61] The Federal District, where policing is not administered at the delegation-level, is excluded.

[62] Along with all other political concerns, I include as zeroes respondents that do not cite a problem.

[63] The cutoff on the 0-10 scale was chosen to broadly match the distribution of responses in 2001. However, the results do not depend upon the choice of cutoff. Unfortunately, vote intentions were not elicited in any survey wave.

[64] The positive interactions are robust to simultaneously controlling for the interaction of an upcoming election with pre-treatment municipal characteristics. The positive effect of local elections in columns (3) and (4) reflect the fact that the positive interactions in columns (1) and (2) are being averaged across in these specifications.

Table 6: Heterogeneous effects of upcoming local elections on concern for public security and institutional confidence, by short-run and long-run municipal homicide measures

	Homicide measure: Homicide shock		Homicide per month last year		Homicides per month last 3 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Public insecurity the major problem in the community						
Upcoming local election	0.024	-0.033	0.034**	0.004	0.034**	0.004
	(0.021)	(0.023)	(0.016)	(0.017)	(0.016)	(0.016)
Homicide measure	-0.011	-0.005	0.009***	0.008***	0.009***	0.009***
	(0.012)	(0.011)	(0.002)	(0.002)	(0.002)	(0.001)
Upcoming local election	0.092***	0.085***	0.002	0.001	0.002	0.001
× Homicide measure	(0.028)	(0.026)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	9,764	9,764	12,541	12,541	12,541	12,541
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.13	0.13	0.11	0.11	0.11	0.11
Upcoming local election mean	0.22	0.22	0.20	0.20	0.20	0.20
Homicide measure mean	0.46	0.46	2.99	2.99	3.06	3.06
Survey year without data	2012	2012	2012	2012	2012	2012
Panel B: Outcome: Low confidence in the municipal mayor						
Upcoming local election	-0.019	0.175	0.000	0.091	0.001	0.093
	(0.042)	(0.110)	(0.036)	(0.056)	(0.038)	(0.058)
Homicide measure	-0.048*	-0.019	0.001	0.001*	0.001***	0.001**
	(0.026)	(0.021)	(0.001)	(0.001)	(0.000)	(0.000)
Upcoming local election	0.114**	0.069	-0.001	0.010	-0.002	0.012
× Homicide shock	(0.048)	(0.146)	(0.009)	(0.012)	(0.015)	(0.021)
Observations	5,925	5,925	7,236	7,236	7,236	7,236
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.34	0.34	0.34	0.34	0.34	0.34
Upcoming local election mean	0.02	0.02	0.03	0.03	0.03	0.03
Homicide measure mean	0.42	0.42	6.33	6.33	6.98	6.98
Survey years without data	2003, 2005	2003, 2005	2003, 2005	2003, 2005	2003, 2005	2003, 2005
State fixed effects		✓		✓		✓

Notes: All specifications include survey year fixed effects, and are estimated using OLS. The number of observations in columns (1) and (2) is lower because voters in municipalities failing to experience any homicide over the two months either side of the survey were dropped. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 7: Heterogeneous effects of upcoming local elections on institutional confidence at higher levels of government, by short-run homicide shocks

	Low confidence in the President		Low confidence in state Governor	
	(1)	(2)	(3)	(4)
Upcoming local election	0.064	0.067**	-0.061	0.052
	(0.039)	(0.033)	(0.040)	(0.090)
Homicide shock	-0.011	-0.007	-0.051**	-0.025
	(0.014)	(0.012)	(0.026)	(0.020)
Upcoming local election	0.046	0.015	-0.005	-0.085
× Homicide shock	(0.044)	(0.041)	(0.062)	(0.158)
Observations	12,352	12,352	5,912	5,912
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.26	0.26	0.32	0.32
Upcoming local election mean	0.17	0.17	0.02	0.02
Homicide shock mean	0.44	0.44	0.42	0.42
Survey years without data			2003, 2005	2003, 2005
State fixed effects		✓		✓

Notes: All specifications include survey year fixed effects, and are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

tually responding to longer-run homicide rates. However, the results in columns (3) and (4) interacting upcoming local elections with longer-term homicide measures reject this possibility. The lower-order terms unsurprisingly show that public security is a greater concern in more violent municipalities. However, the interaction of upcoming elections with the average number of homicides over the last year, or last 3 years, does not significantly affect the salience of public security for voters.

Turning to beliefs about institutional competence, panel B shows that homicides coinciding with upcoming local elections also cause voters to reduce their confidence in the municipal incumbent. Columns (1) and (2) demonstrate that local elections only increase the likelihood that a respondent expresses low confidence in the incumbent mayor when a homicide shock occurs before a local election. The 11 percentage point reduction in mayoral confidence implied by column (1) represents a 34% drop relative to the sample mean. The coefficients for local elections in columns (1) and (2) suggest a somewhat asymmetric relationship where voters do not significantly update about their incumbents when a non-positive homicide shock occurs. Again, long-run homicide trends do not differentially affect voters around elections.

Finally, reinforcing the precinct-level voting behavior in Table 4, Table 7 shows that homicide shocks around elections do not cause voters to lose confidence in higher levels of government. Columns (1)-(4) show no significant change in confidence in either the President or state Governor. In conjunction with the findings for municipal incumbent parties, these tests indicate that voters indeed assign responsibility for local homicides to municipal incumbent parties perceived to be responsible for local crime.

The preceding evidence suggests that homicide both increase the saliency of insecurity (H2a) and induce voters to update about their municipal incumbent (H2b). However, this analysis cannot discern which effect actually induces voters to vote against the incumbent party. I next show that electoral sanctioning at least partially reflects voters updating relative to their prior beliefs.

7.1.2 The position of prior beliefs conditions voter responses to homicide shocks

Voter responses to pre-election homicide shocks may also depend upon the position of their prior beliefs. Where elevated homicide levels are expected, a homicide shock may not cause voters to substantially update their beliefs and behavior. To test this feature of voter learning (H7), I explore whether previous homicide shocks moderate precinct-level electoral sanctioning. Figure 7 shows how the effect of a homicide shock on the change in the incumbent party's vote share varies with the occurrence of a homicide shock before the previous election and whether the current incumbent party differs from the previous incumbent party.[65]

The results indicate that the sanctioning of homicide reflects updating vis-'á-vis prior experiences. The first bar shows that sanctioning is greatest among voters in municipalities experiencing a homicide shock before the current election and which did not experience such a shock before the previous election under a different incumbent party. This effect is larger in magnitude than for the second bar where the current incumbent was in office at the previous election but did not experience a shock. Similarly, the negative effect of a homicide shock is also large if the same incumbent party was in office for consecutive pre-election homicide shocks. Conversely, voters do not punish current incumbents if a previous incumbent from a different party also presided over a homicide shock. In addition to the likely priming effects of crime in the news, these results exhibit voter learning across time from different local violence reports.

7.1.3 Voters with imprecise prior beliefs respond most to homicide shocks

Another prediction of Bayesian belief updating is that voters with relatively imprecise prior beliefs will update most from incumbent party performance signals (H6). Education is widely believed

[65]The regression estimates are provided in Table A13.

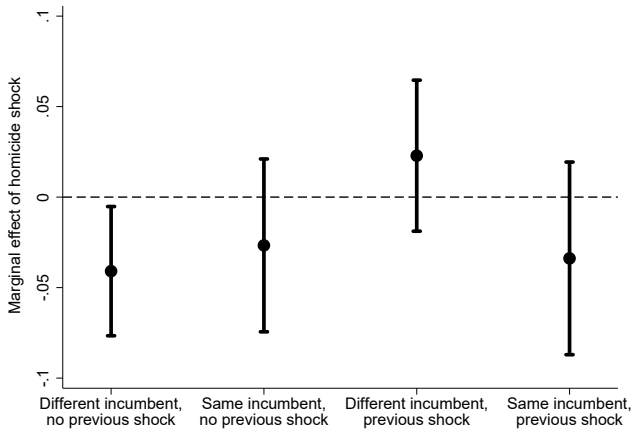


Figure 7: The effect of a homicide shock on the change in incumbent party vote share, by previous homicide shocks and incumbent identity (95% confidence intervals)

Notes: Same incumbent is an indicator for the current incumbent party winning the previous election. Previous shock is an indicator for a homicide shock occurring before the previous election.

to increase political knowledge by providing the ability, opportunity and motivation to acquire information (Barabas et al. 2014; Delli Carpini and Keeter 1996).[66] Using higher education as a proxy for the precision of prior beliefs, I next show—at both the individual and aggregate levels—that the least educated voters are most likely to respond to homicide shocks.

To test whether voters with weaker priors respond more to performance indicators in the news around elections, I first use equation (6) to examine how the effects of short-run homicide shocks on voter beliefs vary by education. Using the ENCUP survey data, I define an education indicator for the 18% of respondents that have completed a university degree ("higher") education. This is positively correlated with news consumption and political knowledge. The marginal effects of an upcoming local election reported in Table 8 suggest that increased concern about public insecurity and reduced confidence in municipal incumbents are most pronounced among less educated voters.[67] Although the triple interaction is not quite statistically significant in the case of public security concerns, the interaction between upcoming local elections and homicide shocks indicate that—netting across coefficients—a pre-election homicide shock only significantly increases the concerns of voters that did not complete upper secondary schooling.

I link these individual-level findings to precinct-level voting outcomes by examining whether the effect of local media covering a homicide shock also varies with the education. To complement

[66] Larreguy, Marshall and Snyder (2016) show that education is positively correlated with political knowledge and holding opinions on different political parties in Mexico.

[67] The homicide within the last month measure produced similar but less stark results.

Table 8: Effect of upcoming local elections on concern about public insecurity and low confidence in incumbent mayors, by homicide shock and completion of higher education

	Public insecurity the major problem in the community		Low confidence in the municipal mayor		Change in incumbent party vote share	
	(1)	(2)	(3)	(4)	(5)	(6)
Upcoming local election	-0.021	-0.048*	-0.036	0.163		
	(0.022)	(0.025)	(0.049)	(0.118)		
Homicide shock	-0.006	-0.005	-0.043*	-0.013	0.0094	0.0111
	(0.015)	(0.013)	(0.026)	(0.022)	(0.0289)	(0.0313)
Higher education	0.082***	0.064***	-0.001	0.008	0.0367**	0.0198
	(0.021)	(0.021)	(0.029)	(0.024)	(0.0151)	(0.0137)
Upcoming local election	0.107***	0.096***	0.143**	0.095		
× Homicide shock	(0.037)	(0.034)	(0.063)	(0.149)		
Upcoming local election	0.037	0.045	0.196***	0.182***		
× Higher education	(0.033)	(0.034)	(0.051)	(0.058)		
Homicide shock × Higher education	0.000	0.010	-0.035	-0.037	-0.0591**	-0.0494*
	(0.026)	(0.024)	(0.040)	(0.035)	(0.0283)	(0.0257)
Upcoming local election	-0.057	-0.073	-0.494***	-0.487***		
× Homicide shock × Higher education	(0.054)	(0.053)	(0.070)	(0.075)		
Local media					0.0027**	0.0031***
					(0.0011)	(0.0012)
Homicide shock × Local media					-0.0036***	-0.0035**
					(0.0013)	(0.0015)
Local media × Higher education					-0.0019**	-0.0013**
					(0.0008)	(0.0006)
Homicide shock × Local media					0.0028*	0.0030**
× Higher education					(0.0016)	(0.0013)
Observations	6,564	6,564	5,916	5,916	30,099	30,099
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	[-0.63,0.50]	[-0.63,0.50]
Outcome mean	0.13	0.13	0.34	0.34	-0.03	-0.03
Homicide measure mean	0.47	0.47	0.42	0.42	0.40	0.40
Higher education measure mean	0.20	0.20	0.16	0.16	0.53	0.53
Upcoming local election mean	0.25	0.25	0.02	0.02		
Local media mean					18.40	18.40
Survey years without data	2003, 2005	2003, 2005	2003, 2005	2003, 2005		
State fixed effects		✓		✓		
Interactive control for development						✓

Notes: The specifications in columns (1)-(4) are estimated using the ENCUP data and include survey year fixed effects, and are estimated using OLS; the number of observations differ between columns due to non-responses. The specifications in columns (5) and (6) are estimated using precinct-level electoral returns and includes neighbor group and year fixed effects, and are estimated using OLS; all observations are weighted by the number of registered voters in the electoral precinct divided by the number of comparison units within each neighbor group. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

the individual-level analysis, I define an indicator for precincts in the top quartile of the higher education distribution (39.5% or more of voters with higher education in the relatively urban neighbors sample). Consistent with the survey results in columns (1)-(4), the triple interaction in column (5) of Table 8 shows that electoral sanctioning induced by local media is almost entirely driven by

precincts lacking highly educated voters. Contrary to the potential confound that the least educated respond most to homicide shocks in the news because local violence afflicts such voters most heavily (Díaz-Cayeros et al. 2011), the higher education triple interaction becomes stronger in column (6) once a triple interaction with an index of precinct-level poverty is simultaneously controlled for.[68]

7.2 Do political information cycles reflect changes in demand for or supply of politically-relevant news?

This article has emphasized political information cycles as an equilibrium outcome. However, from both a theoretical and policy perspective, it is useful to understand whether these cycles reflect changes in voter demand for politically-relevant news or changes in the media's supply of such news around elections. The increased political news consumption at the extensive margin documented in Table 1 suggests that tuning in for the news is a conscious choice, given that radio and television schedules do not change around elections. However, for the many voters increasing consumption along the intensive margin, it is hard to separate increased demand from news providers broadcasting more politically-relevant news.

At least for news relating to local homicides, a first step in separating these effects is to examine changes in coverage around elections. In the absence of comprehensive broadcast media transcripts, I instead utilize Osorio's (2015) dataset using machine-learning techniques to collate reports relating to drug violence from 105 government agencies and national and local newspapers between 2000 and 2010. Broadcast media coverage is likely to be similar given that these sources serve as an important local information source for broadcast media outlets. To examine the extent to which reporting of violent events increases before elections, I test whether the association between the occurrence and reporting of violent events increases within the five months before local elections using the following difference-in-differences specification:

$$\begin{aligned} Reports_{mt} &= \beta_1 Homicides_{mt} + \beta_2 Upcoming\ local\ election_{mt} + \\ &\quad \beta_3 \Big(Upcoming\ local\ election_{mt} \times Homicides_{mt} \Big) + \eta_m + \mu_t + \varepsilon_{imt}, \end{aligned} \tag{7}$$

where $Reports_{mt}$ is a monthly municipal-level count of the number of violent events (including homicides as well as shootings, kidnappings, torture etc.) between DTOs reported for that month,[69]

[68] This measure is a standardized scale composed of 2010 Census indicators of socioeconomic development.

[69] The outcome was aggregated up from Osorio's (2015) weekly count. Similar results obtain when using reports of violent enforcement, arrests, drug seizures, asset seizures, and gun seizures.

and μ_m and μ_t represent municipality and month-year fixed effects.

Table 9: Association between monthly municipal homicide counts and government agency and newspaper reports on violence between gangs, by upcoming local elections

	Reports of inter-DTO violence				
	(1)	(2)	(3)	(4)	(5)
Homicides	0.234***	0.224***	0.231***	0.234***	0.224***
	(0.025)	(0.023)	(0.025)	(0.025)	(0.023)
Upcoming local election			-0.015*	-0.036	-0.201***
			(0.009)	(0.049)	(0.072)
Homicides			0.011	0.011	0.014
× Upcoming local election			(0.015)	(0.015)	(0.012)
Observations	308,360	308,360	308,360	308,360	308,360
Outcome range	[0,102]	[0,102]	[0,102]	[0,102]	[0,102]
Outcome mean	0.15	0.15	0.15	0.15	0.15
Outcome std. dev.	1.43	1.43	1.43	1.43	1.43
Homicides mean	0.44	0.44	0.44	0.44	0.44
Homicides mean	3.33	3.33	3.33	3.33	3.33
Upcoming local election mean	0.15	0.15	0.15	0.15	0.15
Additional fixed effects		State-month-year		Election-month-year	Election-state-month-year

Notes: All specifications include municipality and year-month fixed effects, and are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

The results in Table 9 confirms that media coverage of homicides is high, but is not significantly greater before elections. Column (1) first shows that for each homicide that occurs in a municipality, the number of violent events reported increases by 0.23. Since not all newspapers are studied and reports only pertain to drug-related violence, this estimate almost certainly substantially understates the true association. Nevertheless, the large positive effect clearly suggests a strong association. Turning to differential coverage around elections, column (3) shows that reporting is not significantly greater in the five months preceding local elections.[70] Both sets of results are precisely estimated and robust to including state-month-year fixed effects (and their interaction with upcoming local elections). These findings indicate that media coverage of homicides is not sensitive to the electoral cycle, and thus suggest that the sanctioning of pre-election homicides is more likely driven by increased voter demand for politically-relevant news before elections than changes in the supply of such news. Nevertheless, to the extent that the tone of the news may

[70]Unreported estimates show similar effects when focusing on the months immediately before the election.

change around elections, this tentative conclusion should be treated with caution.

7.3 Interpreting voter behavior

The preceding analysis suggests that voters generally respond in homicide shock in a Bayesian manner, based on weak priors. In contrast, the evidence is inconsistent with recency bias driving the results: rather than discard or discount information that they consumed in advance of the election, voters never update from homicide shocks outside of electoral campaigns, even in their immediate aftermath. If anything, political information cycles may instead explain apparent voter myopia.

Given that voters are updating their beliefs following a pre-election homicide shock, what exactly are they updating about? One possibility is that voters update their beliefs from observing the incumbent party's response, rewarding those revealed as good types and sanctioning those revealed to be bad types (Ashworth and Bueno de Mesquita 2014*b*; Cole, Healy and Werker 2012). However, given that sanctioning is driven by the least educated voters, who are least likely to be able to extract such a sophisticated signal (e.g. Alt, Lassen and Marshall 2016), this seems relatively unlikely. Moreover, contrary to an explanation where the incumbent or challenger parties used their radio and television ads to exploit the issue, unreported results show that electoral sanctioning of homicide shocks does not vary with the proportion of incumbent ads that a precinct receives.

Rather, the most compelling interpretation is that poorly-informed voters regard a homicide shock reported in the news as a signal of incumbent party incompetence. This is consistent with the evidence of voter learning among the least informed and differentiating across levels of government and municipalities with and without their own police force. More sophisticated voters neglect such signals. However, although voters behave as Bayesians with weak prior beliefs along multiple dimensions, the strength of their response appears inconsistent with its weakness as an indicator of longer-run performance. This suggests that the least informed voters may not be fully Bayesian in that they exhibit some degree of overconfidence, such that they systematically overestimate the precision of the signal (e.g. Ortoleva and Snowberg 2015). Although this is a common phenomenon in behavioral studies, further work is required to precisely disentangle the cognitive source of voter updating.

8 Conclusion

This study demonstrates the importance of the timing of voter news consumption for understanding how voters hold governments to account. Since many voters follow relatively sharp political

information cycles, whereby most politically-relevant information is consumed prior to elections, voting behavior can be highly responsive to salient performance indicators, like local homicides, in the news before elections. Leveraging a wealth of fine-grained individual- and precinct-level data, and identification strategies isolating three complementary components of the theory, I show that voters indeed consume more news before elections, and that pre-election homicide shocks substantially reduce support for the municipal incumbent party among poorly informed voters exposed to local media. Conversely, there is little evidence that long-run performance homicide indicators affect electoral outcomes.

While documenting responses to pre-election information, extant studies do not link the effects to the timing of voter news consumption. Nevertheless, they suggest that political information cycles may play a central role in explaining why voters often hold governments accountable for salient short-term indicators of performance, other than violence, in a wide variety of contexts. News revealed about incumbent performance before elections on other valence issues may similarly impact voting behavior. Ferraz and Finan (2008) and Larreguy, Marshall and Snyder (2017) show respectively that Brazilian and Mexican municipal incumbent parties are punished electorally if audits reports reveal malfeasant behavior in office in the run-up to elections, but only in the presence of local media stations. Brollo (2009) finds that such sanctioning in Brazil decreases with months until the election, although she attributes this to greater scope for incumbents to respond to such reports. Furthermore, Italian deputies were only punished for alleged criminal wrongdoing following the "Clean Hands" investigation, which was widely reported in the news before the 1992 and 1994 elections (Chang, Golden and Hill 2010). Economic outcomes paint a similar picture in developed and developing contexts, where voters respond to pre-election economic indicators (e.g. Achen and Bartels 2004*b*; Healy and Lenz 2014; Roberts and Wibbels 1999).

Consequently, understanding the origins of political information cycles demands further work probing the forces underlying equilibrium information consumption. In particular, voter demand for information remains poorly understood, especially in developing contexts where the marginal effects of information on electoral behavior are often substantial. Similarly, little is known empirically about when and how media stations report different types of news (although see Gentzkow and Shapiro 2010; Lawson 2002), especially with respect to the content and tone of media coverage. This paper focused primarily on identifying equilibrium cycles and estimating their electoral implications, but these fundamental questions require attention.

Voting on the basis of the news before elections may be normatively problematic for democracy. In contrast with models emphasizing the value of information for improving democratic representation (e.g. Besley and Burgess 2002; Besley and Prat 2006) or suggesting that the aggre-

gation of individual information can yield efficient outcomes (Feddersen and Pesendorfer 1997), the case of punishing exogenous homicide shocks prior to elections—which are uncorrelated with longer-run homicide trends—suggests that voters could be committing attribution errors by removing competent incumbents on the basis of noisy short-term performance indicators (see also Achen and Bartels 2004*b*). Future research should illuminate whether voters indeed elect worse incumbents on the basis of such information, which is itself likely to depend upon how voters view incumbents on average (Ashworth and Bueno de Mesquita 2014*b*), and whether sanctioning varies with incumbent traits that were previously unobserved by voters. Furthermore, my findings also provide clear incentives for politicians to release bad news when voter engagement is low (e.g. Durante and Zhuravskaya forthcoming; Eisensee and Strömberg 2007). Nevertheless, the political information cycles documented here thus add to an increasing body of work challenging the orthodoxy that an informed electorate necessarily enhances electoral accountability (see Ashworth and Bueno de Mesquita 2014*a*).

Regardless of the welfare implications, it is clear that political parties, NGOs, and media outlets should strategically target incumbent performance information toward the least informed voters around elections—when voters consume political news. If voters substitute short-term performance indicators for longer-term measures that they intend to evaluate (Healy and Lenz 2014), it is essential that the news relate to horizons beyond proximate security or economic shocks. Although recent interventions have attempted to achieve this, often by delivering leaflets or newspapers (e.g. Arias et al. 2017; Banerjee et al. 2011), broadcast media appears to be the most effective tool for dissemination (Larreguy, Marshall and Snyder 2017). Consequently, a key challenge is encouraging political actors and media stations to overcome sensationalist and political biases in favor of longer-term performance indicators.

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A Appendix

Contents

A.1 Formal model illustrating the voter updating process

This section develops a simple decision-theoretic selection model (alla Fearon 1999) to clarify formally the theoretical implications discussed in the main paper. In particular, the model incorporates salience and learning considerations to examine how information about incumbent performance affects the voting behavior of forward-looking voters seeking to choose the best candidate.

By consuming information, forward-looking voters learn about the contemporary political world. Specifically, let a voter receive n signals about incumbent performance in office, and thus the suitability of the incumbent party for continuing in office (or "competence").[71] For simplicity, each signal s of incumbent performance is an independent draw from a Normal distribution $N(\mu, \sigma^2)$ with known variance $\sigma^2 > 0$. The mean of the distribution μ represents the unknown true level of incumbent competence, while a larger variance reflects the possibility that different media sources distort or under-report certain types of news (e.g. Besley and Prat 2006; Gentzkow and Shapiro 2006; Mullainathan and Shleifer 2005), fail to serve all segments of the market (Prat and Strömberg 2005), or are dwarfed by news that does not pertain to the voter's incumbent party (Larreguy, Marshall and Snyder 2017). The voter also possesses a prior belief $N(\delta, \tau^2)$ over incumbent competence. For simplicity the challenger's level of competence is normalized to 0, and I assume that information about the incumbent does not cause voters to also update about the challenger.[72] The voter updates their belief about the incumbent's competence downwards (upwards) when $\delta > (<)\bar{s}$, where $\bar{s} = \sum_{t=1}^{n} s_t / n$ is the average signal received by a voter.

The voter also receives a shock $b \in \mathbb{R}$ toward the incumbent. This shock b may be positive or negative, and could derive from partisanship, clientelistic ties, or other valence factors that helped the incumbent party win the last election. Ultimately, individuals vote according to both their bias and expectations of politician competence in office. To capture issue salience, the voter weights the importance of competence by the weighting function $w(n) > 0$, which is increasing in n.[73] For simplicity, I assume that the voter is risk-averse and simply maximizes the sum of (expected) competence and bias.[74]

[71] Politicians are assumed to perform according to their underlying competence level μ or 0 when in office.

[72] The results could be easily extended to allow for voters to have a distribution of beliefs over the challenger. If I allowed beliefs about the incumbent and challenger to be correlated, then the results would be unaffected for a sufficiently small correlation.

[73] Similar results would hold if the weighting also depended on the content of the news, i.e. $w(n, \bar{s})$, where most plausibly $w_{\bar{s}} < 0$ and $w_{n\bar{s}} < 0$. Changes in the weight attached to beliefs about incumbent competence could also reflect a voter's certainty about the information; in such a setting, more signals would then cause voters to increasingly voter according with the signal because it relatively increases the precision of their belief about the incumbent's competence (relative to other factors affecting vote choice).

[74] Abstracting from risk-aversion presents the results particularly clear. If the voter were risk-averse, information would help the incumbent by reducing uncertainty about the utility voters would expect to receive from electing

Upon receiving n signals, a voter learns from the news about the competence of the incumbent party. Updating their prior belief distribution, the voter's posterior belief distribution is given by the following standard Normal learning result:

$$N\left(\frac{\frac{\delta}{\tau^2}+\frac{n\bar{s}}{\sigma^2}}{\frac{1}{\tau^2}+\frac{n}{\sigma^2}}, \left(\frac{1}{\tau^2}+\frac{n}{\sigma^2}\right)^{-1}\right) \tag{A1}$$

where $\bar{s}$ is the mean signal received by the voter. The extent to which signals about incumbent performance, particularly when noisy or relatively uninformative (i.e. high σ^2), affect voter posterior beliefs depends upon the strength of their prior beliefs. A voter with weak priors (i.e. large τ^2) are mostly likely to update their beliefs. While a noisy signal should have limited effect on well-informed voters who recognize that a short-term shock is not a good reflection of incumbent competence, a voter with a weak prior does not know whether a short-term shock reflects long-term performance. To the extent that uninformed voters believe that such information may be informative about incumbent competence, their posterior beliefs—both the mean and variance—about incumbent competence change.

The expected utility associated with the incumbent politician is then given by integrating over this posterior distribution. Combining this with the bias b toward the incumbent, a voter with bias b votes for the incumbent when:

$$B \equiv b + w(n)\frac{\delta\sigma^2 + n\bar{s}\tau^2}{\sigma^2 + n\tau^2} \geq 0 \tag{A2}$$

Intuitively, a voter is more likely to re-elect the incumbent when the bias b toward the incumbent is high and the expected competence of the incumbent is high. The relative importance attached to these factors is reflected by $w(n)$. The individual-level implications could be aggregated into party-level vote shares by integrating over the distribution of voter priors and biases.

To understand how information impacts the voters propensity of voting for the incumbent, I differentiate B with respect to the number of signals, n:

$$\frac{\partial B}{\partial n} = \underbrace{w(n)\frac{\sigma^2\tau^2(\bar{s}-\delta)}{(\sigma^2+n\tau^2)^2}}_{Learning\ effect} + \underbrace{w'(n)\frac{\delta\sigma^2+n\bar{s}\tau^2}{\sigma^2+n\tau^2}}_{Salience\ effect}. \tag{A3}$$

The first term represents the learning effect: how the impact of new information, by changing

them. This could easily be achieved by adopting a risk-averse utility function (for tractability a constant absolute risk-aversion functional form could easily map Normal posterior beliefs into a utility function that separates the utility associated with the mean and variance).

the voter's posterior belief, affects their likelihood of supporting the incumbent. The following proposition analyzes the comparative static implications of the learning effect:

Proposition 1. *If $\bar{s} < \delta$, such that information does not help the incumbent, then the learning effect impacts voting behavior as follows:*

1. *The likelihood that the voter votes for the incumbent decreases with the number of signals received (n).*

2. *When $\frac{1}{\tau^2} < \frac{n}{\sigma^2}$, the learning increases in the precision of the signal ($\frac{1}{\sigma^2}$), decreases in the precision of the prior ($\frac{1}{\tau^2}$), and (if $\sigma^2 - n\tau^2 < \sqrt{2n\tau^2\sigma^2}$) increases in their interaction.*

The reverse results hold when $\bar{s} > \delta$.

Proof: Focusing on the learning effect sets the salience effect to zero. For part 1, given $w(n) > 0$ and all variances are positive, it is easy to see that the learning effect, $L \equiv w(n)\frac{\sigma^2\tau^2(\bar{s}-\delta)}{(\sigma^2+n\tau^2)^2}$, is positive when $\bar{s} < \delta$. Part 2 is established by taking subsequent derivatives. The cross-partial (i.e. the partial of the learning effect) with respect to σ^2 is given by:

$$\frac{\partial L}{\partial \sigma^2} = \frac{w(n)(\bar{s}-\delta)\tau^2[n\tau^2-\sigma^2]}{(\sigma^2+n\tau^2)^3} > 0, \tag{A4}$$

which, given $\bar{s} < \delta$, is negative when $n\tau^2 < \sigma^2$. Similarly,

$$\frac{\partial L}{\partial \tau^2} = -\frac{w(n)(\bar{s}-\delta)\sigma^2[n\tau^2-\sigma^2]}{(\sigma^2+n\tau^2)^3} < 0, \tag{A5}$$

by the same conditions. Finally,

$$\frac{\partial^2 L}{\partial \tau^2 \partial \sigma^2} = -\frac{w(n)(\bar{s}-\delta)\sigma^2[2n\tau^2\sigma^2-(\sigma^2-(n\tau^2)^2)]}{(\sigma^2+n\tau^2)^3} > 0, \tag{A6}$$

provided that $\sigma^2 - n\tau^2 < \sqrt{2n\tau^2\sigma^2}$. The multiplicative nature of the comparative statics ensures that the reverse results hold when $\bar{s} > \delta$. ■

When $\bar{s} < (>)\delta$, the voter updates negatively (positively) about the competence of their incumbent. Consider the case where $\bar{s} < \delta$. The average signal of incumbent performance that they receive from the news suggests that the incumbent's true level of competence is below their prior δ. Consequently, part 1 unsurprisingly states that when the news suggests that incumbent performance is low, a voter is less likely to re-elect the incumbent. This reflects the decrease in the

posterior belief over the competence of the incumbent, while the posterior belief about the challenger is unaffected. Part 2 further shows that the punishment of the incumbent is increasing in the precision of the signals and decreasing in the precision of the prior—provided that the prior is sufficiently weak ($\frac{1}{\tau^2} < \frac{n}{\sigma^2}$). Furthermore, under an additional condition, the impact of a precise signal is strongest when voter priors are weakest. Intuitively, when news coverage is particularly credible or informative relative to their prior beliefs, voters are less likely to believe that the incumbent has sufficient competence to merit re-election.

The second term in equation (A3) represents the salience effect: the impact of information increasing the relative importance of incumbent performance to the voter. This increases support for the incumbent if the posterior belief is positive (and thus exceeds the normalized belief about the challenger's competence). This opens the possibility that even if voters receive a negative signal, such that $\bar{s} < \delta$, the benefits of priming an issue on which the incumbent initially scored well could outweigh the negative learning effect. The following result clarifies this insight, and describes several key comparative static predictions in terms of vote choice:

Proposition 2. *If $\bar{s} < \delta$, such that information does not help the incumbent, then the salience effect impacts voting behavior as follows:*

1. *The likelihood that the voter votes for the incumbent increases (decreases) with the number of signals received (n) when the posterior belief about incumbent competence, $\frac{\delta\sigma^2+n\bar{s}\tau^2}{\sigma^2+n\tau^2}$, is positive (negative).*

2. *A negative salience effect increases in the precision of the signal ($\frac{1}{\sigma^2}$), decreases in the precision of the prior ($\frac{1}{\tau^2}$), and (if $\frac{1}{\tau^2} < \frac{n}{\sigma^2}$) decreases in their interaction.*

Proof: Focusing on the salience effect sets the learning effect to zero. For part 1, given $w'(n) > 0$ and all variances are positive, it is easy to see that the salience effect, $S \equiv w'(n)\frac{\delta\sigma^2+n\bar{s}\tau^2}{\sigma^2+n\tau^2}$, is positive when the posterior belief $\frac{\delta\sigma^2+n\bar{s}\tau^2}{\sigma^2+n\tau^2} > 0$ (or when $\delta\sigma^2 + n\bar{s}\tau^2 > 0$, given the denominator is positive). Part 2 is again established by taking subsequent derivatives. The cross-partial (i.e. the partial of the salience effect) with respect to σ^2 is given by:

$$\frac{\partial S}{\partial \sigma^2} = \frac{w'(n)n\tau^2[\delta - \bar{s}]}{(\sigma^2 + n\tau^2)^2} > 0. \tag{A7}$$

Similarly,

$$\frac{\partial S}{\partial \tau^2} = -\frac{w'(n)n\sigma^2[\delta - \bar{s}]}{(\sigma^2 + n\tau^2)^2} < 0. \tag{A8}$$

Finally,

$$\frac{\partial^2 S}{\partial \tau^2 \partial \sigma^2} = \frac{w'(n)n[\delta - \bar{s}](\sigma^2 - n\tau^2)}{(\sigma^2 + n\tau^2)^3} < 0, \tag{A9}$$

provided that $\sigma^2 - n\tau^2 < 0$. The multiplicative nature of the comparative statics again ensures that the reverse results hold when $\bar{s} > \delta$. ■

The first part of the proposition reiterates that the direction of the salience effect depends upon the voter's posterior belief—namely if it is above that of the challenger, i.e. $\frac{\delta\sigma^2 + n\bar{s}\tau^2}{\sigma^2 + n\tau^2} > 0$. The magnitude of the negative impact of the salience effect is increasing in the precision of the signal $(1/\sigma^2)$ and increasing in the weakness of the voter's prior $(1/\tau^2)$. The interaction shows that such results trade-off.

Finally, I consider when the salience effect overpowers the learning effect. In particular,

Proposition 3. *If $\bar{s} < \delta$, the salience effect compounds the negative learning effect when the posterior belief about incumbent competence, $\frac{\delta\sigma^2 + n\bar{s}\tau^2}{\sigma^2 + n\tau^2}$, is positive (negative). The learning effect dominates a confounding salience effect when $\frac{w'(n)}{w(n)} < \frac{\sigma^2\tau^2(\delta - \bar{s})}{(\delta\sigma^2 + n\bar{s}\tau^2)(\sigma^2 + n\tau^2)}$*

Proof: The results follow simply from part 1 of Proposition 2, and setting $L < S$. ■

This result unsurprisingly shows that when the salience effect is negative, it compounds the negative learning effect—and that this again requires that the posterior belief about the incumbent is below that of the challenger. Even if this does not hold, the second element of the proposition notes new information may still have a negative effect if the learning effect dominates the salience effect, which requires that the relative increase in salience is sufficiently small or the signal significantly departs from the prior.

A.2 Months and years of municipal elections

Table A1 reports the municipal elections potentially covered by the survey and aggregate elections in the main analysis.

A.3 Data description

A.3.1 ENCUP survey data

Upcoming local election. Indicator coded 1 for respondents living in a state/municipality with an upcoming local election occurring within the year of the survey. States/municipalities where an election has already occurred within the year of the survey are coded 0.

Table A1: Municipal elections, 1999-2013, by state

State	Election dates
Aguascalientes	*August 2001*, *August 2004*, August 2007, July 2010, July 2013.
Baja California	*June 2001*, *August 2004*, *August 2007*, July 2010, July 2013.
Baja California Sur	*February 1999*, **February 2002**, February 2005, February 2008, February 2011.
Campeche	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Chiapas	October 2001, October 2004, October 2007, July 2010, July 2012.
Chihuahua	July 2001, July 2004, July 2007, July 2010, July 2013.
Coahuila	September 1999, September 2002, September 2005, October 2008, July 2010, July 2013.
Colima	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Distrito Federal	*July 2000, July 2003, July 2006, July 2009, July 2012*.
Durango	July 2001, July 2004, July 2007, July 2010, July 2013.
Guanajuato	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Guerrero	October 1999, October 2002, October 2005, October 2008, July 2012.
Hidalgo	November 1999, November 2002, November 2005, November 2008, July 2011.
Jalisco	November 2000, **July 2003**, July 2006, July 2009, July 2012.
Estado de México	July 2000, **March 2003**, **March 2006**, July 2009, July 2012.
Michoacán	November 2001, November 2004, October 2007, October 2011.
Morelos	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Nayarit	July 1999, July 2002, July 2005, July 2008, July 2011.
Nuevo León	*July 2000*, **July 2003**, July 2006, July 2009, July 2012.
Oaxaca	August 2001, August 2004, August 2007, July 2010, *July 2013*.
Puebla	November 2001, November 2004, November 2007, July 2010, July 2013.
Querétaro	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Quintana Roo	*February 1999*, **February 2002**, February 2005, February 2008, July 2010, July 2013.
San Luis Potosí	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Sinaloa	*November 2001*, November 2004, October 2007, July 2010, July 2013.
Sonora	July 2000, **July 2003**, July 2006, July 2009, July 2012.
Tabasco	October 2000, October 2003, October 2006, October 2009, July 2012.
Tamaulipas	October 2001, November 2004, November 2007, July 2010, July 2013.
Tlaxcala	*November 2001*, November 2004, October 2007, July 2010, July 2013.
Veracruz	*September 2000*, September 2004, September 2007, July 2010, July 2013.
Yucatán	May 2001, May 2004, May 2007, May 2010, July 2012.
Zacatecas	July 2001, July 2004, July 2007, July 2010, July 2013.

Notes: Emboldened election are counted as upcoming local elections in the survey analysis. State-level elections were held in Hidalgo in February 2002 without concurrent municipal elections, and are counted as upcoming local elections. Italicized elections are not included in the sample for the homicide shocks analysis due to data unavailability (or exclusion in the case of the Federal District). Except in the cases of Baja California 2001 and 2004 and Oaxaca 2013, missingness reflects the fact that data from the preceding election required to define the change in vote share was not available. For Baja California 2001 and 2004, the precinct numbering changed across elections and thus cannot be matched. For Oaxaca 2013, precinct level data was unavailable.

Watch and listen to news and political programs ever/monthly/weekly/daily. Indicator coded 1 for a respondent that answers that they watch political programs or listen to news at least ever/once a month/at least once a week/daily. ("¿Qué tan seguido escucha noticias o ve programas sobre política?")

Watch and listen to news and political programs scale. 5-point scale from 0 to 4, with values corresponding to levels of watching and listening to new and political programs (in ascending order).

Topical political knowledge. First factor from a factor analysis containing the following questions: What is the name of the youth movement that recently started in Mexico? (2012) Where was the plan to build an airport that was subsequently abandoned due to local pressure? (2003, 2005) Which political party intends to charge VAT on medicines, food, and tuition? (2001) Which party holds your state governorship? (2001, 2003, 2005, 2012) What is the name of your state governor? (2001)

Education. 5-point variable, where 0 is less than completed primary education, 1 is a maximum education level of completing high school, 2 is a maximum level of completing lower secondary education, 3 is a maximum level of complete secondary education (*preparatoria*), and 4 is at least a university degree.

Homicide shock. This indicator is coded 1 if either the number of homicides in the two months prior to the month of the survey (including the survey month) or the two months prior to the survey month exceed those in the two months immediately after the month of the survey, based on the INEGI monthly homicide statistics for the occurrence of homicides among a municipality's residents. In 2005, the indicator is coded using the current month if the day of the month is greater than the 16th. (Note that homicides figures are subject to reclassification across time.)

Homicides last year. Average number of residents suffering a homicide per month within the municipality in the preceding 12 months (excluding the current month). (Note that homicides figures are subject to reclassification across time.)

Homicides last 3 years. Average number of residents suffering a homicide per month within the municipality in the preceding 36 months (excluding the current month). (Note that homicides figures are subject to reclassification across time.)

Public insecurity the major problem in the community. Indicator coded 1 if, in an open response, a respondent lists violence, crime or public security as the main problem facing their community (including as 0s respondents that listed no problem).

Low confidence in mayor. Indicator coded 1 for respondents answering that their confidence in the municipal mayor is 5 or below on a scale from 0 to 10 (for the 2003, 2005, and 2012 surveys) or

expressing no or almost no confidence in the municipal mayor (in 2001), in response to a question asking about the level of confidence that respondents have in the listed institutions.

Number of organizations. The number of organizations that a respondent reports being a member of, or previously being a member of. The number of possible organizations slightly varies across survey.

Organizations talk about politics. Indicator coded 1 for respondents that answer that politics is discussed at the organizations they are a member of.

Number of group meetings. The number of political organizations at which an individual has attended a meeting during the last year.

Discuss community problems. A scale measuring the regularity with which respondents discuss problems in the community with friends and neighbors, ranging through never (coded 0), occasionally (coded 1) and frequently (coded 2).

Incumbent win margin (lag). The difference in vote share between the incumbent and second-placed finisher at the previous municipal mayoral election (or an election held later in the year of the survey). In *Usos y Custombres* in Oaxaca, the incumbent win margin is set to the maximum of 1.

ENPV (lag). The effective number of political parties (by vote share) at the previous municipal mayoral election (or an election held later in the year of the survey). In *Usos y Custombres* in Oaxaca, ENPV is set to the maximum of 1.

Incumbent won (lag). Indicator coded 1 for municipalities where the incumbent party was re-elected at the most recent election (or an election held later in the year of the survey).

Incumbent vote share (lag). The municipal vote share of the incumbent party at the most recent election (or an election held later in the year of the survey).

Police per voter (lag). Total number of municipal security employees in the previous year (in thousands), divided by the total number of registered voters.

A.3.2 Precinct and municipal homicide and electoral data

Change in incumbent party vote share. Change in the precinct-level share of all votes cast for the incumbent between the current municipal election and the prior municipal election (3 or 4 years earlier). When multiple parties form an incumbent coalition, the incumbent vote share is determined by the vote share of the largest party/coalition containing an incumbent party at the next election in terms of vote share.

Incumbent party win. Indicator coded 1 if the incumbent party wins the municipal election. In the case of coalitions, is defined similarly to the above.

Change in turnout. Change in the precinct-level turnout rate between the current municipal election and the prior municipal election (3 or 4 years earlier).

Change in incumbent vote share (registered). Change in the precinct-level share of votes, as a share of all registered voters, cast for the incumbent between the current municipal election and the prior municipal election (3 or 4 years earlier).

Homicide shock. Defined in equation (2) of the main paper, using INEGI homicide statistics for intentional homicides that occurred in each month to residents of a given municipality. One- and three-month versions are similarly defined. (Note that homicides figures are subject to reclassification across time.)

Placebo homicide shock (6 months earlier). Defined as in equation (2) of the main paper, with the exception that all months are shifted 6 months forward in time. (Note that homicides figures are subject to reclassification across time.)

Average monthly homicide rate (12 months/3 years before election). Average number of residents suffering a homicide per month within the municipality in the 12 months/3 years prior to the municipal election, again based on INEGI homicide data. (Note that homicides figures are subject to reclassification across time.)

Post-2006. Indicator coded 1 for elections held since December 2006.

No municipal police force. Indicator coded 1 for municipalities without a municipal police force under its direct control. This category includes municipalities that work solely with state police or federal police, work with the community, run security using a private or other service, or have no service at all. Municipalities that share police forces or use civil associations were excluded because channels of accountability are hard to discern. These categorizations were homogenized across the 2000, 2002, 2004, 2011 and 2013 ENGM surveys. Missing years were imputed according to the following rules: I first used the last available data, and if no previous coding was available took the nearest year in the future.

Calderón Presidency. Indicator coded one for elections in the years 2007-2012.

PAN/PRD. Indicator coded 1 for PAN/PRD municipal incumbents.

A.3.3 Precinct local media coverage data

Local media. Number of AM radio, FM radio or television stations, with an emitter based in the precinct's municipality, covering at least 20% of the precinct population (as defined by detailed population data—block-level population in urban areas, and rural locality locations).

Local AM radio/FM radio/television. Number of AM radio/FM radio/television stations, with an emitter based in the precinct's municipal, covering at least 20% of the precinct population

(as defined by detailed population data—block-level population in urban areas, and rural locality locations).

Non-local media. Number of AM radio, FM radio or television stations, with an emitter based outside the precinct's municipality, covering at least 20% of the precinct population (as defined by detailed population data—block-level population in urban areas, and rural locality locations).

High higher education. Indicator coded 1 for electoral precincts where more than 40% of residents had experienced higher education in 2010, according to the 2010 Census.

A.4 Map of municipalities included in different samples

In separate figures, Figure A1 shades in red the municipalities that appear at least once in each of the main empirical analyses—the survey sample, the homicide shock sample, and the local media sample.

A.5 Additional analyses

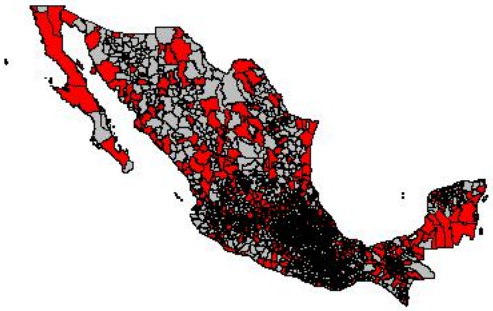
The following subsections present the results of additional analyses cited in the main paper.

A.5.1 Assessing the identification assumptions

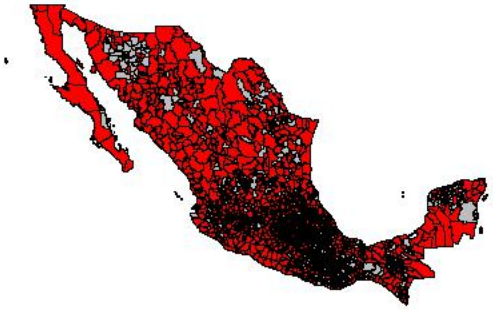
Tables A2-A10 show the results of balance tests for the three main sets of empirical findings in the paper: Table A2 shows that upcoming local elections are well balanced across individual and municipal characteristics in the ENCUP survey data; Tables A6 and A7 shows that homicide shocks are well balanced across a wide variety of covariates, where municipality fixed effects are excluded for time-invariant Census and geographic variables; and Table A10 shows that the number of local media stations is well-balanced across these same covariates.

In addition, Tables A3 and A4 show that homicides and their interaction with upcoming local elections are well-balanced in the ENCUP surveys. Moreover, Table A5 demonstrates that neither changes in upcoming local elections nor changes in measures of local violence predict whether a municipality is included in any given year. In each panel the outcome is an indicator for whether a municipality is included in that survey wave, conditional on a municipality appearing in at least one of the four ENCUP surveys.

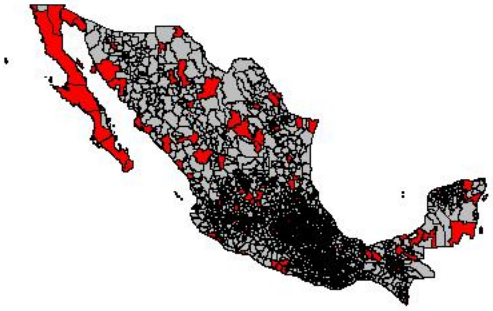
I also provide additional tests to support the exogeneity of homicide shocks. First, using a simple specification containing municipality and year fixed effects, Table A8 shows that the number of police per voter does not systematically vary across election and non-election years. Second, Table A9 shows that election outcomes are uncorrelated with homicide counts in the two months



(a) Municipalities in the ENCUP survey samples



(b) Municipalities in the homicide shock sample



(c) Municipalities in the local media neighbor sample

Figure A1: Municipalities included in each empirical analysis (shaded in red)

after elections. In particular, columns (3) and (4) find no correlation between the identity of the winning party and post-election homicides either throughout the sample or during the Calderón administration. This does not conflict with Dell (2015), who focuses on drug-related homicides over the *subsequent* mayor's term, or as many months as possible of that mayor's term.

A.5.2 Drug-related homicides

As noted in the main text, the Calderón government released monthly municipal data on the number of drug-related deaths between 2007 and 2011. However, there are data issues with such data. First, these numbers are contentious (see Heinle, Rodríguez Ferreira and Shirk 2014), and do not necessarily follow the homogeneous coroner's report criteria used by INEGI. Second, the limited availability of this data, combined with the definition of a homicide shock that requires at least one drug-related death over the four-month window around elections, substantially reduces the sample size by around 75%. More generally, it is not clear theoretically whether voters should respond more or less to drug-related homicides, as opposed to other homicides. In fact, voters might think that these are less relevant to them than more arbitrary murders which constitute around 50% of totals homicides even during the drug war (only a tiny fraction of such homicides as domestic). Nevertheless, I use the drug-related homicide data as a robustness check.

The results in Table A11 report the effect of drug-related homicide shocks just before an election, defined according to equation (2) but instead using drug-related homicides. Given that only five years of data exist, and thus many municipalities only appear once, I use state fixed effects instead of municipality fixed effects. Although the large drop in sample size unsurprisingly reduces precision substantially, the point estimates are relatively similar—negative, and large—to those reported in Table 2. These findings thus further reinforce the claim in the main text that voters respond similarly to different types of homicide.

A.5.3 Difference-in-differences approach to identifying the effects of pre-election homicides

The results in the main text focus on homicide shocks coded as a binary variable. A key advantage of this approach is that it is highly short-term, and thus accurately maps political information cycles. By exploiting idiosyncratic shifts in monthly homicide counts, I am able to generate plausibly exogenous variation. However, I also now consider an alternative approach to capturing the effects of short-run shocks around local elections.

In particular, I utilize a difference-in-differences design to identify how proportional changes in the number of homicides around elections affects vote choice. Since the homicide *level*, as opposed to the short-term *change* exploited in the main paper, just before an election is highly correlated

Table A2: Balance of upcoming local elections in the ENCUP surveys over 18 individual and municipal level variables

	Female	Catholic	Age	Education	Employed last year	Own econ. in month	Number of org.s	Org.s talk about politics	Number of group meetings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Upcoming local election	-0.007	-0.011	-0.100	0.202***	-0.015	-0.029	-0.066	-0.013	-0.084
	(0.011)	(0.020)	(0.394)	(0.075)	(0.016)	(0.021)	(0.077)	(0.021)	(0.103)
Observations	15,976	11,983	15,976	11,756	11,756	12,322	15,976	12,576	15,976
Outcome mean	0.55	0.81	40.76	1.70	0.47	1.57	1.06	0.26	1.52
Local election mean	0.16	0.20	0.16	0.18	0.18	0.20	0.16	0.20	0.16
	Discuss community problems	Homicide shock	Homicide last year	Homicide last 3 years	Incumbent win margin (lag)	ENPV (lag)	Incumbent won (lag)	Incumbent vote share (lag)	Police per voter (last year)
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Upcoming local election	0.005	-0.022	1.607	1.607	0.022	0.011	0.094	0.012	-0.063
	(0.029)	(0.081)	(1.080)	(1.090)	(0.016)	(0.066)	(0.064)	(0.011)	(0.178)
Observations	12,576	12,664	15,941	15,941	15,976	15,976	15,976	15,778	13,666
Outcome mean	0.70	0.44	4.50	4.82	0.15	2.57	0.61	0.48	2.30
Local election mean	0.20	0.17	0.16	0.16	0.16	0.16	0.16	0.17	0.16

Notes: All specifications include survey year fixed effects, and are estimated using OLS. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A3: Balance of homicide shock in the ENCUP surveys over 18 individual and municipal level variables

	Female	Catholic	Age	Education	Employed last year	Own econ. in month	Number of org.s	Org.s talk about politics	Number of group meetings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Homicide shock	-0.002	-0.008	-0.267	-0.010	-0.006	0.024	0.026	-0.010	0.029
	(0.007)	(0.012)	(0.273)	(0.053)	(0.012)	(0.016)	(0.064)	(0.014)	(0.062)
Observations	12,664	9,522	12,664	9,464	9,464	9,591	12,664	9,764	12,664
Outcome mean	0.55	0.81	40.58	1.79	0.47	1.57	1.05	0.26	1.45
Homicide shock mean	0.44	0.44	0.44	0.45	0.45	0.46	0.44	0.46	0.44
	Discuss community problems	Upcoming local election	Homicide last year	Homicide last 3 years	Incumbent win margin (lag)	ENPV (lag)	Incumbent won (lag)	Incumbent vote share (lag)	Police per voter (last year)
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Homicide shock	-0.015	-0.010	-2.357*	-3.143*	0.002	-0.032	-0.000	0.004	-0.139
	(0.020)	(0.036)	(1.267)	(1.860)	(0.010)	(0.042)	(0.041)	(0.006)	(0.106)
Observations	9,764	12,664	12,664	12,664	12,664	12,664	12,521	12,606	10,584
Outcome mean	0.69	0.17	5.62	6.03	0.15	2.56	0.54	0.48	2.17
Homicide shock mean	0.46	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.45

Notes: All specifications include survey year fixed effects, and are estimated using OLS. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A4: Interactive balance of upcoming local election and homicide shock in the ENCUP surveys over 17 individual and municipal level variables

	Female	Catholic	Age	Education	Employed last year	Own econ. in month	Number of org.s	Org.s talk about politics	Number of group meetings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Upcoming local election	-0.007	-0.028	-0.458	0.156	-0.007	-0.012	-0.076	-0.039	-0.092
	(0.018)	(0.031)	(0.595)	(0.116)	(0.025)	(0.034)	(0.108)	(0.028)	(0.123)
Homicide shock	-0.005	-0.017	-0.307	-0.015	-0.003	0.033*	0.060	-0.008	0.068
	(0.008)	(0.013)	(0.319)	(0.061)	(0.014)	(0.020)	(0.068)	(0.015)	(0.060)
Homicide shock	0.022	0.042	0.218	0.023	-0.014	-0.043	-0.215*	-0.014	-0.249
× Upcoming local election	(0.023)	(0.032)	(0.859)	(0.160)	(0.032)	(0.047)	(0.128)	(0.032)	(0.188)
Observations	12,664	9,522	12,664	9,464	9,464	9,591	12,664	9,764	12,664
Outcome mean	0.55	0.81	40.58	1.79	0.47	1.57	1.05	0.26	1.45
Upcoming local election mean									
Homicide shock mean									

	Discuss community problems	Upcoming local election	Homicide last year	Homicide last 3 years	Incumbent win margin (lag)	ENPV (lag)	Incumbent won (lag)	Incumbent vote share (lag)	Police per voter (last year)
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	
Upcoming local election	-0.031	-0.429	-1.097	0.056**	0.071	0.159*	0.008	-0.157	
	(0.047)	(1.504)	(1.748)	(0.023)	(0.095)	(0.091)	(0.017)	(0.257)	
Homicide shock	-0.009	-3.110*	-4.139*	0.009	-0.018	0.020	0.003	-0.176	
	(0.025)	(1.585)	(2.267)	(0.012)	(0.053)	(0.048)	(0.008)	(0.123)	
Homicide shock	-0.031	4.610**	6.067**	-0.036	-0.085	-0.110	0.004	0.210	
× Upcoming local election	(0.056)	(2.256)	(2.816)	(0.035)	(0.126)	(0.139)	(0.025)	(0.332)	
Observations	9,764	12,664	12,664	12,664	12,664	12,521	12,606	10,584	
Outcome mean	0.69	5.62	6.03	0.15	2.56	0.54	0.48	2.17	
Upcoming local election mean									
Homicide shock mean									

Notes: All specifications include survey year fixed effects, and are estimated using OLS. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A5: Predictors of municipalities included in each survey wave

Panel A: Linear predictors	Surveyed municipality indicator				
	(1)	(2)	(3)	(4)	(5)
Local election	0.016				
	(0.037)				
Homicide within last month		-0.020			
		(0.039)			
Homicide shock			0.036		
			(0.039)		
Homicides last year				-0.001	
				(0.001)	
Homicides last 3 years					-0.000
					(0.001)
Observations	2,092	2,088	1,351	2,088	2,088
Outcome mean	0.46	0.46	0.52	0.46	0.46
Panel B: Election-homicide interactions	Surveyed municipality indicator				
	(1)	(2)	(3)	(4)	
Local election	0.032	0.050	0.021	0.020	
	(0.055)	(0.075)	(0.042)	(0.042)	
Homicide within last month	-0.017				
	(0.040)				
Local election × Homicide within last month	-0.029				
	(0.070)				
Homicide shock		0.052			
		(0.043)			
Local election × Homicide shock		-0.124			
		(0.102)			
Homicides last year			-0.001		
			(0.001)		
Local election × Homicides last year			-0.003		
			(0.004)		
Homicides last 3 years				-0.000	
				(0.001)	
Local election × Homicides last 3 years				-0.002	
				(0.004)	
Observations	2,088	1,351	2,088	2,088	
Outcome mean	0.46	0.52	0.46	0.46	

Notes: All specifications include municipality and survey-year fixed effects, and are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A6: Municipal-level balance on 101 variables over pre-election homicide shocks

Outcome	Effect of homicide shock			Outcome	Effect of homicide shock		
	Coef.	SE	Obs.		Coef.	SE	Obs.
Area (km^2)	424.5535*	(219.1869)	3,494	Victims with complete primary schooling before election	0.9600	(9.1598)	2,118
Electorate	5159.277	(6024.1536)	3,494	Victims with secondary schooling before election	0.9925	(5.5046)	2,118
Electorate density	-0.0002	(0.0005)	3,494	Single victims before election	0.0223	(4.7211)	2,169
Local media	-1.0323	(0.0534)	3,487	Job-related homicides before election	0.9489	(1.4437)	1,631
Non-local media	-0.2996	(1.2079)	3,487	Organs examined after homicide before election	2.3932	(9.4195)	2,001
Incumbent vote share (lag)	-0.0002	(1.3293)	3,459	Family-incident homicides before election	0.0489	(0.0839)	2,217
Incumbent win (lag)	0.0389	(0.0562)	2,985	Urban homicide victims before election	1.3901	(10.838)	1,870
Municipality win margin (lag)	0.0035	(0.0141)	3,459	Non-Mexican homicide victims before election	-0.0303	(0.1554)	2,264
Municipality ENPV (lag)	-0.0226	(0.0468)	3,494	Neighbor average homicide shock	0.0050	(0.0348)	3,243
Turnout (lag)	-0.0173*	(0.0098)	3,298	Electorate-weighted neigh. ave. hom. shock	-0.0061	(0.0349)	3,484
PRI incumbent	-0.0721	(0.0504)	3,494	Average non-homicide deaths (prior 3 years)	-2.7841	(2.6206)	3,314
PAN incumbent	0.0497	(0.0512)	3,494	Average non-homicide deaths (prior year)	-4.1514	(2.9176)	3,314
PRD incumbent	0.0244	(0.0380)	3,494	Average child mortalities (prior 3 years)	-0.0516	(0.3486)	2,760
Mayor's party aligned with President	0.0667	(0.0472)	3,494	Average child mortalities (prior year)	0.1766	(0.3781)	2,760
Mayor's party aligned with Governor	-0.0792	(0.0523)	3,494	Total municipal spending	24.6886	(87.6162)	2,078
Mayor's party aligned with President and Governor	0.0076	(0.0296)	3,494	Police per voter	0.0555	(0.0897)	3,494
Homicides 12 months before election	-2.0812	(2.9771)	3,314	Occupants per dwelling	-0.0200	(0.0153)	3,460
Homicides 11 months before election	-1.5810	(3.6363)	3,314	Occupants per room	0.0017	(0.0128)	3,460
Homicides 10 months before election	-2.2831	(3.4014)	3,314	Share with 2 bedrooms	-0.0083	(0.0076)	3,460
Homicides 9 months before election	-2.1495	(3.2739)	3,314	Share 3+ bedrooms	-0.0066	(0.0081)	3,460
Homicides 8 months before election	-2.9404	(2.6120)	3,314	Share female	-0.0003	(0.0008)	3,460
Homicides 7 months before election	-2.0478	(2.8407)	3,314	Share working age	0.0006	(0.0019)	3,460
Homicides 6 months before election	-1.9702	(3.0893)	3,314	Children per woman	-0.0045	(0.0188)	3,460
Homicides 5 months before election	-1.5124	(2.2152)	3,314	Share born out of state	-0.0034	(0.0142)	3,460
Homicides 4 months before election	-0.7409	(2.8034)	3,314	Share Catholic	-0.0009	(0.0053)	3,460
Homicides 3 months before election	-1.0332	(2.9280)	3,314	Share indigenous speakers	0.0013	(0.0036)	3,460
Average homicides (prior 3 years)	-0.7327	(1.8893)	3,314	Years of schooling	-0.0142	(0.0866)	3,460
Average homicides (prior year)	-1.8318	(2.8832)	3,494	Female years of schooling	0.0181	(0.0869)	3,460
Top homicide quartile (prior year)	0.0080	(0.0424)	3,494	Male years of schooling	0.0067	(0.0921)	3,460
Top homicide decile (prior year)	0.0063	(0.0266)	3,494	Share illiterate	0.0003	(0.0028)	3,460
Presence of DTO	0.0054	(0.0422)	2,653	Share with no schooling	-0.0004	(0.0026)	3,460
Average drug-related homicides (prior year)	0.0066	(0.0141)	3,494	Share incomplete primary school	0.0004	(0.0026)	3,460
Reports of inter-DTO violence before election	-0.4688	(1.1220)	2,565	Share complete primary school	-0.0012	(0.0057)	3,460
Reports of violent enforcement before election	0.0294	(0.1492)	2,565	Share incomplete secondary school	0.0015	(0.0073)	3,460
Reports of arrests before election	0.0969	(1.0665)	2,565	Share complete secondary school	0.0023	(0.0077)	3,460
Reports of drug seizures before election	0.5839	(1.4817)	2,565	Share higher education	0.0034	(0.0082)	3,460
Reports of asset seizures before election	-0.3354	(0.3618)	2,565	Share economically active	-0.0026	(0.0032)	3,460
Reports of gun seizures before election	0.4688	(0.6213)	2,565	Share without health care	-0.0012	(0.0061)	3,460
Gun-related homicides before election	-0.1918	(6.8767)	3,494	Share state workers health care	0.0027	(0.0022)	3,460
Chemical substance-related homicides before election	-0.0279	(0.0219)	3,494	Share running water	-0.0060	(0.0067)	3,460
Hanging-related homicides before election	0.1386	(0.2538)	3,494	Share drainage	-0.0030	(0.0053)	3,460
Drowning-related homicides before election	0.1105**	(0.0496)	3,494	Share washing machine	-0.0091	(0.0103)	3,460
Explosives-related homicides before election	0.0049	(0.0063)	3,494	Share landline telephone	-0.0109	(0.0147)	3,460
Smoke/fire-related homicides before election	0.0328	(0.0404)	3,494	Share radio	-0.0039	(0.0065)	3,460
Cutting object-related homicides before election	0.6508**	(0.2836)	3,494	Share fridge	-0.0050	(0.0078)	3,460
Blunt object-related homicides before election	0.1541	(0.1029)	3,494	Share cell phone	-0.0017	(0.0104)	3,460
Delayed homicide registrations before election	1.8262	(7.3719)	3,494	Share television	-0.0020	(0.0032)	3,460
Male homicide victims before election	1.7658	(8.6080)	2,262	Share car or truck	-0.0036	(0.0100)	3,460
Age of homicide victims before election	1.5388*	(0.8059)	2,225	Share computer	-0.0046	(0.0108)	3,460
Victims with no schooling before election	0.3240	(0.3977)	2,118	Share internet	-0.0038	(0.0098)	3,460
Victims with incomplete primary schooling before	1.4145	(9.7034)	2,118				

Notes: Specifications include municipality and year fixed effects, and are estimated using OLS. The time-invariant variables—area, media coverage, and the 2010 Census variables (occupants per dwelling through share internet)—exclude municipality fixed effects. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A7: Precinct-level balance on 106 variables over pre-election homicide shocks

Outcome	Effect of homicide shock			Outcome	Effect of homicide shock		
	Coef.	SE	Obs.		Coef.	SE	Obs.
Electorate	81.1990	(53.5095)	174,622	Victims with complete primary schooling before election	0.8803	(6.6975)	145,965
Municipality electorate	5241.6772	(4741.8763)	174,330	Victims with secondary schooling before election	0.9487	(4.0218)	145,965
Electorate density	18.4433	(41.3544)	172,011	Single victims before election	-0.0251	(3.4588)	147,395
Incumbent vote share (lag)	0.0005	(0.0072)	174,622	Job-related homicides before election	0.9524	(1.0067)	129,680
Incumbent win (lag)	0.0410	(0.0414)	145,890	Organs examined after homicide before election	2.3184	(6.8473)	142,448
Win margin (lag)	0.0011	(0.0090)	174,622	Family-incident homicides before election	0.0485	(0.0605)	147,050
Municipality win margin (lag)	0.0034	(0.0110)	173,715	Urban homicide victims before election	1.2799	(7.5544)	127,256
ENPV (lag)	-0.0204	(0.0318)	174,619	Non-Mexican homicide victims before election	-0.0328	(0.1140)	149,867
Municipality ENPV (lag)	-0.0218	(0.0364)	174,622	Neighbor average homicide shock	0.0040	(0.0268)	170,659
Turnout (lag)	-0.0179**	(0.0074)	165,136	Electorate-weighted neigh. ave. hom. shock	-0.0065	(0.0272)	174,563
Municipality turnout (lag)	-0.0179**	(0.0074)	164,622	Average non-homicide deaths (prior 3 years)	-2.8278	(2.0218)	174,148
PRI incumbent	-0.0710*	(0.0390)	174,622	Average non-homicide deaths (prior year)	-4.1908*	(2.2467)	174,148
PAN incumbent	0.0499	(0.0392)	174,622	Average child mortalities (prior 3 years)	-0.0474	(0.2579)	138,882
PRD incumbent	0.0229	(0.0296)	174,622	Average child mortalities (prior year)	0.1795	(0.2808)	138,882
Mayor's party aligned with President	0.0667*	(0.0366)	174,622	Total municipal spending	24.4864	(66.508)	114,581
Mayor's party aligned with Governor	-0.0790*	(0.0405)	174,622	Police per voter	0.0592	(0.0694)	174,330
Mayor's party aligned with President and Governor	0.0065	(0.0229)	174,622	Area (km^2)	4.5409*	(2.4392)	172,011
Homicides 12 months before election	-2.0558	(2.3129)	174,148	Local media	-1.0474	(1.2066)	172,546
Homicides 11 months before election	-1.5767	(2.8334)	174,148	Non-local media	-0.2794	(1.3447)	172,546
Homicides 10 months before election	-2.2772	(2.6496)	174,148	Occupants per dwelling	-0.0189	(0.0157)	174,287
Homicides 9 months before election	-2.1626	(2.5544)	174,148	Occupants per room	0.0010	(0.0127)	174,287
Homicides 8 months before election	-2.9409	(2.0342)	174,148	Share with 2 bedrooms	-0.0065	(0.0074)	174,156
Homicides 7 months before election	-2.0593	(2.2104)	174,148	Share 3+ bedrooms	-0.0050	(0.0080)	174,156
Homicides 6 months before election	-1.9516	(2.4000)	174,148	Share female	-0.0003	(0.0008)	174,167
Homicides 5 months before election	-1.4975	(1.7223)	174,148	Share working age	0.0007	(0.0019)	174,167
Homicides 4 months before election	-0.7296	(2.1827)	174,148	Children per woman	-0.0028	(0.0182)	174,287
Homicides 3 months before election	-1.0166	(2.2786)	174,148	Share born out of state	-0.0044	(0.0129)	174,167
Average homicides (prior 3 years)	-0.7374	(1.4691)	174,148	Share Catholic	-0.0006	(0.0048)	174,167
Average homicides (prior year)	-1.8248	(2.2650)	174,622	Share indigenous speakers	0.0011	(0.0035)	174,156
Top homicide quartile (prior year)	0.0067	(0.0330)	174,622	Years of schooling	-0.0100	(0.0896)	174,287
Top homicide decile (prior year)	0.0067	(0.0206)	174,622	Female years of schooling	0.0154	(0.0905)	174,287
Presence of DTO	0.0053	(0.0308)	135,384	Male years of schooling	0.0038	(0.0966)	174,287
Average drug-related homicides (prior year)	0.0069	(0.0110)	174,622	Share illiterate	0.0002	(0.0028)	174,156
Reports of inter-DTO violence before election	-0.4559	(0.8231)	131,171	Share with no schooling	-0.0005	(0.0026)	174,156
Reports of violent enforcement before election	0.0341	(0.1092)	131,171	Share incomplete primary school	0.0005	(0.0026)	174,156
Reports of arrests before election	0.1244	(0.7872)	131,171	Share complete primary school	-0.0013	(0.0057)	174,156
Reports of drug seizures before election	0.6271	(1.0910)	131,171	Share incomplete secondary school	0.0011	(0.0073)	174,156
Reports of asset seizures before election	-0.3248	(0.2641)	131,171	Share complete secondary school	0.0016	(0.0077)	174,156
Reports of gun seizures before election	0.4924	(0.4583)	131,171	Share higher education	0.0026	(0.0084)	174,156
High-risk electoral precinct	0.0019	(0.0034)	52,498	Share economically active	-0.0029	(0.0032)	174,167
Gun-related homicides before election	-0.1909	(5.4119)	174,622	Share without health care	-0.0020	(0.0059)	174,167
Chemical substance-related homicides before election	-0.0284*	(0.0172)	174,622	Share state workers health care	0.0028	(0.0021)	174,167
Hanging-related homicides before election	0.1279	(0.1965)	174,622	Share running water	-0.0055	(0.0065)	174,156
Drowning-related homicides before election	0.1098***	(0.0386)	174,622	Share drainage	-0.0038	(0.0051)	174,156
Explosives-related homicides before election	0.0050	(0.0049)	174,622	Share washing machine	-0.0084	(0.0100)	174,156
Smoke/fire-related homicides before election	0.0321	(0.0314)	174,622	Share landline telephone	-0.0113	(0.0143)	174,156
Cutting object-related homicides before election	0.6495***	(0.2220)	174,622	Share radio	-0.0040	(0.0064)	174,156
Blunt object-related homicides before election	0.1553*	(0.0796)	174,622	Share fridge	-0.0043	(0.0075)	174,156
Delayed homicide registrations before election	1.8104	(5.8018)	174,622	Share cell phone	-0.0017	(0.0100)	174,156
Male homicide victims before election	1.6872	(6.3566)	149,857	Share television	-0.0019	(0.0032)	174,156
Age of homicide victims before election	1.5006***	(0.5803)	148,789	Share car or truck	-0.0031	(0.0099)	174,156
Victims with no schooling before election	0.3211	(0.2870)	145,965	Share computer	-0.0048	(0.0109)	174,156
Victims with incomplete primary schooling before	1.3313	(7.0974)	145,965	Share internet	-0.0041	(0.0099)	174,156

Notes: Specifications include municipality and year fixed effects, and are estimated using OLS. The time-invariant variables—area, media coverage, and the 2010 Census variables (occupants per dwelling through share internet)—exclude municipality fixed effects. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A8: Effect of local elections on municipal public security employment

	Municipal public security employees per 1000 voters	
	(1)	(2)
Local election year	0.006	-0.038
	(0.069)	(0.082)
Observations	9,655	9,655
Outcome mean	4.23	4.23
Outcome range	[.01,201.86]	[.01,201.86]
Local election year mean	0.31	0.31
Municipality-specific time trends		✓

Notes: All specifications include municipality and survey-year fixed effects, and are estimated using OLS. Municipalities with no employees are excluded. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A9: Correlation between election outcomes and post-election homicides

	Average homicides in the two months after an election			
	(1)	(2)	(3)	(4)
Incumbent party win	4.315			
	(9.473)			
Change in incumbent party vote share		-7.606		
		(6.251)		
PAN win			0.183	-19.195
			(6.357)	(25.776)
PRI win			1.124	1.905
			(4.920)	(19.214)
PRD win			-8.656	-13.411
			(10.742)	(52.524)
Observations	3314	174148	3314	1558
Outcome mean	18.90	18.91	18.90	27.84
Election outcome mean	-0.05	0.56		

Notes: All specifications include municipality and year fixed effects, and are estimated using OLS. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A10: Precinct-level balance over 105 variables over the number of local media stations

Outcome	Effect of homicide shock			Outcome	Effect of homicide shock		
	Coef.	SE	Obs.		Coef.	SE	Obs.
Homicide shock	-0.0005	(0.0004)	33,460	Male homicide victims before election	-0.0028	(0.0096)	31,705
Area (km^2)	-0.0018	(0.0059)	33,460	Age of homicide victims before election	-0.1094	(0.3309)	31,407
Electorate	-21.5210	(15.1923)	33,460	Victims with no schooling before election	-0.0004	(0.0006)	31,455
Municipal electorate	-43.8548	(39.2136)	33,425	Victims with incomplete primary schooling before	-0.0023	(0.0103)	31,455
Electorate density	1.5194	(23.8498)	33,430	Victims with complete primary schooling before election	-0.0022	(0.0094)	31,455
Distance from centroid to municipal head (log)	-0.0020	(0.0036)	33,460	Victims with secondary schooling before election	-0.0015	(0.0061)	31,455
Non-local media	-0.0133	(0.1847)	33,460	Single victims before election	-0.0004	(0.0042)	31,456
Incumbent vote share (lag)	-0.0005	(0.0005)	33,460	Job-related homicides before election	-0.0008	(0.0019)	29,230
Incumbent win (lag)	-0.0005	(0.0004)	27,161	Organs examined after homicide before election	-0.0008	(0.0095)	30,979
Win margin (lag)	0.0001	(0.0005)	33,460	Family-incident homicides before election	0.0001	(0.0002)	30,957
Municipality win margin (lag)	0.0001	(0.0001)	33,460	Urban homicide victims before election	-0.0071	(0.0084)	27,949
ENPV (lag)	0.0023	(0.0024)	33,460	Non-Mexican homicide victims before election	0.0000	(0.0002)	31,705
Municipality ENPV (lag)	0.0000	(0.0003)	33,460	Neighbor average homicide shock	0.0003*	(0.0002)	32,222
Turnout (lag)	0.0001	(0.0006)	32,478	Electorate-weighted neigh. ave. hom. shock	0.0004*	(0.0002)	33,460
Municipality turnout (lag)	-0.0000*	(0.0000)	32,519	Average non-homicide deaths (prior 3 years)	-0.0124	(0.0180)	33,460
PRI incumbent	0.0001	(0.0003)	33,460	Average non-homicide deaths (prior year)	-0.0109	(0.0176)	33,460
PAN incumbent	-0.0004	(0.0004)	33,460	Average child mortalities (prior 3 years)	0.0019	(0.0018)	25,250
PRD incumbent	0.0002	(0.0002)	33,460	Average child mortalities (prior year)	0.0004	(0.0011)	25,250
Mayor's party aligned with President	-0.0002	(0.0003)	33,460	Total municipal spending	-0.2589	(0.3446)	21,524
Mayor's party aligned with Governor	-0.0004	(0.0003)	33,460	Police per voter	0.0004	(0.0005)	33,430
Mayor's party aligned with President and Governor	0.0000	(0.0002)	33,460	Occupants per dwelling	-0.0033	(0.0021)	33,460
Homicides 12 months before election	0.0025	(0.0070)	33,460	Occupants per room	-0.0034**	(0.0014)	33,460
Homicides 11 months before election	0.0021	(0.0102)	33,460	Share with 2 bedrooms	0.0001	(0.0006)	33,460
Homicides 10 months before election	-0.0016	(0.0058)	33,460	Share 3+ bedrooms	0.0010	(0.0007)	33,460
Homicides 9 months before election	0.0018	(0.0066)	33,460	Share female	0.0002	(0.0002)	33,460
Homicides 8 months before election	0.0010	(0.0053)	33,460	Share working age	-0.0003	(0.0002)	33,460
Homicides 7 months before election	-0.0094	(0.0078)	33,460	Children per woman	-0.0011	(0.0019)	33,460
Homicides 6 months before election	-0.0011	(0.0098)	33,460	Share born out of state	0.0003	(0.0005)	33,460
Homicides 5 months before election	-0.0025	(0.0090)	33,460	Share Catholic	0.0001	(0.0006)	33,460
Homicides 4 months before election	-0.0001	(0.0063)	33,460	Share indigenous speakers	0.0001	(0.0002)	33,460
Homicides 3 months before election	0.0006	(0.0054)	33,460	Years of schooling	0.0105	(0.0103)	33,460
Average homicides (prior 3 years)	0.0029	(0.0045)	33,460	Female years of schooling	0.0052	(0.0101)	33,460
Average homicides (prior year)	-0.0007	(0.0068)	33,460	Male years of schooling	0.0148	(0.0105)	33,460
Top homicide quartile (prior year)	0.0000	(0.0000)	33,460	Share illiterate	-0.0002	(0.0001)	33,460
Top homicide decile (prior year)	0.0000	(0.0000)	33,460	Share with no schooling	-0.0002	(0.0002)	33,460
Presence of DTO	-0.0002	(0.0002)	24,916	Share incomplete primary school	0.0002	(0.0002)	33,460
Average drug-related homicides (prior year)	0.0001	(0.0001)	33,460	Share complete primary school	0.0004	(0.0004)	33,460
Reports of inter-DTO violence before election	-0.0002	(0.0028)	24,137	Share incomplete secondary school	0.0007	(0.0007)	33,460
Reports of violent enforcement before election	0.0004	(0.0005)	24,137	Share complete secondary school	0.0010	(0.0008)	33,460
Reports of arrests before election	0.0038	(0.0026)	24,137	Share higher education	0.0011	(0.0011)	33,460
Reports of drug seizures before election	0.0050	(0.0048)	24,137	Share economically active	0.0002	(0.0003)	33,460
Reports of asset seizures before election	0.0013	(0.0010)	24,137	Share without health care	-0.0002	(0.0004)	33,460
Reports of gun seizures before election	0.0005	(0.0009)	24,137	Share state workers health care	0.0003	(0.0003)	33,460
High-risk electoral precinct	0.0011	(0.0011)	7,921	Share running water	0.0013**	(0.0006)	33,460
Gun-related homicides before election	-0.0014	(0.0091)	33,460	Share drainage	0.0006**	(0.0003)	33,460
Chemical substance-related homicides before election	0.0000	(0.0001)	33,460	Share washing machine	0.0006	(0.0009)	33,460
Hanging-related homicides before election	0.0009	(0.0008)	33,460	Share landline telephone	0.0023*	(0.0012)	33,460
Drowning-related homicides before election	0.0000	(0.0001)	33,460	Share fridge	0.0003	(0.0005)	33,460
Explosives-related homicides before election	NA	NA	33,460	Share cell phone	0.0001	(0.0005)	33,460
Smoke/fire-related homicides before election	0.0000	(0.0002)	33,460	Share car or truck	0.0006	(0.0012)	33,460
Cutting object-related homicides before election	0.0000	(0.0008)	33,460	Share computer	0.0011	(0.0013)	33,460
Blunt object-related homicides before election	0.0003	(0.0003)	33,460	Share internet	0.0020*	(0.0012)	33,460
Delayed homicide registrations before election	0.0003	(0.0103)	33,460				

Notes: All specifications include neighbor group and year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters in the electoral precinct divided by he number of comparison units within each neighbor group. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A11: Effect of pre-election drug-related homicide shocks on municipal incumbent electoral outcomes

	Incumbent party win	Change in incumbent party vote share
	(1)	(2)
Drug-related homicide shock	-0.068	-0.020
	(0.074)	(0.017)
Observations	473	38,038
Outcome mean	0.53	-0.05
Homicide shock mean	0.44	0.44

Notes: All specifications are estimated using OLS, and include municipality and year fixed effects. All observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

with the general homicide level, a meaningful test of increased homicides around elections requires a subtler design.[75] I use the difference between the average number of homicides in the two months prior to an election and the average number of homicides over the prior electoral cycle (i.e. preceding 34 months), divided by the average number of homicides over the prior electoral cycle. This captures differences in the magnitude of pre-election shocks across elections, and thus exploits variation in the intensity of these shocks across municipalities. The identifying assumption is that municipalities experiencing different homicide deviations from cycle trends before elections otherwise follow parallel trends in incumbent support.

Table A12 reports estimates from the analog of equation (4), and demonstrates that this deviation measure of homicide shocks non-linearly decreases the incumbent party's vote share. Consistent with the idea that sanctioning is diminishing for a sufficiently large shock, columns (3) and (5) show that logarithmic and squared version of the deviation measure show clear negative but diminishing effects. The linear version in column (1) is also clearly negative but is not quite statistically significant in panel A. The addition of municipality-specific trends substantially increase standard errors but provide similar point estimates.

A.5.4 Differential updating across elections

Table A13 shows the regression estimates underlying Figure 7.

[75] The results of such an approach are thus similar to the small long-run estimates reported in Table 3.

Table A12: Alternative approach to estimating the effect of short-run homicides rates on municipal incumbent electoral outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Incumbent party win						
Proportional deviation from average monthly homicide	-0.0190	-0.0178			-0.0583***	-0.0630*
	(0.0157)	(0.0278)			(0.0213)	(0.0369)
Proportional deviation from average monthly homicide (log)			-0.0779*	-0.0601		
			(0.0424)	(0.1218)		
Proportional deviation from average monthly homicide squared					0.0053***	0.0054**
					(0.0014)	(0.0022)
Observations	2,676	2,676	1,401	1,401	2,676	2,676
Outcome mean	0.55	0.55	0.51	0.51	0.55	0.55
Homicide measure mean	0.32	0.32	-0.63	-0.63	0.32	0.32
Homicide measure standard deviation	1.39	1.39	1.25	1.25	1.39	1.39
Panel B: Outcome: Change in incumbent party vote share						
Proportional deviation from average monthly homicide	0.0001	-0.0014			-0.0079**	-0.0106*
	(0.0026)	(0.0035)			(0.0038)	(0.0057)
Proportional deviation from average monthly homicide (log)			-0.0028	-0.0102		
			(0.0065)	(0.0097)		
Proportional deviation from average monthly homicide squared					0.0011***	0.0011***
					(0.0003)	(0.0004)
Observations	166,341	166,341	82,076	82,076	166,341	166,341
Outcome mean	0.55	0.55	0.51	0.51	0.55	0.55
Homicide measure mean	0.32	0.32	-0.63	-0.63	0.32	0.32
Homicide measure standard deviation	1.39	1.39	1.25	1.25	1.39	1.39
Municipality-specific time trends		✓		✓		✓

Notes: All specifications are estimated using OLS, and include municipality and year fixed effects. All observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A13: The effect of a homicide shock on the change in incumbent party vote share, by previous homicide shocks and incumbent identity

	Change in incumbent party vote share (1)
Homicide shock	-0.041**
	(0.018)
Homicide shock at last election	-0.042*
	(0.025)
Homicide shock × Homicide shock at last election	0.064**
	(0.030)
Incumbent won previous election	-0.047**
	(0.019)
Homicide shock × Incumbent won previous election	0.014
	(0.029)
Homicide shock at last election × Incumbent won previous election	0.036
	(0.031)
Homicide shock × Homicide shock at last election	-0.071
× Incumbent won previous election	(0.044)
Observations	112,805
Outcome range	[-0.90,0.89]
Outcome mean	-0.06
Outcome standard deviation	0.15
Homicide shock mean	0.43
Homicide shock at last election mean	0.40
Incumbent won previous election mean	0.56

Notes: All specifications include municipality and year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.5.5 Learning from the experiences of neighboring municipalities

To identify how comparative performance information impacts voter behavior, I compute the average homicide shock across neighboring municipalities.[76] The exogeneity of homicide shocks within municipalities facilitates a causal interpretation of the interaction between homicide shocks within a voter's municipality and shocks to their neighbors. Consistent with voters benchmarking their incumbent's performance against that of their neighbors, column (1) shows that the probability that an incumbent party is re-elected increases (albeit not quite significantly so) with the average shock experienced by neighboring municipalities. However, the negative interaction between the homicide shock and the neighbor average implies that relative performance considerations do not apply when a voter's own municipality is afflicted by a shock.[77] Plausibly consistent with increased salience, mayoral punishment instead increases with the proportion of shocks experienced by neighbors. The change is vote share follows a similar pattern but is not statistically significant, while columns (2) and (4) report similar results when an electorate-weighted neighboring municipal shock measure is used instead.

A.5.6 Differential impacts of homicide shocks across parties

I also examine differential punishment across parties. Due to the PRI's more extensive clientelistic ties (e.g. Cornelius 1996; Fox 1994; Greene 2007; Magaloni 2006), PRI voters may be less susceptible to performance information, and thus less inclined to punish PRI incumbents for homicide shocks. Table A15 provides tentative evidence consistent with this expectation. Although the interaction terms are not statistically significant, column (1) shows that the PAN and PRD are punished relatively more than the PRI. Although punishment of PRI incumbents is not statistically, columns (2)-(4) register significant punishment of PAN and PRD mayors. This is likely because the PAN and especially PRD control more urban areas where the effects of homicide shocks are larger.

Finally, who do voters turn to to reduce local violence? If reducing violence is a major concern, and voters believe that Calderón's tough stance on drug-related crime may help their municipality (Dell 2015), even PAN mayors may benefit from a homicide shock. Column (5) of panel C finds suggestive evidence for this: although a homicide shock increases the PAN's vote share by 4.4 percentage points during Calderón's presidency, this interaction is not statistically significant.

[76] The results are robust to weighting neighboring municipalities by the size of their registered electorates.

[77] Unreported results separating neighboring incumbents from the same and different parties, but show no meaningful differences.

Table A14: Effect of pre-election homicide shocks on municipal incumbent electoral outcomes, by neighbor average homicide shock

	Incumbent party win	Incumbent party win	Change in incumbent party vote share	Change in incumbent party vote share
	(1)	(2)	(3)	(4)
Homicide shock	0.007	-0.012	-0.001	-0.005
	(0.091)	(0.089)	(0.018)	(0.017)
Neighbor average homicide shock	0.160		0.026	
	(0.106)		(0.020)	
Homicide shock × Neighbor	-0.259*		-0.043	
average homicide shock	(0.152)		(0.029)	
Electorate-weighted neighbor		0.149		0.026
average homicide shock		(0.102)		(0.019)
Homicide shock × Electorate-weighted		-0.225		-0.036
neighbor average homicide shock		(0.150)		(0.028)
Observations	3,243	3,484	170,659	174,563
Outcome mean	0.55	0.55	-0.05	-0.05
Homicide shock mean	0.42	0.42	0.42	0.42
Interaction mean	0.47	0.46	0.47	0.46

Notes: All specifications are estimated using OLS. All observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A15: Effects of pre-election homicide shocks on municipal incumbent electoral outcomes, by incumbent party

	Change in incumbent party vote share (1)	Change in PAN incumbent vote share (2)	Change in PRD incumbent vote share (3)	Change in PRI incumbent vote share (4)	Change in PAN incumbent vote share (5)
Homicide shock	-0.008	-0.037***	-0.047*	-0.009	0.028
	(0.013)	(0.012)	(0.025)	(0.013)	(0.022)
Homicide shock × PAN	-0.016				
	(0.023)				
Homicide shock × PRD	-0.044				
	(0.035)				
Homicide shock × Calderón Presidency					0.044
					(0.043)
Observations	165,920	58,429	20,670	86,821	174,622
Outcome mean	-0.05	-0.06	-0.09	-0.04	-0.05
Homicide shock mean	0.42	0.43	0.44	0.41	0.42

Notes: All specifications are estimated using OLS. All observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.