TUNING IN, VOTING OUT: NEWS CONSUMPTION CYCLES, HOMICIDES, AND ELECTORAL ACCOUNTABILITY IN MEXICO *

JOHN MARSHALL†

FEBRUARY 2024

Indicators of politician performance reported before elections often seem to influence vote choices. I argue that voters' news consumption patterns play a key role in accounting for this phenomenon: periodic attention to news tracks electoral cycles, which induces poorly-informed voters to rely on noisy incumbent performance signals revealed during election campaigns. Examining homicides that precede Mexican municipal elections, I show that: (i) voters consume more news and are better informed before local elections; (ii) pre-election homicide spikes that coincide with voters' greater pre-election news consumption—but not longer-term homicide rates—reduce incumbent party re-election rates; and (iii) such electoral sanctioning is driven by access to local broadcast media stations. Further evidence indicates that voting behavior reflects belief updating when voters are most attentive to news, rather than changes in media coverage of homicides before elections. Voters' news consumption cycles thus help to explain when the media is most persuasive and why electoral accountability is often limited.

*I thank Jim Alt, Eric Arias, Abhijit Banerjee, Allyson Benton, David Broockman, Francisco Cantú, Oeindrila Dube, Raissa Fabregas, Nilesh Fernando, Hernán Flom, Alex Fouirnaies, Jeff Frieden, Thomas Fujiwara, Saad Gulzar, Sebastian Garrido de Sierra, Andy Hall, Torben Iversen, Dorothy Kronick, Joy Langston, Horacio Larreguy, Chappell Lawson, Sandra Ley, Rakeen Mabud, Nicola Mastrorocco, Noah Nathan, Brian Phillips, Pablo Querubín, Carlo Prato, James Robinson, Rob Schub, Alberto Simpser, Jim Snyder, Chiara Superti, Edoardo Teso, Giancarlo Visconti, Julie Weaver, and workshop or conference participants at presentations at APSA, Berkeley, Caltech, Columbia, Harvard, ITAM, LSE, MIT, MPSA, NYU, NEWEPS, Princeton, Stanford, Stanford GSB, Trinity College Dublin, and Yale for illuminating discussions and useful comments. I thank Bruno Avila and Megan Kelly for excellent research assistance.

†Department of Political Science, Columbia University. Email: jm4401@columbia.edu.

1 Introduction

Information has the potential to improve governance by enabling voters to identify suitable politicians to represent them (e.g. Fearon 1999) and motivating politicians to perform well to get re-elected (e.g. Barro 1973; Ferejohn 1986). Indicators of performance on salient issues—such as unemployment, service delivery, and public security—can then serve as helpful signals of an incumbent's competence, alignment with voters' interests, or effort. However, voters are often poorly informed about public affairs (Keefer 2007; Pande 2011), especially regarding local government. This lack of politically-relevant information may explain why electorates frequently fail to hold incumbents to account for their performance in office.[1] Moreover, the welfare losses associated with failures to effectively select and control politicians are likely to be particularly great in the Global South, where there often exist few other constraints on politicians' potential to extract rents.

Viewing low levels of electoral accountability as stemming from limited access to politically-relevant information, scholars and policymakers have focused on increasing the availability of credible information (Khemani et al. 2016). This relies on media outlets disseminating indicators of incumbent performance that voters would not otherwise obtain, and receives some empirical support (Banerjee et al. 2011; Ferraz and Finan 2008; Larreguy, Marshall and Snyder 2020; Snyder and Strömberg 2010). However, as Downs (1957) poignantly argued, individuals often lack incentives to seek out politically-relevant information. Consequently, while informative media reporting may be a precondition for enhancing electoral accountability, the focus on *access* neglects the reality that the availability of media coverage does not necessarily imply that voters actually *consume* politically-relevant news when it is available to them.

I argue that the relationship between incumbent performance in office and electoral accountability critically depends on the *timing* of voters' news consumption. Attention to news is likely to track electoral cycles because voters seek out politically-relevant information as a consumption good (Hamilton 2004), for work (Larcinese 2005), to meet social expectations (Marshall 2019), or out of civic duty (Feddersen and Sandroni 2006). Rather than isolate the causes of news consumption cycles, I focus on their electoral consequences by showing how the interaction between greater attention to news before elections and the occurrence of politically-relevant events reported by the media at this time shapes election outcomes.

Standard models of electoral accountability imply that voters seeking to retain competent or hard-working incumbents will re-elect parties that surpass a performance threshold (e.g. Ashworth 2005; Barro 1973; Fearon 1999; Ferejohn 1986; Rogoff 1990). When Bayesian but inattentive voters principally consume news before elections, their re-election decision will be driven by the

[1] Evidence that voters punish or reward politicians for economic performance (see Anderson 2007), corruption (see Pande 2011), or legislative effort (Humphreys and Weinstein 2012) is mixed. Voters may also punish politicians for adverse outcomes beyond their control (Achen and Bartels 2017).

performance indicators reported at this time, even when these signals are known to reflect a combination of incumbent type or effort *and* idiosyncratic shocks. News consumption cycles could then help explain the apparently large electoral impact of "October surprises," such as the revelation before the 2016 U.S. presidential election that the FBI was continuing to investigate Hillary Clinton, the scandals concerning Keiko Fujimori that emerged before the runoff of Peru's 2016 presidential election, or Ellen Johnson Sirleaf's receipt of the Nobel Peace Prize days before the first round of Liberia's 2011 presidential election. Indeed, around a quarter of voters change their vote choice in the final two months of election campaigns (Le Pennec and Pons forthcoming).

I assess the electoral impact of news consumption cycles by examining the extent to which Mexican voters rely on homicides that occurred shortly before elections to select their municipal government. Public security remains a major concern, as in other Latin American countries; homicides thus represent a salient measure of incumbent party performance for citizens, who are often poorly informed about public affairs (Chong et al. 2015).[2] Focusing on municipal elections between 1999 and 2012, I test the theory's central predictions for news consumption and electoral accountability by leveraging plausibly exogenous variation in: (i) temporal proximity to local elections; (ii) municipal-level homicide spikes occurring before such elections; and (iii) access to local broadcast media stations likely to report such homicides.

First, I demonstrate the existence of cycles in voters' news consumption. Although increased interest in news is often observed around national elections, it is important to causally establish this first step of the theory in the context of Mexican local elections before evaluating the electoral consequences. Exploiting the irregular timing of survey waves with respect to states' staggered electoral cycles to estimate the effect of proximity to upcoming local—municipal and state legislative—elections, I show that voters consumed more news and programs about politics and public affairs via radio and television in the months preceding local elections. Some voters only started consuming news during election campaigns. Greater news consumption is reflected in a 0.5 standard deviations increase in topical knowledge of public affairs.

Second, I leverage short-term volatility in municipal homicides to isolate idiosyncratic variation in the homicide count that voters encounter in the news before elections. Specifically, I compare "shocked" municipalities that experienced more homicides in the 60 days *before* election day to "control" municipalities that experienced at least as many homicides in the 60 days *after* election day. The relative infrequency of homicides within most municipalities make them especially an hard form of crime for politicians to control in the short term. Indeed, the distribution of homicide shocks is consistent with sampling variability, balanced over more than 100 predetermined covariates including incumbent party support and longer-term homicide rates, and incompatible

[2] Mayors could not be re-elected until 2018, but voters often held the incumbent's party responsible for performance in Mexico's party-centric system (Arias et al. 2022; Chong et al. 2015; Larreguy, Marshall and Snyder 2020).

with theories of manipulation by drug trafficking organizations (DTOs) or politicians.

The results demonstrate that pre-election homicides have substantially impacted Mexican election outcomes. Pre-election homicide shocks decreased the municipal incumbent party's probability of winning by 16 percentage points and vote share by 2.4 percentage points. Using this shock to instrument for the homicide rate before the election, each additional homicide per 100,000 people per month in the 60 days preceding the election—corresponding to a 64% increase over the average number of homicides per capita—reduced the incumbent party's re-election probability by 19 percentage points and vote share by 2.8 percentage points. In contrast, panel estimates show that homicide rates over the prior year or incumbent's entire term are not significantly correlated with the party's electoral prospects. Although longer-term homicide rates are more likely to reflect incumbent party competence or effort, these findings indicate that many voters relied on the imprecise indicators of homicides available at the peak of their news consumption before elections.

Third, to establish that such voting behavior is driven by media coverage—rather than word of mouth, policy responses, or campaign responses on the ground—I demonstrate that access to *local* broadcast media induces the negative effect of pre-election homicides on incumbent support. Using radio and television coverage maps, I exploit topographical variation in coverage between neighboring electoral precincts within the same municipality to estimate the effects of access to media outlets based within a precinct's own municipality. The results show that each additional local media station reduced the incumbent party's vote share by 0.4 percentage points in municipalities that experienced a pre-election homicide shock. Access to an additional local media station also increased the incumbent party's vote share by 0.2 percentage points in municipalities that did not experience a pre-election homicide shock. Conversely, electoral precincts covered by few local media stations were not responsive to homicide shocks.

Analyses of belief updating further illustrate the importance of the timing of voters' news consumption. Survey data show that *pre-survey* homicide shocks that coincided with election campaigns increased concern about public security. In contrast, pre-survey homicide shocks did not increase public security concerns when they occurred outside election campaigns, when voters consume less news. Moreover, consistent with voters updating their beliefs from relevant information that they consume, pre-election homicide shocks do not affect support for the incumbent party of municipal governments that did not command a police force.

Alternative potential mechanisms cannot fully account for these results. First, the lack of an effect of recent homicides on voter beliefs outside election campaigns is inconsistent with explanations that solely rely on voters possessing short memories (Sarafidis 2007; Zaller 1992), substituting recent indicators for longer-term performance (Healy and Lenz 2014; Wolfers 2007), or regarding recent events as more informative about incumbent type or effort (Rogoff 1990). Second, pre-election homicides may convey different information if politicians or cartels use crime rates to send signals to voters (Rogoff 1990). While DTOs have strategically targeted politicians in Mexico (e.g. Trejo and Ley 2021), extensive identification checks suggest that this study's empirical design isolates an idiosyncratic component of variation in pre-election homicide rates. Finally, voters' sensitivity to pre-election events could reflect differences in media content before elections, rather than voters' greater attention to news. However, news coverage of homicides does not appear to change before elections: using news reports scraped from Google, I show that the correlation between homicides and the quantity and content of reporting on homicides by newspapers—an important source of content for local broadcast media—is not significantly different before local elections. Moreover, the effect of pre-election homicides does not vary with the share of radio and television ads allocated to the party of the municipal incumbent during election campaigns.

The finding that news consumption cycles induce voters to rely on pre-election incumbent performance indicators makes three main contributions. First, prior studies of other valence issues have similarly found voters to be more responsive to economic growth and unemployment (Achen and Bartels 2017; Fair 1996; Healy and Lenz 2014; Wolfers 2007), corruption revelations (Brollo 2008; Chang, Golden and Hill 2010), and disaster relief (Cole, Healy and Werker 2012) occurring closer to elections.[3] Beyond leveraging exogenous variation in the performance indicator itself, my focus on homicides highlights the understudied electoral consequences of public security—a key concern in many developing contexts. Others have studied the political mobilization effects of individual-level victimization (e.g. Bateson 2012; Bellows and Miguel 2009; Blattman 2009), but the few estimates of aggregate-level crime's effect on vote choice are mixed. Although Israelis turn to right-wing parties after terror attacks (Berrebi and Klor 2008), Ley (2017) documents little correlation between *annual* municipal-level homicide rates—except for DTO violence against politicians—and electoral support for municipal incumbent parties in Mexico.[4] I square these findings by showing that vote choices are principally sensitive to homicides occurring in the months immediately preceding elections, rather than homicides occurring even within the year preceding elections. This suggests that newsworthy events right before elections are an important channel through which election campaign periods influence vote choices.

Second, I substantiate a novel explanation for voters' high sensitivity to pre-election incumbent performance indicators. The news consumption cycles mechanism differs from theories positing that cognitive constraints drive voters' myopic response to economic news *once exposed to it* (Achen and Bartels 2017; Healy and Lenz 2014; Huber, Hill and Lenz 2012; Wolfers 2007). While there remains debate about whether cognitive constraints later affect electoral responses to news

[3] Partisan ads aired closer to elections also have larger impacts (Sides, Vavreck and Warshaw 2022), although the timing effects of non-partisan mobilization campaigns have proved mixed (Murray and Matland 2014; Panagopoulos 2011). These few studies randomizing the timing of exposure cannot distinguish the effects of recency from attention.

[4] Survey data also show a mixed correlation between security evaluations and self-reported vote choice (Altamirano and Ley 2020; Ley 2017; Pérez 2015).

(see e.g. Achen and Bartels 2017; Ashworth, Bueno de Mesquita and Friedenberg 2018), my findings more fundamentally show that periodic news consumption shapes the information voters are exposed to in the first place. By illustrating the additional role of news consumption cycles, this study highlights a separate challenge for electoral accountability: the importance of cultivating persistent engagement with relevant news. It thus complements recent research endogenizing demand for political information, which has found that interest in politics can be activated (Chen and Yang 2019) but may be short-lived (Hobbs and Roberts 2018; Marshall 2019).

Finally, the timing of voters' news consumption helps to explain the varied impacts of information revelation on electoral accountability. Prior studies find large electoral effects of mass media reporting on incumbent policy-making and malfeasance in office (e.g. Banerjee et al. 2011; Ferraz and Finan 2008; Larreguy, Marshall and Snyder 2020; Snyder and Strömberg 2010), while limited dissemination may explain the mixed effects of lower-reach experimental interventions (Dunning et al. 2019). By connecting citizens' news consumption patterns and political accountability, I show that the effect of access to relevant news on electoral accountability is critically moderated by voter attention: media reports primarily matter when they coincide with news consumption peaks. Consequently, my findings help rationalize why incumbents engage in less corruption in anticipation of audits reports that will be released before elections (Bobonis, Cámara Fuertes and Schwabe 2016) or announce fiscal stimuli before elections (Brender and Drazen 2005), in addition to complementing evidence that politicians act differently when media coverage is devoted to other events (Durante and Zhuravskaya 2018; Eisensee and Strömberg 2007; Kaplan, Spenkuch and Yuan 2019). News consumption cycles thereby constrain the media's pro-accountability potential, while emphasizing for policymakers and NGOs the significance of shaping what is reported when.

2 Conceptual framework

Building on studies emphasizing the importance of voters' *access* to news, I argue that the variable impact of performance indicators reported in the news on electoral accountability is shaped by the timing of voters' *consumption* of politically-relevant news.

2.1 News consumption cycles

Political news content is a consumption good for some citizens (Baum 2002; Hamilton 2004), often instilled through early life socialization (Prior 2019), and can inform vocational and personal investment decisions (Larcinese 2005). However, most voters lack the interest or instrumental motivation to incur the costs of acquiring news. Exposure to politically-relevant information is often an externality arising from other activities because individuals seeking to influence election or policy outcomes face incentives to remain "rationally ignorant" in large electorates (Downs 1957). Ultimately, many voters in developed and developing contexts consume news content infrequently and remain poorly informed about both political institutions and current affairs (e.g. Delli Carpini and Keeter 1996; Keefer 2007; Pande 2011).

While baseline news consumption is often low and political interest is an enduring disposition (Prior 2019), voter attention to politically-relevant news may increase significantly before elections. This could reflect several demand-side factors, which do not rely on voters believing that their own ballot will affect electoral outcomes. First, to the extent that politically-relevant news is a consumption good, electoral campaigns are likely to particularly engage voters. Whereas events outside election campaigns often relate to policy issues, campaigns appeal to a broader audience (e.g. Farnsworth and Lichter 2011) and thereby increase attention to news programming more generally. Second, Feddersen and Sandroni (2006) argue that "ethical" independent voters are intrinsically motivated to cast an informed ballot. Voters motivated to acquire information out of civic duty are likely to perceive a greater duty to do so before elections. Third, in the same way that voters experience social pressure to vote (e.g. DellaVigna et al. 2016; Gerber, Green and Larimer 2008), voters in social networks that collectively value political knowledge can acquire information about election campaigns to cultivate a reputation as at least somewhat politically sophisticated (Marshall 2019). These pressures may be greatest around elections when political discussion is more common (Baker, Ames and Renno 2006; Marshall 2019).

Such a news consumption cycle could cause voters—especially those for whom baseline political knowledge is low—to effectively engage with the news for the first time in an electoral cycle right before the election. More politically engaged voters are, in turn, likely to ratchet up their exposure to political news before elections.

2.2 Consequences for electoral accountability

Where voters can access news and use the incumbent performance indicators that they encounter to select or motivate politicians, news consumption cycles may affect vote choices. I extend canonical models of electoral accountability to incorporate news consumption cycles in the context of municipal homicide rates. The model in Appendix Section A.1 proves the claims summarized below.

By consuming news over a given period, voters observe the homicide count in their municipality by aggregating local media reports of homicides. However, while the homicide count may reflect unobservable incumbent party competence and effort in controlling crime, it also reflects unobservable idiosyncratic shocks beyond the incumbent's control. To capture this, let the homicide count in period $j = 1, ..., J$ of mayoral term t in municipality m be given by:

$$H_{m,t,j} = \eta_m + \mu_{t,j} - \theta_{m,t} - a_{m,t} + e_{m,t,j}, \tag{1}$$

where $\eta_m > 0$ is the baseline number of homicides in municipality m, $\mu_{t,j}$ captures common shocks in period j of term t, $\theta_{m,t}$ is the competence of m's incumbent party in term t, $a_{m,t}$ captures costly policy effort undertaken by the incumbent during their term, and $e_{m,t,j}$ is a period-specific iid shock drawn from a distribution with mean zero. Intuitively, $H_{m,t,j}$ is greater for less competent incumbent parties and incumbent parties that expend less effort to control crime.[5] The average homicide count observed by a voter who follows the news for the last N periods of an incumbent's term is $\overline{H}_{m,t}(N) := \frac{1}{N}\sum_{j=1+J-N}^{J} H_{m,t,j}$.

In seeking to select competent incumbent parties or incentivize effort by incumbents, voters re-elect the incumbent party if $\overline{H}_{m,t}(N)$ is sufficiently low. Such a threshold rule emerges in several standard models where voters use performance signals to infer incumbent types or actions. In career concerns models (e.g. Ashworth 2005), voters and politicians know only the distribution of competence across parties. Accordingly, the incumbent party's effort does not vary with competence and can be anticipated by voters. Voters then update about the party's competence after filtering out baseline homicide counts, common shocks, and anticipated effort, before attempting to retain competent incumbent parties by only re-electing parties that oversee sufficiently few homicides.[6] In moral hazard models where the performance signals that voters observe reflect incumbent effort and idiosyncratic shocks (e.g. Ferejohn 1986), voters similarly commit to a re-election rule requiring a sufficiently low homicide count. The threshold is chosen by voters to extract maximal effort, subject to incentivizing incumbent parties to seek to achieve re-election.

The consequence that incumbent parties overseeing higher homicide rates are less likely to be re-elected reflects two sources of variation in $\overline{H}_{m,t}(N)$. First, as desired by voters, the sanctioning of higher homicide rates in part captures the removal of less competent incumbent parties. Second, because homicide counts also reflect factors beyond the incumbent's control, even Bayesian voters will in part sanction incumbent parties on the basis of idiosyncratic increases in homicide counts. This is because voters cannot distinguish poor performance reflecting low incumbent type or effort from adverse shocks beyond their control.

The number of periods in which voters consume news (N) determines the extent to which voters respond to homicides induced by idiosyncratic oscillations beyond the incumbent party's control. As N becomes large, voters can filter out the idiosyncratic shocks to extract the component of the signal driven by competence or effort. The vote choices of informed voters who consume news throughout the electoral cycle are thus expected to be driven by longer-term homicides rates—and their deviation from the baseline rate—which average out idiosyncratic shocks, rather than short-term oscillations beyond the incumbent party's control.

[5] This framework assumes that the incumbent party's term-level effort cannot be recalibrated respond to j-level variation in homicide counts. Besley and Burgess (2002) consider longer periods over which incumbents can respond to mid-term performance indicators.

[6] Pure selection and signaling models where voters cannot perfectly separate types yield similar results.

In contrast, cyclical news consumption can lead voters to predominantly receive performance signals in the periods preceding an election. When N is low, a voter must tolerate error in using homicide counts to infer incumbent party competence or effort. Although Bayesian voters will accordingly place less weight on a smaller number of signals in casting their ballot, they nevertheless sanction incumbent parties partly on the basis of homicides beyond their control because the observed homicide count remains informative. A few noisy signals before elections can thus drive the vote choices of voters with limited knowledge of public affairs.

The following empirical analyses test the central implications of the news consumption cycles argument: (i) news consumption increases before elections; (ii) homicides occurring before elections heavily disproportionately voting behavior; and (iii) such voting behavior is driven by access to media outlets likely to report local homicides. I test these hypotheses in Mexican municipalities, where many voters are poorly informed about public affairs and local homicide rates are a salient performance indicator frequently reported by local media. While some voters also encounter homicides more directly, especially in the small number of municipalities that have experienced sustained and substantial rates of violence, my focus is on the aggregated information that citizens receive through local media.

3 Empirical context

Despite holding regular elections, Mexico was effectively a one-party state until the late 1990s (Magaloni 2006). Mexico has since democratized significantly, with competitive elections between three main political parties—the right-wing National Action Party (PAN), left-wing Party of the Democratic Revolution (PRD), and previously-hegemonic Institutional Revolutionary Party (PRI)—until the emergence of MORENA in 2015. The PRI reclaimed the presidency from the PAN in 2012, after the PRI first lost the presidency in 2000.

Mexico's federal system is divided between three elected administrative layers of government: the federal government, 31 states and Mexico City, and almost 2,500 municipalities. Constitutional reforms in the mid-1990s substantially increased municipal autonomy over the provision of local public services. Municipal spending has since risen to almost 10% of total government expenditures, and is allocated by municipal governments led by a mayor with an in-built copartisan majority on their municipal council.

Municipal mayors were, until recently, elected to (typically) three years non-renewable terms, entering office 2-6 months after election day. Elections are almost always held concurrently with state legislative elections, while less-frequent gubernatorial elections are often held separately. I refer to simultaneous municipal and state legislative elections as "local elections." Municipal elections have become increasingly competitive; fewer than 50% of incumbent parties were re-elected and 31% of elections were won by less than 5 percentage points between 1999 and 2012. Before 2015, the majority of local elections did not coincide with federal elections.[7]

Although incumbent mayors could not seek re-election until 2018, voters sought to hold incumbent parties accountable for the performance of municipal governments in Mexico's party-centric political system. First, voters primarily decide between party labels, rather than individual candidates. While 80% of voters can identify the party of their municipal incumbent (Arias et al. 2022), the probability of correctly naming individual local incumbents is far lower (Larreguy, Marshall and Snyder 2018). Second, differences in candidate selection mechanisms across parties (Langston 2003) and the growing nationalization of Mexican politics (Johnson and Cantú 2020) lead candidate types to be highly correlated within parties. Third, various studies show that voter support for particular candidates respond to information about their party's malfeasance in office (Arias et al. 2022; Chong et al. 2015; Larreguy, Marshall and Snyder 2020).

3.1 Public security institutions

Responsibility for public security is shared across layers of government, and municipal governments play an important role in fighting crime. Both state and federal laws can be used to prosecute criminals, and preventive and investigative police forces exist at the federal level, in each state, and in Mexico City. State police generally investigate crimes such as homicides, while federal officers—and increasingly the army—have focused on organized crime.

Although federal and state police investigate major crimes, municipal police account for more than half of Mexican law enforcement personnel (Sabet 2010). Municipal forces can support higher-level operations and supply valuable information, respond first to criminal incidents, and patrol the streets and maintain public order (Sabet 2010). Approximately three-quarters of municipalities possess their own police force, which enables the governments of these municipalities to influence the medium-term incidence of crime by choosing the local police chief, allocating funds for hiring and training officers, and setting policies including levels of cooperation with other police forces and, potentially, organized crime. Indeed, Dell (2015) and Durante and Gutierrez (2015) show that policy differences across types of mayor affect homicide rates. In the 2010s, and especially after President Calderón left office in 2012, *mando único* reforms increasingly centralized control of local police forces in the hands of state instead of municipal governments.

3.2 Trends in homicide rates

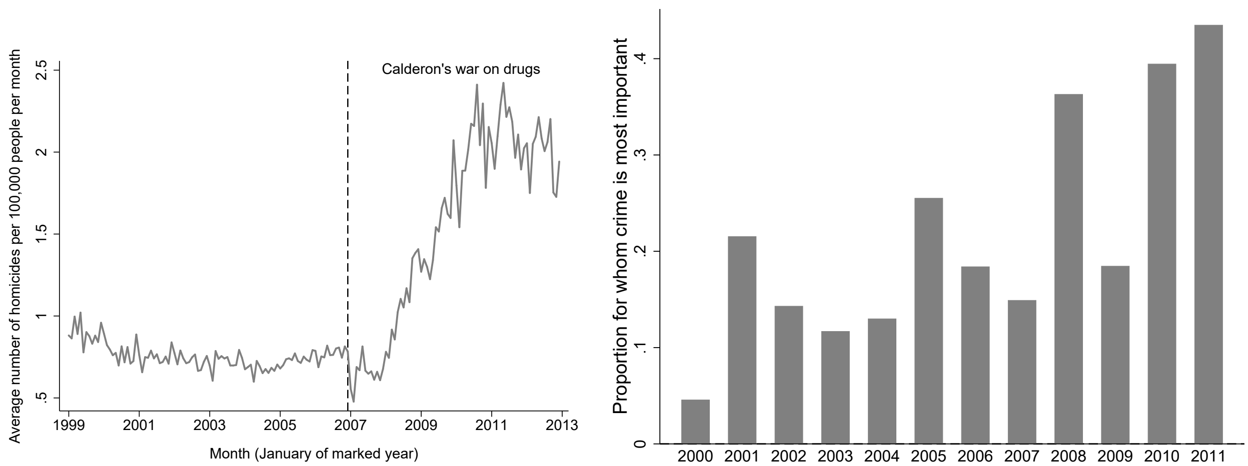
According to the United Nations Office on Drugs and Crime, Mexico has consistently suffered one of the world's highest homicide rates. In 2011, 22.6 people per 100,000 were intentionally

[7] House and Senate elections are held every three years, while the President is elected every six years.

Figure 1: Crime in Mexico

(a) Monthly homicides in the average Mexican municipality, 1999-2012

(b) Voters listing crime as the most important problem that Mexico faces, 2000-2011



Notes: The data in Figure 1a are from INEGI, and monthly averages are weighted by the 2010 municipal population. The data in Figure 1b are from the 2000-2011 Latinobarómetro surveys (no survey was conducted in 1999 or 2012); crime includes concerns about crime/public security and drug trafficking.

murdered. This peak represented the 20th highest homicide rate in the world—slightly less than South Africa, Colombia, and Brazil, and slightly more than Nigeria, Botswana, and Panama. After briefly declining since 2011, the 2017-2021 homicide rates were the highest on record.

Mexico's National Institute of Statistics, Geography, and Information (INEGI) defines intentional homicides as unnatural deaths on the basis of coroner reports.[8] Figure 1a shows that the monthly homicide rate in the average municipality between 1999 and 2012 was substantial, but increased dramatically after President Calderón entered office in December 2006 and began Mexico's "War on Drugs." The homicide clearance rate is only around 20%, and drug-related homicides—which are regionally concentrated—represent around half of the homicides over this period (Heinle, Rodríguez Ferreira and Shirk 2014). Although homicide rates have periodically reached warlike levels in municipalities experiencing conflicts between rival DTOs or between DTOs and the state, homicides remain rare experiences that few citizens encounter directly in most municipalities.

Voters are unsurprisingly concerned about high rates of violent crime. Like many other Central and South American countries, Figure 1b 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 much of the 2000s, public security registered as the most salient issue for voters, alongside the economy.

[8]The federal government's drug-related homicide data suffers from various problems beyond its short time-series (Heinle, Rodríguez Ferreira and Shirk 2014), while voter fears about public security also reflect equally-prevalent non-drug related homicides.

3.3 Media coverage and voter political knowledge

Broadcast media was, until recently, Mexico's primary source of news. According to the 2009 Latinobarometer, 83% of Mexicans received political news from television, 41% from radio, 30% from newspapers, and 41% from family, friends, and colleagues. In 2008, an Ibope/AGB México survey found that around 10% listened to the radio every day, while the average person watched four hours of television a day. In contrast, only 21% of households had internet at home in 2010; 3G coverage expanded substantially after 2011.

In 2009, Mexico contained 852 AM radio stations, 1,097 FM stations, and 1,255 television stations, which generally cover relatively small geographic areas. Most stations are 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 owned-and-operated subsidiaries. Among radio stations, 83% report news more than once a day, and typically report on municipal rather than national issues (Larreguy, Lucas and Marshall 2020). For television stations, identical entertainment content is generally bought from or relayed by network providers. However, while national news is typically provided by television networks, affiliates and regional subdivisions emitting from larger 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 broadcast 3.6 hours of news coverage each weekday (both before and after the 2015 election campaign). Almost half of this news content was devoted to state- or city-specific programming.

Homicides are not always reported in depth, but are "omnipresent" in the local broadcast media (Trelles and Carreras 2012). The 2010 National Insecurity Survey also reports that 87% of respondents learned about public security in the country and in their state from television news programs, while 34%, 29%, and 9% respectively learned from radio news programs, periodicals or newspapers, and the internet. Comparatively few learned from work colleagues (5%) or family, friends, and neighbors (13%).

Despite widespread access to politically-relevant news, voters' knowledge of public affairs remains limited. I show below that only half of Mexicans correctly answer basic questions about politics and recent news. Chong et al. (2015) similarly show that voters were generally unaware of mayoral responsibilities and performance in office in 2009. At least in the municipalities where crime is relatively rare and not experienced directly by most citizens, voter appraisals of their municipal government may rely heavily on the news reports that they consume.

4 The existence of news consumption cycles

The core of the news consumption cycles argument rests on voters consuming more politically-relevant news just before elections. This section leverages individual-level survey data to first establish the existence of such news consumption cycles around local elections in Mexico.

4.1 Data

I use four cross-sectional waves of the National Survey of Political Culture and Civil Practices (ENCUP) conducted in November 2001, February 2003, December 2005, and August 2012.[9] The surveys were commissioned by the Mexican Interior Ministry and designed to be nationally representative. Each round conducted face-to-face interviews with stratified probability samples of around 4,500 adults drawn from pre-selected electoral precincts within urban and rural strata in each state. The pooled sample includes up to 15,976 respondents across 523 municipalities.[10]

News consumption is measured in two ways. First, a standard survey question registers the frequency—never, at some point, monthly, weekly, and daily—with which respondents reported watching or listening to the news or programs about politics or public affairs.[11] To understand the margins at which news consumption changes, I code indicators for ever consuming political news and at least weekly consumption, and a 5-point scale (from never to daily). Second, I assess whether self-reported news consumption translates into topical political knowledge, of the form that voters are 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 events and the party of a respondent's state governor.[12] The average respondent answered around half the questions correctly.

4.2 Identification strategy

To estimate the effects of upcoming local elections on politically-relevant news consumption, I leverage the timing of survey waves with respect to state-specific electoral calendars. The ENCUP surveys vary in terms of the number of years between survey rounds (and thus do not track federal elections) and the month within the year when each survey was conducted, while Mexican states

[9] The study was implemented by INEGI in 2001 and 2003, and private firms in 2005 and 2012. A 2008 wave was also conducted, but did not report a respondent's municipality and asked different media consumption questions.

[10] Respondents from Mexico City, where local governance operates differently, are excluded from all analyses to maintain comparability with the electoral results below.

[11] I focus on radio and television, which were by far the most prevalent sources of political information in Mexico. Comparable questions from 2001 were not asked.

[12] Questions are listed in Appendix Section A.2.2. Questions regarding basic knowledge of political institutions were excluded because they are unlikely to be covered directly in the news.

have historically followed distinct election calendars that vary in the month and year of their elections (see Appendix Section A.2.1). Consequently, respondents in different states experienced local election seasons in different survey waves; of Mexico's 31 states, 29 registered a local election in at least one of the survey years, of which 12 held an election in the 6 months after surveys were conducted. Since the dates and locations of the nationwide and non-politically sensitive ENCUP surveys were not selected to coincide (or not) with particular local elections, whether a survey in a given year was administered just before a state's local elections is plausibly exogenous.[13]

Official campaign periods generally last around three months, with party candidate selection concluded one or two months earlier. Consistent with this, political advertising slots have been allocated four months before elections since 2006. Moreover, Appendix Figures A2a and A2b show that Mexican Google searches for "elecciones" (elections) and "alcalde" (mayor), between 2004 and 2012, are significantly greater in the four months preceding local elections. To capture this pre-election period, I code an indicator for respondents facing an upcoming municipal election within four months of the survey. Robustness checks below show that the results do not depend on this particular cutoff.

Leveraging such state $\times$ survey level variation, I estimate the effect of an upcoming local election using the following OLS regression:

$$Y_{i,s,t} = \beta \, Upcoming\ local\ election_{s,t} + \mu_t + \varepsilon_{i,s,t}, \tag{2}$$

where $Y_{i,s,t}$ is a measure of news consumption for respondent i in state s in survey wave t. Survey wave fixed effects, μ_t, adjust for differences in the political knowledge questions across surveys and common period effects that could arise from concurrent federal elections (in 2003 and 2012), presidential elections (in 2012), or national trends in political behavior. Standard errors are clustered by state, and computed using a block bootstrap based on 10,000 resamples.

Supporting the identifying assumption that upcoming local elections are exogenous to other determinants of a respondent's news consumption, I find no evidence to suggest that upcoming local elections are systematically correlated with the types of people who responded to the surveys or the types of location surveyed before local elections. First, Appendix Table A2 shows that upcoming local elections are well-balanced over observable individual, municipal, and state level characteristics; only 4 of 36 tests report a significant difference below the 10% level. Moreover, the results are robust to adjusting for covariate imbalances and including state fixed effects. Second, conditional on a municipality's inclusion in at least one survey wave, Appendix Table A8 shows that neither upcoming local elections nor recent homicides predict a municipality's inclusion in any given ENCUP survey round.

[13] Eifert, Miguel and Posner (2010) use a similar design leveraging the timing of surveys vis-à-vis national elections.

4.3 Upcoming elections increase news consumption and political knowledge

I first document the existence of news consumption cycles graphically. Figure 2 plots the difference in four measures of news consumption between four-month periods before and after local elections relative to all months more than a year from a local election. For outcomes at the extensive and intensive margin, panels (a)-(c) show that self-reported news consumption was significantly greater in the four months preceding a local election. Panel (d) further indicates that increases in news consumption translate into greater knowledge of topical public affairs. Increased news consumption is not detectable earlier in the electoral cycle for any of these outcomes, and quickly reverts to baseline levels after local elections.

Comparing the four months before the election with all other periods, the regression results in Table 1 confirm that local elections significantly increased the likelihood that respondents reported consuming more politically-relevant news at all levels of news consumption. Column (1) of panel A shows that an upcoming local election increased the probability that a voter ever listened to or watched news by 8 percentage points. Column (3) further reports a 12 percentage point increase in weekly news and political programming consumption. Column (5) shows a 0.38 standard deviation increase on the five-point news consumption scale.

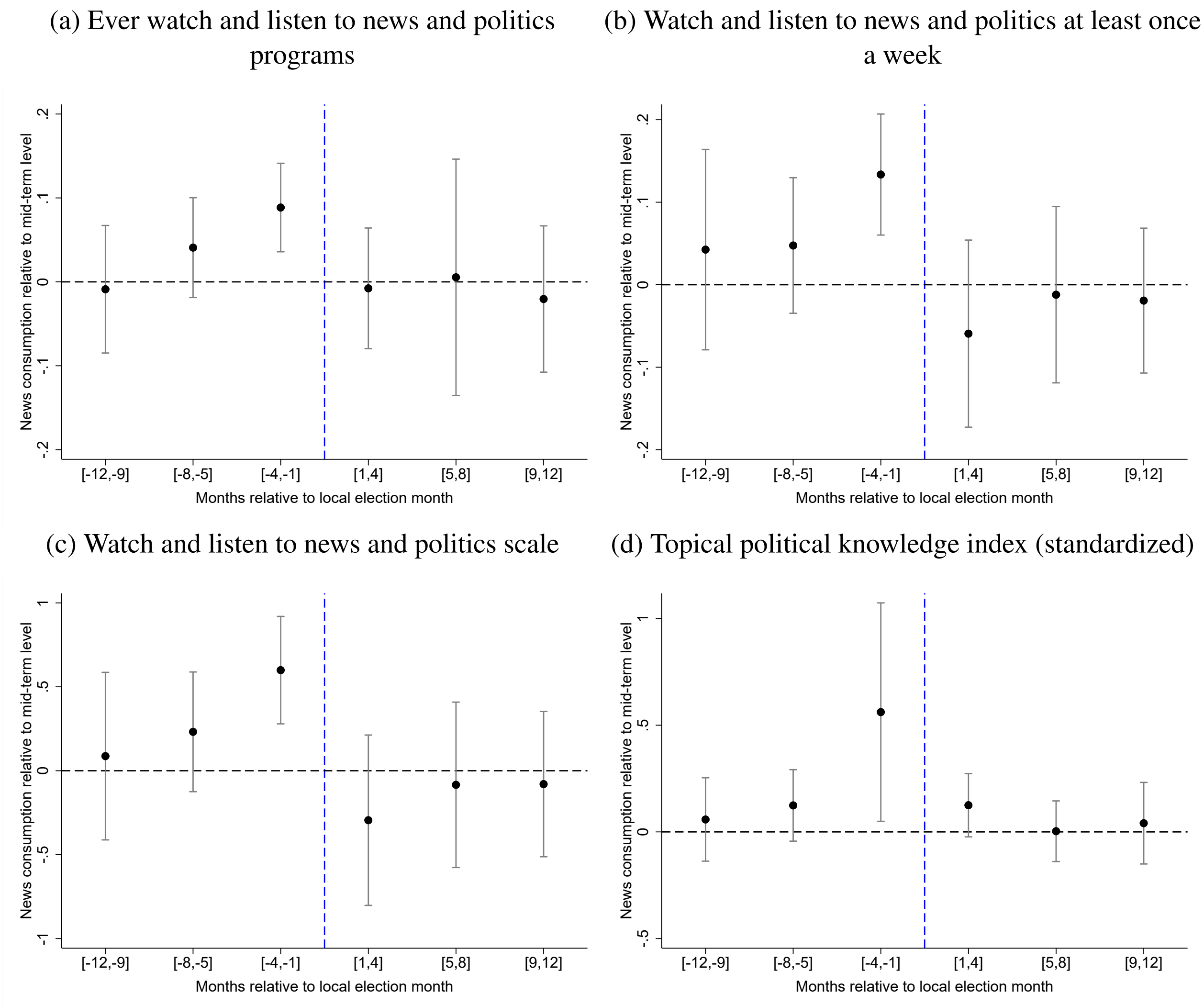
Contrary to the potential concern that social desirability bias could explain the rise in self-reported outcomes, topical knowledge of public affairs mirrors the cycle of increased news consumption prior to local elections. Column (7) of panel A demonstrates that voters facing an upcoming local election became around 0.5 standard deviations—or 16 percentage points—more likely to correctly answer questions testing their political knowledge. This suggests that voters internalize politically-relevant news available before local elections, which is likely to be a prerequisite for news to influence efforts to hold governments accountable.

4.4 Robustness checks

The existence of news consumption cycles is robust at the extensive and intensive margins. First, the even columns in Table 1 include the following covariates to adjust for the few covariate imbalances: separate indicators for all five educational categories, the municipal average number of years of schooling, whether the municipal incumbent won the last election, and whether the state had a PAN governor. Although the sample size declines because education data was not collected in the 2005 survey wave, the point estimates are qualitatively similar.

Second, I conduct a generalized difference-in-differences analysis, which leverages within-state variation in election timing—detailed in Appendix Table A1—by adding state fixed effects that absorb time-invariant differences between states. Identification now relies on respondents in states facing upcoming elections at the time of the survey following parallel trends in political news

Figure 2: News consumption and topical political knowledge by four-month intervals relative to local elections (95% confidence intervals)



Notes: All estimates are from equations analogous to equation (2), but instead include indicators for the three four-month periods before and after a state's local election month. No surveys were conducted in the month that an election was held. The baseline category is the period more than one year away from a local election.

consumption to respondents in states without upcoming elections. This weaker assumption is plausible because states have followed relatively fixed electoral cycles for decades and surveys were not timed to avoid local elections. While standard errors increase when focusing on temporal variation within states, panel B of Table 1 continues to report robust and statistically significant increases in news consumption before local elections.

Third, the results do not depend upon the particular definition of the upcoming election indicator. Panel C of Table 1 instead defines an upcoming local election as a survey conducted within two months of election day. While only 2% of respondents (from three states) ever experience such

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

	Outcomes:							
	Ever watch and listen to news and political programs		**Watch and listen to news and political programs at least once a week**		**Watch and listen to news and political programs scale (never, ever, monthly, weekly, daily)**		**Topical political knowledge index (standardized)**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline specification								
Upcoming local election (4 months)	0.077***	0.072***	0.125***	0.080***	0.558***	0.357***	0.510**	0.225
	(0.016)	(0.018)	(0.022)	(0.024)	(0.102)	(0.107)	(0.255)	(0.183)
Upcoming local election mean	0.07	0.03	0.07	0.03	0.07	0.03	0.06	0.03
Panel B: Difference-in-differences specification								
Upcoming local election (4 months)	0.030*	0.065***	0.056	0.081**	0.315**	0.414***	0.323***	0.217**
	(0.017)	(0.021)	(0.036)	(0.033)	(0.141)	(0.137)	(0.159)	(0.108)
Upcoming local election mean	0.07	0.03	0.07	0.03	0.07	0.03	0.06	0.03
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Panel C: Upcoming local election defined by the two months before local elections								
Upcoming local election (2 months)	0.096***	0.072***	0.124***	0.080***	0.553***	0.357***	0.329*	0.225
	(0.017)	(0.018)	(0.026)	(0.025)	(0.114)	(0.110)	(0.189)	(0.181)
Upcoming local election mean	0.02	0.03	0.02	0.03	0.02	0.03	0.02	0.03
Observations	11,983	7,604	11,983	7,604	11,983	7,604	15,976	11,417
Unique states	31	31	31	31	31	31	31	31
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1,2,3,4}	{0,1,2,3,4}	[-2.11,1.56]	[-2.11,1.56]
Outcome mean	0.86	0.86	0.62	0.62	2.55	2.56	0.00	0.01
Outcome standard deviation	0.34	0.34	0.48	0.49	1.48	1.50	1.00	0.92
Predetermined covariates		✓		✓		✓		✓

Notes: All specifications are estimated using OLS, and include survey wave fixed effects. The outcomes in columns (1)-(6) were not collected in the 2001 survey. The covariates included in even columns are: indicators for primary, lower secondary, upper secondary, and higher educational attainment (not collected in 2005), the municipality's average number of years of schooling, whether the municipal incumbent won the prior election, and whether the state has a PAN governor. Block-bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

a proximate election, the similar point estimates reiterate that voters consume more politically-relevant news before elections. Appendix Table A11 further demonstrates robustness to defining upcoming elections by any number of months between 1 and 12 before a local election.

Finally, Appendix Table A12 shows that the results do not depend on a particular survey wave. This suggests that other events, such as festivals or local scandals, which might have coincided with local elections in particular survey enumeration periods are not driving the results.

5 Local homicides and electoral accountability

Having shown that many voters consume more news just before elections, I next examine whether newsworthy signals of incumbent quality revealed at this time disproportionately influence vote choices. I first leverage idiosyncratic spikes in the occurrence of local homicides that coincide with the months preceding municipal elections to identify the effect of pre-election homicide shocks.

I then compare this with the electoral effect of longer-term homicide rates—a signal that is only observed by voters who regularly consume news throughout the electoral cycle, but is more informative about incumbent competence or effort.

5.1 Data

I use two main data sources to estimate the electoral effects of homicides occurring just before elections. First, electoral returns for municipal elections covering Mexico's c.67,000 electoral precincts come from state electoral institutes. I focus on municipal elections between 1999 and 2012; I thus include a similar period to the ENCUP survey and end the sample before *mando único* reforms integrated many municipal police forces under state control.[14] Incumbent mayors were largely from the PRI (54%), PAN (25%), and PRD (19%).[15] The average turnout rate was 53%. Municipalities without their own police forces, including the *delegaciones* (which have since become municipalities) within Mexico City that had a centralized police force, are excluded from the sample because voters are unlikely to perceive such municipal governments as capable of controlling local violent crime.[16]

Second, I combine the electoral data with the INEGI's individual-level homicide data (described in Section 3.2). While the national homicide rate has changed relatively smoothly over time, this masks considerable heterogeneity within and across municipalities. In larger municipalities, homicide counts have oscillated dramatically over short periods, especially at the height of President Calderón's War on Drugs in 2010. Conversely, the median municipality experienced only one homicide a year between 1999 and 2012. Monthly homicide rates per 100,000 people are computed using the 2010 municipal population throughout.

5.2 Identification strategy

Since homicide rates reflect multiple factors, voters who consume few performance signals are expected to sanction incumbent parties for overseeing high homicide rates when attempting to retain competent or hard-working incumbent parties. To isolate idiosyncratic shocks which occur when voters consume most politically-relevant news from incumbent party competence, incumbent effort, and broader homicide rate trends, I exploit plausibly exogenous short-term oscillations in the homicide count that coincide with local election campaigns.

[14] Before 1999, fine-grained municipal election data is becomes increasingly irregular across states.

[15] For the 26% of cases where the incumbent party won as part of a coalition formed by several parties, the incumbent is defined as the party with the largest vote share at the next election if the coalition changes. Appendix Table A16 shows that the results are robust to restricting attention to incumbents representing the three large parties or a single party. After 2012, large and changing state- and municipal-level coalitions have become increasingly common.

[16] I exclude municipalities without a police force or that rely on state, federal, community, private, or other security forces. Appendix Table A20 reports no electoral effect of homicides in municipalities without police forces.

The identification strategy rests on the logic that, within a given municipality, homicides are relatively rare and unpredictable events that are equally likely to occur before and after elections. Between 1999 and 2012, the monthly homicide change around elections within municipalities was positive in 12.4% of cases and negative in 12.4% of cases. I then compare municipalities experiencing a short-term spike in the number of homicides just before a municipal election, relative to just after that election, to municipalities experiencing at least as many homicides just after the election as before. To ensure a comparison of similar municipalities, I restrict attention to municipalities that experienced at least one homicide around the election. Adopting a similar approach to Ferraz and Finan (2008), leveraging a comparison with homicide spikes that occur *after* the election ensures that post-election homicides cannot influence voting behavior.

Formally, municipality m experiences a pre-election homicide shock if, conditional on at least one homicide occurring in the 60 days either side of election day, m experiences more homicides in the 60 days before election t (including election day) than the 60 days after election day:

$$Homicide\ shock_{m,t} := \begin{cases} 1 & \text{if } \sum_{d=-59}^{0} Homicides_{m,t,d} > \sum_{d=1}^{60} Homicides_{m,t,d} \text{ and } \sum_{d=-59}^{60} Homicides_{m,t,d} > 0, \\ 0 & \text{if } \sum_{d=-59}^{0} Homicides_{m,t,d} \leq \sum_{d=1}^{60} Homicides_{m,t,d} \text{ and } \sum_{d=-59}^{60} Homicides_{m,t,d} > 0, \\ . & \text{if } \sum_{d=-59}^{60} Homicides_{m,t,d} = 0, \end{cases} \tag{3}$$

where d indexes days before ($d \leq 0$) and after ($d > 0$) municipality m's election day in year t. This 120-day window captures the final two months of the campaign and the post-election period before the winner enters office. Figure 3a shows that 75% of 60-day intervals, both before and after election day, experienced 0, 1, or 2 homicides. The 60-day interval was chosen to cover a sufficiently short span that changes in the homicide count are plausibly exogenous and trendless, while also capturing the period of greatest news consumption. A further advantage is that voter registration and candidate selection is completed before this period and the effects of any policy responses to crime are unlikely to have materialized. The results are robust to alternative specifications using pre-election comparison periods as well as 30, 90, and 120 day windows either side of election day.

The mean shocked municipality experienced 1.7 more homicides per 100,000 people—or 7.4 more homicides—over the 60 days preceding election day than non-shocked municipalities. Such shocks doubled the average municipality's typical homicide rate, but rarely constitute major turf wars. Appendix Figure A1b maps the 847 municipalities where coroner reports registered at least one homicide in the 60 days before and after municipal elections; these comparatively large municipalities comprise 69% of Mexico's population and include 64% of the municipalities sampled in the ENCUP surveys. Of these, 520 experienced at least one election with and one election without a homicide shock between 1999 and 2012.

I estimate the effects of homicide shocks that occurred before local elections using OLS regressions of the form:

$$Y_{m,t} = \beta Homicide\ shock_{m,t} + \eta_m + \mu_t + \varepsilon_{m,t}, \tag{4}$$

where $Y_{m,t}$ is either a municipal-level indicator for the incumbent party winning the election or the incumbent party's precinct-level vote share. Although they are not necessary for identification, municipality and election month-year fixed effects—η_m and μ_t, respectively—are included to increase precision and ensure that the estimates do not capture time-invariant municipality characteristics or common election period or seasonal shocks. All observations are weighted by the number of registered voters to capture the average voter's voting behavior within the sample.[17] Since homicide shocks are assigned at the election-level, standard errors are clustered at the municipality $\times$ election level.

5.3 Evidence supporting the identification strategy

The identifying assumption is that the *timing* of homicides over the 60 days either side of election day occurs exogenously. Since state and non-state actors could seek to influence the homicide rate differently before and after elections, I conduct an extensive battery of tests to show that my design supports this assumption.

5.3.1 General exogeneity tests

If homicides are equally likely to fall before and after election day, two testable implications follow: (i) the distribution of homicides across municipalities should be similar in the 60 days before and after election day; and (ii) shocked municipalities should be similar to unshocked municipalities. This subsection tests these implications of the identifying assumption.

First, Figure 3a reports the distribution of homicide counts over the 60-day intervals before and after election day. If homicide counts were manipulated, we might expect to observe greater density around 0 or in the right tail of the distribution before elections. However, a Kolmogorov-Smirnov test fails to reject the null hypothesis of equality of distribution ($p = 0.65$). This is consistent with exogenous sampling variability in homicide counts around local elections.

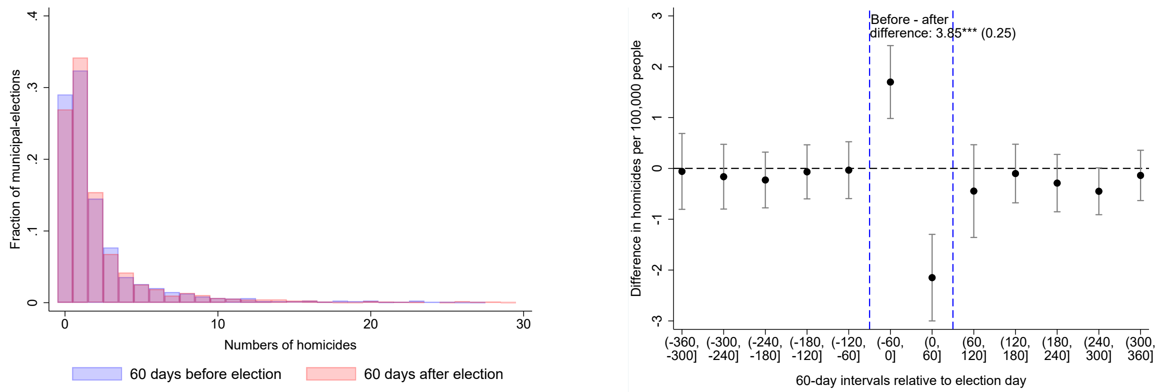
Second, Figure 3b shows that the difference in monthly homicide rates between shocked and non-shocked municipalities is negligible and trendless over the ten months preceding and following the period used to define pre-election homicide shocks. Moreover, the balance tests in Appendix Table A4 confirm that the average homicide rate over the one or three years preceding the period

[17] Appendix Table A15 reports similar results using population weights. Unweighted data would overweight voters in small municipalities.

Figure 3: Distribution of homicides over time and by homicide shock

(a) Distribution of homicides in the 60 days before and after election day

(b) Difference in homicide rate by pre-election homicide shock (95% confidence intervals)



Notes: The 1% of 60-day intervals where homicide counts exceeded 30 are not shown in Figure 3a. Each bar in Figure 3b denotes the difference, based on estimating equation (4), in the 60-day homicide rate per 100,000 people (based on the 2010 population) between shocked and non-shocked municipalities by 60-day intervals before, during, and after the 60-day interval defining homicide shocks around election day. The before-after difference is also estimated with equation (4), using the before-after difference as the outcome.

defining the treatment does not significantly vary with homicide shocks. Further balance tests show that in 2011 and 2012, when disaggregated crime reports coincide with my sample, homicide shocks are uncorrelated with pre-election spikes in other types of crime.

Third, pre-election homicide shocks are more generally uncorrelated with a wide range of pre-determined covariates. Appendix Table A4 reports balance tests over 104 time-varying and time-invariant municipal covariates.[18] Consistent with pre-election shocks isolating exogenous short-term variation in homicide rates, only 6 differences are statistically significant below the 10% level. I further show that all results are robust to adjusting for the few significant imbalances.

5.3.2 Evidence that homicide shocks do not reflect strategic manipulation

I next address several more specific concerns regarding strategic behavior by non-state or state actors that could seek to influence homicide rates around local elections. Manipulation efforts could bias estimates to the extent that they correlate with the incumbent's electoral support.

The first potential source of bias is that DTOs could selectively employ or withhold violence to help elect preferred candidates. However, I find no evidence to suggest that oscillations in homicide counts around elections systematically reflect DTO-related violence or infiltration in a municipality. Appendix Table A4 shows that homicide shocks are balanced across: drug-related homicide rates during the 2006(Dec.)-2011 period, when the Mexican National Security System (SNSP) made

[18] Appendix Table A5 reports analogous tests for precinct-level outcomes.

monthly data available; three measures of DTO presence in Mexico; and Trejo and Ley's (2021) count of DTO attacks on government officials, candidates, and party activists. At the precinct-level, Table A5 further shows that homicide shocks are no more likely to occur in the municipalities containing the 5% of precincts designated by the Federal Election Institute (IFE)—which has since become the National Election Institute—as high-risk, typically due to DTO activity.

Nevertheless, the *types* of homicide could change without affecting overall levels. Gangland killings are typically concentrated among young and uneducated men (e.g. Dell 2015), and are often committed using firearms or more gruesome methods—particularly if they are intended to send a message (e.g. Durán-Martínez 2015). Using the International Classification of Diseases codes in the INEGI's coroner reports, Appendix Table A4 examines the causes of death and victim characteristics associated with the homicides that occurred in the 60 days preceding an election. Notably, shocked and unshocked municipalities are balanced across the share of pre-election homicides caused by a firearm, hanging, drowning, explosives, or cutting objects. Moreover, pre-election homicides in shocked municipalities did not disproportionately afflict young, male, unmarried, or uneducated individuals. This again suggests that homicide shocks did not reflect DTO efforts to sway election outcomes.

A second potential concern is that more capable municipal incumbent parties signal their competence to voters by reducing levels of crime before elections. However, proxies for municipal government *capacity* to reduce crime before elections—including population size and budget, police officers per voter, partisan alignment with the president, and homicide shocks in neighboring municipalities—are not significantly correlated with homicide shocks. Moreover, measures of local political dominance that could proxy for clientelistic ties or depth of DTO infiltration into local politics—such as the incumbent party's prior victory margin or number of consecutive election wins—are not significantly correlated with homicide shocks.

In addition to homicide shocks being uncorrelated with indicators of government capacity to alter homicide rates before elections, I find no evidence of differential *effort* to reduce violent crime. One means through which municipal governments could affect homicide rates is by aiding state and federal interventions. However, Appendix Table A4 shows that homicide shocks are not significantly correlated with Osorio's (2015) measures of violent enforcement against DTOs, drug-related arrests, or asset, drug, and gun seizures in the two months preceding the election. Combined with the lack of contemporaneous spikes in other criminal activities, like robberies, that greater police presence could more easily reduce, homicide shocks do not appear to reflect changes in municipal policies before elections.

Another means by which strategic politicians could alter voter perceptions of homicides before elections is by encouraging police or coroners to delay or prevent homicides from being reported until after election day. Contrary to this concern, Appendix Table A4 also shows that a mismatch between the month in which a coroner determined a homicide to have occurred and the month in which it was registered is equally rare across shocked and non-shocked elections. Further suggesting that coroners did not reclassify homicides as non-homicide deaths or manipulate the date a homicide occurred, Figure A3 shows that the weekly number of homicides registered to have occurred is similar over the 9 weeks before and after election day.

Finally, although new mayors rarely entered office within 60 days of an election, defining homicide shocks using post-election homicides could introduce bias if election outcomes themselves affect post-election homicides rates. However, Appendix Table A9 shows that election outcomes do not predict homicides rates in the 60 days after election day, while Dell (2015) finds no effect of party transitions on homicide patterns during "lame duck" periods. Nevertheless, to ensure that post-election homicides do not drive the results, Table 3 and Figure A4 report similar results when comparing the 60 days before election day with (i) other periods *before* the campaign when voter news consumption was comparatively low or (ii) all periods around the election simultaneously.

5.4 Pre-election homicides reduce incumbent party re-election rates

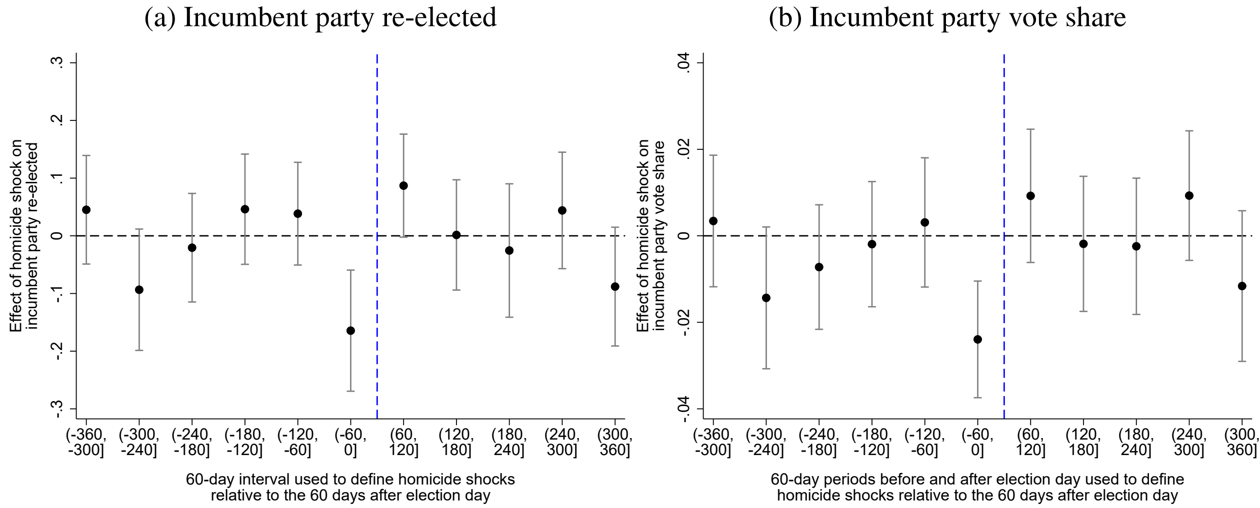
I first report the main finding graphically. Relative to the 60 days after election day, Figure 4 shows that only homicide shocks that occurred in the 60-day interval before the election—when voters consume most news—significantly reduced the incumbent party's probability of re-election and vote share. Homicide spikes that occurred earlier in the year preceding an election did not influence election outcomes, and further suggest that the impact of homicides shortly before election day is not driven by pre-trends. The 60-day intervals after election day serve as placebo tests, demonstrating that homicides which had not yet occurred also did not systematically affect voting behavior.

The regression estimates in Table 2 confirm that idiosyncratic pre-election homicide shocks significantly reduced the municipal incumbent party's electoral prospects. Column (1) of panel A shows that homicide shocks reduced the incumbent party's probability of being re-elected by 16 percentage points. This represents a 27% reduction in the incumbent party's re-election probability, relative to the 60% re-election rate in non-shocked municipalities. Column (1) of panel B reports that this decline reflects a 2.4 percentage point—or 0.2 standard deviation—reduction in the incumbent party's vote share.[19] The fact that the probability of winning changes much more than the vote share reflects the fact that shifts in support can make a big difference in Mexico's highly competitive municipal elections.

To help interpret the effect of pre-election homicides, I use homicide shocks—which approximate $e_{m,t,j}$ in equation (1)—to instrument for the homicide rate over the 60 days preceding election

[19] Appendix Table A14 indicates that the decline in incumbent party support does not reflect significant changes in turnout.

Figure 4: Effect of homicide shocks occurring over the year before and after election day on incumbent party electoral outcomes (95% confidence intervals)



Notes: For each 60-day interval over the 360 days preceding municipal elections, a homicide shock is defined as more homicides occurring in that 60-day period than in the 60 days after election day. Each coefficient is obtained by separately estimating the effect of such homicide shocks using equation (4).

day. The instrumental variable estimates in Appendix Table A13 show that each additional homicide per 100,000 people per month before a municipal election—which equates to a 64% increase in homicides in the average municipality—reduced the incumbent party's probability of winning by 19 percentage points and their vote share by 2.8 percentage points. The first stage is unsurprisingly strong, while the exclusion restriction is plausible for voters who cannot distinguish whether a spike in homicides reflects incumbent quality and/or incumbent effort or an adverse idiosyncratic shock, as standard accountability models assume.

These findings show that homicide spikes which coincide with voters consuming more news before elections can substantially influence voting behavior, even when such spikes are driven by idiosyncratic variation in the homicide rate. The large effects also imply that incumbents cannot respond to fully mitigate these electoral costs in the short term.

5.5 Robustness checks

The large effect of pre-election homicide shocks is robust across various alternative specifications and definitions of homicide shocks. First, column (2) of Table 2 shows that the effect of pre-election homicides does not depend upon including municipality and year fixed effects. Since the empirical design exploits exogenous fluctuations in the homicide count around local elections, these fixed effects are not necessary to identify the effects of pre-election volatility in homicide counts.

Second, the results are not sensitive to time-varying potential confounders. Column (3) of Table

Table 2: Effects of pre-election homicide shocks on municipal incumbent party electoral outcomes

	Baseline specif-ication (1)	No fixed effects (2)	Adjusting for covariates (3)	Municipality -specific trends (4)	State × year fixed effects (5)	Violence bin effects (6)	≤1 homicide per 100,000 per month (7)	Non-DTO states (8)
Panel A: Outcome: Incumbent party re-elected								
Homicide shock	-0.164***	-0.121***	-0.136***	-0.144*	-0.112**	-0.131***	-0.147**	-0.147*
	(0.054)	(0.043)	(0.047)	(0.077)	(0.045)	(0.043)	(0.057)	(0.075)
Observations	2,583	2,583	2,302	2,583	2,580	2,583	2,282	1,511
R^2	0.45	0.01	0.53	0.66	0.52	0.52	0.46	0.45
Outcome mean	0.55	0.55	0.56	0.55	0.55	0.55	0.55	0.54
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.42
Panel B: Outcome: Incumbent party vote share								
Homicide shock	-0.024***	-0.025***	-0.018***	-0.016**	-0.017***	-0.021***	-0.022***	-0.018**
	(0.007)	(0.009)	(0.006)	(0.008)	(0.006)	(0.006)	(0.007)	(0.009)
Observations	149,541	149,541	143,318	149,541	149,541	149,541	137,965	82,670
Unique municipality-elections	2,578	2,578	2,426	2,578	2,578	2,578	2,339	1,506
R^2	0.41	0.01	0.44	0.52	0.44	0.43	0.42	0.36
Outcome mean	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.40
Outcome std. dev.	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.14
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.42

Notes: All specifications are estimated using OLS, and weight observations by the number of registered voters. Column (1) estimates equation (4), which includes municipality and election month-year fixed effects. Column (2) excludes municipality and election month-year fixed effects. Columns (3) includes the covariates in Appendix Table A4, with the exception of variables with greater than 5% missingness and variables characterizing the types of homicides that occurred before the election. Column (4) includes municipality-specific year trends. Column (5) includes state × election year fixed effects. Column (6) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (7) excludes municipalities where the average monthly homicide rate per 100,000 people exceeds 1 over the prior year. Column (8) excludes 13 states with high-level of DTO activity (see footnote 23). Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2 first demonstrates robustness to adjusting for 29 municipal-level covariates, including the indicator for incumbent victory at the previous election over which shocks are imbalanced.[20] Moreover, column (4) adjusts for municipality-specific linear trends to show that the results do not reflect differential trends across municipalities. Standard errors increase because including such trends alongside municipality fixed effects absorbs the 39% of observations from municipalities for which there are only two elections in the sample.[21] Column (5) further demonstrates robustness to including state × year effects, which non-parametrically account for any differences in homicide seasonality, governor actions, or federal policies across states over time.

Third, the results do not reflect shocked municipalities experiencing particular levels or types of homicides. By including fixed effects for the total homicide count, in 10-homicide bins, over

[20] I exclude covariates characterizing pre-election homicides and those with greater than 5% missingness.

[21] While similar in magnitude, the inclusion of quadratic municipality-specific trends—which, alongside municipality fixed effects and linear municipality-specific trends, absorb all variation in treatment in the 67% of observations from municipalities with three or fewer elections in the sample—yields noisier estimates.

Table 3: Alternative definitions of pre-election homicide shocks

	Homicide shock defined by the average homicide rate in the 60 days preceding election day, relative to days to the average homicide rate in days...					Homicide shocks defined using different election day windows:		
	...[-300,-240) *before* election	...[-240,-180) *before* election	...[-180,-120) *before* election	...[-300,-120) *before* election	...[60,120) *after* election	30-day window	90-day window	120-day window
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: Incumbent party re-elected								
Homicide shock	-0.116**	-0.104**	-0.107**	-0.113**	-0.129***	-0.093	-0.100**	-0.104**
	(0.053)	(0.050)	(0.052)	(0.045)	(0.046)	(0.058)	(0.049)	(0.047)
Observations	2,530	2,496	2,472	3,520	2,531	1,717	3,176	3,624
R^2	0.44	0.44	0.44	0.42	0.45	0.46	0.43	0.42
Outcome mean	0.56	0.55	0.55	0.55	0.56	0.57	0.55	0.54
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Homicide shock mean	0.32	0.51	0.43	0.40	0.55	0.45	0.40	0.42
Panel B: Outcome: Incumbent party vote share								
Homicide shock	-0.017**	-0.012	-0.006	-0.012**	-0.021***	-0.017**	-0.017**	-0.019***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.008)	(0.007)	(0.007)
Observations	148,997	148,781	147,228	168,604	148,714	128,683	162,753	169,965
Unique municipality-elections	2,527	2,491	2,470	3,513	2,526	1,717	3,169	3,615
R^2	0.41	0.41	0.40	0.40	0.42	0.41	0.41	0.41
Outcome mean	0.42	0.42	0.42	0.41	0.42	0.42	0.42	0.41
Outcome std. dev.	0.15	0.15	0.15	0.15	0.15	0.14	0.15	0.15
Homicide shock mean	0.32	0.51	0.43	0.40	0.55	0.45	0.40	0.42

Notes: All specifications are estimated using OLS, include municipality and election month-year fixed effects, and weight observations by the number of registered voters. Columns (1)-(5) define homicide shocks by comparing the 60 days preceding election day to the pre- and post-election periods defined at the top of each column. Columns (6)-(8) respectively define homicide shocks over one-, three-, and four-month windows either side of election day. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the 120-day window used to define homicide shocks, column (6) shows that the results are not driven by differences in levels of violence.[22] Furthermore, column (7) excludes municipalities that experienced more than 1 homicide per 100,000 people a month over the 300 days preceding the window defining homicide shocks. This indicates that the results are not driven by the most violent municipalities, where voters are more likely to experience homicides directly. Although homicide shocks are uncorrelated with indicators of DTO presence, I further address the potential concern that homicides reflect strategic behavior by cartels in column (8) by excluding 13 states that have consistently registered high DTO-related crime.[23]

Fourth, although post-election homicide rates were unobservable when voters voted and are not correlated with election outcomes, post-election homicides could be endogenous to election outcomes, changes in policy-making incentives for departing municipal governments, or transition periods more generally. However, to ensure that post-election behavior does not drive the electoral impacts of pre-election homicides, Table 3 shows that the effect of pre-election homicide shocks

[22]Smaller bins of size 1, 2, and 5 similarly yield significant negative effects, at the cost of reducing the sample size by absorbing elections within-bin with no variation in treatment.

[23]These state are: Baja California, Chihuahua, Coahuila, Colima, Durango, Guerrero, Michoacán, Morelos, Nayarit, Nuevo León, Sinaloa, Sonora, and Tamaulipas. Similar results obtain if municipalities registering more than one drug-related homicide per month, over the 2006-2011 period for which data are publicly-available, are excluded.

is similar when a *pre*-election comparison period—when voters pay less attention to the news—is used instead.[24] Columns (1)-(3) report negative effects, for both incumbent re-election and vote share, when 60-day windows from *before* the election campaign are used as the comparison group to define homicide shocks. Column (4) provides more precise estimates when averaging across each of these 60-day intervals preceding the four months of a typical election campaign. To further demonstrate that the election aftermath is not driving the results, column (5) reports similar results when using days 61-120 after the election—a period by when electoral disputes have normally been resolved. Furthermore, Appendix Figure A4 shows that only homicides in the 60-day interval preceding election day significantly predict a lower probability of incumbent re-election when all 60-day intervals in the year before and after election day are included in the same regression. Ultimately, while there are advantages and disadvantages of using pre-campaign and post-election homicide rates to define homicide shocks, the results are not sensitive to the choice of period against which the 60 days preceding the election is compared.

Finally, the results are not driven by the particular operationalization of pre-election homicide shocks. Columns (6)-(8) of Table 3 show that homicide shocks defined by 30-, 90-, 120-day intervals reduce the incumbent party's probability of winning and vote share by similar amounts in these different samples. Appendix Table A17 further reports broadly similar results using a panel fixed effects design to examine deviations between the homicide rate in the 60 days before election day and the homicide rate over the entire preceding electoral cycle.

5.6 No discernible effect of longer-term homicide rates

The news consumption cycles argument implies that many voters predominantly learn about homicides occurring just before elections. In contrast, longer-term homicide rates—which Appendix Table A10 shows to be more informative about future homicide rates—may not shape responsibility attribution if many voters were insufficiently engaged earlier in the electoral cycle to learn about more systematic trends.

To assess voter responsiveness to longer-term performance indicators, I estimate the effects of homicide rates over the year preceding election day and over a mayor's entire term on the incumbent party's electoral prospects using twoway fixed effect regressions. The following panel specification compares changes in support for incumbent parties in municipalities that experienced relatively large increases in their homicide rate between elections to changes in support for incumbent parties in municipalities that experienced relatively small increases in the homicide rate:

$$Y_{m,t} = \beta \textit{Average monthly homicide rate}_{m,t} + \eta_m + \mu_t + \varepsilon_{m,t}. \tag{5}$$

[24] To adjust for homicide rate trends between these more distant comparisons, the first condition defining homicide shocks in equation (3) is demeaned by the mean homicide rate during the pre-election and comparison 60-day intervals.

Table 4: Effects of longer-run homicide rates on municipal incumbent party electoral outcomes

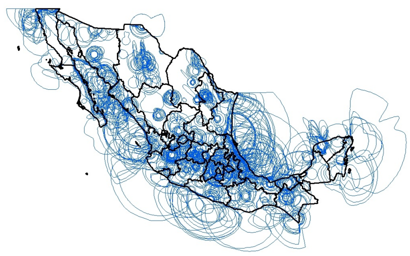
	Outcome:			
	Incumbent party re-elected		**Incumbent party vote share**	
	(1)	(2)	(3)	(4)
Panel A: Effect of average monthly homicide rates 12 months before election				
Average monthly homicide rate	0.0153	-0.0071	0.0008	-0.0019
(last 12 months before election)	(0.0100)	(0.0225)	(0.0014)	(0.0023)
R^2	0.43	0.65	0.41	0.52
Monthly homicide rate mean	1.36	1.36	1.37	1.37
Monthly homicide rate std. dev.	2.43	2.43	2.44	2.44
Panel B: Effect of average monthly homicide rates since last election				
Average monthly homicide rate	0.0325*	-0.0112	0.0035	-0.0033
(mayor's term)	(0.0168)	(0.0360)	(0.0024)	(0.0037)
R^2	0.43	0.65	0.41	0.52
Monthly homicide rate mean	1.19	1.19	1.19	1.19
Monthly homicide rate std. dev.	1.62	1.62	1.63	1.63
Observations	2,583	2,583	149,541	149,541
Unique municipality-elections	2,583	2,583	2,578	2,578
Outcome range	{0,1}	{0,1}	[0,1]	[0,1]
Outcome mean	0.55	0.55	0.42	0.42
Outcome standard deviation	0.50	0.50	0.15	0.15
Municipality-specific time trends		✓		✓

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

Reinforcing Ley's (2017) cross-sectional findings for local elections between 2009 and 2012, the estimates in Table 4 suggest that longer-run homicide rates had little impact on electoral outcomes. Columns (1)-(4) of panel A show that monthly homicides per 100,000 people over the year before election day are not negatively correlated with either the incumbent party's probability of being re-elected or their vote share, regardless of the inclusion of municipality-specific time trends. These generally statistically insignificant point estimates are also an order of magnitude smaller than the IV estimates discussed above. Panel B similarly shows that voters do not sanction homicide rates averaged across the municipal government's entire term. The limited role for long-term homicides rates in shaping voting behavior suggests that relatively few voters updated their appraisals of incumbent performance from homicide reports throughout the electoral cycle.

Figure 5: Media station commercial quality signal coverage areas

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



6 The amplifying effect of local media

The individual- and aggregate-level findings are consistent with the electoral sanctioning of pre-election local homicide shocks being driven by voters' greater consumption of local media content before elections. However, the mass of voters who did not experience crimes directly could also have learned about homicides via word of mouth before elections or reacted to municipal policy responses or election campaigns. To establish the link between pre-election events and news consumption from local media, I next identify the moderating effect of an additional local radio or television station likely to report on homicides in the municipality on electoral responses to pre-election homicides.

6.1 Data

As part of an electoral reform in 2007, the IFE required that all radio and television stations calculate the commercial quality reach of their antennae. Similar to the irregular terrain models used in academic studies (e.g. Olken 2009; Yanagizawa-Drott 2014), the IFE's model for generating coverage maps reflects the local topography and the location, frequency, direction, and power of antennae. The signal inside the commercial quality coverage area—the level of coverage that U.S. media outlets base ad pricing on—is very strong, and reaches most households. Signal quality quickly declines beyond the commercial quality coverage boundary.[25]

Figure 5 maps the commercial quality coverage of each antennae across Mexico. While FM radio, and especially television, stations have limited and primarily urban coverage, AM radio signals can travel considerable distances—particularly when aided by stretches of salt water with high ground conductivity. Following Larreguy, Marshall and Snyder (2020), I combine rural locality-level and urban block-level population data from the 2010 Census with electoral registration data to

[25]The IFE defined the boundary of the coverage area using a 60 dBμ threshold for signal strength. According 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."

define an electoral precinct as covered by a given media station if at least 20% of voters fall within the commercial coverage boundary.

6.2 Identification strategy

Since multiple broadcast media outlets reach most Mexican households, I approximate the probability that voters consume news covering homicides in their municipality by examining the marginal effect of access to an additional *local* media outlet. Media outlets are more likely to cover, and cover in greater detail, news that is local to their audience (e.g. George and Waldfogel 2003; Snyder and Strömberg 2010). Holding fixed the number of non-local media outlets, an additional local outlet then increases the likelihood of voter exposure to local events if local news coverage induces voters to switch toward local outlets (Larreguy, Marshall and Snyder 2020) or local outlets attract new consumers (George and Waldfogel 2006; Prat and Strömberg 2005).

To estimate the electoral effects of increasing the probability of exposure to an additional local media station, I leverage plausibly exogenous topographical variation using a similar design to Olken (2009), Spenkuch and Toniatti (2018), and Yanagizawa-Drott (2014). I restrict attention to comparisons between neighboring electoral precincts within the same municipality which differ in the total number of *local* AM, FM, and television stations—defined as outlets whose antennae are located within the electoral precinct's municipality *and* that provide local content[26]—by which they are covered. The key identifying assumption is that, in expectation, neighboring precincts differ only in the number of commercial quality signals that they receive from local media outlets.[27]

There are several reasons to believe that differences in local media coverage between neighboring electoral precincts are exogenous. First, neighboring precincts often differ in coverage because of physical characteristics such as geographic contours, water, and large objects that aid or impede ground conductivity (for AM radio) and the electromagnetic propagation of "line of sight" radio waves (for FM radio and television) between an antenna and precincts at the coverage boundary. Such differences in topography between antennae and receiver across neighboring precincts are unlikely to correlate with political variables. Second, media stations lack the fine-grained technology required to differentially target neighboring precincts,[28] while voters who locate according to the availability of additional local media are likely to choose locations guaranteeing high-quality coverage away from coverage boundaries.

To implement this design, for each electoral precinct included in the homicide shock sample in Table 2, I identify every set of neighboring precincts within the same municipality that have

[26]Local content excludes television stations solely retransmitting Televisa and TV Azteca national broadcasts.

[27]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.

[28]The power output of AM, FM, and television stations is generally round thousands of watts divisible by five.

access to a *different* number of local media stations. Each such grouping n comprises one "treated" precinct and a set of neighboring "control" precincts. To remove larger precincts that antennae choices could more plausibly have been designed to target and prevent comparisons between urban and non-urban precincts, I restrict attention to precincts with an area of at most 2km^2. This yields 2,186 neighboring groups, containing an average of 2.2 control precincts per election; Appendix Figure A1c maps this sample. The average voter is covered by 5.8, 8.7, and 3.0 local AM, FM, and television stations respectively.[29]

Combining this variation in access to local media between neighboring precincts (within municipalities) with the municipal homicide shocks leveraged above, I use the following OLS regression to identify how local media moderates the electoral effects of pre-election homicide shocks:

$$\begin{aligned} Y_{p,n,m,t} &= \beta_1 \textit{Homicide shock}_{m,t} + \beta_2 \textit{Local media}_{p,n,m} \\ &\quad + \beta_3 \left(\textit{Homicide shock}_{m,t} \times \textit{Local media}_{p,n,m} \right) + \zeta_n + \mu_t + \varepsilon_{p,n,m,t}, \end{aligned} \tag{6}$$

where ζ_n are fixed effects for each group of neighboring precincts. To weight neighbor-election groups equally, each "treated" precinct is weighted by electorate size and "control" precincts are weighted by electorate size divided by the number of controls per neighbor-election set. Reflecting both levels of assignment, standard errors are two-way clustered by municipality-election and neighbor group.

To validate the identifying assumption, I use equation (6) to examine whether local media is balanced across demographic, socioeconomic, and political characteristics. Appendix Table A6 shows that only 6 of these 31 precinct-level variables are significantly correlated with the number of local media stations below the 10% level.[30] These differences are small in magnitude and balanced with respect to key variables, including population density or distance to the municipality head, the number of non-local media stations, and prior incumbent party vote share. I also show below that the results are robust to adjusting for statistically significant imbalances.

6.3 Access to local media increases electoral sanctioning of homicide shocks

Column (1) of Table 5 first reports that the negative effect of pre-election homicide shocks is larger in this subsample of more urban and media-accessible precincts than in the full sample in Table 2. More importantly, column (2) demonstrates that access to local media stations plays a critical role in driving the electoral sanctioning of homicide shocks. The negative interaction coefficient indicates that each additional local media station reduced the vote share of municipal incumbent

[29] Since few new antennae were approved in the 2000s, I apply the 2012 coverage maps to each year in the sample. Appendix Table A19 reports similar results when this assumption is relaxed.

[30] Table A7 shows that homicide shocks remain well-balanced across pre-treatment variables in this subsample.

Table 5: Moderating effect of access to local media on the effect of pre-election homicide shocks on precinct-level incumbent party vote share

	Outcome: Incumbent party vote share							
	No local media interaction	Baseline specif- cation	Interactive non-local media covariate	Interactive imbalanced covariates	Interactive spatial location	Adjust for average media within 50m	Interactions with longer-run homicide rates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Homicide shock	-0.0710***	-0.0029	-0.0024	-0.0031	0.0371	-0.0039		
	(0.0129)	(0.0238)	(0.0291)	(0.1325)	(0.0357)	(0.0239)		
Local media		0.0018**	0.0018**	0.0014**	0.0025***	0.0011	0.0002	0.0003
		(0.0007)	(0.0007)	(0.0007)	(0.0009)	(0.0013)	(0.0004)	(0.0004)
Homicide shock × Local media		-0.0040***	-0.0040***	-0.0031***	-0.0058***	-0.0040***		
		(0.0011)	(0.0011)	(0.0011)	(0.0014)	(0.0012)		
Non-local media			0.0003					
			(0.0009)					
Homicide shock × Non-local media			-0.0000					
			(0.0008)					
Average monthly homicide rate (last 12 months before survey)							0.0004	
							(0.0005)	
Average monthly homicide rate (last 12 months before survey) × Local media							-0.0000	
							(0.0000)	
Average monthly homicide rate (mayor's term)								0.0134
								(0.0103)
Average monthly homicide rate (mayor's term) × Local media								-0.0003
								(0.0004)
Observations	26,134	26,134	26,134	26,134	26,134	10,519	26,134	26,134
Unique municipality-elections	397	397	397	397	397	397	397	397
Unique neighbor groups	2,186	2,186	2,186	2,186	2,186	2,151	2,186	2,186
R^2	0.63	0.64	0.64	0.64	0.65	0.65	0.60	0.60
Outcome range	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]
Outcome mean	0.46	0.46	0.46	0.46	0.46	0.46	0.46	0.46
Outcome standard deviation	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Homicide shock mean	0.36	0.36	0.36	0.36	0.36	0.36	19.34	1.36
Media measure media mean		17.57	17.57	17.57	17.57	17.53	17.57	17.57
Media measure standard deviation		10.30	10.30	10.30	10.30	10.31	10.30	10.30

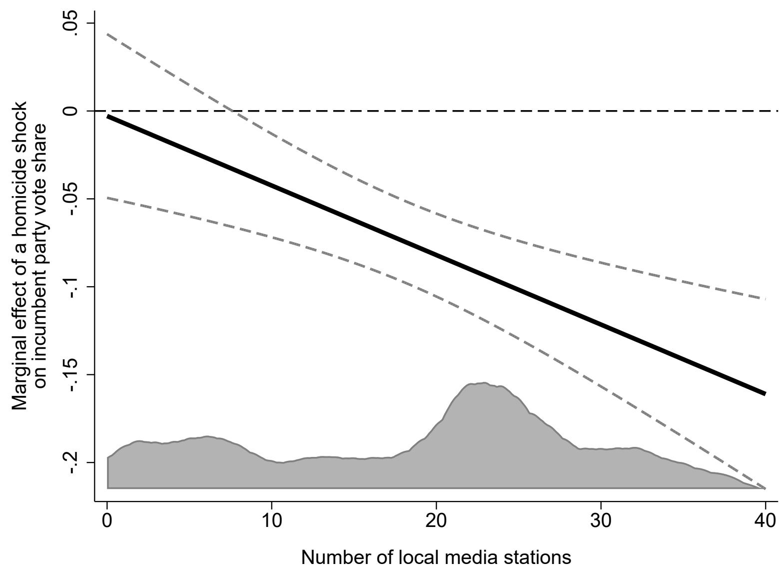
Notes: All specifications are estimated using OLS, and include neighbor group and election month-year fixed effects. All observations are weighted by the number of registered voters, while "control" precincts are further divided by the number of comparison units within each neighbor group. Column (4) includes interactions between homicide shock and the following (standardized) covariates, which are omitted to save space: registered voters, average number of occupants per room, share illiterate, share of households that are economically active, share of households with basic amenities in their home, and share of individuals born in another state. Column (5) includes interactions between homicide shock and third-order polynomials in latitude and longitude and their cross-products. Column (6) restricts control precincts to those with an average distance to media outlets that is within 50 meters of the average distance for the group's corresponding "treated" precinct. Standard errors double clustered by municipality-election and neighbor group are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

parties that experienced a pre-election homicide shock by 0.4 percentage points on average. A standard deviation increase in the number of local media stations decreases the incumbent party's vote share by more than 4 percentage points—almost one third of a standard deviation change in vote share. The smaller positive coefficient on the lower-order local media term indicates that voters rewarded incumbent parties that did not experience a homicide shock before the election.

The results further suggest that local media are necessary for Mexican voters to sanction incumbent parties. The null lower-order effect of a homicide shock indicates that pre-election homicide spikes do not affect vote shares in precincts with limited access to local media. For electoral precincts covered by 8 or more local media stations (75% of the sample), Figure 6 shows that the overall effect of a homicide shock is to significantly reduce the incumbent party's vote share.[31]

[31]Since the identification strategy relies on within-neighbor variation, this represents a linear extrapolation of the

Figure 6: Effect of a homicide shock on change in incumbent party vote share, conditional on the number of local media stations (95% confidence interval)



Notes: Calculated using the lower-order local media and interaction coefficients from column (2) in Table 5. The gray density plot above the *x* axis shows the distribution of the local media variable in the estimation sample.

6.4 Robustness checks

The large moderating effect of access to local media outlets is robust to various identification concerns. First, the results are robust to adjusting for potential confounders. Column (3) of Table 5 interacts pre-election homicide shocks with the number of non-local media outlets covering a precinct to show that precincts with greater access to local media do not respond more to homicide shocks because they possess greater access to non-local media. Column (4) further demonstrates that the results are robust to simultaneously including interactions between a homicide shock and the few (demeaned) covariates over which local media is imbalanced.

Second, I follow Olken (2009) in narrowing variation in access to local media to differences in topography. I conduct two tests: (i) to isolate variation that is independent of a precinct's location, column (5) flexibly adjusts for a precinct's coordinates by including interactions between a homicide shock and (demeaned) cubic polynomials in the latitude and longitude of a precinct's centroid and their cross-products; (ii) to ensure that distance from outlet antennae—and thus precinct location vis-à-vis urban areas where antennae are predominantly located—is not driving the results, column (6) restricts the sample to control precincts for which the average distance from the local media stations that a precinct is covered by differs by less than 50 meters from the average distance from the local media stations that the group's treated precinct is covered by. The robustness of the

average marginal effect.

results in each case suggests that unobservable confounders correlated with space are not driving local media's effects.

Third, the results are not sensitive to particular design parameters. Appendix Table A18 reports similar results using more and less strict definitions of whether a local media stations covers a precinct and tighter and more generous rules for including precincts based on area size. Furthermore, I use the natural logarithm of the number of local media outlets, due to the large number of outlets reaching some precincts. The results imply that a ten percent increase in local media outlets decreased the vote share of shocked incumbent parties by 0.4 percentage points.

Finally, if the results reflect local media coverage of homicides that occurred just before elections, then other types of content that are rarely reported at this time should not alter vote choices. In particular, less recent events are likely to receive less coverage. Indeed, columns (7) and (8) find no significant interaction between longer-run homicide rates and the number of local media outlets. Together, these tests suggest that electoral sanctioning occurs where media outlets are likely to report on local homicides when voters consume most news, and thus illustrate the electoral importance of the nexus between the consumption and supply of politically-relevant news.

7 Mechanisms

The preceding analysis showed that voters consume most political information just before local elections and are highly sensitive to homicides that occur at this time, but only when they have access to media stations likely to report on local homicides. This section substantiates the mechanism—that electoral sanctioning of pre-election homicide shocks reflects belief updating based on news voters encounter when they pay greater attention before elections.

7.1 Voter belief updating tracks news consumption cycles

If news consumption cycles drive voter responsiveness to pre-election homicides, inattentive voters will primarily update their beliefs in response to newsworthy events that occur when they consume most news before elections. In contrast, inattentive voters will engage in limited updating after equally-recent events that occur *outside* elections campaigns.

I assess the belief updating channel by using the ENCUP surveys to examine whether public security concerns increased when homicide shocks coincided with the pre-election period of peak news news consumption. Interacting upcoming local elections with *pre-survey* homicide shocks (defined analogously to pre-election homicide shocks, but with respect to survey dates),[32] the following OLS regression estimates the effect of homicide shocks both outside and during local

[32]Appendix Table A3 shows that pre-survey homicide shocks are balanced across pre-determined covariates.

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

	Outcome: Public insecurity the major problem in the community					
	Interacting homicide measure:					
	Pre-survey homicide shock		Average monthly homicide rate... ...(last 12 months)		...(mayor's term)	
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides measure	-0.009	-0.003	-0.001	0.002	-0.001	0.002
	(0.010)	(0.009)	(0.007)	(0.009)	(0.007)	(0.009)
Upcoming local election (4 months)	0.068		0.075		0.081	
	(0.047)		(0.119)		(0.128)	
Upcoming local election (4 months)	0.054**		0.020		0.014	
× Homicides measure	(0.026)		(0.388)		(0.262)	
Upcoming local election (2 months)		0.014		0.030		0.050
		(0.030)		(0.127)		(0.139)
Upcoming local election (2 months)		0.059**		0.025		0.005
× Homicides measure		(0.029)		(0.388)		(0.273)
Effect of homicide measure when	0.045	0.056	0.018	0.027	0.012	0.007
a local election is upcoming	(0.022)	(0.028)	(0.389)	(0.39)	(0.261)	(0.271)
Observations	10,024	10,024	12,541	12,541	12,541	12,541
Unique states	31	31	31	31	31	31
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.12	0.12	0.10	0.10	0.10	0.10
Homicides measure mean	0.38	0.38	0.71	0.71	0.73	0.73
Upcoming local election mean	0.09	0.04	0.07	0.03	0.07	0.03

Notes: All specifications include survey wave fixed effects, and are estimated using OLS. The outcome was not collected in the 2012 ENCUP survey. Block-bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

election campaigns:

$$\begin{aligned} Security_{i,m,s,t} \;=\; & \beta_1 Upcoming\ local\ election_{s,t} + \beta_2 Homicide\ shock_{m,s,t} \\ & + \beta_3 \left(Upcoming\ local\ election_{s,t} \times Homicide\ shock_{m,s,t}\right) + \mu_t + \varepsilon_{i,m,s,t}, \quad (7) \end{aligned}$$

where $Security_{i,m,s,t}$ is an indicator for respondents who cited crime and insecurity, drug trafficking, or violence as the most important problem for their community to solve.

The results in Table 6 show that pre-survey homicide shocks only increased concern about public security when they occurred just before local elections. The negligible coefficients on the lower-order pre-survey homicide shock term in columns (1) and (2) indicate that an increase in the homicide count in the two months preceding a survey that did not coincide with local election campaigns had no discernible impact on public security concerns. Only highly-engaged voters regularly consumed news in this period, and are unlikely to have substantially updated from additional noisy signals. In contrast, the interaction coefficients and the overall effect—the sum of the lower-order and interaction coefficients for homicide shocks—at the foot of the table show that a pre-survey homicide shock that occurred within four or two months of a local election differentially increased the probability of citing public security as the most important problem facing their community by more than 5 percentage points—an increase of almost 50%, relative to the sample mean.

It remains possible that voters were instead responding to longer-run homicide rates that could also have been reported before local elections. However, interactions with the average number of homicides per month over the prior year or mayor's term—shown in columns (3)-(6)—indicate that there is no differential effect of such homicide rates on public security concerns ahead of elections.

Beyond suggesting that the timing of news consumption drives voter concerns that influence vote choices, these tests are also inconsistent with several alternative explanations for observed voting behavior. One possibility is that voters have short memories and only update from, and respond to, recent events (Sarafidis 2007; Zaller 1992). Similarly, cognitive limitations in aggregating signals might cause voters to overweight recent performance indicators (Healy and Lenz 2014). In either case, pre-election homicides could significantly affect election outcomes because voter beliefs cease to incorporate news they had consumed earlier. Another possibility is that, to the extent that politician types evolve over time, voters might regard more recent indicators as better predictors of future performance (Rogoff 1990).[33] Although Appendix Table A10 shows that longer-term homicide rates are better predictors of future homicide rates, voters may not perceive this. Contrary to recency-based explanations, the survey results in Table 6 show that voters did not update from recent homicide shocks that occurred *outside* local election campaign periods.

7.2 Voters distinguish responsibility across governments

If voters believe that the municipal government is at least partly responsible for local crime because they command the local police, pre-election homicide shocks should primarily be punished in municipalities where the mayor commands a police force. I test this by examining municipalities, and *delegaciones* in Mexico City, without their own police force. Appendix Table A20 shows that homicide shocks had a significantly smaller effect in such municipalities and only significantly harmed the electoral prospects of incumbent parties commanding a municipal police force. In addition to their updated concerns about public security, this suggests that voters are aware of local police forces and are not indiscriminately punishing adverse events that occur when their consumption of

[33] Voters might also anticipate that more competent politicians will improve their performance before elections (Rogoff 1990). However, such behavior is driven by politician anticipation of voters' news consumption cycles. Moreover, I show above that variation in pre-election homicides does not appear to reflect incumbent policies or capacity.

news increases.

Since responsibility for combating crime is split across layers of government, I further establish that sanctioning of municipal governments does not simply reflect sanctioning of the federal government. To differentiate these distinct modes of accountability, I examine heterogeneity in the sanctioning of homicide shocks by incumbent party. Appendix Table A21 shows that pre-election homicide shocks significantly reduced the electoral prospects of both PAN and PRI municipal governments, even though the PRI only held the federal presidency in 2 of the 14 years under study.[34] Similarly, homicide shocks significantly reduce the probability of municipal governments that are not politically aligned with the president by 10 percentage point loss. Since sanctioning is also somewhat larger when municipal and federal governments are aligned, these results further suggest that voters regard municipal governments as partially responsible for homicides rates alongside other levels of government.

7.3 Alternative supply-side interpretations

The preceding analysis suggests that the timing of citizens' news consumption, in combination with access to local media, drives voters' sanctioning of pre-election homicides. Nevertheless, it remains possible that such homicide shocks produce large effects because they are covered differently by the media before elections. The following analyses assess this possibility, finding that the quantity and form of news coverage of homicides did not significantly change before elections and that the effect of pre-election homicide shocks is not moderated by political ads. These results suggest that changes in media content do not drive voters' sensitivity to pre-election events.

7.3.1 No difference in media coverage of violent crime before elections

I first consider news coverage of homicides. If the amount or type of coverage of local homicides in the news also changed before elections, then voters' sensitivity to pre-election homicide shocks could be explained by more articles covering homicides or content that is more likely to attribute responsibility for homicides to municipal governments, rather than citizen attention to politically-relevant news before elections.

In the absence of comprehensive transcripts for local broadcast outlets, I assess this alternative interpretation of the preceding results by examining newspaper reporting of homicides. Radio and television outlets are likely to cover similar events and regularly draw their news content from newspapers.[35] To do so, I assembled an original corpus of online news stories—primarily from the online editions of hundreds of local and national Mexican newspapers—referencing homicides.

[34] Electoral sanctioning was more limited for the PRD, but in a notably smaller sample.

[35] 62% and 56% of radio station news directors in Mexico report sourcing news from national and local news outlets respectively (Larreguy, Lucas and Marshall 2020).

This involved historical queries of Google México by day for 41 terms for homicide in Spanish; the most prevalent were "homicidio", "asesinado", and "asesinato". An article was then classified as reporting a homicide in a given municipality if at least one paragraph contained a homicide term *and* the (full or commonly abbreviated) name of a municipality. Appendix Section A.2.6 provides further information about data collection and classification. This process yielded 8,077 articles reporting a homicide in a given municipality between 1999 and 2012.

This corpus is not without limitations. First, it underestimates the true number of news reports because Google's indexing of websites has become more extensive over time and this archive does not include radio and television reports. Second, measurement error in outcomes based on news reports inevitably arises due to misclassification. To mitigate this concern, I exclude 39 municipalities where the municipality name is shared by their state, is a common noun, or matches one of Mexico's 100 most common surnames.

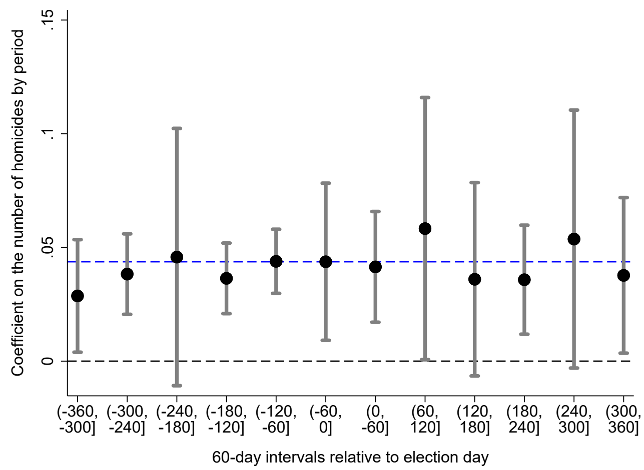
To assess whether the quantity of reporting on homicides changed before local elections, I estimate the correlation between the number of homicides and the number of news reports of homicides in a municipality over the 60-day periods one year either side of each election day in my sample. The following estimating equation includes municipality and election date period fixed effects to adjust for differences in reporting across municipalities and time:

$$\begin{aligned} Reports_{m,k,t} \;=\; & \alpha Homicides_{m,k,t} + \beta\, Upcoming\ local\ election_{s,k,t} \\ & + \gamma\big(Homicides_{m,k,t} \times Upcoming\ local\ election_{s,k,t}\big) + \eta_m + \mu_t + \varepsilon_{m,k,t}, \end{aligned} \tag{8}$$

where $Reports_{m,k,t}$ is the number of articles mentioning homicides in municipality m over 60-day interval k around election date t, and $Upcoming\ local\ election_{s,k,t}$ is an indicator for the 60- or 120-day period preceding election day. I use the same sample of municipalities as Table 2 and again weight observations by the number of registered voters in a municipality. Block-bootstrapped standard errors are clustered at the state level.

Figure 7 first presents the results from a more flexible specification which estimates the correlation between the occurrence and reporting of homicides for each 60-day period. Indicating that the Google articles are capturing reports of local homicides, I detect an average of around 0.04 news articles per homicide that occurs in a given municipality across intervals. Since a single news report could cover multiple related homicides and I cannot measure all local news sources, this statistically significant estimate represents a lower bound on the propensity of newspapers to report a given homicide. More importantly, the comparison across periods demonstrates that reporting rates are similar throughout the year before and after election day. The regression results in column (1) of Table 7 confirm that reporting rates are not significantly different in the 60 days before election day (panel A) or 120 days before election day (panel B) from other periods either side of election

Figure 7: Correlation between the number of municipal homicides and Google-indexed reports of homicides, by interval around local election day (95% confidence interval)



Notes: Coefficients are from a regression analogous to equation (8), except α is more flexibly estimated separately for each 60-day interval 360 day either side of election day. The dashed blue horizontal line indicates the coefficient value for the 60 days up to and including election day.

day.

Having found no evidence to suggest that the quantity of reporting on homicides changes before local elections, I next examine *how* homicides are reported. Specifically, I estimate equation (7) instead using counts of the number of articles about homicides that also satisfy additional features as the outcome, which Appendix Section A.2.6 describes in detail. Whether in the two or four months before local elections, the results in columns (2)-(4) of panels A and B in Table 7 show that the actual number of homicides is not significantly more correlated with the number of articles about homicides that also mention municipal governments, mayors, or municipal police forces at least once. On average, such institutions are only mentioned in around a quarter of articles. Columns (5) and (6) further show that the length of articles about homicides are also unaffected. Finally, columns (7)-(10) classify articles about homicides in terms of the sentiment of the average word in the article (after removing stop words, punctuation, and numbers). I find no evidence to suggest that the sentiment of articles became more negative before elections; if anything, column (9) suggests that there may have been a slight increase in article positivity before elections, which would likely help incumbent parties instead.

Further indicating that there was limited incentive for media outlets to alter the quantity or content of their coverage of homicides, I find no evidence to suggest that audience interest in homicides increased before elections. Appendix Figure A2 shows that Google searches under the theme of "homicides" were indistinguishable in the months preceding local elections relative to the rest of the electoral cycle. Combined with the newspaper coverage analysis, these results suggest that the sanctioning of pre-election homicides is driven by increased news consumption before

Table 7: Correlation between municipal homicide counts and Google-indexed reports of homicides, by upcoming local elections

	Outcome: Number of homicide reports recovered by Google per 60-day interval									
	All	Mentioning municipal government	Mentioning municipal mayors	Mentioning municipal police	At least 500 words	At least 1000 words	With negative sentiment	With very negative sentiment	With positive sentiment	With very positive sentiment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Upcoming local election within 60 days										
Homicides per month	0.042***	0.005***	0.006***	0.007***	0.028***	0.013***	0.007**	0.001**	0.002**	0.000
	(0.010)	(0.002)	(0.002)	(0.002)	(0.006)	(0.004)	(0.002)	(0.000)	(0.001)	(0.000)
Homicides per month	0.002	0.004	0.002	0.002	0.007	0.003	-0.000	0.001	0.005***	0.001
$\times$ 60 days before election day	(0.011)	(0.003)	(0.002)	(0.004)	(0.011)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)
R^2	0.46	0.20	0.23	0.25	0.45	0.35	0.19	0.06	0.18	0.08
Panel B: Upcoming local election within 120 days										
Homicides per month	0.043***	0.005***	0.006***	0.007***	0.029***	0.013***	0.007**	0.001**	0.002**	0.000
	(0.009)	(0.002)	(0.002)	(0.002)	(0.005)	(0.004)	(0.002)	(0.000)	(0.001)	(0.000)
Homicides per month	-0.003	-0.000	0.000	0.001	-0.001	-0.003	-0.001	0.000	0.002*	0.001
$\times$ 120 days before election day	(0.009)	(0.001)	(0.003)	(0.003)	(0.008)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
R^2	0.46	0.20	0.23	0.25	0.44	0.36	0.19	0.05	0.17	0.08
Observations	45,503	45,503	45,503	45,503	45,503	45,503	45,503	45,503	45,503	45,503
Unique states	30	30	30	30	30	30	30	30	30	30
R^2	0.46	0.20	0.23	0.25	0.44	0.36	0.19	0.05	0.17	0.08
Outcome range	[0,27]	[0,6]	[0,5]	[0,6]	[0,16]	[0,8]	[0,8]	[0,3]	[0,4]	[0,1]
Outcome mean	0.06	0.01	0.01	0.02	0.05	0.02	0.01	0.00	0.00	0.00
Outcome standard deviation	0.48	0.12	0.14	0.16	0.36	0.21	0.10	0.03	0.08	0.02
Homicides per month mean	1.84	1.84	1.84	1.84	1.84	1.84	1.84	1.84	1.84	1.84
Homicides per month standard deviation	6.41	6.41	6.41	6.41	6.41	6.41	6.41	6.41	6.41	6.41

Notes: All specifications include municipality and election date period fixed effects, weight observations by the average number of registered voters in a municipality within the sample used in Table 2, and are estimated using OLS. Block-bootstrapped standard errors clustered by state (2,500 replications). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

elections, rather than media outlets selectively reporting or slanting their coverage of homicides before elections.

7.3.2 No moderating effect of campaign ads broadcast on radio and television

Media content could also amplify voter responses to pre-election homicides if political parties incorporate these homicides into their campaign messaging. Voters might therefore respond more to homicides before elections because politicians—especially from opposition parties—increase the salience of these homicides and encourage voters to connect homicides to their appraisals of the municipal government.

Since campaigns are unlikely to target different messages at neighboring electoral precincts, the fact that sanctioning is driven by access to local media already suggests that political campaigns are not driving my findings. However, campaigning occurs on radio and television as well. Following a major reform in 2009, all ads slots—30 second segments designated for federal and local election campaigns—were allocated by the IFE in accordance with the number of parties running and a party's prior vote share in federal and state elections. Due to cross-state spillovers from states conducting local elections, exposure to ads from different parties varies across precincts covered by different local media outlets (see Larreguy, Marshall and Snyder 2018). Even among the ads designated for local candidates, ads generally emphasize national party platforms, concern about general issues (including crime), and candidate characteristics.

To examine whether the effects of homicide shocks are amplified by exposure to opposition ads, Table 8 reports the interaction between a pre-election homicide shock and the (demeaned) share of ads allocated to the party of the municipal government. I focus on the July 2009 and June 2012 elections, where concurrent federal and local elections generated significant within-municipality variation in the share of ads received by municipal incumbent parties. While the reduction in incumbent party vote share continues to hold in this subsample, the results show that the incumbent party's access to pre-election ads did not significantly moderate the effect of homicides shocks on the municipal incumbent party's vote share. This suggests that campaign ads are unlikely to be driving voters' sensitivity to homicides in the news before elections.

8 Conclusion

This study establishes that the timing of voter news consumption plays a key role in shaping electoral accountability. Leveraging complementary sources of plausibly exogenous variation, I demonstrate this in the context of violent crime preceding Mexican elections through several connected findings. First, I identified the existence of cycles where voters' news consumption peaks before local elections. Second, I showed that pre-election homicide spikes—which coincide with

Table 8: Moderating effect of incumbent party ad shares in 2009 and 2012 on the effect of pre-election homicide shocks on precinct-level change in incumbent party vote share

	Outcome: Incumbent party vote share		
	(1)	(2)	(3)
Homicide shock	-0.0315**	-0.0348**	-0.0341**
	(0.0149)	(0.0148)	(0.0148)
Homicide shock × AM incumbent party ad share (demeaned)	0.0624		
	(0.2229)		
Homicide shock × FM incumbent party ad share (demeaned)		0.0496	
		(0.1349)	
Homicide shock × TV incumbent party ad share (demeaned)			-0.0406
			(0.1192)
Observations	32,674	32,674	32,674
Unique municipality-elections	537	537	537
R^2	0.05	0.04	0.04
Outcome range	[0,0.96]	[0,0.96]	[0,0.96]
Outcome mean	0.39	0.39	0.39
Outcome standard deviation	0.13	0.13	0.13
Homicide shock mean	0.46	0.46	0.46
Incumbent party ad share media mean	0.27	0.27	0.26
Incumbent party ad share standard deviation	0.06	0.08	0.08

Notes: All specifications include election month-year fixed effects, and are estimated using OLS. Municipality fixed effects are excluded to avoid dropping municipalities that only enter the sample for either the July 2009 or July 2012 elections. All observations are weighted by the number of registered voters. Lower-order interaction terms are omitted to save space. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

greatest news consumption—increased concerns about public security and substantially reduced the incumbent party's probability of re-election in municipalities with their own police force. In contrast, longer-run homicide trends did not significantly influence voting behavior. Third, I substantiated the attention to news mechanism by demonstrating that vote choices were driven by access to local media outlets when voters consume most news. Fourth, I demonstrate that voters' beliefs are affected by recent homicides that occur before elections, but—contrary to theories based solely on cognitive constraints—are not impacted by equally-recent homicides that occurred at other points in the electoral cycle. Moreover, I find no evidence to suggest that these findings are driven by changes in the quantity or content of media coverage of homicides before elections or by complementarities with election campaigns.

While *access* to information supplied by the media may be necessary for electoral accountability, these findings show that understanding voters' news consumption patterns is just as important. Indeed, the complementarity between voters attention to news and news coverage of pre-election homicide spikes altered incumbent re-election rates in Mexican municipal elections—and in ways that deviate from a counterfactual scenario where voters were similarly responsive to longer-term homicide rates. The large effects align with media reporting on corruption (Banerjee et al. 2011; Ferraz and Finan 2008), although whether these large effect magnitudes extend beyond a context of rising crime or the particularly emotionally-charged issue of local violence warrants further investigation. Nevertheless, the coincidence of relevant news with voter attention may be critical for explaining mixed evidence that information provision affects electoral accountability in developing contexts (Dunning et al. 2019). As the rise of social media in the Global South changes how individuals encounter news, passive exposure to content curated by peers through Facebook or WhatsApp may only accentuate the significance of voter attention cycles. The need for citizens to regularly engage with politically-relevant information may then pose at least as much of a challenge as concerns about whether citizens can effectively process the information when they encounter it (Achen and Bartels 2017).

The reliance of poorly-informed voters on a few signals of incumbent performance before elections weaken accountability linkages in at least two ways that may reduce citizen welfare. First, reliance on indicators that provide little information about incumbent quality limits voters' capacity to retain higher-quality politicians or parties. Second, by restricting voters' monitoring capacity—and offering an alternative microfoundation for the Rogoff (1990) model of pre-election policy signaling that does not rely on evolving politician types—news consumption cycles create incentives for politicians to primarily serve voters' interests when attention to news is high, as other studies have found (Durante and Zhuravskaya 2018; Eisensee and Strömberg 2007; Kaplan, Spenkuch and Yuan 2019). Bobonis, Cámara Fuertes and Schwabe (2016) illustrate this concern, finding that politicians in Puerto Rico adjust their rent seeking in anticipation of audit reports that will be released before elections.

Addressing such challenges is critical for improving governance and economic development, especially where limited constraints on executive behavior in office exist (Khemani et al. 2016). Beyond ensuring that voters can access credible information sources, the attention mechanism driving my findings suggests several implications for policy. On the supply side, more informative news reports could aggregate signals of incumbent performance to contextualize pre-election events within broader trends. However, this treats voters' news consumption cycles as fixed. On the demand side, interventions could instead aim to increase citizen engagement with news outside election campaigns. Since my findings suggest that voters update their beliefs and adjust their voting behavior in sophisticated ways, increasing citizens' news consumption may be most pertinent. Whether attention can be increased through civic education, generating social expectations, nudging latent interest in news, or making news programming easier to consume is a key question for future research.

References

Achen, Christopher H. and Larry M. Bartels. 2017. *Democracy for realists: Why elections do not produce responsive government*. Princeton University Press.

Afzal, Madiha. 2007. "Voter rationality and politician incentives: Exploiting luck in Indian and Pakistani elections." Working paper.

Altamirano, Melina and Sandra Ley. 2020. "The Economy, Security, and Corruption in the 2018 Presidential Election Campaign Issues and Electoral Preferences in Mexico." *Política y gobierno* 27(2).

Anderson, Christopher J. 2007. "The end of economic voting? Contingency dilemmas and the limits of democratic accountability." *Annual Review Political Science* 10:271–296.

Arias, Eric, Horacio Larreguy, John Marshall and Pablo Querubín. 2022. "Priors Rule: When Do Malfeasance Revelations Help Or Hurt Incumbent Parties?" *Journal of the European Economic Association* 20(4):1433–1477.

Ashworth, Scott. 2005. "Reputational dynamics and political careers." *Journal of Law, Economics, and Organization* 21(2):441–466.

Ashworth, Scott, Ethan Bueno de Mesquita and Amanda Friedenberg. 2018. "Learning about voter rationality." *American Journal of Political Science* 62(1):37–54.

Baker, Andy, Barry Ames and Lucio R. Renno. 2006. "Social context and campaign volatility in new democracies: networks and neighborhoods in Brazil's 2002 Elections." *American Journal of Political Science* 50(2):382–399.

Banerjee, Abhijit V., Selvan Kumar, Rohini Pande and Felix Su. 2011. "Do Informed Voters Make Better Choices? Experimental Evidence from Urban India." Working paper.

Barabas, Jason, Jennifer Jerit, William Pollock and Carlisle Rainey. 2014. "The Question (s) of Political Knowledge." *American Political Science Review* 108(4):840–855.

Barro, Robert J. 1973. "The control of politicians: an economic model." *Public choice* 14(1):19–42.

Bateson, Regina. 2012. "Crime victimization and political participation." *American Political Science Review* 106(3):570–587.

Baum, Matthew A. 2002. "Sex, lies, and war: How soft news brings foreign policy to the inattentive public." *American Political Science Review* 96(1):91–109.

Bellows, John and Edward Miguel. 2009. "War and local collective action in Sierra Leone." *Journal of Public Economics* 93(11):1144–1157.

Berrebi, Claude and Esteban F. Klor. 2008. "Are voters sensitive to terrorism? Direct evidence from the Israeli electorate." *American Political Science Review* 102(3):279–301.

Besley, Timothy. 2006. *Principled Agents? The Political Economy of Good Government*. Oxford University Press.

Besley, Timothy and Robin Burgess. 2002. "The political economy of government responsiveness: Theory and evidence from India." *Quarterly Journal of Economics* 117(4):1415–1451.

Blattman, Christopher. 2009. "From violence to voting: War and political participation in Uganda." *American Political Science Review* 103(2):231–247.

Bobonis, Gustavo J., Luis R. Cámara Fuertes and Rainer Schwabe. 2016. "Monitoring Corruptible Politicians." *American Economic Review* 106(8):2371–2405.

Brender, Adi and Allan Drazen. 2005. "Political budget cycles in new versus established democracies." *Journal of Monetary Economics* 52(7):1271–1295.

Brollo, Fernanda. 2008. "Who Is Punishing Corrupt Politicians—Voters or the Central Government? Evidence from the Brazilian Anti-Corruption Program." Working paper.

Chang, Eric C.C., Miriam A. Golden and Seth J. Hill. 2010. "Legislative malfeasance and political accountability." *World Politics* 62(2):177–220.

Chen, Yuyu and David Y. Yang. 2019. "The Impact of Media Censorship: Evidence from a Field Experiment in China." *American Economic Review* 112(3):2294–2332.

Chong, Alberto, Ana De La O, Dean Karlan and Leonard Wantchekon. 2015. "Does Corruption Information Inspire the Fight or Quash the Hope? A Field Experiment in Mexico on Voter Turnout, Choice and Party Identification." *Journal of Politics* 77(1):55–71.

Cole, Shawn, Andrew Healy and Eric Werker. 2012. "Do voters demand responsive governments? Evidence from Indian disaster relief." *Journal of Development Economics* 97(2):167–181.

Coscia, Michele and Viridiana Rios. 2012. Knowing where and how criminal organizations operate using web content. In *Proceedings of the 21st ACM international conference on Information and knowledge management*. pp. 1412–1421.

Dell, Melissa. 2015. "Trafficking Networks and the Mexican Drug War." *American Economic Review* 105(6):1738–1779.

DellaVigna, Stefano, John A. List, Ulrike Malmendier and Gautam Rao. 2016. "Voting to tell others." *Review of Economic Studies* 84(1):143–181.

Delli Carpini, Michael X. and Scott Keeter. 1996. *What Americans Know about Politics and Why It Matters*.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. Addison Wesley.

Dunning, Thad, Guy Grossman, Macartan Humphreys, Susan Hyde, Craig McIntosh and Gareth Nellis. 2019. *Metaketa I: The Limits of Electoral Accountability*. Cambridge University Press.

Durán-Martínez, Angélica. 2015. "To kill and tell? State power, criminal competition, and drug violence." *Journal of Conflict Resolution* 59(8):1377–1402.

Durante, Ruben and Ekaterina Zhuravskaya. 2018. "Attack When the World Is Not Watching? U.S. News and the Israeli-Palestinian Conflict." *Journal of Political Economy* 126(3):1085–1133.

Durante, Ruben and Emilio Gutierrez. 2015. "Fighting crime with a little help from my friends: Party affiliation, inter-jurisdictional cooperation and crime in Mexico." Working paper.

Eifert, Benn, Edward Miguel and Daniel N. Posner. 2010. "Political Competition and Ethnic Identification in Africa." *American Journal of Political Science* 54(2):494–510.

Eisensee, Thomas and David Strömberg. 2007. "News droughts, news floods, and US disaster relief." *Quarterly Journal of Economics* 122(2):693–728.

Fair, Ray C. 1996. "Econometrics and presidential elections." *Journal of Economic Perspectives* 10(3):89–102.

Farnsworth, Stephen J. and S. Robert Lichter. 2011. *The Nightly News Nightmare: Media Coverage of US Presidential Elections, 1988-2008*. Rowman and Littlefield.

Fearon, James D. 1999. Electoral Accountability and the Control of Politicians: Selecting Good Types versus Sanctioning Poor Performance. In *Democracy, Accountability, and Representation*, ed. Adam Przeworski, Susan Stokes and Bernard Manin. Cambridge University Press.

Feddersen, Timothy and Alvaro Sandroni. 2006. "Ethical voters and costly information acquisition." *Quarterly Journal of Political Science* 1(3):287–311.

Ferejohn, John. 1986. "Incumbent performance and electoral control." *Public Choice* 50(1):5–25.

Ferraz, Claudio and Frederico Finan. 2008. "Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes." *Quarterly Journal of Economics* 123(2):703–745.

George, Lisa and Joel Waldfogel. 2003. "Who affects whom in daily newspaper markets?" *Journal of Political Economy* 111(4):765–784.

George, Lisa M. and Joel Waldfogel. 2006. "The New York Times and the market for local newspapers." *American Economic Review* 96(1):435–447.

Gerber, Alan S., Donald P. Green and Christopher W. Larimer. 2008. "Social pressure and voter turnout: Evidence from a large-scale field experiment." *American Political Science Review* 102(1):33–48.

Hamilton, James. 2004. *All the news that's fit to sell: How the market transforms information into news*. Princeton University Press.

Healy, Andrew and Gabriel S. Lenz. 2014. "Substituting the End for the Whole: Why Voters Respond Primarily to the Election-Year Economy." *American Journal of Political Science* 58(1):31–47.

Heinle, Kimberly, Octavio Rodríguez Ferreira and David A. Shirk. 2014. "Drug Violence in Mexico: Data and Analysis Through 2013." Justice in Mexico Project Special Report.

Hobbs, William R. and Margaret E. Roberts. 2018. "How sudden censorship can increase access to information." *American Political Science Review* 112(3):621–636.

Huber, Gregory A., Seth J. Hill and Gabriel S. Lenz. 2012. "Sources of bias in retrospective decision making: Experimental evidence on voters' limitations in controlling incumbents." *American Political Science Review* 106(4):720–741.

Humphreys, Macartan and Jeremy Weinstein. 2012. "Policing Politicians: Citizen Empowerment and Political Accountability in Uganda Preliminary Analysis." Working paper.

Johnson, Paul and Francisco Cantú. 2020. "The Nationalization of Mexican Parties." *Política y gobierno* 27(2).

Kaplan, Ethan, Jörg L Spenkuch and Haishan Yuan. 2019. "Natural disasters, moral hazard, and special interests in Congress." Working paper.

Keefer, Phillip. 2007. Seeing and Believing: Political Obstacles to Better Service Delivery. In *The Politics of Service Delivery in Democracies: Better Access for the Poor*, ed. Shantayanan Devarajan and Ingrid Widlund. Stockholm: Expert Group on Development Issues pp. 42–55.

Khemani, Stuti, Ernesto Dal Bó, Claudio Ferraz, Frederico Shimizu Finan, Corinne Louise Stephenson Johnson, Adesinaola Michael Odugbemi, Dikshya Thapa and Scott David Abrahams. 2016. "Making politics work for development: harnessing transparency and citizen engagement." World Bank Policy Research Reports.

Langston, Joy. 2003. "Rising from the ashes? Reorganizing and unifying the PRI's state party organizations after electoral defeat." *Comparative Political Studies* 36(3):293–318.

Larcinese, Valentino. 2005. "Electoral Competition and Redistribution with Rationally Informed Voters." *BE Journal of Economic Analysis and Policy* 4(1):1–28.

Larreguy, Horacio, Christopher Lucas and John Marshall. 2020. "When do Media Stations Support Political Accountability? A Field Experiment in Mexico." Working paper.

Larreguy, Horacio, John Marshall and James M. Snyder, Jr. 2018. "Leveling the playing field: How campaign advertising can help non-dominant parties." *Journal of the European Economic Association* 16(6):1812–1849.

Larreguy, Horacio, John Marshall and James M. Snyder, Jr. 2020. "Publicizing malfeasance: How local media facilitates electoral sanctioning of Mayors in Mexico." *Economic Journal* 130(10):2291–2327.

Le Pennec, Caroline and Vincent Pons. forthcoming. "How do Campaigns Shape Vote Choice? Multi-Country Evidence from 62 Elections and 56 TV Debates." *Quarterly Journal of Economics* .

Ley, Sandra. 2017. "Electoral Accountability in the Midst of Criminal Violence: Evidence from Mexico." *Latin American Politics and Society* 59(1):3–27.

Magaloni, Beatriz. 2006. *Voting for Autocracy: Hegemonic Party Survival and its Demise in Mexico*. New York: Cambridge University Press.

Marshall, John. 2019. "Signaling sophistication: How social expectations can increase political information acquisition." *Journal of Politics* 81(1):167–186.

Mohammad, Saif M. and Peter D. Turney. 2013. "Crowdsourcing a Word-Emotion Association Lexicon." *Computational Intelligence* 29(3):436–465.

Murray, Gregg R. and Richard E. Matland. 2014. "Mobilization effects using mail: Social pressure, descriptive norms, and timing." *Political Research Quarterly* 67(2):304–319.

Olken, Benjamin A. 2009. "Do television and radio destroy social capital? Evidence from Indonesian villages." *American Economic Journal: Applied Economics* 1(4):1–33.

Osorio, Javier. 2015. "The Contagion of Drug Violence: Spatiotemporal Dynamics of the Mexican War on Drugs." *Journal of Conflict Resolution* 59(8):1403–1432.

Panagopoulos, Costas. 2011. "Timing is everything? Primacy and recency effects in voter mobilization campaigns." *Political Behavior* 33:79–93.

Pande, Rohini. 2011. "Can informed voters enforce better governance? Experiments in low-income democracies." *Annual Review of Economics* 3(1):215–237.

Pérez, Orlando J. 2015. The impact of crime on voter choice in Latin America. In *Latin American Voter: Pursuing Representation and Accountability in Challenging Contexts*, ed. Ryan E. Carlin, Matthew M. Singer and Elizabeth J. Zechmeister. University of Michigan Press Ann Arbor, MI pp. 324–345.

Prat, Andrea and David Strömberg. 2005. "Commercial Television and Voter Information." CEPR Discussion Paper No. 4989.

Prior, Markus. 2019. *Hooked: How Politics Captures People's Interest*. Cambridge University Press.

Rogoff, Kenneth. 1990. "Equilibrium Political Budget Cycles." *American Economic Review* 80(1):21–36.

Sabet, Daniel. 2010. Police Reform in Mexico: Advances and Persistent Obstacles. In *Shared Responsibility: U.S.-Mexico Policy Options for Confronting Organized Crime*, ed. Eric L. Olson, David A. Shirk and Andrew Selee. Woodrow Wilson International Center for Scholars.

Sarafidis, Yianis. 2007. "What have you done for me lately? Release of information and strategic manipulation of memories." *Economic Journal* 117(518):307–326.

Sides, John, Lynn Vavreck and Christopher Warshaw. 2022. "The effect of television advertising in united states elections." *American Political Science Review* 116(2):702–718.

Snyder, Jr, James M. and David Strömberg. 2010. “Press Coverage and Political Accountability.” *Journal of Political Economy* 118(2):355–408.

Sobrino, Fernanda. 2020. “Mexican Cartel Wars: Fighting for the U.S Opioid Market.” Working paper.

Spenkuch, Jorg L. and David Toniatti. 2018. “Political Advertising and Election Outcomes.” *Quarterly Journal of Economics* 133(4):1981–2036.

Trejo, Guillermo and Sandra Ley. 2021. “High-profile criminal violence: Why drug cartels murder government officials and party candidates in Mexico.” *British Journal of Political Science* 51(1):203–229.

Trelles, Alejandro and Miguel Carreras. 2012. “Bullets and votes: Violence and electoral participation in Mexico.” *Journal of Politics in Latin America* 4(2):89–123.

Wolfers, Justin. 2007. “Are Voters Rational? Evidence from Gubernatorial Elections.” Working paper.

Yanagizawa-Drott, David. 2014. “Propaganda and conflict: Evidence from the Rwandan genocide.” *Quarterly Journal of Economics* 129(4):1947–1994.

Zaller, John. 1992. *The Nature and Origins of Mass Opinion*. Cambridge University Press.

A Appendix

Contents

A.1 Integrating news attention cycles into models of electoral accountability

This section extends two standard models of electoral accountability to derive the implications for the voting behavior of voters who cyclically pay attention to news. I first consider a career concerns model where voters seek to select the most competent incumbent (as in Ashworth 2005), before then considering a pure moral hazard model (as in Ferejohn 1986). Both models incorporate voter uncertainty—reflecting imprecise signals—about incumbent type or effort in equilibrium, and ultimately rationalize the core predictions described in Section 2 of the main paper.[1] I first lay out common primitives across the models.

A.1.1 Model primitives

Consider an incumbent party I seeking re-election in municipality m. For simplicity, there are two electoral terms $t \in \{1,2\}$, each containing J discrete periods (e.g. months) during which a noisy signal of the incumbent's underlying competence $\theta_{m,t}$ and/or effort $a_{m,t}$ can be observed by voters. The terms are split by an election, where the accumulated signals observed help a unit mass of voters to decide whether to re-elect incumbent party I or instead elect challenger party C. Parties are motivated to seek office to receive "ego" rents $R > 0$ derived from each term in office.

A signal $H_{m,t,j}$ is the homicide rate in the municipality during period $j = 1,...,J$ of term t, which voters can adduce in a given period by aggregating local media reports. Akin to the performance indicators analyzed by Ashworth (2005), this signal of incumbent competence and effort is given by equation (1), where $e_{m,t,j} \sim \mathcal{N}(0, 1/h_e^2)$ is an iid period-specific normally-distributed shock with precision h_e^2 whose distribution is common knowledge, $\eta_m > 0$ is the municipality's baseline homicide rate, and $\mu_{t,j}$ captures common period shocks. Intuitively, $H_{m,t,j}$ is greater for less competent and low-effort incumbents, but also reflects idiosyncratic factors beyond the incumbent's control. The distribution of incumbent competence is discussed as part of the specific models below, while the cost to I of a unit of effort $a_{m,t} \in [0,\infty)$ is $c(a_{m,t})$, where $c'(\cdot) > 0$, $c''(\cdot) > 0$, and $c(0) = 0$. Such effort could involve allocating more resources to policing, selecting a competent or incorruptible chief of police, or requiring municipal police to work with state of federal security forces. The assumption that I cannot respond to homicide rate realizations is appropriate when the homicide rate is not immediately responsive to I's effort, as in the empirical context studied below.[2]

To capture news attention cycles, I assume voters consume only media reports during the last $N \leq J$ periods before the election. The discussion in the main paper provides various microfoundations for this assumption, while Table 2 demonstrates that a significant share of voters indeed substantially increase their news consumption in the months immediately preceding Mexico's local elections. This suggests that $N \ll J$ for many voters. The following subsections derive the consequences of imposing this assumption for electoral accountability.

[1] Signaling models, such as Rogoff (1990), can also generate similar predictions. For example, if exogenous homicide shocks create an opportunity for high-quality parties to distinguish themselves (e.g. by responding effectively to homicides), then such shocks will help voters to identify high-quality parties. This would reduce the probability of re-election for the average incumbent party if voters supported the incumbent party in the absence of a signal.

[2] See Afzal (2007), Besley (2006) or Besley and Burgess (2002) for models where incumbents respond to signals.

A.1.2 Career concerns model

In the career concerns model, each incumbent and challenger party is ascribed a fixed level of competence $\theta_{m,p}$, where $p \in \{I, C\}$. This is drawn by nature from normal distribution $\mathcal{N}(\bar{\theta}_m, 1/h_\theta)$, where the realization is known neither to parties nor voters. The competence of the party in office in term t is denoted by $\theta_{m,t}$; this is always $\theta_{m,I}$ in term $t = 1$. At the beginning of each term, I can exert effort $a_{m,t}$ to reduce the homicide $H_{m,t,j}$, in order to convince voters that they are sufficiently competent to merit re-election. Looking forward, I maximizes $\delta p(a_{m,t})R - c(a_{m,t})$, where $p(a_{m,t})$ is the probability of re-election and payoffs in the next term are discounted by $\delta \in (0,1)$.

In this model, voters vote for their preferred party for the second term (given the lack of re-election incentives at $t = 2$), based on the sum of their beliefs about the incumbent party's competence and their "ideological" bias, and a common shock. All voters value competence equally, either directly to maintain security, or to address other issues that the ability to reduce homicides may be correlated with. Since $\theta_{m,t}$, $a_{m,t}$, and $e_{m,t,j}$ are unobservable, voters draw inferences about the incumbent's competence $\theta_{m,t}$ from media reports revealing $H_{m,t,j}$. Voters accordingly update their posterior beliefs about incumbent competence from signals $H_{m,t,J-N}, ..., H_{m,t,J}$. The uniformly-distributed ideological bias $\sigma_i \sim \mathscr{U}(B_m - \frac{1}{2\psi_m}, B_m + \frac{1}{2\psi_m})$ of voter i toward I is realized after $a_{m,t}$ is selected, where $B_m \in \mathbb{R}$ is the expected bias toward the incumbent. Each voter i determines their vote choice $v_{i,m} \in \{I, C\}$ by comparing the expected utility of voting for I and C.

The career concerns game's timing can be summarized as:

1. At the beginning of term $t = 1$, the incumbent chooses $a_{m,1}$.
2. Nature draws party competences, $\theta_{m,I}$ and $\theta_{m,C}$, and sets $\theta_{m,1} = \theta_{m,I}$. These realizations are not known to parties or voters.
3. Each voter observes the last N independent homicide count realizations before the election, $H_{m,1,1+J-N}, ..., H_{m,1,J}$.
4. Each voter's ideological bias σ_i is realized, and each voter decides whether to re-elect the incumbent party or not, $v_{i,m} \in \{I, C\}$. The party with the majority of votes wins.
5. At the beginning of $t = 2$, the election winner chooses $a_{m,2}$.
6. Each voter observes the last N independent homicide count realizations, $H_{m,2,1+J-N}, ..., H_{m,2,J}$.

Given the lack of re-election incentives, any incumbent party in the second term ($t = 2$) exerts effort $a^*_{m,2} = 0$. At the election concluding the first term ($t = 1$), voter i then votes for I when the sum of I's expected competence and i's partisan bias exceed C's expected competence:

$$\mathbb{E}[\theta_{m,I} | H_{m,1,1+J-N}, ..., H_{m,1,J}] + \sigma_i \geq \mathbb{E}[\theta_{m,C}]. \tag{A1}$$

Upon observing the homicide rate in the last N periods, i's posterior belief about I's competence is normally distributed with expectation:

$$\mathbb{E}[\theta_{m,I} | H_{m,1,1+J-N}, ..., H_{m,1,J}] = \lambda(N)[\eta_m - \hat{a}_{m,1} - \overline{H}_{m,1}(N)] + [1 - \lambda(N)]\bar{\theta}_m, \tag{A2}$$

where $\hat{a}_{m,1}$ is the voter's conjecture about incumbent effort and $\lambda(N) := \frac{Nh_e^2}{h_\theta^2 + Nh_e^2}$ is the weight attached to the mean observed signal within term t, $\overline{H}_{m,t}(N) := \frac{1}{N}\sum_{j=1+J-N}^{J} H_{m,t,j}$. Rearranging, voter i then votes to re-elect I if the observed homicide rate is sufficiently low: $\overline{H}_{m,1}(N) \leq \frac{\sigma_i}{\lambda(N)} + \eta_m - \bar{\theta}_m - \hat{a}_{m,1}$. Aggregating across voters, the incumbent party's vote share is:

$$V_{m,I}(\overline{H}_{m,1}(N)|a_{m,1}) = \frac{1}{2} + \psi_m B_m + \psi_m \lambda(N) \left[\theta_{m,I} - \bar{\theta}_m + a_{m,1} - \hat{a}_{m,1} - \frac{1}{N} \sum_{j=1+J-N}^{J} e_{m,1,j} \right]. \quad \text{(A3)}$$

The incumbent party's vote share then reflects the average ideological bias B_m and voter inferences about the incumbent's competence—and thus future performance—based on the homicide rate (after filtering out the baseline homicide level η_m and incumbent effort $a_{m,1}$).

At the beginning of the first term, the incumbent chooses their effort to reduce the homicide rate in anticipation of this voting rule. Given the ex ante re-election probability (i.e. before homicide rates and ideological shocks are realized), I solves:

$$a^*_{m,1} = \underset{a_{m,1} \in [0,\infty)}{\text{argmax}} \ \Phi \left(\frac{\frac{B_m}{\lambda(N)} + a_{m,1} - \hat{a}_{m,1}}{h} \right) \delta R - c(a_{m,1}) = \max \left\{ 0, (c')^{-1} \left(\frac{\phi\left(\frac{B_m}{h\lambda(N)}\right) \delta R}{h} \right) \right\} \quad \text{(A4)}$$

where $h := \sqrt{\frac{1}{h_\theta^2} + \frac{N}{h_e^2}}$, $\Phi(\cdot)$ and $\phi(\cdot)$ are the normal cumulative distribution and probability density functions, and voters' expectations of effort are correct in equilibrium ($\hat{a}_{m,1} = a^*_{m,1}$).[3]

The following proposition summarizes the central empirical implications for voting behavior:

Proposition 1. *For any $\theta_{m,I}$ and $a^*_{m,1}$ in the career concerns game, incumbent party I is re-elected if $\overline{H}_{m,1}(N) \leq \frac{B_m}{\lambda(N)} + \eta_m - \bar{\theta}_m - a^*_{m,1}$ and receives vote share $V_{m,I}(\overline{H}_{m,1}(N)|a^*_{m,1})$. In such an equilibrium:*

1. *The incumbent party's probability of victory and vote share decreases in $\overline{H}_{m,1}(N)$.*

2. *As N becomes large, incumbent party electoral prospects are, on average, unaffected by idiosyncratic shocks.*

Proof: follows from above analysis and differentiating the re-election condition and $V_{m,I}$. ■

The proposition establishes the core electoral predictions tested in the main paper. First, voters cast their ballots on the basis of observed homicides in an effort to select competent politicians. Given that voters cannot distinguish the components of the signal that reflect incumbent competence and idiosyncratic shocks, the decision rule not to retain incumbent parties that oversee sufficiently high homicide rates inevitably depends on the random realizations of the idiosyncratic shock. Second, voters' inferences improve as they receive more signals to the point that—in the limit, where $\lim_{N\to\infty} \frac{1}{N} \sum_{j=1+J-N}^{J} e_{m,1,j} = 0$ and $\lim_{N\to\infty} \lambda(N) = 1$—they can perfectly parse the incumbent party's competence $\theta_{m,I}$ from the many signals received and rely entirely on this in forming their posterior beliefs. Combining these implications, the model predicts that the voting behavior of voters who can aggregate across many homicide signals no longer depends on idiosyncratic shocks. In contrast, the voters who consume relatively few signals of incumbent performance have no choice but to partly rely on homicides as a noisy signal of incumbent competence, especially when they are relatively uninformed and non-partisan.

[3] The second-order condition holds when: $\delta R < \left(\frac{1}{h_\theta^2} + \frac{N}{h_e^2} \right) \frac{c''(a^*_{m,1})}{\phi'\left(\frac{B_m}{h\lambda(N)}\right)}$.

A.1.3 Pure moral hazard model

In a pure moral hazard model, $\theta_{m,p}$ is constant across parties (and can thus be incorporated into η_m). Because voters are required to commit to implementing a re-election threshold rule, I abstract from ideological biases to consider a representative voter seeking to minimize the homicide rate in the first period (given their lack of control over identical politicians in the second period).[4]

The timing of the pure moral hazard game is as follows:

1. At the beginning of term $t=1$, the incumbent chooses $a_{m,1}$.
2. The representative voter observes the last N independent homicide count realizations before the election, $H_{m,1,1+J-N},...,H_{m,1,J}$.
3. The representative voter decides whether to re-elect the incumbent party or not, $v_m \in \{I,C\}$.
4. At the beginning of $t=2$, the election winner chooses $a_{m,2}$.
5. Each voter observes the last N independent homicide count realizations, $H_{m,2,1+J-N},...,H_{m,2,J}$.

Since the representative voter anticipates that any incumbent in period $t=2$ will exert effort $a^*_{m,2}=0$, they select a re-election threshold $\hat{H}$ to minimize agency loss in period $t=1$. This requires anticipating that the incumbent will select $a_{m,1}$ to solve:

$$a^*_{m,1} = \underset{a_{m,1}\in[0,\infty)}{\text{argmax}} \left\{ \Pr[\overline{H}_{m,1}(N) \leq \hat{H}]\delta R - c(a_{m,1}) \right\}. \tag{A5}$$

The incumbent's first-order condition (at an interior optimum) implicitly characterizing $a^*_{m,1}(\hat{H})$ is then given by:[5]

$$\phi\left(\frac{\hat{H}+a^*_{m,1}(\hat{H})-b}{\sqrt{\frac{N}{h_e^2}}}\right)\sqrt{\frac{h_e^2}{N}}\delta R = c'\left(a^*_{m,1}(\hat{H})\right). \tag{A6}$$

Voters then choose $\hat{H}$ to ensure that $\phi(0)$, as this maximizes the standard Normal density function ϕ and thus $a_{m,1}$. Bringing together these insights yields the following result:

Proposition 2. *In the pure moral hazard game, the incumbent party I is re-elected if $\overline{H}_{m,1}(N) \leq \hat{H}^*$, where $\hat{H}^* = \eta_m - (c')^{-1}\left(\phi(0)\sqrt{\frac{h_e^2}{N}}\delta R\right)$ and $a^*_{m,1} = (c')^{-1}\left(\phi(0)\sqrt{\frac{h_e^2}{N}}\delta R\right)$. In equilibrium, voters then re-elect I when $\frac{1}{N}\sum_{j=1+J-N}^{J} e_{m,t,j} < 0$.*

Proof: follows from above analysis. ■

This proposition yields a similar decision rule for the representative voter as the heterogeneous voters in the career concerns model. Specifically, voters re-elect the incumbent party if the homicide

[4] The results hold for any level of partisan bias B_m among the representative voter.

[5] It is assumed that the second-order condition holds, such that: $\phi'\left(\frac{\hat{H}+a^*_{m,1}(\hat{H})-b}{\sqrt{\frac{N}{h_e^2}}}\right)\frac{h_e^2}{N}\delta R - c''\left(a^*_{m,1}(\hat{H})\right) < 0$.

rate is sufficiently low. Since the homicide rate reflects a combination of unobserved effort and idiosyncratic shocks, voting behavior in part reflects random realizations of the shocks. Due to the simplified setting of this model where voters lack other motivations for re-electing the incumbent party, voters' prior beliefs and the number of signals consumed do not affect voting behavior. Extensions to more complex environments, however, are likely to generate analogous predictions to the career concerns model.

A.2 Data description

A.2.1 Months and years of municipal elections

Table A1 lists the municipal elections potentially covered by the survey and aggregate elections in the main analysis. In Chiapas, Coahuila, Guerrero, Michoacán, Quintana Roo, Veracruz, and Yucatán, the typical 3-year cycle was adjusted during the sample period by switching to a 2- or 4-year term for a single, usually transitional, electoral cycle. Following a constitutional amendment in 2007, states were subsequently to hold local elections on the same day as federal elections when the state cycle coincides. Consequently, states also changed the month of their elections. Some states holding elections off the federal cycle also altered the year of their elections after the reform.

A.2.2 ENCUP survey data

The ENCUP surveys (www.encup.gob.mx/en/Encup/Encup) were commissioned by the Interior Ministry and designed to be nationally representative, and focus on the country's political culture rather than more contentious questions about elections.[6] Each survey wave 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.[7]

The ENCUP variables used for the main analyses are described below:

- *Watch and listen to news and political programs ever/at least once a week*. Indicator coded 1 for a respondent that answered that they watched or listened to news or programs about politics or public affairs ever/at least once a week. ("¿Qué tan seguido escucha noticias o ve programas sobre política o asuntos públicos?") This question was not asked in the 2001 survey.

- *Watch and listen to news and political programs scale*. 5-point scale from 0 to 4, with values corresponding to levels (never, ever, monthly, weekly, daily) of watching and listening to new or programs about politics or public affairs (in ascending order). This question was not asked in the 2001 survey.

- *Topical political knowledge index (standardized)*. 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

[6] The specific objectives of the study, which does not address elections at all, are enumerated here.

[7] 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 appears to be similar.

Table A1: Mexican municipal election months, 1999-2012, by state

State	Election dates
Aguascalientes	*August 2001*, *August 2004*, August 2007, July 2010.
Baja California	*July 2001*, *August 2004*, *August 2007*, July 2010.
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.
Coahuila	September 1999, September 2002, September 2005, October 2009.
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.
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, November 2007, November 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	October 2001, October 2004, October 2007, July 2010.
Puebla	November 2001, November 2004, November 2007, July 2010.
Querétaro	July 2000, July 2003, July 2006, July 2009, July 2012.
Quintana Roo	*February 1999*, **February 2002**, February 2005, February 2008, July 2010.
San Luis Potosí	July 2000, October 2003, July 2006, July 2009, July 2012.
Sinaloa	*October 2001*, November 2004, October 2007, July 2010.
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.
Tlaxcala	*November 2001*, November 2004, November 2007, July 2010.
Veracruz	*September 2000*, September 2004, September 2007, July 2010.
Yucatán	May 2001, May 2004, May 2007, May 2010, July 2012.
Zacatecas	July 2001, July 2004, July 2007, July 2010.

Notes: Emboldened elections are counted as upcoming local elections in the survey analysis, i.e. ENCUP surveys were conducted within 4 months of local election. Some state-level elections were held without concurrent municipal elections in Hidalgo in all election years, in Coahuila in 2008, in Oaxaca in 2007, and in San Luis Potosí in 2003, and are not 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 of Mexico City); except in the cases of Baja California 2001 and 2004 (where the precinct numbering changed across elections and thus cannot be matched), missingness reflects the fact that data from the preceding election required to define the change in vote share was not available. Off-cycle extraordinary elections are included in the sample, but these few elections are not noted in the table.

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)

- *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; respondents that listed no problem are coded as 0. This question was not asked in the 2012 survey.

- *Upcoming local election (4/2 months)*. Indicator coded 1 for respondents living in a state or municipality with an upcoming local election occurring within 4/2 months of the survey being conducted.

- *Pre-survey homicide shock*. This indicator is coded 1 if the number of homicides in the two months prior to the month of the survey (or the survey month and the following month if the survey was conducted within the first 5 days of the month in 2005, the only wave for which the exact day of the survey is reported) exceed those in the two months immediately after the month of the survey (or the preceding month and survey month itself if the survey was conducted within the last 5 days of the month), based on the INEGI monthly homicide statistics for the occurrence of homicides among a municipality's residents.

- *Average monthly homicide rate (last 12 months)*. Average monthly homicide rate per 100,000 people within the municipality (using INEGI intentional homicide statistics, normalized by the municipal 2010 Census population) over the 12 months preceding the survey (excluding the current month).

- *Average monthly homicide rate (mayor's term)*. Average monthly homicide rate per 100,000 people within the municipality (using INEGI intentional homicide statistics, normalized by the municipal 2010 Census population) over the incumbent party's tenure in office preceding the survey (excluding the current month).

A.2.3 Municipal- and precinct-level homicide and electoral data

The main analyses use the following variables, which derive from electoral data obtained from State Electoral Institutes and homicide data obtained from the INEGI (www.inegi.org.mx):

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

- *Incumbent party vote share*. The precinct-level share of all votes cast for the incumbent at the current municipal election. 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 vote share (share of registered voters)*. 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).

- *Turnout*. The precinct-level turnout rate between the current municipal election and the prior municipal election.
- *Homicide shock*. Defined in equation (3) of the main paper, using INEGI homicide statistics for intentional homicides that occurred in each month to residents of a given municipality. One-, three-, and four-month versions are similarly defined.
- *Pre-election monthly homicide rate*. The average monthly homicides rate per 100,000 people (based on the 2010 municipal population), as registered as occurring in the given municipality by INEGI, in the 60 days before election day.
- *Average monthly homicide rate (last 12 months before election/mayor's term)*. Average number of residents per 100,000 (based on the 2010 municipal population) suffering a homicide per month within the municipality in the 12 months prior to the municipal election/over the mayor's term up until the election, again based on INEGI homicide data.
- *Deviation from average monthly homicide rate*. Difference between the average monthly homicide rate per 100,000 people that occurred in the 2 months before a municipal election and the average monthly homicide rate per 100,000 people that occurred in the 10/34 months preceding that.
- *Proportional deviation from average monthly homicide rate*. Proportional difference between the average monthly homicide rate per 100,000 people that occurred in the 2 months before a municipal election and the average monthly homicide rate per 100,000 people that occurred in the 10/34 months preceding that.
- *AM/FM/TV incumbent party ad share*. Fraction of ads from AM/FM/TV media outlets that cover the precincts that were allocated to the incumbent party in the 2009 and 2012 elections.

A.2.4 Precinct local media coverage data

The main analyses use the following additional variables, which derive from the IFE media outlet coverage data (for the most recent data from the INE, see http://pautas.ine.mx/transparencia/mapas):

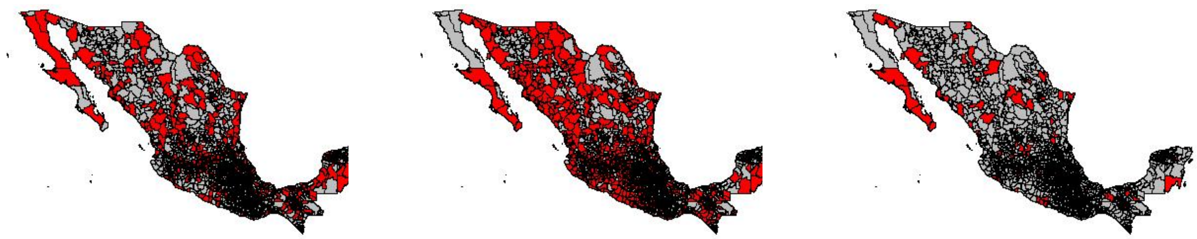
- *Local media*. Number of AM radio, FM radio or television stations, with an emitter based in the precinct's municipality, covering at least 20% of the precinct population (as defined by detailed population data—block-level population in urban areas, and rural locality locations).
- *Non-local media*.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

A.2.5 Map of municipalities included in different samples

In separate subfigures, Figure A1 shades in red the municipalities that appear at least once in each of the main empirical analyses—the ENCUP survey sample, the municipal-level pre-election homicide shock sample, and the precinct-level local media sample.

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

(a) Municipalities in the ENCUP survey samples | (b) Municipalities in the homicide shock sample | (c) Municipalities in the local media neighbor sample



A.2.6 Google articles on homicides

The dataset of news reporting on homicides in Mexico was created according to the following steps:

1. Google Mexico was queried for results for the search term "*x*" on a particular date, with results restricted to those from Mexico. Google was queried because it produced more news article results than restricting the search to Google News. The set of query terms is $X = \{$homicidio, homicida, asesinato, asesinar, asesinado, asesino, asesinaron, matar, matado, mataron, mato, matanza, masacre, masacrado, masacrar, masacro, masacraron, decapitacion, decapitar, decapitado, decapito, decapitaron, estrangulamiento, estrangular, estrangulado, estrangulo, estrangularon, degollacion, degollar, degollado, degollado, degollo, degollaron, ejecuciones, patricidio, patricida, matricidio, matricida, fratricidio, fratricida, feminicidio$\}$. For example, a query of "homicidio" on May 22, 2015 was:

   ```
   https://www.google.com.mx/search?q=homicidio+after:2015-05-22
   +before:2010-05-23+&num=9&start=10&cr=countryMX
   ```

   For each query, up to 10 urls were extracted from the html of each page of results (some queries produced many pages). Queries were staggered by 55 seconds to avoid being blocked.

2. After extracting the urls for all queries, the title, text, and date of each hit was obtained from the html of the corresponding url. All urls from non-Mexican websites were dropped.

3. The text associated with each url was split into paragraphs. Accents were removed and all letters were set to lower case.

4. An article is coded as referring to a homicide in municipality m if there exists at least one paragraph in the article that includes both a homicide term $x \in X$ *and* a reference to $m \in M$. Municipality $m \in M$ is tagged if a paragraph contains the exact text of its full or abbreviated name *and* it satisfies a sequence of filters designed to prevent misclassification (due to common names or words).

5. The final dataset collapses articles by municipality and month to yield a count of the number of articles referencing a homicide in a given month in each municipality.

The following variables are then used for the media reporting analysis:

- *Number of homicide reports recovered by Google per month.* The number of online news articles classified as reporting a homicide in a given municipality in the month.
- *Homicides per month.* The number of intentional homicides that occur in a given municipality in the month, using the INEGI data described above.

To assess whether the "quality" of articles about homicides changes before elections, I consider five additional features of the articles that report on homicides (after scrubbing computer code, accents, and punctuation from the text). Specifically, in addition to mentioning that a homicide occurred in a particular municipality, I classify these articles in terms of: (i) mentioning municipal governments if at least one of the following words/word stems (lower case strings) appears once anywhere in the article: "alcaldia", "gobierno municip", "concejo municipal", "regidor", "sindic", or "cabildo"; (ii) mentioning municipal mayors if at least one of the following words/word stems (lower case strings) appears once anywhere in the article: "president municip", " alcalde ", " alcaldesa ", " regente "; (iii) mentioning municipal police forces if at least one of the following words/word stems (lower case strings) appears once anywhere in the article: "policia municipal", "policiales municipales", "policia local", or "policiales locales"; (iv) having more than 500 words (including stop words); (v) having more than 1,000 words (including stop words); (vi) possessing a negative sentiment if the mean sentiment score of the words in the article (after removing stop words and numbers) is below -0.05; (vii) possessing a very negative sentiment if the mean sentiment score of the words in the article (after removing stop words and numbers) is below -0.1; (viii) possessing a positive sentiment if the mean sentiment score of the words in the article (after removing stop words and numbers) is above 0.05; and (ix) possessing a very positive sentiment if the mean sentiment score of the words in the article (after removing stop words and numbers) is above 0.1. The sentiment score of articles is coded based on a list of around 14,000 Spanish words scored by Canada's National Research Council using crowdsourcing (Mohammad and Turney 2013); the word-specific sentiment scores range from -1 to 1.

A.3 The timing of citizen Google searches

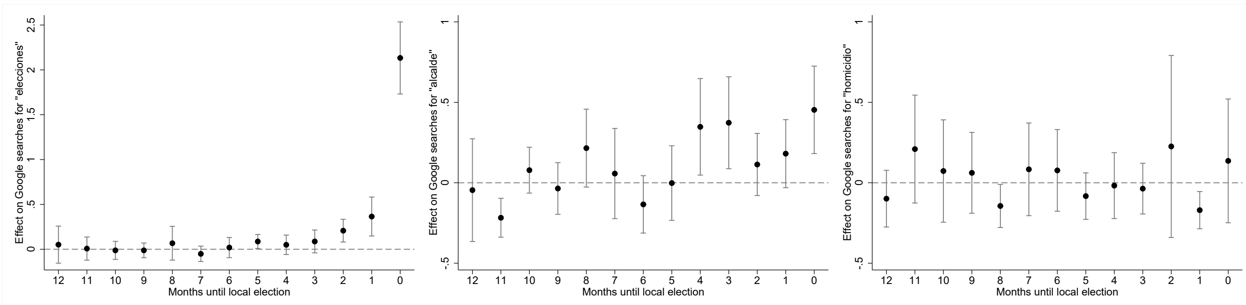
Figure A2 show how the timing of citizen Google searches for the themes "elecciones", "alcalde" (mayor), and "homicidio" vary over the electoral cycle. The point estimates are based on a difference-in-differences regression including state and year-month fixed effects. Identification comes from the fact that states hold local elections in different years and months within election years. The results suggest that, relative to the baseline of of the years preceding the 12 months before an election, interest in elections and local government jumps significantly in the 4-5 months preceding a state's local (i.e. state and municipal) elections. In contrast, searches for homicides are not affected.

A.4 Evidence supporting the identification assumptions

Tables A2-A6 report 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, municipal, and state characteristics in the ENCUP survey data, while Table A3 shows that homicides are well-balanced in the ENCUP surveys; Tables A4 and A5 show that homicide shocks are well

Figure A2: Google search themes, by month until local election (95% confidence intervals)

(a) “Elecciones” (b) “Alcalde” (c) “Homicidio”



Notes: All estimates are from a regression of state-level search intensity on indicators for the number of months until the next local election and state and year-month fixed effects. The baseline category is months more than 12 months before the election. Block-bootstrapped standard errors clustered by state (10,000 replications).

balanced across a wide variety of covariates at both the municipal- and precinct-level, where municipality fixed effects are excluded for time-invariant variables (see table footnotes for details); and Table A6 shows that the number of local media stations is well-balanced across the covariates vary within municipalities or over time, as well as some additional covariates germane to spatial analysis.[8] For each level of analysis, the robustness checks in the main show that all findings are robust to adjusting for imbalanced covariates. This is important because some imbalances—such as education in the ENCUP surveys, alignment with Governor in the municipal data, or proxies for wealth in the local media data—could have spuriously generated the main results. Unreported balance tests further confirm that homicide shocks remain well-balanced within the ENCUP and local media samples.

I also conduct several design-specific exogeneity checks. First, Table A8 shows that upcoming local elections, pre-survey homicide shocks, an indicator of a homicide having occurred in the prior month, and their interactions do not predict municipality's inclusion in the ENCUP in any given year over time. Second, Figure A3 shows that, relative to the week up to election day, homicides were generally no more likely to occur in the other weeks before or after election day within the 120-day window used to define homicide shocks. Third, columns (1)-(4) of Table A9 show that election outcomes are uncorrelated with homicide counts in the two months after elections. Columns (5)-(8) find no significant correlation between the identity of the winning party and the post-election homicide count, either throughout the sample or during the Calderón administration.

A.5 Predicting future homicide rates

My theoretical framework assumes that longer-term homicide rates are more precise indicators than very recent homicides of incumbent party competence. If this assumption holds, longer-term homicides rates should be better predictors of future homicide rates under the same government, since future homicide rates are assumed to be affected by the incumbent party's competence level.

To validate this implication of the model, I restrict attention to incumbent parties that get re-elected. I then estimate the predictive power of longer-term and recent homicides using the follow-

[8]For the local media balance tests, I estimate equation (6) without the interaction with the homicide shock.

Table A2: Balance of upcoming local elections in the ENCUP surveys over 36 individual, municipal, and state variables

	Female (1)	Age (2)	Education (5-point scale) (3)	Employed (4)	Own economic situation (5)	Number of org. memberships (6)	Member of labor union (7)	Org.s talk about politics (8)	Number of group meetings (9)	Discuss community problems (10)	Turnout president since 2000 (11)	Turnout mayor since 2000 (12)
Upcoming local election (4 months)	-0.013	-1.449	0.252**	-0.066	-0.112	0.009	-0.176	-0.023	0.066	-0.025	-0.006	-0.011
	(0.065)	(1.259)	(0.102)	(0.054)	(0.165)	(0.052)	(0.201)	(0.084)	(0.287)	(0.101)	(0.045)	(0.066)
Observations	15,976	15,976	11,747	11,756	12,322	15,976	15,976	12,576	15,976	12,576	15,976	15,976
Unique states	31	31	31	31	31	31	31	31	31	31	31	31
R^2												
Outcome mean	0.55	40.76	1.70	0.47	1.69	0.09	1.06	0.26	1.52	0.70	0.77	0.73
Outcome std. dev.	0.50	15.50	1.20	0.50	0.74	0.28	1.84	0.44	1.91	0.72	0.42	0.44
Upcoming local election mean	0.06	0.06	0.03	0.03	0.07	0.06	0.06	0.07	0.06	0.07	0.06	0.06

	Municipal registered voters (13)	Media outlets in municipality (14)	Municipal share economically active (15)	Municipal share indigenous speakers (16)	Municipal average years of schooling (17)	Municipal share illiterate (18)	Municipal average occupants per dwelling (19)	Municipal share with elec., water, and drainage (20)	Municipal share with a cell phone (21)	Municipal share internet (22)	Total municipal spending (last year) (23)	Municipal police per voter (24)
Upcoming local election (4 months)	163151.689	-6.472	0.014	-0.010	0.688*	-0.025	0.023	0.063	0.037	0.036	161.051	-0.053
	(142553.708)	(5.279)	(0.016)	(0.125)	(0.418)	(0.021)	(0.154)	(0.058)	(0.067)	(0.051)	(238.114)	(1.271)
Observations	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	10,668	13,792
Unique states	31	31	31	31	31	31	31	31	31	31	20	31
R^2												
Outcome mean	247810.24	10.42	0.39	0.07	8.37	0.08	3.99	0.80	0.63	0.19	803.50	2.29
Outcome std. dev.	311511.07	11.82	0.05	0.18	1.61	0.07	0.37	0.20	0.20	0.13	1149.50	2.45
Upcoming local election mean	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.08	0.04

	Pre-survey homicide shock (25)	Ave. monthly homicide rate (last 12 months) (26)	Ave. monthly homicide rate (mayor's term) (27)	PAN incumbent mayor (28)	PRD incumbent mayor (29)	PRI incumbent mayor (30)	Incumbent win margin (lag) (31)	Incumbent won (lag) (32)	Municipal turnout (lag) (33)	PAN incumbent governor (34)	PRD incumbent governor (35)	PRI incumbent governor (36)
Upcoming local election (4 months)	0.107	0.376	0.374	0.011	0.121	-0.106	-0.016	0.111***	0.004	-0.336***	-0.056	0.393
	(0.177)	(0.338)	(0.357)	(0.170)	(0.169)	(0.240)	(0.024)	(0.041)	(0.041)	(0.112)	(0.319)	(0.289)
Observations	12,974	15,941	15,941	15,688	15,688	15,688	15,976	15,833	15,778	15,896	15,896	15,896
Unique states	31	31	31	31	31	31	31	31	31	31	31	31
R^2												
Outcome mean	0.41	1.02	1.00	0.38	0.11	0.49	0.15	0.54	0.55	0.29	0.08	0.61
Outcome std. dev.	0.49	1.84	1.59	0.48	0.31	0.50	0.15	0.50	0.11	0.45	0.27	0.49
Upcoming local election mean	0.07	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06

Notes: All specifications include survey wave fixed effects, and are estimated using OLS. The individual-level variables in columns (1)-(12) are taken from the ENCUP survey. The municipal and state variables in columns (13)-(36) are derived from the 2010 Census, electoral data from state electoral institutes, the IFE's media data, or the INEGI's budget, policing, and homicide datasets. Block-bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Balance of pre-survey homicide shocks in the ENCUP surveys over 36 individual, municipal, and state variables

	Female (1)	Age (2)	Education (5-point scale) (3)	Employed (4)	Own economic situation (5)	Number of org. memberships (6)	Member of labor union (7)	Org.s talk about politics (8)	Number of group meetings (9)	Discuss community problems (10)	Turnout president since 2000 (11)	Turnout mayor since 2000 (12)
Homicide shock	-0.007	0.293	0.000	-0.000	0.016	0.004	0.035	-0.013	0.013	-0.038	-0.019	-0.008
	(0.008)	(0.300)	(0.053)	(0.012)	(0.025)	(0.008)	(0.064)	(0.015)	(0.070)	(0.025)	(0.013)	(0.013)
Observations	12,974	12,974	9,734	9,743	9,842	12,974	12,974	10,024	12,974	10,024	12,974	12,974
Unique states	386	386	303	303	295	386	386	295	386	295	386	386
R^2	0.01	0.01	0.00	0.00	0.01	0.02	0.08	0.07	0.08	0.08	0.02	0.01
Outcome mean	0.55	40.61	1.78	0.47	1.69	0.09	1.05	0.26	1.46	0.69	0.76	0.72
Outcome std. dev.	0.50	15.38	1.21	0.50	0.75	0.29	1.85	0.44	1.89	0.72	0.43	0.45
Upcoming local election mean	0.41	0.41	0.41	0.41	0.38	0.41	0.41	0.38	0.41	0.38	0.41	0.41
	Municipal registered voters (13)	Media outlets in municipality (14)	Municipal share economically active (15)	Municipal share indigenous speakers (16)	Municipal average years of schooling (17)	Municipal share illiterate (18)	Municipal average occupants per dwelling (19)	Municipal share with elec., water, and drainage (20)	Municipal share with a cell phone (21)	Municipal share internet (22)	Total municipal spending (last year) (23)	Municipal police per voter (24)
Homicide shock	-17815.322	-2.744**	-0.005	-0.011	-0.147	0.005	0.021	-0.004	-0.031*	-0.017	-18.092	-0.061
	(29353.314)	(1.258)	(0.004)	(0.011)	(0.126)	(0.005)	(0.025)	(0.015)	(0.016)	(0.011)	(142.081)	(0.111)
Observations	12,974	12,974	12,974	12,974	12,974	12,974	12,974	12,974	12,974	12,974	9,072	11,020
Unique states	386	386	386	386	386	386	386	386	386	386	251	361
R^2	0.01	0.02	0.01	0.00	0.01	0.00	0.00	0.00	0.01	0.01	0.12	0.01
Outcome mean	299108.35	12.46	0.40	0.06	8.66	0.07	3.96	0.83	0.67	0.22	935.27	2.18
Outcome std. dev.	324395.98	12.14	0.04	0.16	1.50	0.07	0.34	0.19	0.18	0.12	1198.78	1.30
Upcoming local election mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.43	0.42
	Upcoming local election (25)	Ave. monthly homicide rate (last 12 months) (26)	Ave. monthly homicide rate (mayor's term) (27)	PAN incumbent mayor (28)	PRD incumbent mayor (29)	PRI incumbent mayor (30)	Incumbent win margin (lag) (31)	Incumbent won (lag) (32)	Municipal turnout (lag) (33)	PAN incumbent governor (34)	PRD incumbent governor (35)	PRI incumbent governor (36)
Homicide shock	0.027	0.176	0.215	-0.042	0.027	0.010	0.001	0.003	0.008	-0.054	0.029	0.039
	(0.029)	(0.123)	(0.132)	(0.042)	(0.030)	(0.042)	(0.011)	(0.044)	(0.010)	(0.040)	(0.019)	(0.045)
Observations	12,974	12,939	12,939	12,836	12,836	12,836	12,974	12,831	12,926	12,894	12,894	12,894
Unique states	386	385	385	382	382	382	386	386	382	384	384	384
R^2	0.05	0.14	0.14	0.03	0.01	0.03	0.01	0.02	0.08	0.09	0.02	0.02
Outcome mean	0.07	1.09	1.08	0.39	0.11	0.48	0.15	0.55	0.54	0.29	0.08	0.60
Outcome std. dev.	0.25	1.70	1.53	0.49	0.31	0.50	0.13	0.50	0.10	0.45	0.27	0.49
Upcoming local election mean	0.41	0.41	0.41	0.40	0.40	0.40	0.41	0.41	0.41	0.41	0.41	0.41

Notes: All specifications include survey wave fixed effects, and are estimated using OLS. The individual-level variables in columns (1)-(12) are taken from the ENCUP survey. The municipal and state variables in columns (13)-(36) are derived from the 2010 Census, electoral data from state electoral institutes, the IFE's media data, or the INEGI's budget, policing, and homicide datasets. Standard errors clustered by municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Balance of pre-election homicide shocks over 104 municipal-level variables

Covariate	Mean	Standard deviation	Effect of homicide shock Coef.	SE	Obs.	Covariate	Mean	Standard deviation	Effect of homicide shock Coef.	SE	Obs.
Electorate	347,093.60	356,428.16	6,566.4	(5,356.6)	2,583	Blunt object-related share of homicides before election	0.02	0.09	-0.0122*	(0.0070)	1,618
Electorate density (log)	0.02	0.04	-0.0002	(0.0002)	2,492	Share of homicide registered in the same month before election	0.85	0.23	-0.0130	(0.0159)	1,618
Incumbent vote share (lag)	0.47	0.09	-0.0066	(0.0070)	2,542	Male share of homicide victims before election	0.87	0.19	0.0101	(0.0169)	1,618
Incumbent win (lag)	0.54	0.50	0.1025**	(0.0432)	2,549	Average age of homicide victims before election	33.73	9.35	0.9624	(0.7957)	1,618
Incumbent vote share victory margin (lag)	-0.14	0.11	0.0096	(0.0112)	2,542	Share of victims with no schooling before election	0.09	0.18	0.0137	(0.0136)	1,618
Effective number of political parties by vote (lag)	2.69	0.60	0.0014	(0.0367)	2,542	Share of victims with incomplete primary schooling before election	0.80	0.24	-0.0257	(0.0175)	1,618
Turnout (lag)	0.52	0.11	-0.0098	(0.0074)	2,382	Share of victims with complete primary schooling before election	0.55	0.30	-0.0261	(0.0229)	1,618
PRI incumbent	0.48	0.50	-0.0726*	(0.0417)	2,583	Share of victims with incomplete secondary schooling before election	0.42	0.28	-0.0076	(0.0227)	1,618
PAN incumbent	0.37	0.48	0.0401	(0.0374)	2,583	Share of single victims before election	0.36	0.27	0.0141	(0.0245)	1,618
PRD incumbent	0.15	0.36	0.0343	(0.0360)	2,583	Job-related share of homicides before election	0.08	0.16	-0.0029	(0.0149)	1,618
Consecutive terms (main party)	1.11	1.21	0.1194	(0.1386)	1,367	Share of homicides where organs were examined before election	0.83	0.27	0.0051	(0.0151)	1,618
Consecutive terms (any coalition party)	1.13	1.22	0.1229	(0.1399)	1,335	Family-incident share of homicides before election	0.03	0.11	-0.0030	(0.0098)	1,618
Mayor's party aligned with President	0.36	0.48	0.0501	(0.0431)	2,583	Urban share of homicide victims before election	0.66	0.38	-0.0044	(0.0178)	1,618
Mayor's party aligned with Governor	0.51	0.50	-0.0905*	(0.0480)	2,583	Non-Mexican share of homicide victims before election	0.01	0.05	0.0067	(0.0044)	1,618
Mayor's party aligned with President and Governor	0.12	0.32	0.0169	(0.0335)	2,583	Neighbor average homicide shock	0.48	0.29	0.0076	(0.0236)	2,464
Total municipal spending	1,218.07	1,311.12	-5.5344	(66.288)	1,698	Average non-homicide deaths (last 3 years)	-35.40	9.67	0.0740	(0.1458)	2,569
Municipal formal employment rate (prior year)	0.17	0.16	-0.0005	(0.0026)	1,339	Average non-homicide deaths (last 12 months before election)	-36.16	9.76	0.2335	(0.1824)	2,571
Police per voter	2.04	1.26	-0.0177	(0.0698)	2,583	Average child mortalities (last 3 years)	1.84	1.00	-0.0293	(0.0409)	2,096
Homicide per 100,000 people 12 months before election	1.35	2.95	0.0613	(0.1888)	2,583	Average child mortalities (last 12 months before election)	1.81	1.03	-0.0365	(0.0440)	2,096
Homicide per 100,000 people 11 months before election	1.32	3.01	-0.1193	(0.2040)	2,583	Area (km^2)	1,493.43	2,630.37	245.04	(217.47)	2,492
Homicide per 100,000 people 10 months before election	1.31	3.34	-0.0255	(0.1882)	2,583	Local media	9.41	10.03	-1.5407*	(0.8611)	2,573
Homicide per 100,000 people 9 months before election	1.22	2.57	-0.1376	(0.1523)	2,583	Non-local media	26.80	23.25	0.4299	(2.1532)	2,573
Homicide per 100,000 people 8 months before election	1.30	2.58	-0.1440	(0.1484)	2,583	Average occupants per room	1.11	0.21	0.0064	(0.0124)	2,554
Homicide per 100,000 people 7 months before election	1.30	2.72	-0.0828	(0.1474)	2,583	Share of households with 2 bedrooms	0.66	0.10	-0.0077	(0.0072)	2,554
Homicide per 100,000 people 6 months before election	1.30	2.60	-0.1007	(0.1430)	2,583	Share of households with 3+ bedrooms	0.77	0.11	-0.0079	(0.0081)	2,554
Homicide per 100,000 people 5 months before election	1.30	3.09	0.0340	(0.1484)	2,583	Share female	0.51	0.01	-0.0008	(0.0006)	2,554
Homicide per 100,000 people 4 months before election	1.36	3.84	0.1192	(0.1509)	2,583	Share working age	0.64	0.03	0.0014	(0.0022)	2,554
Homicide per 100,000 people 3 months before election	1.34	2.72	-0.1526	(0.1589)	2,583	Average children per woman	2.32	0.33	0.0019	(0.0199)	2,554
Average monthly homicide rate (mayor's term)	1.19	1.62	-0.0293	(0.0898)	2,583	Average years of schooling	8.78	1.37	-0.0196	(0.0829)	2,554
Average monthly homicide rate (last 12 months before election)	1.36	2.43	0.0957	(0.1440)	2,583	Share illiterate	0.06	0.05	0.0011	(0.0028)	2,554
Top homicide quartile (last 12 months before election)	0.22	0.42	0.0012	(0.0340)	2,583	Share economically active	0.41	0.04	-0.0025	(0.0026)	2,554
Top homicide decile (last 12 months before election)	0.09	0.28	-0.0152	(0.0293)	2,583	Share born out of state	0.20	0.16	0.0168	(0.0168)	2,554
Number of DTOs (Coscia and Rios 2012)	1.45	1.75	0.0816	(0.1288)	1,990	Share Catholic	0.84	0.09	-0.0049	(0.0065)	2,554
Presence of a DTO (Coscia and Rios 2012)	0.57	0.50	-0.0231	(0.0395)	1,990	Share indigenous speakers	0.04	0.11	0.0061	(0.0048)	2,554
Number of DTOs (Osorio 2015)	0.96	1.66	0.0258	(0.1704)	1,903	Share without health care	0.33	0.10	0.0012	(0.0065)	2,554
Presence of a DTO (Osorio 2015)	0.39	0.49	-0.0008	(0.0508)	1,903	Share with electricity, running water, and drainage	0.85	0.17	-0.0104	(0.0089)	2,554
Number of DTOs (Sobrino 2020)	0.93	1.59	-0.1969	(0.1327)	1,704	Share with a washing machine	0.69	0.17	-0.0071	(0.0096)	2,554
Presence of a DTO (Sobrino 2020)	0.37	0.48	-0.0656	(0.0538)	1,704	Share with a landline telephone	0.46	0.17	-0.0161	(0.0130)	2,554
Criminal electoral violence events (Trejo and Ley 2021)	0.08	0.31	-0.0410	(0.0326)	2,507	Share with a radio	0.81	0.10	-0.0062	(0.0067)	2,554
Reports of inter-DTO violence (Osorio 2015) before election	3.22	10.93	-0.9839	(0.8988)	1,903	Share with a fridge	0.85	0.13	-0.0068	(0.0069)	2,554
Reports of violent enforcement (Osorio 2015) before election	0.46	1.60	0.1150	(0.1781)	1,903	Share with a cell phone	0.68	0.16	-0.0051	(0.0090)	2,554
Reports of arrests (Osorio 2015) before election	4.58	9.51	1.1203	(1.4106)	1,903	Share with a television	0.94	0.08	-0.0052	(0.0035)	2,554
Reports of drug seizures (Osorio 2015) before election	5.43	12.85	2.0040	(1.8206)	1,903	Share with a car or truck	0.46	0.14	-0.0101	(0.0090)	2,554
Reports of asset seizures (Osorio 2015) before election	1.41	3.62	0.6110	(0.5430)	1,903	Share with a computer	0.30	0.13	-0.0055	(0.0088)	2,554
Reports of gun seizures (Osorio 2015) before election	1.41	4.17	0.9363	(0.6512)	1,903	Share with internet	0.22	0.11	-0.0059	(0.0079)	2,554
Gun-related share of homicides before election	0.51	0.32	0.0083	(0.0284)	1,618	Robbery shock	0.37	0.48	0.1174	(0.1014)	328
Chemical substance-related share of homicides before election	0.00	0.03	0.0020	(0.0029)	1,618	Property theft shock	0.45	0.50	-0.0342	(0.1109)	314
Hanging-related share of homicides before election	0.08	0.16	0.0100	(0.0129)	1,618	Carjacking shock	0.39	0.49	-0.1734	(0.1462)	83
Drowning-related share of homicides before election	0.01	0.07	0.0031	(0.0037)	1,618	Criminal injury shock	0.60	0.49	0.0627	(0.1079)	314
Explosives-related share of homicides before election	0.00	0.01	0.0011	(0.0014)	1,618	Sexual assault shock	0.55	0.50	0.0462	(0.1235)	226
Smoke/fire-related share of homicides before election	0.01	0.04	0.0022	(0.0047)	1,618	Kidnapping shock	0.60	0.49	-0.0595	(0.1813)	65
Cutting object-related share of homicides before election	0.18	0.23	-0.0111	(0.0223)	1,618	Other crime shock	0.52	0.50	0.1924*	(0.1027)	347

Notes: All specifications are estimated using OLS, include municipality and election month-year fixed effects, and weight observations by the number of registered voters. Homicide share variables are included only where at least one homicide occurs in the two months preceding the election. The time-invariant variables—area, media coverage, and the 2010 Census variables (average occupants per dwelling-share with internet)—and pre-election shocks regarding other types of crime (data are only available from 2011 onward) exclude fixed effects. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Balance of pre-election homicide shocks over 114 precinct-level variables

Covariate	Mean	Standard deviation	Effect of homicide shock Coef.	SE	Obs.	Covariate	Mean	Standard deviation	Effect of homicide shock Coef.	SE	Obs.
Electorate	1,963.94	2,325.75	5.7207	(38.5365)	149,541	Smoke/fire-related share of homicides before election	0.01	0.04	0.0021	(0.0037)	130,014
Municipal electorate	346,931.60	356,211.14	6,610.3	(4,367.9)	149,541	Cutting object-related share of homicides before election	0.18	0.24	-0.0103	(0.0177)	130,014
Electorate density (log)	7.43	2.22	-0.0063	(0.0056)	147,619	Blunt object-related share of homicides before election	0.02	0.09	-0.0121	(0.0056)	130,014
Municipal electorate density (log)	0.02	0.04	-0.0001	(0.0002)	147,619	Share of homicide registered in the same month before election	0.85	0.23	-0.0123	(0.0127)	130,014
Incumbent vote share (lag)	0.47	0.13	-0.0072	(0.0056)	149,541	Male share of homicide victims before election	0.87	0.19	0.0099	(0.0134)	130,014
Municipal incumbent vote share (lag)	0.47	0.09	-0.0065	(0.0057)	148,671	Average age of homicide victims before election	33.77	9.68	0.9151	(0.6314)	130,014
Incumbent win (lag)	0.54	0.50	0.1025***	(0.0352)	149,097	Share of victims with no schooling before election	0.09	0.19	0.0135	(0.0109)	130,014
Incumbent vote share victory margin (lag)	-0.19	0.15	0.0126*	(0.0072)	149,541	Share of victims with incomplete primary schooling before election	0.80	0.25	-0.0254*	(0.0139)	130,014
Municipal incumbent vote share victory margin (lag)	-0.14	0.11	0.0097	(0.0090)	148,671	Share of victims with complete primary schooling before election	0.55	0.30	-0.0254	(0.0182)	130,014
Effective number of political parties by vote (lag)	2.56	0.60	0.0157	(0.0254)	149,539	Share of victims with incomplete secondary schooling before election	0.42	0.29	-0.0076	(0.0181)	130,014
Municipal effective number of political parties by vote (lag)	2.69	0.60	0.0020	(0.0297)	148,671	Share of single victims before election	0.36	0.28	0.0149	(0.0195)	130,014
Turnout (lag)	0.52	0.12	-0.0104*	(0.0058)	140,496	Job-related share of homicides before election	0.08	0.16	-0.0028	(0.0119)	130,014
Municipal turnout (lag)	0.52	0.11	-0.0103*	(0.0059)	139,900	Share of homicides where organs were examined before election	0.83	0.27	0.0055	(0.0121)	130,014
PRI incumbent	0.48	0.50	-0.0725**	(0.0339)	149,541	Family-incident share of homicides before election	0.03	0.11	-0.0030	(0.0078)	130,014
PAN incumbent	0.37	0.48	0.0393	(0.0304)	149,541	Urban share of homicide victims before election	0.65	0.39	-0.0043	(0.0142)	130,014
PRD incumbent	0.15	0.36	0.0349	(0.0292)	149,541	Non-Mexican share of homicide victims before election	0.01	0.05	0.0067*	(0.0035)	130,014
Consecutive terms (main party)	1.09	1.21	0.1163	(0.1065)	90,959	Neighbor average homicide shock	0.48	0.29	0.0081	(0.0191)	146,522
Consecutive terms (any coalition party)	1.12	1.22	0.1202	(0.1072)	90,089	Average non-homicide deaths (last 3 years)	-35.44	9.66	0.0767	(0.1182)	149,510
Mayor's party aligned with President	0.36	0.48	0.0492	(0.0351)	149,541	Average non-homicide deaths (last 12 months before election)	-36.20	9.74	0.2371	(0.1477)	149,520
Mayor's party aligned with Governor	0.51	0.50	-0.0925**	(0.0388)	149,541	Average child mortalities (last 3 years)	1.84	1.00	-0.0293	(0.0325)	130,379
Mayor's party aligned with President and Governor	0.12	0.32	0.0153	(0.0272)	149,541	Average child mortalities (last 12 months before election)	1.82	1.03	-0.0375	(0.0350)	130,379
Total municipal spending	1,208.12	1,310.01	-4.9447	(53.375)	104,371	Latitude	21.39	3.51	0.0434	(0.2331)	149,219
Municipal formal employment rate (prior year)	0.16	0.15	-0.0004	(0.0020)	91,894	Longitude	-100.76	4.06	0.2087	(0.3127)	149,219
Police per voter	2.03	1.26	-0.0169	(0.0565)	149,541	Area (km^2)	15.55	123.80	6.5284***	(2.3929)	147,619
Homicide per 100,000 people 12 months before election	1.35	2.96	0.0632	(0.1535)	149,541	Municipal area (km^2)	1,486.03	2,614.25	230.9051	(214.51)	147,619
Homicide per 100,000 people 11 months before election	1.32	3.03	-0.1173	(0.1656)	149,541	Local media	9.55	10.31	-1.6494*	(0.8614)	147,847
Homicide per 100,000 people 10 months before election	1.31	3.36	-0.0283	(0.1526)	149,541	Non-local media	27.26	23.60	0.5401	(2.1467)	147,847
Homicide per 100,000 people 9 months before election	1.22	2.55	-0.1365	(0.1234)	149,541	Average occupants per room	1.08	0.30	0.0097	(0.0119)	149,219
Homicide per 100,000 people 8 months before election	1.30	2.59	-0.1436	(0.1203)	149,541	Share of households with 2 bedrooms	0.67	0.14	-0.0071	(0.0067)	149,098
Homicide per 100,000 people 7 months before election	1.30	2.73	-0.0817	(0.1196)	149,541	Share of households with 3+ bedrooms	0.77	0.15	-0.0078	(0.0075)	149,098
Homicide per 100,000 people 6 months before election	1.31	2.60	-0.0992	(0.1162)	149,541	Share female	0.51	0.02	-0.0011*	(0.0006)	149,108
Homicide per 100,000 people 5 months before election	1.30	3.09	0.0332	(0.1205)	149,541	Share working age	0.64	0.05	0.0013	(0.0022)	149,108
Homicide per 100,000 people 4 months before election	1.36	3.86	0.1206	(0.1228)	149,541	Average children per woman	2.35	0.46	0.0005	(0.0187)	149,219
Homicide per 100,000 people 3 months before election	1.34	2.72	-0.1510	(0.1293)	149,541	Average years of schooling	8.84	2.19	-0.0488	(0.0846)	149,219
Average monthly homicide rate (mayor's term)	1.19	1.63	-0.0288	(0.0728)	149,541	Share illiterate	0.06	0.07	0.0012	(0.0028)	149,098
Average monthly homicide rate (last 12 months before election)	1.37	2.44	0.0960	(0.1169)	149,541	Share economically active	0.41	0.06	-0.0028	(0.0026)	149,108
Top homicide quartile (last 12 months before election)	0.22	0.42	0.0015	(0.0276)	149,541	Share born out of state	0.19	0.18	0.0150	(0.0159)	149,108
Top homicide decile (last 12 months before election)	0.09	0.28	-0.0153	(0.0238)	149,541	Share Catholic	0.84	0.11	-0.0043	(0.0056)	149,108
Number of DTOs (Coscia and Rios 2012)	1.42	1.75	0.0840	(0.1020)	127,317	Share indigenous speakers	0.03	0.12	0.0062	(0.0046)	149,098
Presence of a DTO (Coscia and Rios 2012)	0.56	0.50	-0.0229	(0.0313)	127,317	Share without health care	0.34	0.13	0.0015	(0.0061)	149,108
Number of DTOs (Osorio 2015)	0.94	1.65	0.0221	(0.1331)	123,343	Share with electricity, running water, and drainage	0.86	0.23	-0.0110	(0.0087)	149,098
Presence of a DTO (Osorio 2015)	0.39	0.49	-0.0008	(0.0400)	123,343	Share with a washing machine	0.70	0.20	-0.0083	(0.0094)	149,098
Number of DTOs (Sobrino 2020)	0.92	1.58	-0.1932*	(0.1020)	106,149	Share with a landline telephone	0.48	0.24	-0.0187	(0.0125)	149,098
Presence of a DTO (Sobrino 2020)	0.36	0.48	-0.0647	(0.0414)	106,149	Share with a radio	0.81	0.12	-0.0068	(0.0064)	149,098
Criminal electoral violence events (Trejo and Ley 2021)	0.08	0.31	-0.0412	(0.0265)	148,034	Share with a fridge	0.85	0.16	-0.0076	(0.0067)	149,098
High-risk electoral precinct	0.02	0.13	0.0023	(0.0021)	50,527	Share with a cell phone	0.67	0.20	-0.0052	(0.0083)	149,098
Reports of inter-DTO violence (Osorio 2015) before election	3.16	10.87	-0.9894	(0.7080)	123,343	Share with a television	0.94	0.09	-0.0051	(0.0034)	149,098
Reports of violent enforcement (Osorio 2015) before election	0.45	1.58	0.1155	(0.1399)	123,343	Share with a car or truck	0.46	0.20	-0.0107	(0.0089)	149,098
Reports of arrests (Osorio 2015) before election	4.50	9.46	1.1198	(1.1020)	123,343	Share with a computer	0.31	0.21	-0.0077	(0.0088)	149,098
Reports of drug seizures (Osorio 2015) before election	5.31	12.74	2.0274	(1.4343)	123,343	Share with internet	0.23	0.19	-0.0081	(0.0080)	149,098
Reports of asset seizures (Osorio 2015) before election	1.37	3.59	0.6121	(0.4249)	123,343	Robbery shock	0.37	0.48	0.1173	(0.1019)	19,393
Reports of gun seizures (Osorio 2015) before election	1.38	4.14	0.9466*	(0.5118)	123,343	Property theft shock	0.45	0.50	-0.0302	(0.1113)	18,857
Gun-related share of homicides before election	0.51	0.33	0.0075	(0.0225)	130,014	Carjacking shock	0.39	0.49	-0.1684	(0.1462)	4,315
Chemical substance-related share of homicides before election	0.00	0.03	0.0020	(0.0023)	130,014	Criminal injury shock	0.60	0.49	0.0622	(0.1085)	19,165
Hanging-related share of homicides before election	0.08	0.16	0.0104	(0.0103)	130,014	Sexual assault shock	0.55	0.50	0.0537	(0.1239)	15,369
Drowning-related share of homicides before election	0.01	0.07	0.0029	(0.0030)	130,014	Kidnapping shock	0.60	0.49	-0.0485	(0.1812)	6,012
Explosives-related share of homicides before election	0.00	0.01	0.0011	(0.0012)	130,014	Other crime shock	0.52	0.50	0.2039**	(0.1023)	19,716

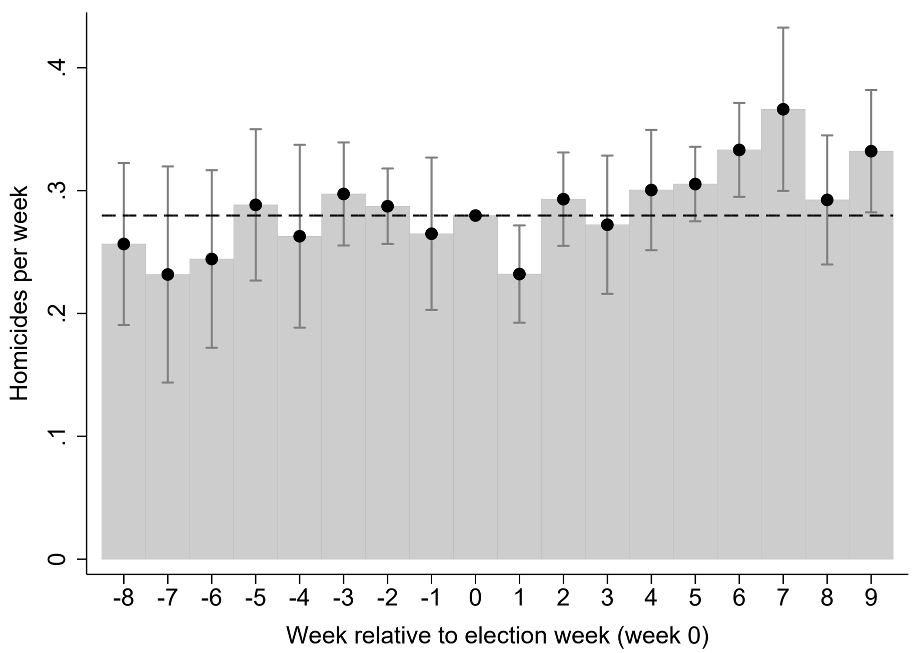
Notes: All specifications are estimated using OLS, include municipality and election month-year fixed effects, and weight observations by the number of registered voters. Homicide share variables are included only where at least one homicide occurs in the two months preceding the election. The time-invariant variables—area, media coverage, and the 2010 Census variables (average occupants per dwelling-share with internet)—and pre-election shocks regarding other types of crime (data are only available from 2011 onward) exclude fixed effects. Standard errors clustered by municipality-election in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Balance of the number of local media stations over 31 precinct-level variables

Covariate	Mean	Standard deviation	Effect of local media Coef.	SE	Obs.	Covariate	Mean	Standard deviation	Effect of local media Coef.	SE	Obs.
Latitude	23.08	4.51	0.0001	(0.0001)	26,134	Share working age	0.66	0.04	-0.0003	(0.0002)	26,134
Longitude	-101.52	4.69	-0.0001	(0.0000)	26,134	Average children per woman	2.19	0.33	-0.0020	(0.0016)	26,134
Area (km^2)	0.54	0.48	-0.0036	(0.0051)	26,134	Average years of schooling	9.60	1.78	0.0145	(0.0093)	26,134
Electorate	1,932.57	1,370.27	-23.4784*	(12.9640)	26,134	Share illiterate	0.03	0.03	-0.0003*	(0.0001)	26,134
Electorate density (log)	8.41	0.77	-0.0108	(0.0070)	26,106	Share economically active	0.42	0.04	0.0004*	(0.0002)	26,134
Distance from centroid to municipal head (log)	8.17	0.80	-0.0036	(0.0024)	26,134	Share born out of state	0.18	0.12	0.0009**	(0.0004)	26,134
Non-local media	17.45	17.65	-0.0446	(0.1376)	26,134	Share Catholic	0.82	0.09	-0.0003	(0.0003)	26,134
Incumbent vote share (lag)	0.50	0.13	-0.0001	(0.0005)	26,134	Share indigenous speakers	0.02	0.04	0.0001	(0.0001)	26,134
Incumbent vote share victory margin (lag)	-0.20	0.15	-0.0005	(0.0005)	26,134	Share without health care	0.30	0.10	0.0000	(0.0004)	26,134
Effective number of political parties by vote (lag)	2.42	0.55	0.0006	(0.0017)	26,134	Share with electricity, running water, and drainage	0.94	0.11	0.0015**	(0.0006)	26,134
Turnout (lag)	0.49	0.11	0.0003	(0.0004)	25,076	Share with a washing machine	0.76	0.13	-0.0001	(0.0006)	26,134
High-risk electoral precinct	0.03	0.17	0.0012	(0.0011)	7,618	Share with a fridge	0.91	0.09	0.0001	(0.0005)	26,134
Average occupants per room	0.99	0.24	-0.0037***	(0.0013)	26,134	Share with a cell phone	0.75	0.10	0.0000	(0.0005)	26,134
Share of households with 2 bedrooms	0.69	0.12	-0.0004	(0.0005)	26,134	Share with a car or truck	0.53	0.19	0.0006	(0.0010)	26,134
Share of households with 3+ bedrooms	0.80	0.14	0.0005	(0.0006)	26,134	Share with a computer	0.38	0.18	0.0013	(0.0010)	26,134
Share female	0.51	0.02	0.0001	(0.0001)	26,134						

Notes: All specifications are estimated using OLS, and include neighbor group and election month-year fixed effects. All observations are weighted by the number of registered voters, while “control” precincts are further divided by the number of comparison units within each neighbor group. Homicide share variables are included only where at least one homicide occurs in the two months preceding the election. There is no variation in the number of explosive-related homicides or non-homicide pre-election crime shocks (for which overlapping data are only available for one election) across local media in this sample. Standard errors double clustered by municipality-election and neighbor group are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A3: Weekly homicide rates around local elections (95% confidence intervals)



Notes: Estimates from an OLS regression of the weekly homicide rate on indicators for week (relative to the baseline category of the week up to election day) and municipality $\times$ election fixed effects. Standard errors clustered by municipality-election.

Table A7: Balance of pre-election homicide shocks over 104 precinct-level variables (in the local media sample)

Covariate	Mean	Standard deviation	Effect of homicide shock Coef.	SE	Obs.	Covariate	Mean	Standard deviation	Effect of homicide shock Coef.	SE	Obs.
Local media	17.57	10.3	0.0029	(0.0049)	26,134	High-risk electoral precinct	0.03	0.17	-0.0070	(0.0081)	7,618
Latitude	23.08	4.51	0.0000	(0.0000)	26,134	Reports of inter-DTO violence (Osorio 2015) before election	5.49	15.52	-3.2204*	(1.6529)	22,275
Longitude	-101.52	4.69	0.0000	(0.0000)	26,134	Reports of violent enforcement (Osorio 2015) before election	0.72	1.96	-0.2197	(0.2861)	22,275
Area (km^2)	0.54	0.48	0.0015	(0.0017)	26,134	Reports of arrests (Osorio 2015) before election	7.27	11.59	0.6012	(2.1057)	22,275
Municipal area (km^2)	15,053.63	24,466.70	-126.7228	(145.3529)	26,134	Reports of drug seizures (Osorio 2015) before election	8.81	18.45	4.4239	(5.3958)	22,275
Electorate	1,932.57	1,370.27	33.1573	(47.9158)	26,134	Reports of asset seizures (Osorio 2015) before election	1.71	3.11	0.3620	(0.4668)	22,275
Municipal electorate	465,600.53	334,220.99	17208.0506*	(6883.2409)	26,134	Reports of gun seizures (Osorio 2015) before election	2.2	4.97	-0.1906	(0.7301)	22,275
Electorate density (log)	8.41	0.77	0.0108	(0.0150)	26,106	Gun-related share of homicides before election	0.51	0.29	0.0473	(0.0456)	25,107
Municipal electorate density (log)	0.05	0.1	-0.0051**	(0.0020)	26,134	Chemical substance-related share of homicides before election	0	0.01	0.0000	(0.0016)	25,107
Distance from centroid to municipal head (log)	8.17	0.8	-0.0006	(0.0007)	26,134	Hanging-related share of homicides before election	0.07	0.13	-0.0275	(0.0172)	25,107
Non-local media	17.45	17.65	0.0001	(0.0021)	26,134	Drowning-related share of homicides before election	0	0.02	0.0016	(0.0023)	25,107
Incumbent vote share (lag)	0.5	0.13	-0.0476***	(0.0126)	26,134	Explosives-related share of homicides before election	0	0			25,107
Municipal incumbent vote share (lag)	0.49	0.09	-0.0414***	(0.0112)	26,063	Smoke/fire-related share of homicides before election	0	0.02	-0.0022	(0.0038)	25,107
Incumbent win (lag)	0.59	0.49	0.0858	(0.0774)	26,112	Cutting object-related share of homicides before election	0.18	0.21	-0.0108	(0.0288)	25,107
Incumbent vote share victory margin (lag)	-0.2	0.15	0.0603***	(0.0182)	26,134	Blunt object-related share of homicides before election	0.02	0.08	-0.0126	(0.0098)	25,107
Municipal incumbent vote share victory margin (lag)	-0.15	0.12	0.0636***	(0.0189)	26,063	Share of homicide registered in the same month before election	0.84	0.2	0.0546**	(0.0246)	25,107
Effective number of political parties by vote (lag)	2.42	0.55	0.1371***	(0.0513)	26,134	Male share of homicide victims before election	0.88	0.17	0.0016	(0.0211)	25,107
Municipal effective number of political parties by vote (lag)	2.5	0.55	0.1421***	(0.0540)	26,063	Average age of homicide victims before election	33.5	8.21	1.8157	(1.2276)	25,107
Turnout (lag)	0.49	0.11	-0.0181*	(0.0099)	25,076	Share of victims with no schooling before election	0.09	0.15	0.0180	(0.0182)	25,107
Municipal turnout (lag)	0.5	0.1	-0.0175*	(0.0097)	25,045	Share of victims with incomplete primary schooling before election	0.76	0.22	0.0008	(0.0260)	25,107
PRI incumbent	0.52	0.5	-0.0262	(0.0682)	26,134	Share of victims with complete primary schooling before election	0.51	0.25	0.0202	(0.0213)	25,107
PAN incumbent	0.39	0.49	0.0148	(0.0634)	26,134	Share of victims with incomplete secondary schooling before election	0.4	0.24	0.0340	(0.0269)	25,107
PRD incumbent	0.11	0.31	-0.0284	(0.0309)	26,134	Share of single victims before election	0.35	0.25	0.0368	(0.0446)	25,107
Consecutive terms (main party)	1.28	1.2	-0.1075	(0.1521)	15,771	Job-related share of homicides before election	0.07	0.12	-0.0234	(0.0229)	25,107
Consecutive terms (any coalition party)	1.28	1.2	-0.0938	(0.1567)	15,593	Share of homicides where organs were examined before election	0.8	0.24	0.0608**	(0.0309)	25,107
Mayor's party aligned with President	0.38	0.48	0.0484	(0.0694)	26,134	Family-incident share of homicides before election	0.04	0.11	0.0097	(0.0158)	25,107
Mayor's party aligned with Governor	0.52	0.5	-0.1733**	(0.0679)	26,134	Urban share of homicide victims before election	0.68	0.35	0.0543**	(0.0259)	25,107
Mayor's party aligned with President and Governor	0.08	0.27	-0.0212	(0.0448)	26,134	Non-Mexican share of homicide victims before election	0.02	0.09	0.0206	(0.0132)	25,107
Total municipal spending	1,650.58	1,264.12	132.3293	(95.5203)	17,223	Neighbor average homicide shock	0.47	0.31	0.0628	(0.0516)	25,151
Municipal formal employment rate (prior year)	0.21	0.17	0.0058	(0.0038)	15,962	Average non-homicide deaths (last 3 years)	-35.03	8.71	0.0216	(0.2262)	26,134
Police per voter	2	1.16	0.2757***	(0.0836)	26,134	Average non-homicide deaths (last 12 months before election)	-35.9	8.76	0.2274	(0.2613)	26,134
Homicide per 100,000 people 12 months before election	1.7	3.34	-0.4450	(0.4459)	26,134	Average child mortalities (last 3 years)	1.57	0.84	-0.0564	(0.0487)	23,315
Homicide per 100,000 people 11 months before election	1.64	3.74	-0.6962	(0.5224)	26,134	Average child mortalities (last 12 months before election)	1.55	0.84	-0.0236	(0.0634)	23,315
Homicide per 100,000 people 10 months before election	1.63	3.44	-0.3993	(0.4246)	26,134	Average occupants per room	0.99	0.24	0.0001	(0.0004)	26,134
Homicide per 100,000 people 9 months before election	1.43	2.95	-0.5072	(0.3199)	26,134	Share of households with 2 bedrooms	0.69	0.12	-0.0006**	(0.0002)	26,134
Homicide per 100,000 people 8 months before election	1.43	2.82	-0.5078*	(0.3044)	26,134	Share of households with 3+ bedrooms	0.8	0.14	-0.0005***	(0.0002)	26,134
Homicide per 100,000 people 7 months before election	1.49	3	-0.5828**	(0.2822)	26,134	Share female	0.51	0.02	0.0000	(0.0000)	26,134
Homicide per 100,000 people 6 months before election	1.52	2.78	-0.5214	(0.3452)	26,134	Share working age	0.66	0.04	-0.0002**	(0.0001)	26,134
Homicide per 100,000 people 5 months before election	1.53	2.8	-0.6521**	(0.3276)	26,134	Average children per woman	2.19	0.33	-0.0010	(0.0011)	26,134
Homicide per 100,000 people 4 months before election	1.6	3.32	-0.2367	(0.3181)	26,134	Average years of schooling	9.6	1.78	-0.0041	(0.0007)	26,134
Homicide per 100,000 people 3 months before election	1.65	3.4	-0.0352	(0.3112)	26,134	Share illiterate	0.03	0.03	-0.0001	(0.0001)	26,134
Average monthly homicide rate (mayor's term)	1.36	2.1	-0.3138	(0.1998)	26,134	Share economically active	0.42	0.04	-0.0001*	(0.0001)	26,134
Average monthly homicide rate (last 12 months before election)	19.34	37.99	-4.0190	(3.9431)	26,134	Share born out of state	0.18	0.12	0.0001	(0.0002)	26,134
Top homicide quartile (last 12 months before election)	0.29	0.45	-0.0345	(0.0701)	26,134	Share Catholic	0.82	0.09	-0.0001	(0.0001)	26,134
Top homicide decile (last 12 months before election)	0.14	0.35	0.0074	(0.0536)	26,134	Share indigenous speakers	0.02	0.04	-0.0001	(0.0001)	26,134
Number of DTOs (Coscia and Rios 2012)	2.31	1.99	-0.2248	(0.1957)	22,991	Share without health care	0.3	0.1	0.0001	(0.0001)	26,134
Presence of a DTO (Coscia and Rios 2012)	0.79	0.41	-0.0077	(0.0489)	22,991	Share with electricity, running water, and drainage	0.94	0.11	0.0002	(0.0002)	26,134
Number of DTOs (Osorio 2015)	1.51	1.92	-0.3170	(0.1987)	22,275	Share with a washing machine	0.76	0.13	-0.0001	(0.0001)	26,134
Presence of a DTO (Osorio 2015)	0.56	0.5	0.0174	(0.0735)	22,275	Share with a fridge	0.91	0.09	0.0001	(0.0001)	26,134
Number of DTOs (Sobrino 2020)	1.88	2.2	-0.3846*	(0.2329)	19,177	Share with a cell phone	0.75	0.1	0.0000	(0.0002)	26,134
Presence of a DTO (Sobrino 2020)	0.59	0.49	0.1550**	(0.0721)	19,177	Share with a car or truck	0.53	0.19	0.0001	(0.0004)	26,134
Criminal electoral violence events (Trejo and Ley 2021)	0.18	0.44	-0.0244	(0.0614)	25,982	Share with a computer	0.38	0.18	0.0000	(0.0004)	26,134

Notes: All specifications are estimated using OLS, and include neighbor group and election month-year fixed effects. All observations are weighted by the number of registered voters, while "control" precincts are further divided by the number of comparison units within each neighbor group. Homicide share variables are included only where at least one homicide occurs in the two months preceding the election. There is no variation in the number of explosive-related homicides or non-homicide pre-election crime shocks (for which overlapping data are only available for one election) across local media in this sample. Standard errors double clustered by municipality-election and neighbor group are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Predictors of municipalities included in each survey wave

	Outcome: municipality surveyed in ENCUP survey wave				
	(1)	(2)	(3)	(4)	(5)
Upcoming local election	-0.037		-0.012		0.017
	(0.059)		(0.092)		(0.115)
Pre-survey homicide shock		-0.053	-0.047		
		(0.039)	(0.041)		
Pre-survey homicide shock × Upcoming local election			-0.083		
			(0.123)		
Homicide in last month				-0.020	-0.017
				(0.039)	(0.039)
Homicide in last month × Upcoming local election					-0.072
					(0.130)
Observations	2,092	1,364	1,364	2,088	2,088
Unique municipalities	523	460	460	522	522
Outcome mean	0.46	0.53	0.53	0.46	0.46

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

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

	Outcome: Number of homicides in the 60 days after election day							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incumbent party re-elected	7.749	-2.556*						
	(6.359)	(1.432)						
Incumbent party vote share			26.109	0.413				
			(24.437)	(5.925)				
PRI win					-3.494	2.854	-14.893	5.253
					(4.430)	(1.762)	(18.820)	(3.690)
PRD win					-0.487	1.345	-7.418	2.590
					(6.474)	(1.870)	(19.335)	(4.824)
Other win					-10.488	1.762	-13.356	4.021
					(7.824)	(1.779)	(15.981)	(3.542)
Average number of homicides per month		3.065***		3.058***		3.061***		3.119***
(10 months preceding treatment window)		(0.141)		(0.143)		(0.141)		(0.156)
Observations	2,583	2,583	2,583	2,583	2,582	2,582	959	959
R^2	0.63	0.98	0.63	0.98	0.63	0.98	0.73	0.99
Outcome range	[0,807]	[0,807]	[0,807]	[0,807]	[0,807]	[0,807]	[0,807]	[0,807]
Outcome mean	18.09	18.09	18.09	18.09	18.09	18.09	29.93	29.93
Outcome standard deviation	66.02	66.02	66.02	66.02	66.02	66.02	97.16	97.16

Notes: All specifications are estimated using OLS, include municipality and election month-year fixed effects, and are estimated using OLS, and weight observations by the number of registered voters. The omitted category in columns (2) and (3) is PAN victories. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Predictors of average monthly homicide rates under the next municipal administration run by the same incumbent party

	Outcome: Future average monthly homicide rate (mayor's term)		
	(1)	(2)	(3)
Average monthly homicide rate (mayor's term)	0.689***		0.676***
	(0.073)		(0.139)
Average monthly homicide rate (last 60 days before election)		0.291***	0.008
		(0.077)	(0.069)
Observations	1,200	1,200	1,200
R^2	0.68	0.59	0.68
Outcome mean	1.58	1.58	1.58
Outcome std. dev.	2.13	2.13	2.13
Average monthly homicide rate (mayor's term) mean	1.26		1.26
Average monthly homicide rate (last 60 days before election) mean		1.57	1.57

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

ing regression:

$$\begin{aligned} \textit{Average monthly homicide rate}_{m,t+1} &= \beta_1 \textit{Average monthly homicide rate}_{m,t} \\ &\quad + \beta_2 \textit{Pre election monthly homicide rate}_{m,t} + \mu_t + \varepsilon_{m,t} \end{aligned} \tag{A.7}$$

where *Pre election monthly homicide rate*$_{m,t}$ is the average number of homicides in the two months preceding the election and μ_t are election month-year fixed effects.

In line with the assumptions driving the theoretical model, column (3) in Table A10 shows that the longer-term average is a much stronger predictor of the homicide rate in the next administration than pre-election homicides. While columns (1) and (2) show that both indicators are correlated with the future homicide rate, there is no significant correlation with pre-election homicides after adjusting for the longer-term homicide rate.

A.6 Robustness checks for the individual-level survey results

Table A11 shows that the ENCUP information consumption results are robust to defining upcoming local elections by any number of months between 1 and 12 before the election. Consistent with the postulated news consumption cycles, the upcoming election effect declines as periods further from the election are included in the definition of upcoming elections.

Table A12 further shows that the effect of upcoming local elections on political news consumption is robust to excluding all responses from each of the 2001, 2003, 2005, and 2012 surveys.

Table A11: Effects of upcoming local elections on political news consumption and topical political knowledge, by number of months before the election

	Number of months before election used to define an upcoming local election											
	1 month	2 months	3 months	4 months	5 months	6 months	7 months	8 months	9 months	10 months	11 months	12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Outcome: Watch and listen to news and political programs ever												
Upcoming local election	0.096***	0.096***	0.077***	0.077***	0.045	0.045	0.043*	0.054**	0.054**	0.054**	0.037**	0.037**
	(0.017)	(0.017)	(0.016)	(0.016)	(0.031)	(0.031)	(0.026)	(0.025)	(0.025)	(0.025)	(0.016)	(0.016)
Observations	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983
Outcome mean	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86
Outcome standard deviation	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34	0.34
Upcoming local election mean	0.02	0.02	0.07	0.07	0.18	0.18	0.27	0.29	0.29	0.29	0.40	0.40
Panel B: Outcome: Watch and listen to news and political programs weekly												
Upcoming local election	0.124***	0.124***	0.125***	0.125***	0.071	0.071	0.060	0.076**	0.076**	0.082**	0.081***	0.081***
	(0.026)	(0.026)	(0.022)	(0.022)	(0.048)	(0.048)	(0.038)	(0.036)	(0.036)	(0.037)	(0.026)	(0.027)
Observations	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983
Outcome mean	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62
Outcome standard deviation	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48
Upcoming local election mean	0.02	0.02	0.07	0.07	0.18	0.18	0.27	0.29	0.29	0.29	0.40	0.40
Panel C: Outcome: Watch and listen to news and political programs scale												
Upcoming local election	0.553***	0.553***	0.558***	0.558***	0.315	0.315	0.292*	0.363**	0.363**	0.378**	0.355***	0.355***
	(0.113)	(0.114)	(0.102)	(0.101)	(0.211)	(0.210)	(0.171)	(0.161)	(0.163)	(0.165)	(0.110)	(0.110)
Observations	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983	11,983
Outcome mean	3.20	3.20	3.20	3.20	3.20	3.20	3.20	3.20	3.20	3.20	3.20	3.20
Outcome standard deviation	1.93	1.93	1.93	1.93	1.93	1.93	1.93	1.93	1.93	1.93	1.93	1.93
Upcoming local election mean	0.02	0.02	0.07	0.07	0.18	0.18	0.27	0.29	0.29	0.29	0.40	0.40
Panel D: Outcome: Topical political knowledge												
Upcoming local election	0.482***	0.329*	0.510**	0.510**	0.322**	0.322**	0.205*	0.221**	0.199*	0.188*	0.135*	0.135*
	(0.039)	(0.188)	(0.253)	(0.253)	(0.131)	(0.131)	(0.119)	(0.112)	(0.109)	(0.106)	(0.076)	(0.077)
Observations	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976	15,976
Outcome mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Outcome standard deviation	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Upcoming local election mean	0.02	0.02	0.06	0.06	0.15	0.15	0.22	0.23	0.24	0.25	0.34	0.34
Unique states	31	31	31	31	31	31	31	31	31	31	31	31

Notes: All specifications are estimated using OLS and include survey wave fixed effects. The outcomes in panels A-C were not collected in the 2001 survey. Block-bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7 Robustness checks for the electoral effect of pre-election homicides

A.7.1 Instrumental variable estimates

The instrument variable estimates are from 2SLS estimates of the following structural equation:

$$Y_{m,t} = \beta \textit{Pre-election monthly homicide rate}_{m,t} + \eta_m + \mu_t + \varepsilon_{m,t}. \tag{A8}$$

If homicide shocks only influence election outcomes through their effect on the pre-election homicide rate, β identifies the effect of an additional homicide per 100,000 people per month before a municipal election induced by idiosyncratic homicide shocks. The first stage and instrumental variable estimates are reported in Table A13.

A.7.2 Effects on aggregate electoral turnout

Table A14 examines how homicide shocks affect turnout and incumbent vote share (as a proportion of registered voters). The results show that a homicide shock does not significantly affect turnout, but does significantly reduce the incumbent vote share as a proportion of registered voters. Given

Table A12: Effect of upcoming local elections on political news consumption and topical political knowledge, excluding each ENCUP survey wave

	Outcomes:			
	Ever watch and listen to news and political programs	**Watch and listen to news and political programs at least once a week**	**Watch and listen to news and political programs scale (never, ever, monthly, weekly, daily)**	**Topical political political index (standardized)**
	(1)	(2)	(3)	(4)
Panel A: Excluding the 2001 ENCUP survey				
Upcoming local election (4 months)	0.077***	0.125***	0.558***	0.596***
	(0.016)	(0.022)	(0.101)	(0.066)
Observations	11,983	11,983	11,983	11,983
Unique states	31	31	31	31
Outcome mean	0.86	0.62	3.20	-0.01
Outcome standard deviation	0.34	0.48	1.93	1.08
Upcoming local election mean	0.07	0.07	0.07	0.07
Panel B: Excluding the 2003 ENCUP survey				
Upcoming local election (4 months)	0.068***	0.125***	0.561***	0.521*
	(0.019)	(0.027)	(0.127)	(0.268)
Observations	7,620	7,620	7,620	11,613
Unique states	31	31	31	31
Outcome mean	0.88	0.63	3.24	-0.00
Outcome standard deviation	0.33	0.48	1.86	1.01
Upcoming local election mean	0.07	0.07	0.07	0.06
Panel C: Excluding the 2005 ENCUP survey				
Upcoming local election (4 months)	0.096***	0.124***	0.553***	0.329*
	(0.017)	(0.026)	(0.114)	(0.190)
Observations	7,763	7,763	7,763	11,756
Unique states	31	31	31	31
Outcome mean	0.86	0.62	3.20	0.01
Outcome standard deviation	0.35	0.49	1.95	0.92
Upcoming local election mean	0.03	0.03	0.03	0.03
Panel D: Excluding the 2012 ENCUP survey				
Upcoming local election (4 months)	0.077***	0.125***	0.558***	0.510**
	(0.016)	(0.022)	(0.103)	(0.254)
Observations	8,583	8,583	8,583	12,576
Unique states	31	31	31	31
Outcome mean	0.85	0.62	3.16	0.00
Outcome standard deviation	0.35	0.49	1.98	0.98
Upcoming local election mean	0.09	0.09	0.09	0.07

Notes: All specifications are estimated using OLS and include survey wave fixed effects. The outcomes in columns (1)-(3) were not collected in the 2001 survey. Block-bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the limits of ecological inference, these results tentatively suggest that the decline in incumbent vote share (as a proportion of those that turned out) principally reflects vote switching. Nevertheless, individual-level voting records would be required to establish the degree to which the results reflect voters who always turn out switching away from the incumbent party relative to homicide shocks inducing those initially leaning toward the incumbent not to turn and those initially leaning toward

Table A13: Instrumental variable estimates of the effect of pre-election homicide rates on municipal incumbent party electoral outcomes

	Baseline specif-ication (1)	No fixed effects (2)	Adjusting for covariates (3)	Municipality -specific trends (4)	State × year fixed effects (5)	Violence bin effects (6)	≤1 homicide per 100,000 per month (7)	Non-DTO states (8)
Panel A: Outcome: Incumbent party re-elected								
Pre-election monthly homicide rate	-0.194**	-0.125**	-0.144***	-0.217*	-0.115**	-0.149***	-0.198**	-0.184*
	(0.078)	(0.060)	(0.052)	(0.126)	(0.049)	(0.052)	(0.083)	(0.098)
Observations	2,583	2,583	2,302	2,583	2,580	2,583	2,282	1,511
R^2	-0.15	-0.65	0.38	0.44	0.34	0.32	0.34	0.38
Outcome mean	0.55	0.55	0.56	0.55	0.55	0.55	0.55	0.54
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.42
Pre-election monthly homicide rate mean	1.54	1.54	1.53	1.54	1.54	1.54	1.05	0.99
Pre-election monthly homicide rate std. dev.	3.16	3.16	3.13	3.16	3.17	3.16	1.23	1.12
First stage *F* statistic	21.6	10.2	114.1	19.1	44.1	119.2	140.2	138.6
First stage coefficient	0.849***	0.966***	0.945***	0.667***	0.972***	0.876***	0.743***	0.795***
	(0.183)	(0.302)	(0.088)	(0.153)	(0.146)	(0.080)	(0.063)	(0.067)
Panel B: Outcome: Incumbent party vote share								
Pre-election monthly homicide rate	-0.028***	-0.026**	-0.019***	-0.023**	-0.018***	-0.024***	-0.029***	-0.022*
	(0.010)	(0.012)	(0.007)	(0.012)	(0.007)	(0.008)	(0.010)	(0.011)
Observations	149,541	149,541	143,318	149,541	149,541	149,541	137,965	82,670
Unique municipality-elections	2,578	2,578	2,426	2,578	2,578	2,578	2,339	1,506
R^2	0.27	-0.32	0.40	0.49	0.39	0.37	0.39	0.35
Outcome mean	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.40
Outcome std. dev.	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.14
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.42
Pre-election monthly homicide rate mean	1.55	1.55	1.54	1.55	1.55	1.55	1.06	0.99
Pre-election monthly homicide rate std. dev.	3.18	3.18	3.16	3.18	3.18	3.18	1.30	1.12
First stage *F* statistic	32.5	10.0	180.5	57.9	68.0	181.8	215.0	214.5
First stage coefficient	0.848***	0.964***	0.955***	0.665***	0.968***	0.874***	0.738***	0.793***
	(0.149)	(0.305)	(0.071)	(0.087)	(0.117)	(0.065)	(0.050)	(0.054)

Notes: All specifications are estimated using 2SLS, and weight observations by the number of registered voters. Column (1) includes municipality and election month-year fixed effects. Column (2) excludes municipality and election month-year fixed effects. Columns (3) includes the covariates in Appendix Table A4, with the exception of variables with greater than 5% missingness and the variables characterizing the types of homicides that occurred before the election. Column (4) includes municipality-specific year trends. Column (5) includes state × election year fixed effects. Column (6) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (7) excludes municipalities where the prior average monthly homicide rate per 100,000 people exceeds 1 over the prior year. Column (8) excludes states with high-level of DTO activity (see footnote 23). Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

abstention to turn out for non-incumbent parties (see e.g. Spenkuch and Toniatti 2018).

A.7.3 Weighting by population instead of registered voters

Table A15 replicates the main estimates of the effect of pre-election homicides on voting behavior, weighting observations by population in the 2010 Census. Given the high correlation between registered voters and total population, the point estimates are unsurprisingly similar.

Table A14: Effect of pre-election homicide shocks on precinct-level turnout

	Turnout		**Incumbent party vote share (share of registered voters)**	
	(1)	(2)	(3)	(4)
Homicide shock	-0.004		-0.014***	
	(0.003)		(0.004)	
Pre-election monthly homicide rate		-0.005		-0.016***
		(0.004)		(0.005)
Observations	149,541	149,541	149,541	149,541
Unique municipality-elections	2,578	2,578	2,578	2,578
R^2	0.61	0.60	0.43	0.37
Outcome range	[0.01,1]	[0.01,1]	[0,0.98]	[0,0.98]
Outcome mean	0.53	0.53	0.22	0.22
Outcome std. dev.	0.12	0.12	0.09	0.09
Homicide shock mean	0.41	0.41	0.41	0.41
First stage F statistic		103.0		103.0

Notes: Odd-numbered specifications estimate equation (4) using OLS and even-numbered specifications estimate equation (A8) using 2SLS. All observations are weighted by the number of registered voters. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7.4 Restricting incumbent coalitions

Table A16 demonstrates that the results are robust to restricting the set of incumbent coalitions considered for the main analysis. First, columns (1) and (2) restrict attention to PAN, PRD, and PRI incumbents, demonstrating that the results are not driven by the few mayors from smaller parties. Second, and consistent with a clearer assignment of responsibility, columns (3) and (4) report slightly larger effects, especially on vote share, when restricting attention to single-party incumbents or incumbent party coalitions that remained intact when seeking re-election.

A.7.5 Estimating correlations with homicide counts around elections simultaneously

An alternative approach to estimating the effects of pre-election homicides simultaneously estimates the correlation between voting outcomes and proximate homicide counts occurring before or after the election using the following OLS regression:

$$Y_{m,t} = \sum_{j=-6}^{5} \beta_j Homicides_{m,t,j} + \eta_m + \mu_t + \varepsilon_{m,t}, \tag{A9}$$

where $Homicides_{m,t,j}$ is the number of homicides reported in the 60-day interval j and $j = -1$ denotes the 60 day interval before election day. The results in Figure A4 show that only homicides in the 60-day interval immediately preceding election day significantly predict a lower probability of incumbent re-election. However, these estimates should be treated with caution due to multicollinearity—the correlation in homicide counts between any pre-election period always ex-

Table A15: Reduced form and instrumental variables estimates of the effect of pre-election homicides on municipal incumbent electoral outcomes, using 2010 population weights

	Baseline specif- ication (1)	No fixed effects (2)	Adjusting for covariates (3)	Municipality -specific trends (4)	State × year fixed effects (5)	Violence bin effects (6)	≤1 homicide per 100,000 per month (7)	Non- DTO states (8)
Panel A: Outcome: Incumbent party re-elected								
Reduced form estimates								
Homicide shock	-0.158***	-0.120***	-0.133***	-0.142**	-0.115***	-0.128***	-0.142***	-0.147**
	(0.050)	(0.040)	(0.045)	(0.072)	(0.042)	(0.040)	(0.053)	(0.069)
Instrumental variables estimates								
Pre-election monthly homicide rate	-0.189***	-0.130**	-0.147***	-0.223*	-0.122***	-0.156***	-0.194**	-0.188**
	(0.071)	(0.056)	(0.051)	(0.122)	(0.047)	(0.051)	(0.076)	(0.091)
Observations	2,593	2,593	2,292	2,593	2,590	2,593	2,237	1,515
Unique municipality-elections	2,593	2,593	2,292	2,593	2,590	2,593	2,237	1,515
R^2	-0.04	-0.57	0.40	0.45	0.35	0.33	0.37	0.38
Outcome mean	0.56	0.56	0.56	0.56	0.56	0.56	0.56	0.55
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41
Pre-election monthly homicide rate mean	1.4	1.4	1.4	1.4	1.4	1.4	1.0	0.9
Pre-election monthly homicide rate std. dev.	2.9	2.9	2.8	2.9	2.9	2.9	1.2	1.1
First stage F statistic	25.9	14.4	127.7	20.5	51.4	132.2	168.9	153.0
First stage coefficient	0.845***	0.933***	0.907***	0.648***	0.957***	0.836***	0.736***	0.788***
	(0.167)	(0.246)	(0.081)	(0.142)	(0.134)	(0.073)	(0.057)	(0.064)
Panel B: Outcome: Incumbent party vote share								
Reduced form estimates								
Homicide shock	-0.022***	-0.023***	-0.017***	-0.013*	-0.017***	-0.019***	-0.020***	-0.017**
	(0.007)	(0.009)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.008)
Instrumental variables estimates								
Pre-election monthly homicide rate	-0.026***	-0.023**	-0.018***	-0.020*	-0.018***	-0.023***	-0.027***	-0.021**
	(0.009)	(0.011)	(0.007)	(0.011)	(0.006)	(0.008)	(0.010)	(0.011)
Observations	149,745	149,745	143,375	149,745	149,745	149,745	136,380	82,670
Unique municipality-elections	2,593	2,593	2,420	2,593	2,593	2,593	2,311	1,515
R^2	0.30	-0.21	0.41	0.50	0.39	0.38	0.39	0.35
Outcome mean	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.40
Outcome std. dev.	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.14
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41	0.41	0.41
Pre-election monthly homicide rate mean	1.4	1.4	1.4	1.4	1.4	1.4	1.0	1.0
Pre-election monthly homicide rate std. dev.	2.9	2.9	2.9	2.9	2.9	2.9	1.2	1.1
First stage F statistic	39.5	15.5	198.4	67.2	77.5	195.3	254.2	231.5
First stage coefficient	0.856***	0.969***	0.927***	0.659***	0.965***	0.843***	0.739***	0.792***
	(0.136)	(0.246)	(0.066)	(0.080)	(0.109)	(0.060)	(0.046)	(0.052)

Notes: All specifications are estimated using OLS, and weight observations by the number of registered voters. Column (1) estimates equation (4), which includes municipality and election month-year fixed effects. Column (2) excludes municipality and election month-year fixed effects. Columns (3) includes the covariates in Appendix Table A4, with the exception of variables with greater than 5% missingness and the variables characterizing the types of homicides that occurred before the election. Column (4) includes municipality-specific year trends. Column (5) includes state × election year fixed effects. Column (6) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (7) excludes municipalities where the prior average monthly homicide rate per 100,000 people exceeds 1 over the prior year. Column (8) excludes states with high-level of DTO activity (see footnote 23). Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Robustness to restricting incumbent coalitions

	Outcome: Incumbent win		Outcome: Incumbent vote share	
	PAN, PRD, or PRI incumbent (1)	Single-party incumbent or same coalition (2)	PAN, PRD, or PRI incumbent (3)	Single-party incumbent or same coalition (4)
Homicide shock	-0.163***	-0.153**	-0.024***	-0.024***
	(0.056)	(0.061)	(0.007)	(0.008)
Observations	2,336	1,858	142,004	109,168
Unique municipality-elections	2,336	1,858	2,331	1,853
R^2	0.45	0.54	0.41	0.46
Outcome mean	0.56	0.53	0.42	0.42
Outcome std. dev.	0.50	0.50	0.14	0.15
Homicide shock mean	0.42	0.41	0.42	0.41

Notes: All specifications are estimated using OLS, include municipality and election month-year fixed effects, and weight observations by the number of registered voters. Columns (1) and (2) include only elections where the PAN, PRD, or PRI are part of the incumbent party coalition. Columns (3) and (4) include only observations where the incumbent party is not a coalition or where the incumbent coalition remains intact for the current election. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ceeds 0.9—and the lack of a compelling source of identifying variation.

A.7.6 Panel fixed effects approach

The results in the main text focus on pre-election homicide shocks coded as a binary variable. This approach is theoretically appealing because it guarantees that voters could not have observed the post-election homicide rate and, more importantly, because its short-term nature both captures differences relative to the municipality's baseline level of homicides *and* closely maps to cycles in political information consumption. Moreover, it is empirically appealing because month-to-month variation in homicide shocks is both plausibly exogenous to a wide variety of possible confounds and uncorrelated with the broader trends in homicide rates that this article seeks to differentiate the effects of short-term spikes around elections from. While Table 3 shows that the results are also robust to using pre-election comparison periods, I now consider another approach to capturing the effects of short-run shocks around local elections that also does not rely on homicides that occur after election day.

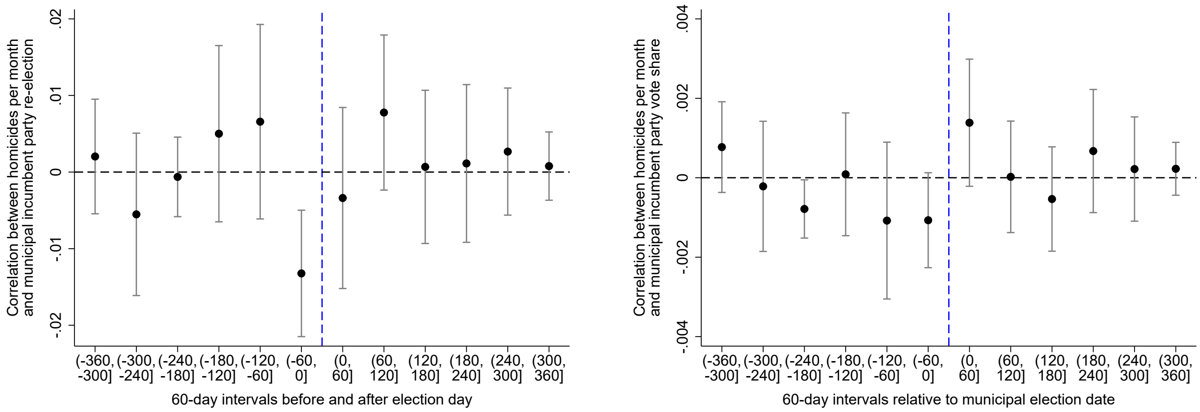
Since the homicide rate, as opposed to the idiosyncratic short-term shock exploited in the main paper, just before an election is highly correlated with the general homicide rate and broader trends, a meaningful test of increased homicides around elections requires a subtler design.[9] I use a panel fixed effects design to estimate how *deviations* in the homicide rate just before elections, relative

[9]The results of such an approach are thus similar to the small long-run estimates reported in Table 4.

Figure A4: Effect of pre-election homicides during 60-day intervals before and after election day (95% confidence intervals)

(a) Incumbent party re-elected

(b) Incumbent party vote share



Notes: Each coefficient is obtained from a single OLS regression of the outcome on homicide counts in each 60-day interval, as well as municipality and election month-year fixed effects.

to the homicide rate occurring earlier in the electoral cycle, affect vote choice. Specifically, I first use the raw difference between the average monthly homicide rate per 100,000 people in the two months prior to an election and the average monthly homicide rate per 100,000 people over the prior electoral cycle (i.e. preceding 34 months). A second measure then takes the proportional difference by normalizing these deviations by the average monthly homicide rate per 100,000 people over the prior electoral cycle. Finally, a third approach codes indicators for above- and below-median municipalities according to the two preceding variables. These measures seek to capture differences in the magnitude of pre-election homicide rate deviations across elections, and thus exploit variation in the intensity of these shocks across municipalities.

Table A17 reports the two-way fixed effect estimates from the analog of equation (5). The results, using this alternative (and less design-based) approach to capturing pre-election homicide rate deviations, also suggest that pre-election homicides decrease the incumbent party's vote share. Although not every estimate is statistically significant, column (1) firsts show that an increase in the raw pre-election deviation decreases the probability of the incumbent winning and the incumbent's vote share. These effects are clearer when separating above and below median deviations in column (2). Columns (3) and (4) report similar results when considering the proportional deviation instead.

A.8 Robustness checks for the moderating effect of local media

A.8.1 Alternative design parameters

Table A18 shows that the effects of local media are robust to alternative specifications that vary: (i) the definition of whether a precinct is covered by a media outlet (from 20% to 50% or 100% of voters receiving a commercial quality coverage signal); (ii) the restriction on precinct area (from at most 2km^2 to 1km^2, 5km^2, or unrestricted); and (iii) use the natural logarithm of the number of local media outlets (plus 1) instead of the number of local media outlets in the main specification.

Table A17: Panel fixed effects approach to estimating the effect of pre-election deviations in homicide rates on municipal incumbent electoral outcomes

	(1)	(2)	(3)	(4)
Panel A: Outcome: Incumbent party re-elected				
Deviation from average monthly homicide rate	-0.0028			
	(0.0080)			
Above median deviation from average monthly homicide rate		-0.1008**		
		(0.0448)		
Proportional deviation from average monthly homicide rate			-0.0115*	
			(0.0067)	
Above median proportional deviation from average monthly homicide rate				-0.1250***
				(0.0388)
Observations	2,583	2,583	2,485	2,485
Unique municipalities	847	847	806	806
Outcome mean	0.55	0.55	0.56	0.56
Homicide rates measure mean	0.39	0.49	0.42	0.49
Homicide rate measure std. dev.	2.20	0.50	2.01	0.50
Panel B: Outcome: Incumbent party vote share				
Deviation from average monthly homicide rate	-0.0009			
	(0.0011)			
Above median deviation from average monthly homicide rate		-0.0223***		
		(0.0082)		
Proportional deviation from average monthly homicide rate			-0.0020*	
			(0.0012)	
Above median proportional deviation from average monthly homicide rate				-0.0269***
				(0.0080)
Observations	149,541	149,541	148,895	148,895
Unique municipalities	847	847	842	842
Outcome mean	0.55	0.55	0.55	0.55
Homicide rates measure mean	0.39	0.49	0.43	0.49
Homicide rate measure std. dev.	2.21	0.50	2.03	0.50

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

A.8.2 Time-varying measure of local media

The main analysis applies the 2012 media coverage maps to each year in the sample. While relatively few new AM, FM, or television antennae started broadcasting between 1999 and 2012, some new permits were granted at the beginning and end of this period. To demonstrate robustness to adding this temporal variation in access to local media, I compute media coverage separately for each year to take into account media outlet entry. The set of control precincts within a given neighbor group that differ in access to local media from a given treated precinct can now vary over time. Accordingly, I define neighbor group fixed effects by the single treated precinct within each set. Unsurprisingly given the limited variation over time, Table A19 reports similar moderating effects of an additional local media outlet as found in the main paper.

Table A18: Specification tests for the moderating effect of access to local media on the effect of pre-election homicide shocks on precinct-level incumbent party vote share

	Outcome: Incumbent party vote share					
	50% precinct coverage (1)	100% precinct coverage (2)	1km^2 area restriction (3)	5km^2 area restriction (4)	No area restriction (5)	Logged media in main specification (6)
Homicide shock	-0.0165	-0.0153	0.0050	-0.0088	0.0074	0.0418
	(0.0261)	(0.0164)	(0.0282)	(0.0220)	(0.0109)	(0.0333)
Local media	0.0011	0.0011	0.0015**	0.0019***	0.0013**	
	(0.0009)	(0.0007)	(0.0007)	(0.0007)	(0.0005)	
Homicide shock × Local media	-0.0026**	-0.0023***	-0.0042***	-0.0036***	-0.0034***	
	(0.0011)	(0.0009)	(0.0013)	(0.0011)	(0.0007)	
Local media (log)						0.0216***
						(0.0079)
Homicide shock × Local media (log)						-0.0431***
						(0.0115)
Observations	27,335	43,598	22,045	31,514	97,388	26,134
Unique municipality-elections	376	598	314	470	1,186	397
Unique neighbor groups	2,477	4,061	1,846	2,621	7,756	2,186
R^2	0.64	0.61	0.63	0.62	0.51	0.64
Outcome mean	[0,0.87]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.97]	[0,0.88]
Outcome mean	0.46	0.44	0.46	0.45	0.43	0.46
Outcome standard deviation	0.15	0.15	0.14	0.15	0.16	0.15
Homicide measure mean	0.39	0.41	0.34	0.36	0.40	0.36
Media measure media mean	19.33	17.38	18.61	16.86	12.13	17.57
Media measure standard deviation	10.59	10.89	10.03	10.48	10.43	10.30

Notes: All specifications are estimated using OLS, and include neighbor group and election month-year fixed effects. All observations are weighted by the number of registered voters, while "control" precincts are further divided by the number of comparison units within each neighbor group. Instead of requiring that 20% of the electorate in a precinct is covered by a media outlet, columns (1) and (2) respectively define coverage by 50% or 100% coverage. Columns (3)-(5) vary the precinct maximum area sample restriction. Standard errors double clustered by municipality-election and neighbor group are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.9 Additional mechanisms results

A.9.1 Differential effects of homicide shocks by presence of a municipal police force

Table A20 compares the effects of pre-election homicide shocks across municipalities with and without their own police.[10] Municipalities without police forces, which includes all *delegaciones* within Mexico City (which became municipalities in 2016), were excluded from the main analysis. The results show that the effect of pre-election homicide shocks is concentrated in municipalities with their own police force. The test at the foot of the table indicates that pre-election homicide

[10]Municipalities without a police force include 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 National Census of Municipal Governments surveys. Missing years were imputed according to the following rule: I first used the last available data, and if no previous coding was available took the nearest year in the future.

Table A19: Moderating effect of access to local media on the effect of pre-election homicide shocks on precinct-level incumbent party vote share, using time-varying media outlet presence

	Outcome: Incumbent party vote share							
	No local media interaction (1)	Baseline spec. (2)	Interactive non-local media covariate (3)	Interactive imbalanced covariates (4)	Interactive spatial location (5)	Adjust for average media within 50m (6)	Interactions with longer-run homicide rates (7)	(8)
Homicide shock	-0.0756	-0.0103	-0.0116	-0.0111	0.0460	-0.0103		
	(0.0127)	(0.0238)	(0.0274)	(0.1444)	(0.0342)	(0.0238)		
Local media		0.0007	0.0008	0.0008	0.0014	-0.0036	-0.0036	-0.0036
		(0.0014)	(0.0014)	(0.0013)	(0.0013)	(0.0034)	(0.0017)	(0.0017)
Homicide shock × Local media		-0.0045	-0.0044	-0.0038	-0.0065	-0.0044		
		(0.0013)	(0.0013)	(0.0013)	(0.0015)	(0.0015)		
Non-local media			0.0007					
			(0.0011)					
Homicide shock × Non-local media			0.0000					
			(0.0009)					
Average monthly homicide rate (last 12 months before survey)							0.0000	
							(0.0003)	
Average monthly homicide rate (last 12 months before survey) × Local media							0.0000	
							(0.0000)	
Average monthly homicide rate (mayor's term)								0.0023
								(0.0068)
Average monthly homicide rate (mayor's term) × Local media								0.0000
								(0.0003)
Observations	23,047	23,047	23,047	23,047	23,047	8,196	23,047	23,047
Unique municipality-elections	392	392	392	392	392	389	392	392
Unique neighbor groups	2,002	2,002	2,002	2,002	2,002	1,746	2,002	2,002
R^2	0.63	0.64	0.64	0.64	0.66	0.66	0.60	0.60
Outcome range	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]	[0,0.88]
Outcome mean	0.46	0.46	0.46	0.46	0.46	0.46	0.46	0.46
Outcome standard deviation	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Homicide shock mean	0.35	0.35	0.35	0.35	0.35	0.36	19.81	1.39
Media measure media mean		15.10	15.10	15.10	15.10	14.84	15.10	15.10
Media measure standard deviation		8.79	8.79	8.79	8.79	8.69	8.79	8.79

Notes: All specifications are estimated using OLS, and include neighbor group and election month-year fixed effects. All observations are weighted by the number of registered voters, while "control" precincts are further divided by the number of comparison units within each neighbor group. Column (4) includes interactions between homicide shock and the following (standardized) covariates, which are omitted to save space: registered voters, average number of occupants per room, share illiterate, share of households that are economically active, share of households with basic amenities in their home, and share of individuals born in another state. Column (5) includes interactions between homicide shock and third-order polynomials in latitude and longitude and their cross-products. Column (6) restricts control precincts to those with an average distance to media outlets that is within 50 meters of the average distance for the group's corresponding "treated" precinct. Standard errors double clustered by municipality-election and neighbor group are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

shocks did not significantly impact electoral behavior in municipalities without their own police force. As noted in the main paper, this is consistent with voters recognizing that mayors have little control over crime in such cases.

A.9.2 Differential effects of homicide shocks across parties

While the results suggest that voters are sanctioning municipal incumbent parties for pre-election homicide shocks, voters may instead be holding the federal government to account. The following analyses show that voters are not simply sanctioning the federal incumbent party, although—in line with Ley's (2017) for DTO-related electoral violence—the effect of homicide shocks are somewhat more pronounced where the parties governing a municipality and the country are aligned.

I first establish that voters sanction municipal governments of all political stripes. Subsetting the sample by incumbents from the PAN, PRD, and PRI, columns (1)-(3) of Table A21 show that

Table A20: Effects of pre-election homicide shocks, by presence of municipal police force

	Incumbent party win (1)	**Incumbent party vote share** (2)
Homicide shock	-0.165***	-0.023***
	(0.052)	(0.007)
No municipal police force	-0.095	-0.020
	(0.120)	(0.016)
Homicide shock × No municipal police force	0.170*	0.034*
	(0.097)	(0.019)
Observations	2,880	180,420
Unique municipality-elections	2,880	2,875
R^2	0.45	0.39
Outcome range	[0,1]	[0,1]
Outcome mean	0.60	0.42
Homicide shock mean	0.42	0.42
No municipal police force mean	0.172	0.173
Homicide shock + Homicide shock × No municipal police force	0.005	0.011
	(0.081)	(0.017)

Notes: All specifications are estimated using OLS, include municipal and election month-year fixed effects, and weight observations by the number of registered voters. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the party of each municipal incumbent experiences significant reductions in their re-election probability. Although the effect is a little larger for the PAN, it is clear that voters distinguish the government of their municipality from the federal government. Unreported results further find no significant effect of homicide shocks on the municipal-level vote share of the federal incumbent party.

While voters attribute responsibility to municipal incumbents, vertical political alignment could still moderate the effect of pre-election homicide shocks. In line with Dell (2015), voters might believe that municipal incumbent parties are more responsible for local crime rates when their alignment with the federal government facilitates joint crime-reduction efforts. Accordingly, I code an indicator for elections where the municipal incumbent is from the same party as the president. For all but two of the years between 1999 and 2012 that comprise my sample, the PAN held the Mexican presidency; in 1999 and 2000, the PRI held the presidency. Column (4) of Table A21 shows that voter sanctioning of municipal incumbents is notably larger when they are aligned with the federal incumbent. In addition to heterogeneity by the presence of the municipal police force, this result further suggests that voters perceive differences in the capacity of different municipal governments to control homicides.

A.9.3 Alternative measure of news reports of violence between DTOs

To assess the robustness of the finding that the rate of reporting on homicides in the news does not increase during election campaigns, I replicate the analysis in Table 7 using a more expan-

Table A21: Effects of pre-election homicide shocks on municipal incumbent electoral outcomes, by incumbent party type

	PAN incumbents (1)	PRD incumbents (2)	PRI incumbents (3)	All incumbents (4)
Panel A: Outcome: Incumbent party re-elected				
Homicide shock	-0.162**	-0.050	-0.127**	-0.098*
	(0.070)	(0.060)	(0.051)	(0.059)
Homicide shock × Aligned with federal incumbent				-0.145
				(0.106)
Observations	601	429	1,356	2,583
Outcome range	[0,1]	[0,1]	[0,1]	[0,1]
Outcome mean	0.53	0.46	0.61	0.55
Outcome standard deviation	0.50	0.50	0.49	0.50
Homicide shock mean	0.41	0.52	0.39	0.41
Panel B: Outcome: Incumbent party vote share				
Homicide shock	-0.024*	0.003	-0.030***	-0.018**
	(0.013)	(0.014)	(0.010)	(0.008)
Homicide shock × Aligned with federal incumbent				-0.011
				(0.014)
Observations	51,099	20,355	72,084	149,541
Unique municipality-elections	607	431	1,355	2,578
Outcome range	[0,0.97]	[0,0.98]	[0,1]	[0,1]
Outcome mean	0.42	0.36	0.44	0.42
Outcome standard deviation	0.14	0.15	0.14	0.15
Homicide shock mean	0.41	0.52	0.39	0.41

Notes: All specifications are estimated using OLS, and include year fixed effects. Only column (4) includes municipality fixed effects, since the subsamples in columns (1)-(3) include many municipalities with only a single election. All observations are weighted by the number of registered voters. Standard errors clustered by municipality-election are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Correlation between monthly municipal homicide counts and government agency and newspaper reports on violence between gangs, by upcoming local elections

	Outcome: Number of inter-DTO violence reports per month					
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides per month	0.222***	0.175***	0.219***	0.172***	0.218***	0.172***
	(0.023)	(0.013)	(0.029)	(0.016)	(0.028)	(0.016)
Homicides per month ×			0.023	0.019		
Upcoming local election (4 months)			(0.032)	(0.023)		
Homicides per month ×					0.036	0.033
Upcoming local election (2 months)					(0.031)	(0.025)
Observations	110,957	110,957	110,957	110,957	110,957	110,957
Unique states	330	330	330	330	330	330
R^2	0.66	0.81	0.66	0.81	0.66	0.81
Outcome range	[0,102]	[0,102]	[0,102]	[0,102]	[0,102]	[0,102]
Outcome mean	0.28	0.28	0.28	0.28	0.28	0.28
Outcome standard deviation	2.13	2.13	2.13	2.13	2.13	2.13
Homicides per month mean	0.93	0.93	0.93	0.93	0.93	0.93
Homicides per month standard deviation	5.15	5.15	5.15	5.15	5.15	5.15
State × month-year fixed effects		✓		✓		✓

Notes: All specifications are estimated using OLS, include municipality and month-year fixed effects, and weight observations by the average number of registered voters in a municipality within the sample used in Table 2. Block-bootstrapped standard errors clustered by state (2,500 replications). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

sive sources of news reports. Specifically, I use news reports of violent crime assembled by Osorio (2015). His team first identified almost 42,000 reports of violence between DTOs within the archives of 105 national and local newspapers and federal and local government agencies between 2000 and 2010. He then automated the identification of incidents of violence between DTOs within the text—using Spanish grammatical structure to identify a perpetrator, type of action, and target of that action—and tagged the municipality and date of the reports.[11] Violent events include homicides as well as shootings, kidnappings, and torture.

This dataset approximates local news coverage of the principal forms of violence in Mexico across all municipalities over 11 years, but is also limited in several ways. First, classification algorithms are imperfect and rely on the source data representing all parts of the country accurately. Although such concerns are unavoidable, I validate below that the incidence of homicides is strongly correlated with reports of violence between DTOs. Second, Osorio's (2015) dataset reflects newspaper and government information, rather than radio and television broadcasts. Broadcast media coverage is, however, likely to cover similar events, especially given that newspapers produce considerable original content and 62% and 56% of radio station news directors in Mexico report sourcing news from national and local news outlets respectively (Larreguy, Lucas and Marshall 2020). Third, the dataset codes the reporting of events, but not how the events were reported. It is thus best suited to measuring selective reporting, although this likely correlates with the tone of news reports.

[11] Osorio (2015) also measures violent enforcement by the state. I exclude such cases because deaths caused by law enforcement are rarely classified as homicides. The results are not affected by combining both types of DTO violence.

Using the same regression specification as in the main paper, Table A22 reports similar results obtain using the broader set of news reports complied by Osorio (2015). The first two columns show that the actual incidence of homicides is strongly correlated with the number of reports of violence between DTOs, suggesting that this alternative measure of news coverage is capturing reporting on violent crime. However, columns (3)-(6) again find no evidence to suggest that such reporting is significantly greater in the month preceding local elections.