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

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I show that voter news consumption tracks electoral cycles, inducing performance indicators revealed before elections to substantially shape electoral selection. Examining homicides occurring before Mexican municipal elections, I demonstrate that: (i) voters consume more politically-relevant news before local elections; (ii) incumbent party re-election rates are reduced by pre-election homicide spikes, but not longer-term homicide rates; and (iii) electoral sanctioning increases with local media coverage. Such news consumption cycles reflect belief updating—rather than short memories—among voters demanding politically-relevant news before elections. By highlighting the importance of information acquisition cycles, these findings help explain variation in media's electoral influence and information's impact on electoral accountability.

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1 Introduction

Information has the potential to improve governance by enabling informed voters to select suitable politicians to represent them. Given the difficulty of directly observing an incumbent's competence or alignment with voter interests, performance on salient issues like economic growth, redistribution, public security, or corruption can serve as valuable signals of an incumbent's continuing suitability for office (Fearon 1999; Rogoff 1990). However, particularly in developing contexts, voters are often poorly informed about politics and current affairs (e.g. Keefer 2007; Pande 2011), or do not receive relevant information pertaining to their incumbent party (Larreguy, Marshall and Snyder 2018*b*; Snyder and Strömberg 2010).[1] This lack of politically-relevant information may explain why voters often fail to hold incumbents to account for their performance in office.[2]

Scholars and policymakers have responded by proposing that increasing access to media should improve electoral selection and accountability. This claim rests on media outlets reporting relevant indicators of incumbent performance that voters would not otherwise obtain, and receives some empirical support (see Larreguy and Marshall forthcoming; Prat and Strömberg 2013).[3] However, while it may be a precondition for efficient electoral selection, the focus on *access* neglects the reality that the availability of media coverage does not necessarily imply that voters actually choose to *consume* politically-relevant news.

In contrast with behavioral theories emphasizing voters' short memories (Zaller 1992), this article argues that the relationship between incumbent performance in office and electoral selection reflects the *timing* of Bayesian voters' news consumption. News consumption is likely to track electoral cycles if, before elections, either: (i) voters actively seek out politically-relevant information, e.g. for its consumption value (e.g. Baum 2002; Hamilton 2004), for work (Larcinese 2005), to impress others (e.g. Marshall 2019), or out of civic duty (e.g. Feddersen and Sandroni 2006); or (ii) media outlets increase their supply of such information. Regardless of whether such news consumption cycles are driven by demand or supply, I show that relevant incumbent performance indicators reported by media outlets before elections—when voters consume most news—have the potential to significantly influence election outcomes. Indeed, a simple career concerns model

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

[2] Evidence that voters in consolidating democracies punish or reward politicians for economic performance (see Anderson 2007; Duch and Stevenson 2008), levels of public security (Vivanco et al. 2015), malfeasance in office (see Ashworth 2012; Larreguy and Marshall forthcoming), or legislative effort (Humphreys and Weinstein 2012) is mixed.

[3] Various studies now compellingly show that media programming affects voting behavior (e.g. Adena et al. 2015; DellaVigna and Kaplan 2007; DellaVigna et al. 2014; Enikolopov, Petrova and Zhuravskaya 2011; Larreguy, Marshall and Snyder 2018*b*; Martin and Yurukoglu 2017; Snyder and Strömberg 2010).

predicts that Bayesian voters in low information environments will seek to retain high-quality incumbents by applying a performance cutoff rule based on the few noisy performance signals that they observe before elections, even when such signals reflect both performance-relevant characteristics and idiosyncratic shocks. These news consumption cycles could, for example, help explain the apparently large electoral impact of "October surprises" like the revelation shortly before the 2016 U.S. presidential election that the FBI was continuing to investigate Hillary Clinton.

This article examines the extent to which news consumption cycles drive voting behavior in the context of Mexican voters using local homicide rates to identify high-quality municipal incumbent parties. Following significant democratizing and decentralizing reforms in the 1990s, Mexican elections have become competitive at national and sub-national levels, while responsibility for public service delivery such as policing is increasingly assigned to municipal, rather than state or national, governments. Public security is a major concern for Mexican voters, as in other Latin American nations, and thus local homicides represent a salient measure of incumbent party performance.[4] Moreover, since many voters are poorly informed about public affairs and a significant portion of the electorate lacks strong partisan ties (Chong et al. 2015; Greene 2011), there is scope for pre-election crime reports to influence voting behavior.

Leveraging plausibly exogenous variation in temporal proximity to local elections, municipal-level homicide spikes occurring before elections, and then access to local broadcast media stations, I test the key features of the news consumption cycles argument at the individual and aggregate levels. First, to demonstrate the existence of cycles in political news consumption in this context, I exploit the irregular timing of survey waves with respect to Mexican states' staggered electoral cycles to isolate variation in the proximity of local—municipal and state legislative—elections. I indeed find that voters report consuming more political news through radio and television in the months preceding local elections. For some less-informed voters, they effectively only consume political news during elections campaigns. These changes are reflected in a 0.2 standard deviation increase in topical knowledge of public affairs.

Second, I leverage month-to-month volatility in homicide counts within municipalities to isolate the idiosyncratic component, from the quality-based component, of the pre-election homicide count that voters observe. Specifically, I compare "shocked" municipal elections that experienced more homicides in the two months preceding the election to "control" elections from within the same municipality that experienced at least as many homicides in the two months after the election. The distribution of homicide shocks is consistent with random sampling variability, balanced over

[4] Although municipal mayors were only permitted to seek re-election in 2018, voters generally hold the incumbent's party responsible for performance in Mexico's party-centric system (e.g. Arias et al. 2019; Chong et al. 2015; Larreguy, Marshall and Snyder 2018*b*).

99 predetermined covariates, and inconsistent with manipulation by politicians or drug trafficking organizations (DTOs). Based on the career concerns model's assumption that voters cannot differentiate whether a spike in homicides reflects a low-quality incumbent party or an idiosyncratic adverse shock, I then employ an instrumental variables strategy using the idiosyncratic component—a pre-election homicide shock—to instrument for the homicide rate immediately before elections.

Despite being uncorrelated with broader homicide levels and trends, spikes in the pre-election incidence of homicides driven by the idiosyncratic component have substantially affected Mexican election outcomes between 1999 and 2013. Reduced form estimates show that pre-election homicide shocks reduced the incumbent party's probability of winning by 10 percentage points, while the instrumental variable estimates indicate that each additional homicide per 100,000 people reduced the incumbent party's re-election probability by 8 percentage points. Furthermore, and consistent with most voters relying on the pre-election signals available when they actually consume news, panel fixed effects estimates show that longer-run homicide rates—likely a better proxy for incumbent competence, but which is unlikely to be observed by voters predominantly consuming news before elections—are not significantly correlated with an incumbent party's electoral prospects. Additional analyses indicate that the parties of incumbent mayors, rather than higher levels of government, are sanctioned electorally (see also Vivanco et al. 2015).

Third, to demonstrate that electoral sanctioning derives from media coverage, I join individual-level news consumption patterns with aggregate-level voting behavior by showing that *local* broadcast media drive the negative effect of pre-election homicides on incumbent support. Local broadcasters report politically-relevant news that voters may not be able to access otherwise, and local crime receives substantial media coverage (e.g. Osorio 2015; Trelles and Carreras 2012). Using radio and television coverage maps, I leverage variation in signal access across neighboring electoral precincts to plausibly identify the effects of broadcast media in relatively urban areas (see also Larreguy, Marshall and Snyder 2018*b*; Spenkuch and Toniatti forthcoming). On average, I show that each additional local media station based within a precinct's own municipality reduces the incumbent party's vote share by 0.3 percentage points following a pre-election homicide shock. While incumbent parties are rewarded to a lesser degree for not experiencing homicide shocks, precincts covered by few local media stations do not significantly punish homicide shocks. Neither non-local media stations nor party ad shares on accessible outlets can account for these findings.

Examination of the mechanisms suggests that voter behavior is driven by voters demanding more news before elections and updating from the events reported in the news at this time. First, survey data shows that pre-survey homicide shocks that coincide with election campaigns increase concern about public security. However, pre-survey homicide shocks do not increase public secu-

rity concerns when they occur outside election campaigns, when voters consume significantly less news. The latter finding is inconsistent with alternative explanations based on voters' short memories (Zaller 1992), naive "emotional" reactions (Achen and Bartels 2017), or policy responses designed to rectify bad outcomes (Brollo 2009; Grossman and Michelitch 2018). Moreover, voting behavior is consistent with belief updating based on proxies for voter prior beliefs, based on homicide shocks before the previous election. These findings echo extant studies documenting relatively sophisticated voter updating in India (Banerjee et al. 2011), Italy (Kendall, Nannicini and Trebbi 2015), and Mexico (Arias et al. 2019).

Second, I leverage difference-in-differences designs to show that receiving local homicide information reflects news consumption cycles driven by voter demand for election-oriented content, rather than greater media reporting on local homicides before elections. Google search data indicate that voter interest in elections, but not homicides or crime, increase before local elections. In contrast, the likelihood that newspapers report on violent crime does not change before elections.

The overarching finding that news consumption cycles induce poorly-informed voters to heavily weight recent performance indicators makes several contributions. First, the findings extend a large literature documenting persuasive effects of broadcast media content (see Larreguy and Marshall forthcoming and Prat and Strömberg 2013 for detailed reviews), and especially the importance of local news (Ferraz and Finan 2008; Larreguy, Marshall and Snyder 2018*b*; Snyder and Strömberg 2010). In contrast with extant studies focusing on differential *access* to media, I demonstrate that the effects of access to media also vary systematically with the timing of voter news *consumption*. By highlighting the importance of news consumption patterns for election outcomes, my findings advance a nascent literature exploring the consequences of endogenizing political information acquisition (Chen and Yang forthcoming; Hobbs and Roberts 2018; Marshall 2019). Moving beyond political knowledge and attitudes, I show that information acquisition incentives—driven here by election cycles—can also alter electoral outcomes.

Second, integrating cyclical news consumption into models of electoral selection helps account for the mixed evidence that incumbent performance information influences voting behavior. For example, the timing of information consumption may explain why pre-election corruption revelations have produced larger electoral impacts than revelations occurring earlier in a electoral cycle (Bobonis, Cámara Fuertes and Schwabe 2016; Brollo 2009; Chang, Golden and Hill 2010), why voters overweight recent economic performance in evaluating presidents at the end of their term (Achen and Bartels 2017; Healy and Lenz 2014), and why short-term economic fluctuations have induced electoral volatility (Roberts and Wibbels 1999). Furthermore, the importance of local media coverage in my findings reiterates the notably larger effects of the media's dissemination

of incumbent performance information (Banerjee et al. 2011; Ferraz and Finan 2008; Keefer and Khemani 2014; Larreguy, Marshall and Snyder 2018*b*) relative to dissemination campaigns that provide information to voters in person (e.g. Chong et al. 2015; de Figueiredo, Hidalgo and Kasahara 2013; Dunning et al. forthcoming; Humphreys and Weinstein 2012). Nevertheless, this article still tempers the pro-accountability potential of the media by illustrating how dependent voter responses are on timing information dissemination campaigns to coincide with voter engagement.

Third, my findings add to a growing literature highlighting the political economy implications of crime. At the voter level, the media's agenda setting role has been shown to shape perceptions of a given level of crime (Mastrorocco and Minale 2018). This article demonstrates that variation in criminal activity also remains a root cause of voter beliefs and politician approval, but that its effects are moderated by the extent to which media market are incentivized to report local news (Larreguy, Marshall and Snyder 2018*b*; Snyder and Strömberg 2010). I further show that homicides spikes are sufficiently important to enough voters to alter electoral outcomes. My finding that even the idiosyncratic component of crime influences electoral selection complements recent studies suggesting that organized crime affects electoral outcomes (Buonanno, Prarolo and Vanin 2016; Vivanco et al. 2015) and the policies pursued by a government once in office (Di Cataldo and Mastrorocco 2019), while also chiming with evidence that politics in turn shapes criminal presence and activity (Acemoglu, De Feo and De Luca forthcoming; Dell 2015) and creates incentives to threaten politicians (Alesina, Piccolo and Pinotti 2019; Ley 2018).

Finally, the importance of news consumption cycles contributes to the broader political economy literature emphasizing cyclical political behavior and outcomes. Previous studies have observed that economic policies and output (e.g. Brender and Drazen 2005), the distribution of aid (Faye and Niehaus 2012), and corruption levels (Bobonis, Cámara Fuertes and Schwabe 2016) reflect anticipated electoral cycles. My findings similarly chime with the strategic decision of politicians to act surreptitiously when the media is covering other major events (Durante and Zhuravskaya 2018; Eisensee and Strömberg 2007). However, unlike cycles driven by politician actions, I show that cyclical voting behavior driven by news consumption also has electoral implications.

2 Theoretical framework

In contrast with studies emphasizing the importance of voter *access* to news (Ferraz and Finan 2008; Larreguy, Marshall and Snyder 2018*b*; Snyder and Strömberg 2010), this section proposes that the impact of performance indicators reported in the news on electoral selection depends upon the timing of voter news *consumption*. I first argue that news consumption follows a cycle

where many voters only seriously encounter politically-relevant information—performance indicators pertaining to salient characteristics of incumbents that a voter can sanction electorally—just before elections. Integrating this news consumption cycle into a simple career concerns model, I then show that poorly-informed Bayesian voters rely on noisy indicators of incumbent competence or alignment with voter interests in the news just before they cast their ballot. The model, which posits that voters that primarily consume news before elections sanction incumbent parties for homicides both within and beyond the incumbent's control, informs the empirical strategy.

2.1 Causes of political information cycles

Voters seeking to influence outcomes of mass elections generally face strong incentives to remain "rationally ignorant" (Downs 1957). However, there are a number of reasons to believe that consumption of politically-relevant news increases before elections.

First, to the extent that political news is a consumption good (e.g. Baum 2002; Hamilton 2004), electoral campaigns are likely to particularly engage voters. While coverage of political events out of election season is often more focused on specific policy issues, dramatic campaigns appeal to a broader audience (e.g. Farnsworth and Lichter 2011).

Second, voters may acquire political information before elections for strategic reasons beyond affecting election outcomes. Marshall (2019) shows that voters in social networks that collectively value knowledge about politics acquire information about election campaigns to cultivate a reputation as politically sophisticated. Such dynamics may be especially strong around elections when political discussion is more common (Baker, Ames and Renno 2006; Marshall 2019). Similarly, voters may acquire information ahead of elections to improve decision-making in other domains (Larcinese 2005), e.g. if investment choices depend upon political risks. The information consumed for such social and vocational reasons may also inform vote choices.

Third, voters may feel a civic duty to become informed around elections. Feddersen and Sandroni (2006) argue that "ethical" independent voters are intrinsically motivated to cast an informed ballot, where information acquisition is concentrated among voters facing the lowest acquisition costs. A large body of research suggests that a voter's civic duty is correlated with political engagement and participation (e.g. Blais 2000). Relatedly, Degan (2006) suggests that voters acquire information to avoid the psychic costs of voting for non-preferred candidates.

Fourth, voters without instrumental or intrinsic motivations could still consume more information around elections simply because the media devote more time and space to political news, advertising, and specific election programming. Even for uninterested voters, such information becomes difficult to avoid—especially where there are relatively few channels available (Prior

2007) or such information is recast as entertainment (Baum 2002)—and performance indicators are increasingly placed in the context of appraising incumbent politicians and parties (Semetko and Valkenburg 2000).

Regardless of whether politically-relevant news consumption is driven by voter demand for information or media supply of information, such consumption is likely to increase prior to elections. News consumption cycles would cause many voters—especially in developing contexts where baseline political knowledge is comparatively low (Keefer 2007; Pande 2011)—to essentially engage with the news before the election for the first time in an electoral cycle. Relatively politically engaged voters are, in turn, likely to ratchet up their exposure to political news.

2.2 Consequences for electoral selection

Provided that voters can access relevant and credible news and seek to use incumbent performance indicators to elect better politicians, news consumption cycles may influence electoral selection. The following career concerns model, with multiple signals pertaining to a single task, shows that this is particularly true among poorly-informed voters that have little alternative to using noisy pre-election news to inform their vote choice. The model is applied to a setting where voters use homicide rates to infer the competence of their municipal incumbent party.

2.2.1 Setup

Consider an incumbent party I seeking re-election in a given municipality. For simplicity, there are two electoral terms $t \in \{1,2\}$, each containing J discrete periods (e.g. month) during which a noisy signal of the incumbent's underlying competence θ_I can be observed.[5] The signal $H_{t,j}$ is the homicide rate in the municipality during period $j = 1,...,J$ of term t. The terms are split by an election, where the signal helps a unit mass of voters to decide whether to re-elect I or instead elect challenger party C.

The incumbent party seeks to retain office. For each term in office, I receives "ego" rents $R > 0$. Each incumbent party is ascribed a fixed level of competence θ_I drawn by nature from normal distribution $\mathcal{N}(\hat{\theta}, 1/h_\theta)$, where the realization is known neither to I nor voters.[6] At the beginning of each term, I can exert costly effort $a_t \in [0,\infty)$ to reduce the homicide rate, in order to convince voters that they are sufficiently competent to merit re-election. 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 cost of I's effort is $c(a_t)$, where

[5] This reflects the simplifying two-period assumption, although the results generalize to infinitely-repeated games.
[6] Signaling models instead assume that the incumbent's type is only known to the incumbent (e.g. Rogoff 1990).

$c'(\cdot) > 0$, $c''(\cdot) > 0$, and $c(0) = 0$. Looking forward, I maximizes $\delta p(a_t)R - c(a_t)$, where $p(a_t)$ is the probability of re-election and payoffs in the next term are discounted by $\delta \in (0,1)$. Challenger party C's competence is a random draw from the same pool as the incumbent party.

Voters can observe the homicide rate in a given period by aggregating media reports. Akin to the performance indicators analyzed by Ashworth (2005) and Holmström et al. (1999), this signal of incumbent competence is given by:

$$H_{t,j} = b - \theta_I - a_t + e_{t,j}, \tag{1}$$

where $e_{t,j} \sim \mathcal{N}(0, 1/h_e)$ is an iid period-specific shock whose distribution is common knowledge, and $b > 0$ is the baseline homicide rate. Intuitively, $H_{t,j}$ is greater for less competent and low-effort incumbents, but also reflects idiosyncratic factors beyond the incumbent's control.

Finally, voters vote for their preferred party for the second term, based on the sum of their beliefs about the party's competence and their "ideological" bias. 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 $(\theta_I, a_t, e_{t,j})$ are unobservable, voters draw inferences about θ_I from media reports revealing $H_{j,t}$. To capture news consumption cycles, I assume that voters only consume media reports during the last $N \leq J$ periods before the election. Voters accordingly update their posterior beliefs about incumbent competence from signals $H_{t,J-N}, ..., H_{t,J}$. The uniformly-distributed ideological bias $\sigma_i \sim \mathcal{U}(B - \frac{1}{2\psi}, B + \frac{1}{2\psi})$ of voter i toward I is realized after a_t is selected, where $B \in \mathbb{R}$ is the mean bias toward the incumbent. Each voter i determines their vote choice $v_i \in \{I, C\}$ by comparing the expected utility of voting for I and C.

The game's timing can be summarized as:

1. Nature draws incumbent party competence θ_I and challenger party competence θ_C from $\mathcal{N}(\hat{\theta}, 1/h_\theta)$ in a municipality. These fixed realizations are not known to parties or voters.

2. At the beginning of term $t = 1$, the incumbent chooses a_1.

3. Each voter observes the last N independent homicide count realizations before the election, $H_{1,J-N}, ..., H_{1,J}$, and each voter's ideological bias σ_i is realized.

4. Each voter decides whether to re-elect the incumbent party or not, $v_i \in \{I, C\}$. The party with the majority of votes wins.

5. At the beginning of $t = 2$, the election winner chooses a_2.

6. Each voter observes the last N independent homicide count realizations, $H_{2,J-N}, ..., H_{2,J}$.

2.2.2 Equilibrium and empirical implications

Working backwards, any incumbent party in the second term ($t = 2$) exerts effort $a_2^* = 0$, given that they lack re-election incentives. At the election completing the first term ($t = 1$), voter i then votes for I ($v_i = I$) when:

$$\mathbb{E}[\theta_I | H_{1,J-N}, ..., H_{1,J}, \hat{a}_1] + \sigma_i \geq \mathbb{E}[\theta_C], \tag{2}$$

where $\hat{a}_1$ is the voter's conjecture about incumbent effort that needs to be filtered out. Upon observing the homicide rate in the last N periods, i' posterior belief about incumbent competence is normally distributed with expectation:

$$\mathbb{E}[\theta_I | H_{1,J-N}, ..., H_{1,J}, \hat{a}_1] = \lambda(N)[b - \hat{a}_1 - \overline{H}_1(N)] + [1 - \lambda(N)]\hat{\theta}, \tag{3}$$

where $\lambda(N) := \frac{Nh_e}{h_\theta + Nh_e}$ is the weight attached to the mean observed signal, $\overline{H}_t(N) := \frac{1}{N}\sum_{j=J-N}^{J} H_{t,j}$. Rearranging, voter i then votes to re-elect I if the observed homicide rate is sufficiently low: $\overline{H}_1(N) \leq \frac{\sigma_i}{\lambda(N)} + b - \hat{\theta} - \hat{a}_1$. Aggregating across voters, the incumbent party's vote share is:

$$V_I = \frac{1}{2} + \psi B + \psi\lambda(N)[b - \hat{\theta} - \hat{a}_1 - \overline{H}_1(N)]. \tag{4}$$

Intuitively, the incumbent's vote share reflects average ideological bias B and voter inferences about the incumbent's competence—and thus future performance—based on the homicide rate (after filtering out the baseline homicide level b and incumbent effort a_1^*).

At the beginning of the first term, the incumbent selects their effort to reduce the number of homicides in anticipation of this voting rule. Given the ex ante re-election probability (i.e. before homicides are realized), the incumbent solves:

$$\max_{a_1 \in [0,\infty)} \left[1 - \Phi\left(\frac{\hat{a}_1 - a_1 - \frac{B}{\lambda(N)}}{h}\right)\right] \delta R - c(a_1), \tag{5}$$

where $h := \sqrt{\frac{1}{h_\theta} + \frac{N}{h_e}}$ and $\Phi(\cdot)$ is the normal cumulative distribution function. In an interior equilibrium, voters' expectations of effort are correct ($\hat{a}_1 = a_1^*$), implying that effort is:

$$a_1^* = (c')^{-1}\left(\frac{\phi\left(-\frac{B}{h\lambda(N)}\right)\delta R}{h}\right), \tag{6}$$

where $\phi(\cdot)$ is the normal probability distribution function.[7] As previous career concern models have shown (Ashworth 2005), effort is thus increasing in discounted rents δR, the precision of voter prior beliefs (h_θ), and the signals' precision (h_e).

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

Proposition 1. *Incumbent party I is re-elected if* $\overline{H}_1(N) \leq \frac{B}{\lambda(N)} + b - \hat{\theta} - a_1^*$, *and receives vote share* $V_I = \frac{1}{2} + \psi B + \psi\lambda(N)[b - \hat{\theta} - a_1^* - \overline{H}_1(N)]$. *In such an equilibrium:*

1. *Incumbent victory and vote share decreases in* $\overline{H}_1(N)$, *and the magnitude of this decline increases in* ψ *and decreases in* h_θ.

2. *As N becomes large, incumbent electoral prospects are, on average, unaffected by performance signals.*

Proof: follows straightforwardly from the analysis above. ■

The result that incumbents overseeing higher homicide rates are less likely to be re-elected reflects two sources of variation in homicide rates. First, as desired by voters, the sanctioning of higher homicide rates in part captures deselection of less competent incumbents. Second, because voters can only imperfectly extract underlying competence from the signals received, Bayesian voters will also in part select incumbents on the basis of exogenous increases in homicide counts. In contrast, voters anticipate incumbent effort to reduce homicide rates; such efforts are thus both inefficient and ineffective in equilibrium.

Importantly, the number of signals received (N) determines the extent to which voters respond to homicides induced by incumbent competence and idiosyncratic oscillations beyond the incumbent's control. When N is large, the idiosyncratic shocks average out (i.e. $\lim_{N\to\infty} \frac{1}{N}\sum_{j=J-N}^{J} e_{1,j} = 0$) and the posterior belief is driven by the signals received (i.e. $\lim_{N\to\infty} \lambda(N) = 1$). In this case, voters can parse the incumbent party's competence θ_I from the many signals received and avoid selecting incumbent parties on the basis of random oscillations in homicide rates beyond their control. Among highly informed voters, longer-term homicide rates (i.e. the sum of many signals) are thus expected to drive vote choices.

In contrast, when N is small, voters will select incumbent parties partly on the basis of homicides beyond their control. Intuitively, voters that receive a small number of signals before elections can only imperfectly infer incumbent party competence, and must tolerate error when attempting to elect competent politicians—especially when competence is important relative to ideology (i.e. high ψ). Although voters place less weight on a small number of signals in casting their ballot

[7] The second-order condition holds when: $\delta R < \left(\frac{1}{h_\theta} + \frac{N}{h_e}\right) \frac{c''(a_1^*)}{\phi'\left(\frac{B}{h\lambda(N)}\right)}$.

(as $\lambda'(N) > 0$), the observed homicide rates remains informative about underlying competence. As Proposition 1 demonstrates, voters are particularly reliant on homicide rates when they possess imprecise priors beliefs (i.e. low h_θ). In short, a few noisy signals before elections can 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: whether political news consumption increases just before elections; whether homicides reported in the news before elections influence voting behavior; and whether such voting behavior is driven by access to local media outlets likely to report such information. These hypotheses are examined in Mexico, where many voters remain poorly informed about public affairs, in the context of local homicide rates—an important performance indicator on a salient topic frequently reported by local media—ahead of municipal elections.

3 Municipal governance, homicide rates, and media in Mexico

Despite holding regular elections, Mexico was effectively a one-party state until the late 1990s (e.g. Cornelius 1996; 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).[8] The PRI reclaimed the presidency from the PAN in 2012, after the PAN broke the PRI's stranglehold on the presidency in 2000. Despite growing partisan alignment, often induced by clientelistic exchanges, many Mexicans have yet to develop strong ties to specific political parties (Greene 2011; Lawson and McCann 2005).

Mexico's federal system is divided between three administrative and elected layers of government: the federal government, 31 states (and the Federal District of Mexico City), and approximately 2,500 municipalities. Constitutional reforms in the mid-1990s substantially increased mayoral autonomy over the provision of local public services (see Diaz-Cayeros, González and Rojas 2006), inducing municipal spending to rise to around 20% of total government spending.[9]

Municipal mayors have typically been elected every three years to non-renewable terms,[10] and entered office between three and seven months after election day. Municipal elections are almost always held concurrently with state legislative elections, although gubernatorial elections are often held separately. I refer to simultaneous municipal and state legislative elections as "local elections."

[8] Since 2015, a fourth major competitor—MORENA—has emerged, and swept the 2018 elections.

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

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

Such local elections have become increasingly competitive, with fewer than 50% of municipal incumbent parties being re-elected and 31% of elections being won by less than 5 percentage points. The majority of local elections do not coincide with federal elections.[11]

Although incumbent mayors could not seek re-election, Mexico's party-centric system ensures that voters hold incumbent parties accountable for the performance of individual politicians. First, voters primarily decide between party labels, not individual candidates. Arias et al. (2019) report that 80% of voters can identify the party of their municipal incumbent; this far exceeds the probability of correctly naming individual local incumbents (Chong et al. 2015; Larreguy, Marshall and Snyder 2018*a*). Second, differences in candidate selection mechanisms across parties at the state level ensure that candidate choices are highly correlated within parties (Langston 2003). Third, various studies find that voters select parties on the basis of an aligned incumbent's malfeasance in office (Arias et al. 2019; Chong et al. 2015; Larreguy, Marshall and Snyder 2018*b*).

3.1 Public security forces

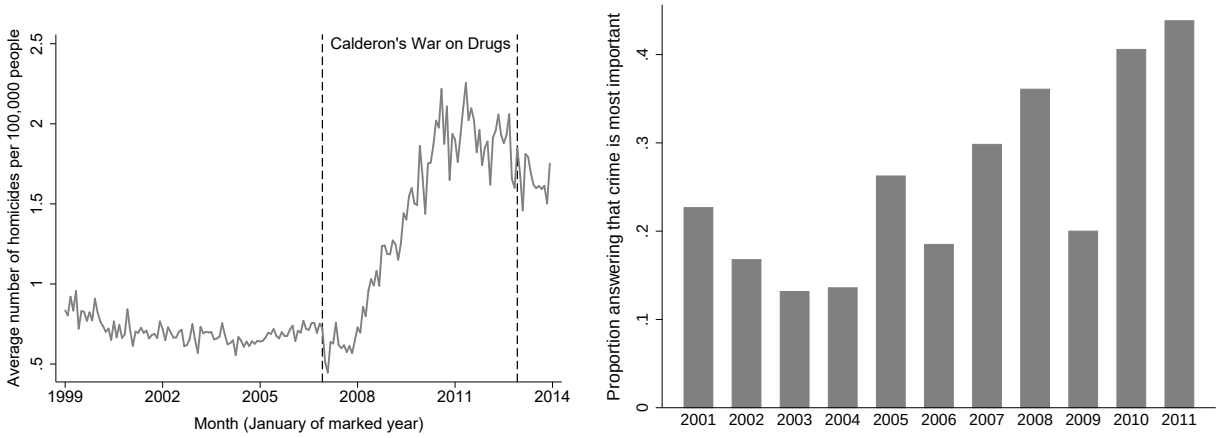
Responsibility for public security is shared across levels of government, and like other federal systems such as the United States, mayors play an important role in fighting crime. Both state and federal laws can be used to prosecute criminals, and uniformed (preventive) and investigative police forces exist at both the federal level and in each of Mexico's 31 states and the Federal District of Mexico City. State and federal police, and increasingly the army, are responsible for investigating major crimes in different jurisdictions. State police investigate state crimes such as homicides, while federal officers focus on organized crime (Reames 2003).

However, around three-quarters of municipalities also possess their own police force. Municipal forces support higher-level operations and supply information, but their principal role is preventive: they primarily patrol the streets and maintain public order, address administrative issues, and respond first to criminal incidents (Reames 2003; Sabet 2010).[12] In municipalities with their own police force, mayors can influence the medium-term incidence of crime. Mayors choose the local police chief and set local policies, including levels of cooperation with other forces. PAN mayors played a key role in supporting President Calderón's "kingpin" strategy that cracked down on Mexico's DTOs (Dell 2015), while political alignment across neighboring municipalities has reduced rates of violent crime (Durante and Gutierrez 2015). Municipal police are by far the most numerous, accounting for more than half of Mexican enforcement personnel (Sabet 2010).

Given the number of municipal police officers and their "on-the-streets" presence, it is not

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

[12] A central police force controlled by the mayor of Mexico City covers all delegations within the Federal District.



(a) Trends in monthly homicides in the average Mexican municipality, 1999-2013

(b) Voters listing crime as the most important problem that Mexico faces, 2001-2011

Figure 1: Crime in Mexico

Notes: The data in Figure 1a are from INEGI, and monthly averages are weighted by 2015 municipal population. The data in Figure 1b are from the 2001-2011 Latinobarómetro surveys; crime includes concerns about crime/public security, drug trafficking, violence, or terrorism/political violence.

surprising that municipal police are the foremost police force voters' minds. When asked which types of law enforcement authorities they are aware of, the 2010 National Insecurity Survey (ENSI) reports that while 75% of voters could identify municipal police, only 38% and 48% respectively recognized state and federal forces.

3.2 Trends in homicide rates

According to the United Nations Office on Drugs and Crime (UNODC), Mexico suffers one of the world's highest homicide rates. In 2011, 22.6 people per 100,000 were intentionally murdered. This represents the 20th highest homicide rate in the world—slightly less than South Africa, Colombia, and Brazil, and slightly more than Nigeria, Botswana, and Panama. After briefly declining since 2011, 2017 was the highest yet recorded.

Figure 1a plots the intentional homicide rate per month occurring in the average municipality between 1999 and 2013. Mexico's National Institute of Statistics, Geography, and Information (INEGI) defines intentional homicides as unnatural deaths on the basis of coroner reports.[13] The

[13] Although there is no official measure of drug-related homicides enshrined in Mexican law, the federal government has sporadically released monthly data. This data also suffers from various problems beyond its short time-series (see Heinle, Rodríguez Ferreira and Shirk 2014), while voter fears about public security also reflect equally-prevalent non-drug related homicides (of which only a tiny fraction occur within families).

monthly homicide rate has been substantial throughout this period, but increased dramatically after President Calderón entered office in December 2006 and began Mexico's "War on Drugs." The homicide clearance rate is only approximately 20% (México Evalúa 2012), and drug-related homicides—which are regionally concentrated—represent around half of the homicides over this period (Heinle, Rodríguez Ferreira and Shirk 2014). However, many municipalities only rarely experience a homicide; in fact, only one homicide occurs in the median municipality each year.

Unsurprisingly, voters are concerned about Mexico's high rates of violent crime. Like many other Latin American countries, 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 most of the 2000s, public security registered as the most salient issue for voters, ahead of the economy.

3.3 Media coverage and voter political knowledge

As in most developing countries, broadcast media is voters' primary source of news. According to the 2009 Latinobarometer, 83% of Mexicans receive political information from television, 41% from radio, 30% from newspapers, and 41% from family, friends, and colleagues. In 2008, around 10% listened to radio every day, while the average person watched 4 hours of television a day (Ibope/AGB México 2009). In contrast, only 21% had access to the internet at home in 2010, while 3G coverage only starting to expand in 2011.[14] Broadcast media coverage thus plays a central role in determining whether voters receive politically-relevant information.

In 2009, Mexico contained 852 AM radio stations, 1,097 FM stations, and 1,255 television stations, which generally cover relatively small geographic areas (see Figure 3 below). Most stations form part of broader regional and national radio or television networks—such as Grupo ACIR, Radiorama, Televisa, and TV Azteca—where affiliates share branding or are owned-and-operated subsidiaries (Larreguy, Marshall and Snyder 2018*b*). Among radios, 83% report news more than once a day, and typically report on municipal rather than national issues (Larreguy, Lucas and Marshall 2016). For television stations, identical entertainment content is generally bought from or relayed by network providers. However, while national news is typically centrally provided, affiliates and regional subdivisions emitting from major cities within each state also provide significant local news content. Of the 52 distinct television channels (excluding Mexico City's 24-hour news channels) for which schedules were available in 2015, the average channel broadcast 3.6 hours of news coverage each weekday (both before and after the June elections). Slightly less than half of this news is devoted to state- or city-specific programming. Private radio and television outlets

[14] Even in 2010 and 2011, 60% of Latinobarómetro respondents reported never using the internet.

rely on advertising revenues to support themselves, and thus face strong incentives to tailor their programming to local audiences (Larreguy, Marshall and Snyder 2018*b*).

Homicides are not always reported in great depth, but are regularly covered in print (Osorio 2015) and are "omnipresent" in the local broadcast media (Trelles and Carreras 2012).[15] Based on a survey of all local radio stations conducted in 2016 and 2017 as part of another study (Larreguy, Lucas and Marshall 2016), 96% report regularly covering local security stories, while Table 9 below demonstrates that newspaper reporting of violent crime indeed tracks the homicide rate. The 2010 ENSI survey also reports that 87% of respondents learn about public security in the country and in their state from television news programs, while 34%, 29%, and 9% respectively learn from radio news programs, periodicals or newspapers, and the internet.[16] Furthermore, 82% of voters believe that television news has the most important influence on public opinion.

Despite claiming reasonable levels of attention to political news, voters' knowledge of public affairs is limited. I show below that only half of Mexicans can answer basic questions about politics. Chong et al. (2015) similarly report that voters are generally unaware of mayoral responsibilities and performance in office. This lack of knowledge suggests that many voters may possess weak prior beliefs about their incumbent party's suitability for office, and may thus rely heavily on news reports when they choose to follow the news.

4 The existence of political news consumption cycles

The core of the political information cycles argument rests on voters consuming more politically-relevant news just prior to elections. While some voters only start consuming politically-relevant news during election campaigns, others may consume more than before. This section first estimates the effect of upcoming local elections on such consumption patterns, providing survey-level evidence for the existence of such political information cycles.

4.1 Data

I use four cross-sectional waves of the National Survey of Political Culture and Civil Practices (ENCUP) conducted over several weeks in November 2001, February 2003, December 2005, and

[15] For example, see this local news report of an gangland-style murder in Monterrey. The same news station also reports on domestic murders, e.g. this case of a woman strangling her partner. News programs may continue to report on arrests and the rare cases that go to trial, especially when the defendants are found guilty.

[16] Comparatively few learn from work colleagues (5%) or family, friends, and neighbors (13%).

August 2012.[17] The surveys were commissioned by the Interior Ministry and designed to be nationally representative. Each round draws stratified random samples of around 4,500 Mexican voters for face-to-face interviews from pre-selected electoral precincts within urban and rural strata.[18] The pooled sample includes up to 15,976 respondents across 523 municipalities from within 31 states. The Federal District is excluded from all analyses to maintain comparability with the electoral results.

Political news consumption is measured in two ways. First, I examine the frequency with which voters report watching or listening to the news, programs about politics, or programs about public affairs.[19] To understand the margins at which consumption changes, I distinguish 5 consumption intensities: never, at some point, at least monthly, at least weekly, and daily. I code indicators for ever consuming political news, at least weekly consumption, and a 5-point scale (from 0 to 4). Second, I assess whether such self-reported news consumption translates into greater political knowledge. I focus specifically on topical political knowledge that voters are more likely to encounter through the media (Barabas et al. 2014). Topical political knowledge is measured as the first (standardized) factor from a set of indicators coding correct responses to simple factual questions regarding recent political events and the partisanship of a respondent's state governor.[20] The average respondent answered around half the questions correctly. An advantage of this measure is that voters cannot over-report their knowledge.

4.2 Identification strategy

To identify the effects of upcoming local elections on politically-relevant news consumption, I leverage the irregular timing of survey waves with respect to state-specific electoral calendars (see also Eifert, Miguel and Posner 2010). The timing of ENCUP surveys varies across both the number of years between survey rounds (and thus does not track federal elections) and the month within the year when the survey was conducted, while Mexican states have historically followed different electoral cycles that vary in both the month and year of their elections.[21] Of Mexico's 31 states, 29

[17] 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 provide a respondent's municipality and asked different media consumption questions.

[18] See Appendix section A.1.2 for additional sampling information.

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

[20] In 2001, 2003, 2005, and 2012 respectively, respondents were asked 3, 2, 2, and 2 topical questions (see Appendix section A.1.2). Questions regarding basic knowledge of political institutions were excluded because they are unlikely to be covered directly in the news.

[21] See Appendix Table A1 for a full list of municipal elections by month. In Chiapas, Coahuila, Guerrero, Michoacán, Quintana Roo, Veracruz, and Yucatán, the typical 3-year cycle was adjusted over the sample period by switching to a 2 or 4-year term for a single electoral cycle. Moreover, following a constitutional amendment in 2007,

register a local election in at least one of the survey years, while 12 states hold an election within the 6 months after the survey was conducted. Provided that the ENCUP surveys and their locations were not strategically timed to coincide (or not) with particular elections, whether a survey was administered just before a state's local elections is plausibly exogenous.

Although there is some variation across states, official campaigns last around 3 months with internal candidate selection concluded a month or two earlier. Consistent with this, political advertising slots have been allocated 4 months priors to elections after 2006 (Larreguy, Marshall and Snyder 2018*a*) and Table 8 below shows that Mexican Google searches for "elecciones" since 2004 rise significantly in the 4 months preceding state elections. Accordingly, I code an indicator for respondents facing an upcoming municipal election, and typically a simultaneous state legislative election, within 4 months of the survey. Appendix Table A10 shows that the results are robust to defining upcoming elections by any number of months between 1 and 12, while I also report similar results using the number of months until a local election.

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

$$Y_{i,s,t} = \beta \, Upcoming\ local\ election_{s,t} + \mu_t + \varepsilon_{i,s,t}, \tag{7}$$

where $Y_{i,s,t}$ is a measure of political news consumption for respondent i in state s at survey year t. Survey fixed effects, μ_t, control for differences in the difficulty of 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. Bootstrapped standard errors are clustered by state.

Importantly, there is no evidence to suggest that the ENCUP surveys were strategically timed with respect to local elections or that local election campaigns alter the types of people that responded to the surveys. First, Appendix Table A2 shows that upcoming local elections are well-balanced over observable individual and municipal level characteristics: 3 of 18 tests reports a significant difference at the 10% level. I show below that the results are robust to controlling for these imbalances. Second, conditional on a municipality's inclusion in at least one survey wave, Marshall (2019) has demonstrated that neither upcoming local elections nor violence predict a municipality's inclusion in any given survey round.

Although upcoming local elections are plausibly exogenous to the timing of the survey, I also

states were subsequently mandated 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. To reduce constant electoral competition, some states holding off-cycle elections also homogenized elections after the reform.

implement a generalized difference-in-differences design—by adding state fixed effects, λ_s, to equation (7)—to demonstrate robustness. This design instead relies on the parallel trends assumption that states facing upcoming elections at the time of the survey follow similar trends in political news consumption to states without upcoming elections. This assumption is plausible in a context where states have had relatively fixed electoral cycles for decades. However, this design changes the identifying variation and reduces statistical power: because most states never experience an ENCUP survey within 4 months of an election, there is only within-state variation for a few states.

4.3 Upcoming elections increase news consumption and political knowledge

The results in panel A-C of Table 1 demonstrate that local elections increase the likelihood that a voter reports consuming any political information at all as well as the level of information consumed. Column (1) of panel A shows that an upcoming local election increases the probability that a voter listens to or watches news at all by 8 percentage points. Only consuming news before elections is likely to represent a conscious acquisition choice by voters that had previously avoided the relatively extensive news coverage also available outside election campaigns. Column (1) in panel B further reports a 12 percentage point increase in weekly news and political programming consumption. The 0.3 standard deviation increase on the five-point scale in column (1) of panel C is also statistically significant. These findings suggest that voters with both lower and higher baseline levels of engagement consume more political information prior to statewide elections.

Panel D shows that political knowledge mirrors the cycle of increased information consumption prior to elections. Contrary to potential concerns about social desirability bias, column (1) demonstrates that voters facing an upcoming local election are more than a quarter of a standard deviation—or 9 percentage points—more likely to correctly answer a question testing their political knowledge.[22] This increase suggests that voters actively engage with news before elections by internalizing political information, a likely prerequisite for it to influence voting behavior.

Columns (2)-(8) demonstrate the robustness of such political information cycles at the extensive and intensive margins of political news consumption. First, to address the imbalances, column (2) shows that the findings are qualitatively similar when fixed effects for all five educational categories, whether the municipal won the last election, and a PAN governor are included. This reduces the sample because education was not measured in 2005. Second, column (3) reports the generalized difference-in-differences results including state fixed effects. Although state fixed effects effectively drop observations from states that did not experience an upcoming election at the time

[22]Interacting upcoming local elections with baseline covariates for both consumption and knowledge outcomes suggests that the same voters consuming more news also become more knowledgeable about politics.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: Watch and listen to news and political programs ever								
Upcoming local election	0.077***	0.077***	0.030*					
	(0.016)	(0.019)	(0.017)					
Upcoming local election				0.096***	0.077***			
(two-month)				(0.017)	(0.019)			
Months until next election						-0.002**	-0.001	-0.001
						(0.001)	(0.001)	(0.001)
Observations	11,983	7,683	11,983	11,983	7,683	11,983	7,683	11,983
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	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
Independent variable mean	0.07	0.03	0.07	0.02	0.03	17.66	18.37	17.66
Panel B: Outcome: Watch and listen to news and political programs weekly								
Upcoming local election	0.125***	0.091***	0.056					
	(0.022)	(0.028)	(0.036)					
Upcoming local election				0.124***	0.091***			
(two-month)				(0.026)	(0.028)			
Months until next election						-0.004***	-0.005***	-0.003**
						(0.001)	(0.002)	(0.002)
Observations	11,983	7,683	11,983	11,983	7,683	11,983	7,683	11,983
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.62
Outcome standard deviation	0.48	0.49	0.48	0.48	0.49	0.48	0.49	0.48
Independent variable mean	0.07	0.03	0.07	0.02	0.03	17.66	18.37	17.66
Panel C: Outcome: Watch and listen to news and political programs scale								
Upcoming local election	0.433***	0.321***	0.246**					
	(0.078)	(0.094)	(0.106)					
Upcoming local election				0.438***	0.321***			
(two-month)				(0.087)	(0.094)			
Months until next election						-0.015***	-0.015***	-0.011**
						(0.004)	(0.006)	(0.005)
Observations	11,983	7,683	11,983	11,983	7,683	11,983	7,683	11,983
Outcome range	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}	{0,1,2,3,4}
Outcome mean	2.55	2.56	2.55	2.55	2.56	2.55	2.56	2.55
Outcome standard deviation	1.48	1.50	1.48	1.48	1.50	1.48	1.50	1.48
Independent variable mean	0.07	0.03	0.07	0.02	0.03	17.66	18.37	17.66
Panel D: Outcome: Topical political knowledge								
Upcoming local election	0.510**	0.241	0.323**					
	(0.251)	(0.176)	(0.161)					
Upcoming local election				0.329*	0.241			
(two-month)				(0.190)	(0.175)			
Months until next election						-0.005	-0.004*	-0.002
						(0.003)	(0.002)	(0.003)
Observations	15,976	11,533	15,976	15,976	11,533	15,976	11,533	15,976
Outcome range	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]
Outcome mean	0.00	0.01	0.00	0.00	0.01	0.00	0.01	0.00
Outcome standard deviation	1.00	0.92	1.00	1.00	0.92	1.00	0.92	1.00
Independent variable mean	0.06	0.03	0.06	0.02	0.03	18.79	19.51	18.79
Unique states	31	31	31	31	31	31	31	31
Control variables		✓			✓		✓	
State fixed effects			✓					✓

Notes: All specifications include survey-year fixed effects, and are estimated using OLS. The outcomes in panels A-C were not collected in the 2001 survey. The control variables, which are included to address imbalances, are indicators for education levels (which were not collected in 2005), whether the incumbent won the prior election, and a PAN governor. Bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

of any survey (more than three quarters of the sample), the results are similar and generally remain statistically significant in spite of the reduced precision. Third, the results do not depend upon the particular definition of the upcoming election indicator. Columns (4)-(5) instead define an upcoming local election as a survey conducted within two months of election day. While only 2% of observations (from 3 states) experience such an election,[23] the larger point estimates provide further confidence that voters indeed consume most political information before elections. Columns (6)-(8) similarly confirm that voters exhibit greater news consumption and political knowledge as the number of months until the next local election decreases. Fourth, Appendix Table A11 shows that the results do not depend on the inclusion of any particular survey wave. This suggests that the findings do not reflect events, e.g. festivals or local scandals, that could have coincided with survey enumeration periods during local election campaigns (Eifert, Miguel and Posner 2010).

5 Local homicides and municipal electoral selection

Having shown that voters consume significantly more news just before elections, I next test the model's prediction that signals of incumbent quality covered in the news at this time influence the voting behavior of Mexico's generally poorly-informed electorate. Specifically, I exploit plausibly exogenous spikes in the occurrence of homicides, a widely-covered news event, that coincide with the months before elections when voters consume most news. I use this idiosyncratic component of the per-period homicide rate—capturing $e_{j,t}$ in equation (1)—to instrument for the pre-election homicide rate $\overline{H}_1(N)$, and thereby leverage only the exogenous component driving voters' re-election rule in Proposition 1. I contrast the electoral effect of such pre-election homicide rates with longer-term homicide rates—a more precise signal of incumbent party competence only observed by highly-informed voters consuming politically-relevant news throughout the electoral cycle.

5.1 Data

I use two main data sources to examine the electoral effects of homicides occurring just before Mexican elections. First, electoral returns for municipal, state, and federal elections covering Mexico's c.67,000 electoral precincts were assembled from the Federal Electoral Institute (IFE) and state electoral institutes. I focus primarily on municipal elections between 1999 and 2013, for which data is widely available across all states; state and federal electoral returns are used to illuminate responsibility attribution across layers of government. Since municipal elections

[23] Since only 3 states have within-state variation, state fixed effects are not added.

generally occur every three years, the full dataset contains around four elections per municipality.

Second, I combine the electoral data with the INEGI's monthly municipal homicide data (described in section 3.2). While the national homicide rate has changed relatively smoothly over time, this masks considerable heterogeneity within and across municipalities. Within larger municipalities, homicide counts can oscillate dramatically, e.g. from 394 in September to 477 in October before falling to 242 in November and again rising to 309 in December in Ciudad Juárez, Chihuahua, at the height of the War on Drugs in 2010. Conversely, the median municipality experienced only one homicide a year between 1999 and 2013. Homicide rates per 100,000 people are normalized by the 2015 municipal population throughout.

I focus on the two incumbent electoral performance outcomes identified in Proposition 1: an indicator for whether the municipal incumbent party won the election, and the precinct-level change in the incumbent party's vote share.[24] Reflecting their persisting state-level power (Langston 2003), 53% of incumbent mayors in the sample are from the PRI; 29% are PAN and 12% are PRD. As noted above, voters generally select candidates on the basis of their parties in Mexico's party-centric system.[25] The average incumbent party experiences a 5.7 percentage point decline in their vote share, but still wins 54% of races. Turnout rates are typically around 60%. Municipalities without their own police forces, including the delegations of the Federal District, are excluded from the analysis because mayors have limited capacity to affect homicide rates.[26]

5.2 Identification strategy

The homicide rate that voters observe reflects unobservable incumbent competence and effort as well as unobservable idiosyncratic shocks. Because voters cannot distinguish these sources of homicides, Proposition 1 shows that Bayesian voters who consume few performance signals will sanction incumbent parties for idiosyncratic shocks while attempting to select competence incumbents. Beyond predicting that homicide shocks will drive voting behavior, the model's separation of incumbent-specific and exogenous drivers of homicide counts suggests an IV strategy leveraging idiosyncratic pre-election homicide shocks to instrument for the pre-election homicide rate.

To plausibly isolate idiosyncratic shocks that are independent of incumbent party competence, incumbent effort, and broader homicide rate trends *and* occur when voters consume most

[24] Appendix Table A12 shows similar results using incumbent vote share levels.

[25] For the 32% of cases where the incumbent won as part of a coalition formed by several parties, I define the incumbent as the party with the largest vote share at the following election if the coalition changes. Table A13 shows that the results are robust to restricting attention to just incumbents representing the three large parties or a single party.

[26] Based on National Census of Municipal Governments (ENGM) surveys, I exclude municipalities without a police force or relying on state, federal, community, private, or other security forces. Appendix A.1.2 describes the imputation procedure. Appendix Table A16 reports no electoral sanctioning of homicides in municipalities without police forces.

politically-relevant news, I exploit short-term deviations from trend in the homicide count around local elections. This approach rests on the logic that, within a given municipality, homicides are unpredictable and volatile rare events that follow no trend across adjacent months, and are thus equally likely to occur before and after elections.[27] Specifically, I compare municipalities experiencing a short-term spike in the number of homicides just before a municipal election, relative to just after that election, to otherwise similar municipalities experiencing at least as many homicides just after the election as before. Following Ferraz and Finan (2008) Ferraz and Finan's approach of comparing the effects of federal audit reports that were released just before and just after Brazilian municipal elections, I ensure that the comparison group captures similar municipalities (in homicide levels as well as other characteristics) by focusing on municipalities experiencing at least one homicide around the election.[28]

Formally, municipality m is defined as experiencing a pre-election homicide shock if, conditional on at least one homicide occurring over the four months spanning the penultimate month before election t and the second month after election t, m experiences more homicides in the two months before election t than the two months after the election:

$$\textit{Homicide shock}_{m,t} := \begin{cases} 1 & \text{if } Homicides_{m,t,j-2} + Homicides_{m,t,j-1} > Homicides_{m,t,j} + Homicides_{m,t,j+1} \\ & \text{and } \sum_{j'=j-2}^{j+1} Homicides_{m,j',t} > 0, \\ 0 & \text{if } Homicides_{m,t,j-2} + Homicides_{m,t,j-1} > Homicides_{m,t,j} + Homicides_{m,t,j+1} \\ & \text{and } \sum_{j'=j-2}^{j+1} Homicides_{m,t,j'} > 0, \\ . & \text{if } \sum_{j'=j-2}^{j+1} Homicides_{m,t,j'} = 0, \end{cases} \tag{8}$$

where j indexes months around a municipal election. Since most elections are held on the first Sunday of the month, this 4-month window captures the final 2 months of the campaign and the post-election period before the winner enters office.[29] The 2-month period was chosen to cover a sufficiently short span that month-to-month changes in the homicide count are plausibly exogenous and trendless, while also capturing the period of greatest political engagement. A further advantage is that voter registration is completed before this period. Table 2 reports similar results using 1, 3,

[27] Across municipalities since 1999, the monthly homicide change was positive in 11.86% of cases and negative in 11.79% of cases. In election months, these shares are 11.90% and 11.87%. In the month preceding an election, the shares are 11.87% and 11.86%.

[28] Municipalities with no homicides around local elections less violent, developed, and politically competitive than municipalities experiencing only a single homicide over the 4-month window I focus on.

[29] For the 9% of elections held on the 16th or later of a given month, I use months $j-1$ and j for the pre-election homicide count and $j+1$ and $j+2$ for the post-election count. Appendix Table A13 shows that the results are robust to dropping elections not held during the first half of the month.

or 4-month periods either side of elections.

On average, a homicide shock equates to an increase of around 4 homicides per month, or 2 homicides per 100,000 people, in shocked municipalities relative to non-shocked municipalities. Shocks thus double the average municipality's monthly homicide rate. Appendix Figure A1b maps the sample of 1,335 mostly larger municipalities with their own police force that register at least one homicide in the two months before and after municipal elections. Of these, 906 experience at least one election with and one election without a homicide shock between 1999 and 2013. In around two thirds of municipalities, the monthly homicide count is below 5.

I first estimate the reduced form effects of idiosyncratic homicide shocks occurring just prior to an election in municipality m in election year t using regressions of the form:

$$Y_{m,t} = \beta Homicide\ shock_{m,t} + \eta_m + \mu_{s,t} + \varepsilon_{m,t}, \tag{9}$$

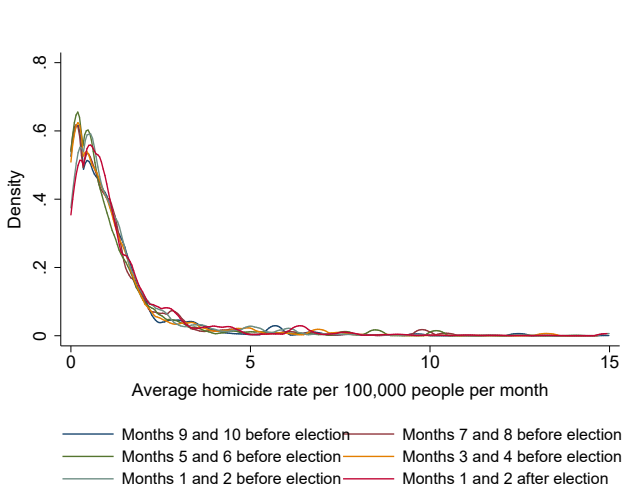
where $Y_{m,t}$ is either a municipal-level indicator for the incumbent party winning the election or the precinct-level change in the incumbent party's vote share relative to the previous election. Although they are not required for identification, municipality and state $\times$ year fixed effects—respectively, η_m and $\mu_{s,t}$—are included to increase precision and ensure that the estimates do not capture time-invariant municipality characteristics or (state-specific) common shocks. I show below that similar results obtain without these controls. All observations are weighted by the number of registered voters, and standard errors are clustered by municipality.

Building on the career concerns model, I next use idiosyncratic pre-election homicide shocks—the exogenous component of $\overline{H}_{m,t}(N)$—to instrument for the homicide rate per 100,000 people in the two months preceding the election (i.e. $\overline{H}_{m,t}(2)$) in the following structural equation:

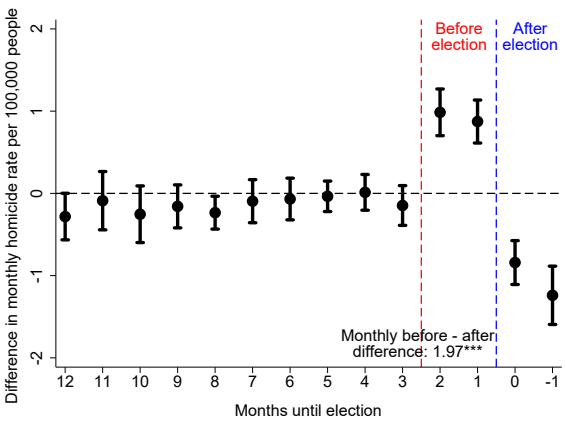
$$Y_{m,t} = \beta Pre\text{-}election\ homicide\ rate_{m,t} + \gamma Prior\ homicide\ rate_{m,t} + \eta_m + \mu_{s,t} + \varepsilon_{m,t}. \tag{10}$$

Assuming that homicide shocks only influence outcomes by increasing the two-month pre-election homicide rate, β identifies the effect of an additional homicide per 100,000 people before a municipal election induced by idiosyncratic homicide shocks.[30] Given the mechanical relationship between homicide shocks ad pre-election homicide rates, the exclusion restriction is almost an accounting identity. The *Prior homicide rate*$_{m,t}$ term—the average monthly homicide rate over the 10 months preceding the election window—proxies for the competence component of $H_{m,t,j}$ that voters struggle to infer when they principally consume news before elections, and thus strengthens the first stage, while the fixed effects capture the b and a_t^* components of $H_{m,t,j}$.

[30] If homicide shocks are orthogonal to other determinants of homicide rates, monotonicity holds by construction.



(a) Distribution of homicide rates by relation to election



(b) Difference in the homicide rate between municipalities with and without a pre-election homicide shock, by month until the election (95% confidence intervals)

Figure 2: Distribution of homicides by homicide shock and over time

Notes: The few cases of monthly homicide rates exceeding 15 per 100,000 are not shown in Figure 2a to facilitate graphical representation. Each bar in Figure 2b denotes the difference, based on estimating equation (9), in the homicide rate per 100,000 people (based on the 2015 population) between shocked and non-shocked municipalities in the ten months prior to the period defining the homicide shock around the election. The before-after comparison is the difference in the monthly homicide rate over the two month before and after the election.

5.3 Evidence supporting the identification strategy

The identifying assumption is that the *timing* of homicides around elections occurs exogenously. Although month-to-month changes in homicide rates appear idiosyncratic at first glance, differences in homicide shocks across elections in a given municipality may not have occurred by chance. This subsection validates the plausibility of the identifying assumption.

Consistent with sampling variability, Figure 2a shows that the distributions of two-month homicide rates prior to the period defining a shock, just before elections, and just after elections are similar. If homicide counts reflect strategic manipulation, we might instead expect to observe a spike in the density around 0 or modes in the tail of the distributions just before the election. However, comparing the distribution of homicides 2 months before the election with other 2-month periods, Kolmogorov-Smirnov tests fail to reject equality of distribution in each case. Appendix Figure A2 further demonstrates that these distributions do not differ by homicide shock status.

I next show that pre-election homicide shocks are not systematically correlated with a wide variety of observable pre-treatment covariates. First, Appendix Table A4 conducts balance tests

over 99 time-varying and time-invariant municipal covariates.[31] Only 9 differences are statistically significant at the 10% level. Most notably, homicide shocks are well-balanced across political variables including the incumbent's previous vote share, the competitiveness of the previous election, and the incumbent's party identity, in addition to indicators of municipal governance, socioeconomic development, demography, and access to media. I discuss key tests in detail below.

Second, shocked municipalities experience indistinguishable homicide rate levels and trends from non-shocked municipalities before elections. Figure 2b shows that the difference in monthly homicide rates between treated and untreated precincts is both relatively constant over the 10 months preceding the period used to define homicide shocks. The balance tests in Appendix Table A4 also confirm that the average homicide rate over the prior 1 or 3 years preceding the period defining the treatment do not significantly vary with homicide shocks. The balance tests further show that, for the 2011-2013 period when disaggregated crime reports coincide with my sample, homicide shocks are not generally correlated with pre-election spikes in other types of major crime. Contrary to concerns about strategic reporting, a mismatch between the month in which a homicide occurred and was reported is equally rare across shocked and non-shocked elections. Furthermore, I show below that a placebo homicide shock defined 6 months prior to the election does not influence electoral outcomes, and that the results are robust to controlling for pre-election monthly homicide rates.

Third, I also find no evidence to support the more specific concern that DTOs alter homicide rates around elections to signal their preferred electoral outcome. While Alesina, Piccolo and Pinotti (2019) and Ley (2018) find evidence that political attacks follow electoral cycles, Appendix Table A4 shows that pre-election homicide shocks are not significantly correlated with the drug-related homicide rates in the prior year, for the 2006(Dec.)-2011 period when monthly data was made publicly available by the Mexican National Security System. Homicide shocks are also balanced across municipalities registering more than one drug-related homicide in any pre-election year over this period, where DTOs are present, and in municipalities where a politician has been killed. At the precinct-level, Appendix Table A5 shows that shocks are no more likely to occur in the municipalities containing the 5% of precincts designated by the IFE as high-risk (typically high DTO activity locations). Moreover, Table 2 shows that the results are robust to removing municipalities with high levels of drug-related homicides and states with high DTO presence.

Nevertheless, the *types* of homicide could change without affecting overall levels. Gangland killings are typically concentrated among young and uneducated men (Dell 2015), and are of-

[31] Municipality fixed effects are excluded for the time-invariant 2010 Census variables and 2011-2013 variables defining shocks over other types of crime. Appendix Table A5 shows analogous tests for precinct-level outcomes.

ten committed using firearms or more gruesome methods—particularly if they intend to send a message. Using the International Classification of Diseases codes in INEGI's coroner reports, Appendix Table A4 also examines the causes of death and victim characteristics associated with homicides occurring in the two months before an election. Notably, homicide shocks are balanced with respect to the share of pre-election homicides caused by a firearm, hanging, drowning, explosives, or cutting objects. Furthermore, pre-election homicides did not disproportionately afflict young, male, unmarried, or uneducated individuals that are most likely to be involved with organized crime in shocked municipalities.

Fourth, I find no evidence to suggest that homicide rates change before elections because high-capacity governments can crack down on crime prior to elections to win votes. Although no expert on municipal politics that I interviewed believed that municipal governments crack down on local homicides in the short-term, I examine this concern more systematically. First, proxies for state capacity—including municipality size and budget, police per voter, alignment with governors or the president, and neighbor homicide shocks—are uncorrelated with homicide shocks. This suggests no differential ability to engage in such crackdowns. Second, using Osorio's (2015) newspaper-based measures of DTOs crackdowns, Appendix Table A4 further demonstrates that homicide shocks are not significantly correlated with violent enforcement, drug-related arrests, asset seizures, drug seizures, and gun seizures in the two months before the election.

Finally, defining homicide shocks using post-election homicides could introduce bias if election outcomes influence post-election homicides rates. However, because new mayors enter office more than 2 months after elections, municipalities are not governed by different incumbents in the post-election period. Moreover, Appendix Table A6 shows that election outcomes themselves do not predict homicides rates in the 2 months after an election, while Dell (2015) finds no effect of party transitions on homicide patterns during "lame duck" periods. Post-election homicides are also uncorrelated with interactions between election outcomes and such pre-determined covariates. Nevertheless, Appendix Table A15 reports similar results examining pre-election deviations from trend, based only on homicides occurring before the election.

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

The reduced form estimates in Table 2 show that idiosyncratic pre-election homicide shocks substantially reduce the municipal incumbent party's electoral prospects. Column (1) of panel A indicates that such homicide shocks reduce the incumbent party's probability of being re-elected by 10 percentage points. This represents a 20% reduction in the incumbent party's re-election probability, relative to municipalities not experiencing a pre-election homicide shock. Column (1) of

panel B reports that this decline reflects a 1.6 percentage point reduction in the incumbent party's vote share. Appendix Table A9 suggests that this relatively small, but consequential, decline in incumbent party vote share largely reflects greater turnout for non-incumbent parties.

The IV results in Table 3 further estimate the local average treatment effect of an additional homicide per 100,000 people before elections. Column (1) of panels A and B respectively indicate that such an increase before a municipal election reduces the incumbent party's probability of winning by 8 percentage points and the party's vote share by 1.5 percentage points. The strong first stage at the foot of each panel indicates that, on average, a pre-election homicide shock entails around 1 more homicide per 100,000 people a month, or a 0.6 standard deviation increase in the homicide rate. These large effects support the model's prediction that homicide spikes, which coincide with poorly-informed voters consuming news before elections, can substantially influence voting behavior, even when driven by the idiosyncratic component of the homicide rate.

These findings survive a wide variety of robustness checks. First, the results are not sensitive to possible violations of the identifying assumption. Column (2) shows that the results do not depend upon including municipality and year fixed effects. However, the greatest identification threat reflects time-varying unobservables. To address possible concerns, column (3) of Table 2 first demonstrates robustness to including 27 time-varying municipal-level covariates.[32] Columns (4) and (5) further include PAN-, PRD-, and PRI-specific and municipality-specific linear trends to show that the results do not reflect differential trends across different types of incumbent parties or municipalities. Although the inclusion of many municipality-specific time trends reduce estimation precision, the point estimate is similar in magnitude. Column (6) similarly includes election-month fixed effects to ensure that seasonality in homicide rates does not drive the results. Finally, I conduct a placebo test where a homicide shock is defined six months earlier, according to equation (9). The results in column (7) show that such pre-campaign shocks, which could be indicative of pre-trends, do not affect incumbent electoral performance.

Second, the results do not reflect municipalities experiencing particular levels or types of homicides. By including fixed effects for the total number of homicides over the four-month window used to define homicide shocks (in 10-homicide bins), column (8) provides further evidence that the results are not driven by differences in homicide levels.[33] Moreover, column (9) excludes the 13 elections experiencing more than 25 homicides per month before or after the election to demonstrate that the results do not simply reflect municipalities like Ciudad Juárez that experience many homicides. Although homicide shocks are uncorrelated with indicators of DTO presence, I further

[32] I exclude covariates characterizing pre-election homicides and those with greater than 95% missingness.

[33] Bins of size 1 and 5 similarly yield significant effects, at the cost of fully-explaining municipality-elections with a unique large number of homicides.

Table 2: Reduced form effects of pre-election homicide shocks on municipal incumbent electoral outcomes

	Baseline spec.	No fixed effects	Additional control variables	Incumbent party trends	Municipality -specific trends	Election month effects	Placebo 6 months earlier	Violence bin effects	Fewer than 25 homicides per month	Few drug homicides	Non-DTO states	One-month shock	Three-month shock	Four-month shock
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Outcome: Incumbent party win														
Homicide shock	-0.099***	-0.081**	-0.090***	-0.090***	-0.076	-0.099***		-0.075**	-0.085***	-0.099***	-0.081**			
	(0.031)	(0.036)	(0.028)	(0.031)	(0.051)	(0.031)		(0.029)	(0.032)	(0.031)	(0.040)			
Placebo homicide shock							-0.013							
							(0.039)							
Homicide shock (one month)												-0.085**		
												(0.042)		
Homicide shock (three month)													-0.090***	
													(0.032)	
Homicide shock (four month)														-0.066**
														(0.039)
Observations	2,838	2,838	2,671	2,533	2,838	2,838	2,402	2,838	2,825	2,838	1,881	1,831	3,488	3,975
Unique municipalities	905	905	863	822	905	905	819	905	904	905	615	619	1,062	1,170
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.54	0.55	0.54	0.56	0.54	0.54
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Homicide shock mean	0.41	0.41	0.40	0.41	0.42	0.41	0.40	0.41	0.41	0.41	0.40	0.44	0.44	0.42
Panel B: Outcome: Change in incumbent party vote share														
Homicide shock	-0.016**	-0.016**	-0.012*	-0.013*	-0.013	-0.016**		-0.018**	-0.017**	-0.016**	-0.023***			
	(0.007)	(0.007)	(0.006)	(0.007)	(0.010)	(0.007)		(0.007)	(0.007)	(0.007)	(0.008)			
Placebo homicide shock							-0.011							
							(0.011)							
Homicide shock (one month)												-0.025***		
												(0.009)		
Homicide shock (three month)													-0.015**	
													(0.007)	
Homicide shock (four month)														-0.020***
														(0.008)
Observations	166,355	166,355	162,061	156,907	166,355	166,355	144,993	166,355	156,794	166,355	101,365	141,453	180,031	188,049
Unique municipalities	905	905	895	822	905	905	819	905	904	905	615	619	1,062	1,170
Outcome range	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.87]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]
Outcome mean	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.06
Outcome std. dev.	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.15	0.14	0.14	0.14	0.15	0.15
Homicide shock mean	0.41	0.41	0.40	0.41	0.43	0.41	0.40	0.41	0.41	0.41	0.40	0.44	0.43	0.42

Notes: All specifications are estimated using OLS, and all observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. Column (1) is the baseline specification show in equation (9), which includes municipality and state × year fixed effects. Column (2) excludes municipality and state × year fixed effects. Columns (3) includes the controls 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 incumbent party-specific incumbent year trends. Column (5) includes municipality-specific year trends. Column (6) includes election month fixed effects. Column (7) uses a placebo homicide shock defined six months before the election. Column (8) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (9) excludes municipalities averaging more than 25 homicides per month over the two months either before or after the election. Column (10) excludes municipalities that average more than one drug-related homicide a month over the 12 months before an election between 2006 and 2011. Column (11) excludes states with high-level of DTO activity (see footnote 34). Columns (12)-(14) respectively define homicide shocks over one-, three-, and four-month windows. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 3: Instrumental variable estimates of the effect of pre-election homicides on municipal incumbent electoral outcomes

	Baseline spec.	No fixed effects	Additional control variables	Incumbent party trends	Municipality -specific trends	Election month effects	Violence bin effects	Fewer than 25 homicides per month	Few drug homicides	Non-DTO states	One-month shock	Three-month shock	Four-month shock
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: Outcome: Incumbent party win													
Pre-election homicide rate	-0.084***	-0.073**	-0.087***	-0.078**	-0.077	-0.084***	-0.079**	-0.083***	-0.084***	-0.076	-0.113*	-0.132**	-0.111*
	(0.031)	(0.034)	(0.029)	(0.031)	(0.060)	(0.031)	(0.034)	(0.031)	(0.031)	(0.055)	(0.061)	(0.053)	(0.059)
Observations	2,838	2,838	2,671	2,533	2,838	2,838	2,838	2,831	2,838	1,881	1,831	3,488	3,975
Unique municipalities	905	905	863	822	905	905	905	903	905	615	619	1,062	1,170
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.54	0.56	0.54	0.54
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Pre-election homicide rate mean	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.0	1.7	1.5	1.4
Pre-election homicide rate std. dev.	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.0	3.1	1.3	3.2	3.0	3.0
Homicide shock mean	0.42	0.42	0.42	0.43	0.42	0.42	0.42	0.42	0.42	0.42	0.44	0.44	0.43
First stage coefficient	1.059***	1.090***	1.030***	1.057***	0.861***	1.059***	1.026***	1.069***	1.059***	0.770***	0.733***	0.573***	0.494***
	(0.090)	(0.112)	(0.083)	(0.095)	(0.116)	(0.090)	(0.069)	(0.090)	(0.090)	(0.062)	(0.084)	(0.076)	(0.076)
First stage *F* statistic	139.0	94.1	154.6	122.5	54.7	139.0	219.7	138.2	139.0	153.6	76.1	56.9	41.8
Panel B: Outcome: Change in incumbent party vote share													
Pre-election homicide rate	-0.015**	-0.015**	-0.011*	-0.012*	-0.015	-0.015**	-0.018**	-0.015**	-0.015**	-0.030***	-0.035***	-0.027**	-0.041**
	(0.006)	(0.007)	(0.006)	(0.007)	(0.011)	(0.006)	(0.007)	(0.006)	(0.006)	(0.011)	(0.013)	(0.011)	(0.014)
Observations	166,355	166,355	162,061	156,907	166,355	166,355	166,355	166,292	166,355	101,365	141,453	180,031	188,049
Unique municipalities	905	905	895	822	905	905	905	905	905	615	619	1,062	1,170
Outcome range	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.87]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]
Outcome mean	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.05	-0.06
Outcome std. dev.	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.15	0.15
Pre-election homicide rate mean	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.0	1.7	1.5	1.4
Pre-election homicide rate std. dev.	3.1	3.1	3.1	3.2	3.1	3.1	3.1	3.0	3.1	1.3	3.2	3.0	3.0
Homicide shock mean	0.42	0.42	0.42	0.43	0.43	0.42	0.42	0.42	0.42	0.41	0.44	0.44	0.43
First stage coefficient	1.059***	1.089***	1.030***	1.057***	0.858***	1.059***	1.026***	1.070***	1.059***	0.768***	0.731***	0.571***	0.498***
	(0.088)	(0.113)	(0.081)	(0.093)	(0.093)	(0.088)	(0.068)	(0.089)	(0.088)	(0.061)	(0.081)	(0.075)	(0.075)
First stage *F* statistic	144.4	92.5	163.2	128.6	84.8	144.4	230.8	143.8	144.4	160.3	81.3	58.4	43.6

Notes: All specifications instrument for the pre-election homicide rate using 2SLS, and all observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. Column (1) is the baseline specification show in equation (10), which includes municipality and state × year fixed effects and the prior average monthly homicide rate per 100,000 people. Column (2) excludes municipality and state × year fixed effects. Columns (3) includes the controls 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 incumbent party-specific incumbent year trends. Column (5) includes municipality-specific year trends. Column (6) includes election month fixed effects. Column (7) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (8) excludes municipalities averaging more than 25 homicides per month over the two months either before or after the election. Column (9) excludes municipalities that average more than one drug-related homicide a month over the 12 months before an election between 2006 and 2011. Column (10) excludes states with high-level of DTO activity (see footnote 34). Columns (11)-(13) respectively define homicide shocks over one-, three-, and four-month windows. The placebo implemented in Table 2 is excluded. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

address potential concerns that homicides reflect strategic behavior by excluding the elections most vulnerable to political violence by DTOs. Specifically, column (10) excludes municipalities that average more than one drug-related murder a month over any pre-election year between 2006 and 2011 (when such data were registered), while column (11) excludes 9 states with high DTO-related drug crime.[34] In neither case are the estimates substantially affected.

Third, the results are not driven by the instrument's operationalization. Columns (12)-(14) shows that homicide shocks defined by 1, 3, or 4 month comparisons also reduce the incumbent's probability of winning and vote share. Furthermore, Appendix Table A15 reports broadly similar results using a panel fixed effects design to examine deviations between the homicide rate in the two months before the election and the homicide rate over the entire preceding electoral cycle.

5.5 No discernible effect of longer-run homicide rates

The political information cycles argument implies that many voters respond to homicides that occur just before elections. In contrast, longer-run homicide metrics that are more informative about incumbent competence may not affect voters' responsibility attribution if—as already shown—many voters are insufficiently engaged at other times in the political cycle to learn about more systematic trends in homicide levels.

To examine voter responsiveness to longer-run homicide rates, I estimate the effects of homicides over the prior year and prior (three-year) electoral cycle on the incumbent party's electoral prospects using a panel fixed effects design. The following regression 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 did not:

$$Y_{m,t} = \beta \textit{Average monthly homicide rate}_{m,t} + \eta_m + \mu_{s,t} + \varepsilon_{m,t}. \tag{11}$$

As a robustness check, municipality-specific time trends are included to account for differential trends in incumbent support across municipalities experiencing different homicide rates.

The results in Table 4 indicate that longer-run homicide rates have had limited impact on electoral outcomes. Columns (1), (2), (5), and (6) of panel A show that homicides over the year before the election are not significantly correlated with either the incumbent's probability of being re-elected or their vote share, regardless of the inclusion of municipality-specific time trends. Moreover, the point estimates are an order of magnitude smaller than for homicide shocks. Columns (3), (4), (7), and (8) include a quadratic term, but also find little suggestion that any effect is non-

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

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

	Incumbent party win				Change in incumbent party vote share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effect of average monthly homicide rates 12 months before election								
Average monthly homicide rate	0.0081	0.0168	0.0218	0.0186	-0.0034	0.0013	0.0011	-0.0004
(12 months before election)	(0.0119)	(0.0143)	(0.0163)	(0.0224)	(0.0022)	(0.0040)	(0.0033)	(0.0063)
Average monthly homicide rate			-0.0001	-0.0000			0.0000	0.0000
(12 months before election) squared			(0.0001)	(0.0001)			(0.0000)	(0.0000)
Monthly homicide rate mean	1.39	1.39	1.39	1.39	1.39	1.39	1.39	1.39
Monthly homicide rate std. dev.	2.30	2.30	2.30	2.30	2.31	2.31	2.31	2.31
Panel B: Effect of average monthly homicide rates since last election								
Average monthly homicide rate	0.0216	0.0280	0.0548*	0.0362	-0.0038	0.0060	0.0067	0.0059
(3 years before election)	(0.0157)	(0.0250)	(0.0283)	(0.0432)	(0.0024)	(0.0051)	(0.0043)	(0.0087)
Average monthly homicide rate			-0.0006	-0.0002			-0.0001	0.0000
(3 years before election) squared			(0.0004)	(0.0006)			(0.0001)	(0.0001)
Monthly homicide rate mean	1.24	1.24	1.24	1.24	1.24	1.24	1.24	1.24
Monthly homicide rate std. dev.	1.67	1.67	1.67	1.67	1.68	1.68	1.68	1.68
Observations	2,838	2,838	2,838	2,838	166,355	166,355	166,355	166,355
Unique municipalities	905	905	905	905	905	905	905	905
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]
Outcome mean	0.55	0.55	0.55	0.55	-0.05	-0.05	-0.05	-0.05
Outcome standard deviation	0.50	0.50	0.50	0.50	0.14	0.14	0.14	0.14
Municipality-specific time trends		✓		✓		✓		✓

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

linear. Panel B similarly shows that voters do not sanction homicide rates averaged across the full electoral cycle. The evidence thus suggests that, consistent with few voters consistently consuming news throughout the electoral cycle, longer-run homicide rates play limited role in electoral selection.

5.6 Voters distinguish responsibility across levels of government

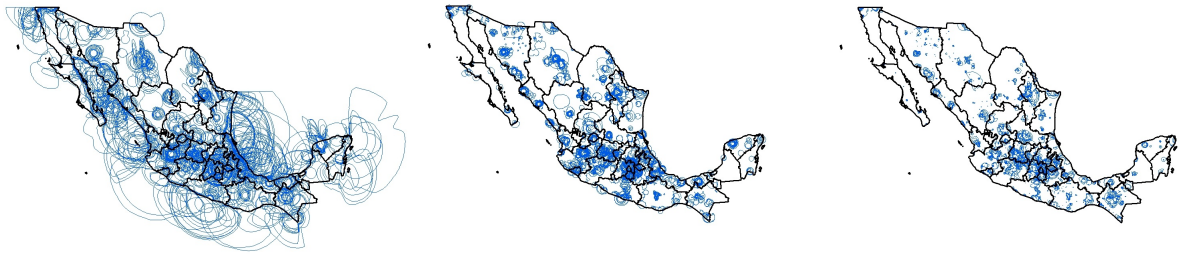
Although mayors play an important role in local public security, state and federal governments are also important players. Voter removal of poorly-performing mayoral incumbent parties could then reflect broader punishment of the party controlling higher office. However, if voters believe that mayors are primarily responsible for local crime or that their actions are weakly correlated with co-partisan actions at higher levels, they may update less about national parties following local homicide shocks. I next disentangle this assignment of responsibility.

If voters believe that the mayor is responsible for local crime because they control the local police, pre-election homicide shocks should only be punished in municipalities with their own police force. To test this, I add municipalities (and delegations in the Federal District) without their own police force to the sample. Consistent with voters recognizing that elevated homicides rates may be beyond the control of municipal mayors without local police forces, Appendix Table A16 shows that homicide shocks only significantly harm the electoral prospects of incumbent parties that command a local police force. This result suggests that voters are are aware of local police forces and do not indiscriminately punishing adverse events.

Nevertheless, if the parties of municipal mayors, state deputies, state Governors, or the President are correlated, the substantial electoral penalties documented at the municipal level could instead reflect punishment of higher political actors. The results in Appendix Table A17 suggest that, if anything, pre-election homicides increase the municipal vote share of the state deputy's, governor's, and president's parties', and thus provide no support for punishment at higher levels of government spilling over to municipal elections. Alternatively, voters could restrict punishment for state and federal politicians to their corresponding races. However, pre-election homicides reduce neither the vote share of state nor federal deputies in concurrent state and federal elections.

6 The amplifying effect of local media

The individual- and aggregate-level findings suggest that the electoral punishment of pre-election local homicides principally reflects increased voter news consumption before elections. However, this electoral response could also reflect voters becoming more likely to hear about crime via word



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

Figure 3: Media station commercial quality signal coverage areas

of mouth before elections or through on the ground election campaigns, rather than greater news consumption through the media. To examine the extent to which voting behavior depends on access to local media, this section combines pre-election homicide shocks with within-municipality variation in media signal coverage across electoral precincts to estimate the effect of an additional local radio or television station likely to report on homicides in the municipality. Given media outlet control of whether and how news is reported (e.g. Besley and Prat 2006; Gentzkow and Shapiro 2006; Mullainathan and Shleifer 2005) and that market segmentation may necessitate multiple outlets to report on an event to reach most voters (Barabas and Jerit 2009; Prat and Strömberg 2005), the likelihood that voters consume news covering homicides in their municipality is likely to increase with the number of local media outlets that voters have access to.

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—the level of coverage that U.S. media outlets base advertising sales on—of their antennae for the purpose of allocating political ad slots before the election (see Durante and Gutierrez 2015; Larreguy, Marshall and Snyder 2018*a*). The signal inside the commercial quality coverage area is very strong, and should cover virtually all households. However, signal quality quickly declines beyond the commercial quality coverage boundary.[35]

Figure 3 maps the commercial quality coverage of each antennae. While FM radio, and especially television, stations have limited and primarily urban coverage, AM radio stations can travel considerable distances—particularly when aided by stretches of sea water with high elec-

[35]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."

trical ground conductivity. Virtually all electoral precincts are covered by at least one media outlet, but the number of outlets—both emitting from inside and outside a precinct's municipality—providing commercial quality signals varies considerably. Following Larreguy, Marshall and Snyder (2018*b*), I combine rural locality- and urban block-level population data from the 2010 Census with electoral registration data to 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

To estimate the electoral effects of an additional media station covering local homicides, I leverage geographic variation in the number of local media outlets an electoral precinct has access to. Like Larreguy, Marshall and Snyder (2018*b*) and Spenkuch and Toniatti (forthcoming), I compare neighboring electoral precincts within the same municipality that 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[36]—by which they are covered. Figure 4 illustrates the identification strategy graphically for a television station covering neighboring precincts 1571 and 1583 within a municipality in Oaxaca. The key assumption identifying this intent to treat effect of increasing the probability of exposure to an additional local media station is that neighboring precincts differ only because precinct 1583 receives a commercial quality signal from a local television station that does not cover precinct 1571.[37]

More generally, for each electoral precinct, I identify the set of neighboring precincts from within the same municipality that have access to a different number number of local media stations. Each such grouping n is thus defined by a single "treated" precinct and a set of neighboring "control" precincts. Focusing on municipalities included in the homicide shock sample produced 2,623 neighboring groups, containing an average of 2.3 control units per election. The average precinct in this sample is covered by 6.3, 9.0, and 3.3 local AM, FM, and television stations respectively. The total number of local media stations covering a precinct ranges from 0 to 40.[38]

There are good reasons to believe that, among neighboring electoral precincts, local media coverage at the commercial quality boundary is exogenous. First, neighboring precincts often differ in coverage because of physical characteristics such as water, geographic contours, and large objects that aid or impede ground conductivity (in the case of AM radio) and "line of sight" radio

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

[37] 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.

[38] Given that data from the Secretariat of Communications and Transportation show that the number of media stations has barely changed since 2003, I continue to pool the years 1999 to 2013.

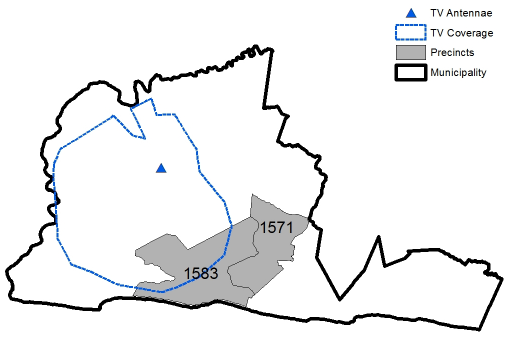


Figure 4: Identification strategy example

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

waves (in the case of FM radio and television) between an antenna and precincts at the coverage boundary (Larreguy, Marshall and Snyder 2018*b*; Olken 2009). Second, given that media stations lack the technology to differentiate media markets at fine-grained levels,[39] and voters who locate according to the availability of additional local media are unlikely to choose to live close to a coverage boundary (preferring a location guaranteeing high-quality coverage), it is unlikely that coverage reflects strategic sorting by either media stations or voters around coverage boundaries. Finally, I restrict attention to neighboring precincts with an area of less than 2km^2. This removes larger precincts where media outlets could more plausibly target their signals, and prevents stark comparisons between urban and suburban, or suburban and rural, precincts that could respond differently to homicide shocks. The final sample of precincts is shown in Figure A1c.

Combining this within-neighbor variation in local media (within municipalities) coverage with the homicide shocks leveraged above, I use the following specification to estimate the interaction between pre-election homicide spikes and local media coverage:

$$\begin{aligned} Y_{p,n,m,t} &= \beta_1 Homicide\ shock_{m,t} + \beta_2 Local\ media_{p,n,m} \\ &\quad + \beta_3 \Big(Homicide\ shock_{m,t} \times Local\ media_{p,n,m} \Big) + \zeta_n + \mu_{s,t} + \varepsilon_{p,n,m,t}, \end{aligned} \tag{12}$$

where ζ_n is a fixed effect for each set of neighboring precincts. To weight neighbor-election sets 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.

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

To assess the claim that differences in local media coverage between neighboring precincts reflect plausibly exogenous topological features and are not subject to strategic sorting, I use equation (12) to demonstrate balance across demographic, socioeconomic, homicide, and political municipal and precinct characteristics.[40] Appendix Table A7 shows that only 10 of these 93 variables are significantly correlated with the number of local media stations at the 10% level. In particular, there are no significant differences in key indicators of rural-urban geography (such as precinct area, electorate size, or distance to the municipality head), the number of non-local media stations, homicide rates, or prior electoral behavior. I show below that the results are robust to controlling interactively for the statistically significant imbalances.

6.3 Local media increase electoral punishment of homicide shocks

Column (1) of Table 5 first shows that, while slightly larger in magnitude, the average effect of pre-election homicide shocks is similar in this sample of more urban precincts to the full sample in Table 2.

Column (2) demonstrates that access to local media stations plays a critical role in driving the electoral sanctioning of homicide shocks. Indicating that local media are necessary for Mexican voters to sanction incumbent party on the basis of pre-election homicides, the null lower-order effect of a homicide shock shows that pre-election homicide spikes do not affect vote shares in precincts with limited access to local media. However, the statistically significant negative interaction coefficient implies that each additional local media station reduces the vote share of incumbents experiencing a pre-election homicide shock by 0.3 percentage points. For more than 20 local media stations (around half the sample), Figure 5 shows that the overall effect of a homicide shock is to substantially reduce the municipal incumbent party's vote share.[41] The smaller, but nevertheless statistically significant, positive coefficient on the lower-order local media term suggests that voters reward incumbents that do not experience a homicide shock before the election, but to a lesser degree than incumbents overseeing homicide shocks are punished. The natural logarithm of the number of local media stations used in column (3) indicates that each additional local media station alters the incumbent party's vote share by approximately 3%.

The findings are robust to various potential threats to identification. First, column (4) interactively controls for 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

[40] Table A8 shows that homicide shocks remain well-balanced across pre-treatment variables in this subsample.

[41] Since the identification strategy relies on within-neighbor variation, this represents a linear extrapolation of the average marginal effect.

Table 5: Effect of homicide shocks on precinct-level change in incumbent party vote share, moderated by access to local and non-local media

	Outcome: Change in incumbent party vote share						
	No local media interaction	Baseline spec.	Logarithmic transformation	Interactive non-local media control	Interactive imbalanced controls	Average control media within 50m	50% precinct coverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Homicide shock	-0.0254	0.0246	0.0444	0.0358	0.0091	-0.0168	0.0305
	(0.0213)	(0.0296)	(0.0463)	(0.0344)	(0.0270)	(0.0232)	(0.0279)
Local media		0.0020*		0.0022**	0.0022**	-0.0005	0.0008
		(0.0011)		(0.0010)	(0.0010)	(0.0011)	(0.0012)
Homicide shock × Local media		-0.0033**		-0.0037**	-0.0037**	-0.0017	-0.0031**
		(0.0016)		(0.0015)	(0.0015)	(0.0010)	(0.0015)
Local media (log)			0.0217*				
			(0.0115)				
Homicide shock × Local media (log)			-0.0286				
			(0.0175)				
Non-local media				0.0000			
				(0.0008)			
Homicide shock × Non-local media				-0.0004			
				(0.0011)			
Observations	29,704	29,704	29,704	29,704	29,704	11,578	33,010
Unique municipalities	118	118	118	118	118	115	123
Outcome range	[-0.63,0.50]	[-0.63,0.50]	[-0.63,0.50]	[-0.63,0.50]	[-0.63,0.50]	[-0.63,0.50]	[-0.63,0.50]
Outcome mean	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04	-0.04
Outcome standard deviation	0.13	0.13	0.13	0.13	0.13	0.13	0.13
Homicide shock mean	0.40	0.40	0.40	0.40	0.40	0.39	0.39
Media measure media mean		18.65	18.65	18.65	18.65	18.70	20.46
Media measure standard deviation		10.31	10.31	10.31	10.31	10.32	10.84

	100% precinct coverage	1km area restriction	5km area restriction	No area restriction	Longer-run homicides	
	(8)	(9)	(10)	(11)	(12)	(13)
Homicide shock	0.0210	0.0243	0.0192	0.0229		
	(0.0188)	(0.0306)	(0.0261)	(0.0150)		
Local media	0.0016*	0.0018*	0.0023**	0.0007	0.0005	0.0006
	(0.0009)	(0.0011)	(0.0010)	(0.0007)	(0.0005)	(0.0005)
Homicide shock × Local media	-0.0032***	-0.0032*	-0.0031**	-0.0021**		
	(0.0012)	(0.0017)	(0.0015)	(0.0010)		
Average monthly homicide rate					0.0700	
(12 months before election)					(0.0957)	
Average monthly homicide rate					-0.0039	
(12 months before election) × Local media					(0.0030)	
Average monthly homicide rate						0.0138
(3 years before election)						(0.0103)
Average monthly homicide rate						-0.0005
(3 years before election) × Local media						(0.0004)
Observations	51,370	25,252	35,295	101,298	29,704	29,704
Unique municipalities	198	91	140	375	118	118
Outcome range	[-0.64,0.61]	[-0.63,0.47]	[-0.63,0.53]	[-0.90,0.89]	[-0.63,0.50]	[-0.63,0.50]
Outcome mean	-0.05	-0.04	-0.04	-0.04	-0.04	-0.04
Outcome standard deviation	0.13	0.13	0.13	0.15	0.13	0.13
Homicide shock mean	0.41	0.38	0.41	0.44	1.59	15.31
Media measure media mean	18.45	19.68	17.88	13.21	18.65	18.65
Media measure standard deviation	11.26	9.99	10.47	10.76	10.31	10.31

Notes: All specifications include neighbor group and state × year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters divided by the number of comparison units within each neighbor group. The (standardized) interactive controls in column (5), which are omitted to save space are: registered voters, municipal police per capita, an indicator for the top quartile of the homicide distribution, average number of occupants per room, average years of schooling for men, share illiterate, share of households with basic amenities in their home, and share of households with a computer. 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. Instead of requiring that 20% of the electorate in a precinct is covered by a media outlet, columns (7) and (8) instead respectively define coverage by 50% or 100% coverage. Columns (9)-(11) vary the precinct maximum area sample restriction. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

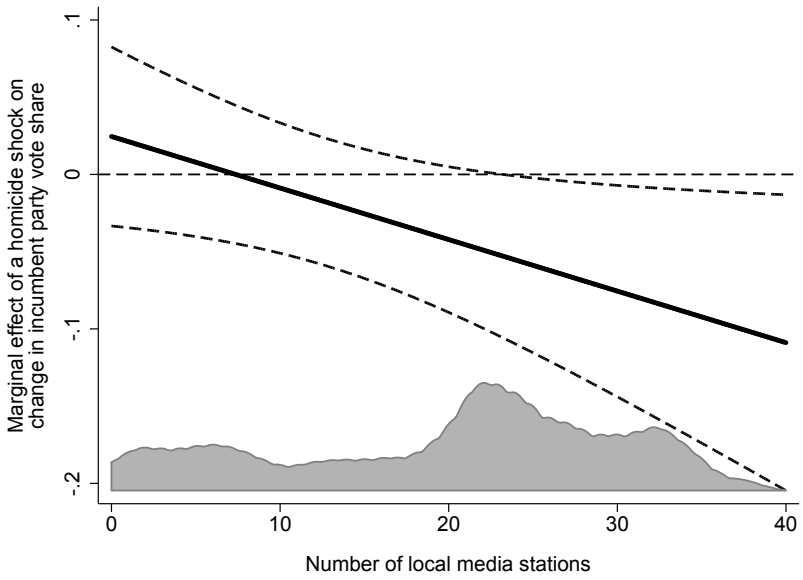


Figure 5: Effect of a homicide shock on change in incumbent party vote share, conditional on the number of local media stations (95% confidence interval)

Notes: Calculated using the estimates from column (2) of Table 5. The gray density plot above the x axis plots the distribution of the local media variable in the estimation sample.

greater access to non-local media. Second, column (5) similarly demonstrates that the results are robust to controlling interactively for (standardized versions of) the 8 variables with significant imbalances over local media. Third, to ensure that distance from radio antennae—and thus the location of precincts within municipalities more generally—is not driving the results, column (6) restricts the sample to "control" precincts that are no more than 50 meters nearer to, or further from, the average local media station of each neighboring group's "treated" precinct. The results of this Olken (2009)-type approach provide similar, but noisier, estimates in this smaller sample. Fourth, the results are not sensitive to particular design choices. Columns (7) and (8) first demonstrate that the estimates are not sensitive to the 20% cutoff chosen to define whether a local media station covers a precinct. Columns (9)-(11) further show that the results are robust to considering tighter restrictions (precincts with an area of 1km or less) or more generous restrictions (5km or less or no area restriction) on the maximum precinct area permitted.

Moreover, if the results indeed reflect local media covering homicide shocks that occur just before elections, then content that media outlets have weak incentives to report should not alter vote choices. In particular, news that is not recent is likely to be broadcasted less. In line with these expectations, columns (12) and (13) find no significant interaction between longer-run homicide rates and access to local media. Nevertheless, voters could still be influenced by content that also

references homicide shocks, beyond the news and political programming of local media outlets. Mexico's (nationally-oriented) election campaign ads are the most likely sources. However, Appendix Table A19 reports no evidence that the share of broadcast media ads allocated to the party of the municipal incumbent moderates the effects of homicide shocks. These tests suggest that electoral sanctioning occurs where media outlets are likely to report on local homicides at the time when voters consume most news, and thus emphasize the electoral importance of the intersection between the consumption and supply of politically-relevant news.

7 Mechanisms

The preceding analysis showed that voters consume most political information just before elections, and are responsive to homicides that occur at this time, but only when covered by enough local media stations likely to report such news. This section substantiates the mechanisms driving these news consumption cycles. First, and consistent with voter belief updating, I show that voter security concerns only increase following homicide spikes that occur before elections and that electoral responses to pre-election homicide shocks vary systematically with heterogeneity in proxies for voters' prior beliefs. Second, Google search and media reporting data indicate that political information cycles are more likely driven by increases in voter demand for news before elections than increases in media coverage of homicide around elections.

7.1 Voter belief updating tracks news consumption cycles

News consumption cycles inducing Bayesian updating represents one potential mechanism driving the responsiveness of voting behavior to pre-election homicides. The model proposed in section 2 predicts that news consumption before elections will result in voters principally updating their beliefs in response to events occurring during this period. As explained below, this prediction is not consistent with alternative interpretations of results, such as voters possessing short memories.

I first test this channel by using the ENCUP survey data to examine whether concern about public security increases when homicide shocks coincide with the pre-election period during which news consumption is greatest. Leveraging plausible exogenous variation in both upcoming local elections and *pre-survey* homicide shocks (defined analogously to pre-election homicide shocks, but with respect to surveys),[42] the following interactive specification estimates the effect of homi-

[42]Pre-survey homicide shocks are coded as occurring if, relative to the two months after the survey was conducted, more homicides occurred either in the two months preceding the survey month or the preceding month and survey month itself. Moreover, Table A3 shows that short-run homicide shocks are balanced across individual-level and

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					
			Average monthly homicide rate...			
	Homicide shock		...(last 12 months)		...(last 3 years)	
	(1)	(2)	(3)	(4)	(5)	(6)
Homicides measure (varies by column)	0.001	0.004	-0.003	-0.000	-0.001	0.004
	(0.011)	(0.010)	(0.007)	(0.008)	(0.011)	(0.013)
Upcoming local election (4 months)	0.060***		0.064		0.077	
	(0.016)		(0.881)		(0.084)	
Upcoming local election (4 months)	0.106***		0.029		0.016	
× Homicides measure	(0.014)		(2.910)		(0.116)	
Upcoming local election (2 months)		0.002		-0.005		0.038
		(0.019)		(0.869)		(0.091)
Upcoming local election (2 months)		0.132***		0.064		0.017
× Homicides measure		(0.016)		(2.909)		(0.126)
Observations	9,764	9,764	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	0.13	0.13	0.11	0.11	0.11	0.11
Local election mean	0.08	0.03	0.07	0.03	0.07	0.03
Homicide measure mean	0.46	0.46	0.68	0.68	0.70	0.70

Notes: All specifications include survey-year fixed effects, and are estimated using OLS. The outcome was not collected in the 2012 ENCUP survey. Homicide shocks are not defined for municipalities that do not experience homicides around the survey. Bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

cide shocks outside and during local election campaigns:

$$\begin{aligned} Security_{i,m,s,t} &= \beta_1 Upcoming\ local\ election_{s,t} + \beta_2 Homicide\ shock_{m,t} \\ &\quad + \beta_3 \Big(Upcoming\ local\ election_{s,t} \times Homicide\ shock_{m,t} \Big) + \mu_t + \varepsilon_{i,m,s,t}, \end{aligned} \tag{13}$$

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

The results in Table 6 demonstrate that homicide shocks increased concern about public security, but only when they occur just before local elections. Indeed, the negligible coefficients on the lower-order pre-survey homicide shock term in columns (1) and (2) show that an increase in the homicide count immediately before surveys that do not coincide with local election campaigns has

municipal covariates in the ENCUP sample.

no discernible impact on public security concerns. Only the most engaged voters consume news in this period, who are unlikely to substantially update from noisy signals. In contrast, the statistically significant interaction coefficients indicate that a pre-survey homicide shock that occurs within 4 or 2 months of a local election increases concerns about public security by more than 10 percentage points. This almost doubles the number of respondents citing public insecurity as the most important problem in their community. The lower-order upcoming local election coefficients suggest that beliefs change much less when few homicides occur before local elections. Combined with the local media effect estimates, these results imply that the timing of news consumption drives voter concerns that in turn translate into vote choice.

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

A second test of voter updating examines heterogeneity in response to homicide shocks with respect to voters' prior beliefs. Allowing prior expectations of competence, $\hat{\theta}$, to vary across voters, the model predicts that a homicide shock is more likely to reduce the incumbent party's probability of winning when $\hat{\theta}$ was initially high for the incumbent party or low for challenger parties. I test this prediction in the electoral data using the following reduced form specification:

$$\begin{aligned} Y_{m,t} &= \beta_1 \textit{Homicide shock}_{m,t} + \beta_2 \textit{Lower expectations}_{m,t} \\ &\quad + \beta_3 \Big(\textit{Homicide shock}_{m,t} \times \textit{Lower expectations}_{m,t} \Big) + \eta_m + \mu_{s,t} + \varepsilon_{m,t}, \end{aligned} \tag{14}$$

where $\textit{Lower expectations}_{m,t}$ is a proxy for relatively low expectations of incumbent competence based on pre-election homicide shocks before the previous election. Incumbent expectations are coded as relatively low when: (i) the current incumbent party was in power before the previous election and oversaw a homicide shock; (ii) the incumbent party in power before the previous election was from a different party and did not oversee a homicide shock; or (iii) there were no homicides around the previous election.

Consistent with voter belief updating, Table 7 shows that electoral sanctioning of pre-election homicide shocks is smaller where expectations were relatively low. The sum of the homicide shock coefficients at the foot of columns (1) and (2) indicate that the incumbent's probability re-election probability and vote share still decline when expectations are low, but that the effect is no longer statistically significant. Although the interaction term is only statistically significant at the $p < 0.1$ level in column (2), the negative effect is more than 3 times smaller in magnitude.

Table 7: Differential reduced form effects of pre-election homicides on municipal incumbent electoral outcomes, by voters' prior expectations

	Incumbent party win (1)	Change in Incumbent party vote share (2)
Homicide shock	-0.180***	-0.033***
	(0.066)	(0.011)
Lower expectations	-0.034	-0.021**
	(0.062)	(0.009)
Homicide shock × Lower expectations	0.120	0.025*
	(0.084)	(0.015)
Observations	2,838	166,355
Unique municipalities	905	905
Outcome range	{0,1}	[-0.96,0.89]
Outcome mean	0.55	-0.05
Outcome standard deviation	0.50	0.14
Homicide shock mean	0.41	0.41
Lower expectations mean	0.67	0.67
Homicide shock + Homicide shock × Lower expectations	-0.060	-0.008
	(0.042)	(0.008)

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

Beyond supporting the belief updating interpretation, these results are also inconsistent with several alternative explanations that could have accounted for observed electoral behavior. First, one alternative explanation is that voters have short memories and only update from, and respond to, recent events (e.g. Zaller 1992), and thus pre-election homicides would drive election outcomes because voter beliefs cease to reflect news that they consumed earlier. Second, voters may similarly only respond to pre-election homicide shocks because incumbents have time to win back disgruntled voters when homicides occur earlier in their terms (e.g. Brollo 2009; Grossman and Michelitch 2018). Third, another possibility is that pre-election homicide spikes could induce emotional electoral responses that do not rely on voters updating—whether in a naive or sophisticated manner—about incumbent quality (e.g. Achen and Bartels 2017). However, contrary to the prediction of each alternative explanation, the survey results in Table 6 show that recent homicide shocks

do not cause voters to react when the shock occurs outside local election campaign periods. The results thus indicate that voters meaningfully update their beliefs in relatively sophisticated ways, and vote accordingly, but only during the periods when many voters regularly consume news likely to report local homicides.

7.2 Voter interest in elections drives the effects of news consumption cycles

Thus far, this article has considered news consumption cycles as an equilibrium outcome that could reflect increased voter demand for, and/or increased media supply of, politically-relevant news before elections. While the increased political news consumption at the extensive margin documented in Table 1 suggests that tuning in to watch or listen to news before elections is a conscious choice by voters, it is hard to separate whether increased voter interest in politically-relevant news or greater media outlet supply of politically-relevant news before elections drive increased consumption at the intensive margin. This section finds greater support for the demand mechanism, suggesting that the preceding results are driven by voters updating from reports of homicides that coincide with the period during which voters watch more news due to their interest in upcoming elections.

To assess whether voter interest drives increased news consumption before elections, I first leverage 2004-2013 state-level internet search data from Google Trends.[43] An increase in election-related or crime-related search themes before elections would suggest that voter interest in these topics frequently covered by the news may underpin the increased news consumption before elections shown in Table 1.[44] I examine this channel by estimating the following difference-in-differences specifications:

$$Term_{s,t} = \beta\, Upcoming\ local\ election_{s,t} + \gamma Month\ of\ election_{s,t} + \eta_s + \mu_t + \varepsilon_{s,t}, \tag{15}$$

where $Term_{s,t}$ is the relative frequency of citizen searches—normalized within states to range from 0 to 100, relative to the least and most popular themes covered in Table 8—falling within a given theme in state s in month t. By controlling for the month of the election itself, β captures the increase in relative theme searches in the 2 or 4 months preceding the election relative to all months other than the election month itself. Bootstrapped standard errors are clustered by state.

The results in Table 8 suggest that increased news consumption before elections is driven by voter interest in elections, rather than attempts to learn about crime. Column (1) of panel A reports

[43] Data before 2004 was not available. More recent search data was excluded to match the elections in Table 2.

[44] While searches could be media-induced, they are still a behavioral indicator of independent user interest.

Table 8: Upcoming local elections and Google search terms

	Google search topics:														
	"elecciones"			"homicidio"			"narcotráfico"			"crimen"			"seguridad"		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Panel A: Upcoming local election within 2 months															
Upcoming local election (2 months)	3.193***	3.212***	2.322***	-0.219	-0.226	-0.252**	0.236	0.271	0.138	-0.023	-0.014	0.068*	0.005	0.006	-0.075*
	(0.836)	(0.840)	(0.320)	(0.163)	(0.166)	(0.112)	(0.531)	(0.536)	(1.088)	(0.065)	(0.066)	(0.037)	(0.146)	(0.144)	(0.042)
Month of election	25.274***	25.293***	22.267***	-0.102	-0.108	0.006	-0.723	-0.689	-1.000	0.145	0.153	0.069	0.160	0.161	0.140*
	(1.688)	(1.684)	(2.167)	(0.155)	(0.155)	(0.055)	(0.512)	(0.514)	(1.070)	(0.106)	(0.105)	(0.050)	(0.264)	(0.264)	(0.072)
Pre-election outcome mean	4.74	4.74	3.56	0.64	0.64	0.42	11.41	11.41	16.89	0.36	0.36	0.23	0.63	0.63	0.37
Pre-election outcome std. dev.	8.78	8.78	7.58	1.91	1.91	0.71	15.13	15.13	17.26	1.11	1.11	0.70	1.68	1.68	0.71
Upcoming local election mean	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06
Panel B: Upcoming local election within 4 months															
Upcoming local election (4 months)	2.063***	2.085***	1.751***	-0.186	-0.193	-0.143**	0.318	0.357	0.283	-0.022	-0.013	0.022	0.027	0.028	-0.019
	(0.480)	(0.471)	(0.220)	(0.122)	(0.123)	(0.068)	(0.565)	(0.574)	(1.109)	(0.045)	(0.047)	(0.034)	(0.108)	(0.110)	(0.035)
Month of election	25.328***	25.350***	22.263***	-0.109	-0.116	0.007	-0.707	-0.670	-0.996	0.144	0.153	0.068	0.162	0.163	0.141**
	(1.691)	(1.706)	(2.188)	(0.154)	(0.154)	(0.054)	(0.533)	(0.533)	(1.050)	(0.106)	(0.106)	(0.050)	(0.262)	(0.263)	(0.071)
Pre-election outcome mean	4.70	4.70	3.53	0.65	0.65	0.42	11.53	11.53	17.15	0.37	0.37	0.23	0.63	0.63	0.37
Pre-election outcome std. dev.	8.96	8.96	7.83	1.93	1.93	0.72	15.27	15.27	17.42	1.13	1.13	0.71	1.68	1.68	0.72
Upcoming local election mean	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Observations	5,669	5,669	3,365	5,669	5,669	3,365	5,669	5,669	3,365	5,669	5,669	3,365	5,669	5,669	3,365
Unique states	32	32	32	32	32	32	32	32	32	32	32	32	32	32	32
Outcome range	[0,100]	[0,100]	[0,100]	[0,43]	[0,43]	[0,43]	[0,100]	[0,100]	[0,100]	[0,25]	[0,25]	[0,25]	[0,39]	[0,39]	[0,39]
State-specific time trends		✓			✓			✓			✓			✓	
2010-2013 data only			✓			✓			✓			✓			✓

Notes: All specifications include state and year-month fixed effects, and are estimated using OLS. Bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

that searches for the elections topic increase by more than one third of a standard deviation in the two months preceding local elections. Panel B shows a somewhat smaller average increase over the 4 months preceding local elections. Column (2) shows that these estimates are robust to including state-specific linear time trends, while column (3) shows that the results still since 2010—when 3G internet became widely available. In contrast, columns (4)-(15) report negligible and generally statistically insignificant changes in searches relating to crime and security. These findings are consistent with a demand-based channel, whereby interest elections stimulate news consumption that also informs voters about the recent homicides shown to influence voting behavior.

Even if increased news consumption before local elections reflects voter interest in the elections, the substantial electoral effects of homicides at the same time could still be driven by media outlets reporting on homicides more frequently at this time. In the absence of comprehensive broadcast media transcripts, I use newspaper data from Osorio (2015) to examine whether reporting on incidents of violent crime—a proxy for media reporting on homicides—increases before elections. Osorio (2015) uses machine-learning techniques to identify reports relating to drug violence from 105 government agencies and national and local newspapers between 2000 and 2010. The resulting report counts capture the frequency of coverage, but not its tone. Broadcast media coverage is likely to be similar, given that newspapers serve as an important information source for broadcast media outlets in developing contexts (Keefer and Khemani 2016).[45] I estimate the correlation between the number of homicides in a given month and reporting of violent events in the same month, both outside and within the 2 or 4 months preceding local elections, using the following difference-in-differences specification:

$$Reports_{m,t} = \beta_1 Homicides_{m,t} + \beta_2 \Big(Homicides_{m,t} \times Upcoming\ local\ election_{s,t} \Big) + \eta_m + \mu_{s,t} + \varepsilon_{m,t}, \quad (16)$$

where $Reports_{m,t}$ is a month-year municipal-level count of the number of violent events (including homicides as well as shootings, kidnappings, torture etc.) between DTOs reported for that month,[46] and η_m and $\mu_{s,t}$ are, respectively, municipality and state $\times$ month-year fixed effects. To mirror the aggregate electoral estimates, I use the same sample of municipalities and weight observations by the mean number of registered voters across the elections covered in the electoral analysis.

The results in Table 9 show a strong positive correlation between homicides and media coverage violent events, but—consistent with the lack of greater interest in homicides among voters—

[45] In a nationwide survey of local newspapers and radio stations in Mexico, 64% report sourcing news from other local media outlets (Larreguy, Lucas and Marshall 2016).

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

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

	Number of inter-DTO violence reports per month-year		
	(1)	(2)	(3)
Homicides per month	0.182***	0.182***	0.181***
	(0.023)	(0.024)	(0.023)
Homicides per month × Upcoming local election (4 months)		-0.000	
		(0.012)	
Homicides per month × Upcoming local election (2 months)			0.015
			(0.012)
Observations	117,650	117,650	117,650
Unique municipalities	905	905	905
Outcome range	[0,102]	[0,102]	[0,102]
Outcome mean	0.28	0.28	0.28
Outcome std. dev.	2.12	2.12	2.12
Homicides per month mean	0.93	0.93	0.93
Homicides per month std. dev.	5.24	5.24	5.24

Notes: All specifications include municipality and state × year-month 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. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

that coverage are no different before elections. Column (1) first demonstrates that for each homicide that occurs in a municipality, the number of violent events reported increases by 0.18. Since not all newspapers are studied and reports only pertain to drug-related violence, this estimate likely understates the propensity of newspapers to report homicides. Turning to differential coverage around elections, columns (2) and (3) indicate that reporting is not significantly greater in the 2 or 4 months preceding local elections. This is broadly consistent with surveys of media outlets in 2017, which indicate that while politics in general is covered more before elections, only 18% (5%) of outlets report on public security issues more (less) than usual before elections (Larreguy, Lucas and Marshall 2016). Both sets of results are precisely estimatedand robust to including state-month-year fixed effects (and their interaction with upcoming local elections). These findings indicate that media coverage of homicides is relatively insensitive to the electoral cycle, and thus suggest that the sanctioning of pre-election homicides is more likely driven by increased voter demand for politically-relevant news before elections than changes in the supply of such news.

8 Conclusion

This study finds, for a central issue in a developing context where politically-relevant knowledge is low, that the timing of voters' political news consumption—specifically, greater demand for news before elections—plays a key role in explaining voter beliefs and electoral selection. While access to local is a necessary condition, I find that information acquisition plays a central role in understanding when information reported in the news drives election outcomes.

I leverage multiple complementary sources of plausibly exogenous variation to support the underlying career concerns model's key predictions. First, I demonstrate that Mexican voters' political news consumption increases before local elections. Second, I then show that pre-election homicide spikes, which coincide with greater news consumption, substantially reduce the municipal incumbent party's probability of re-election. In contrast, voters are not sensitive to longer-run homicide trends; such indicators are likely to be more informative, but can only be observed by attentive voters consuming local news throughout the electoral cycle. Third, I support the news consumption channel by demonstrating that vote choices are principally driven by access to the local media outlets likely to report municipal crime. Finally, I provide additional support for news consumption cycle mechanism by showing that voting behavior is consistent with belief updating from limited information, rather than voters possessing short memories, and providing evidence suggesting that pre-election news consumption is principally driven by voter demand for election-related news.

Such news consumption cycles highlight the importance of understanding how voter demand for news and editorial decisions to report local news can shape electoral outcomes. In this regard, this article advances extant studies that have predominantly focused on access to media. Indeed, by showing that the timing of news reporting significantly influences electoral selection, my findings add nuance to our understanding of media persuasion, help explain the mixed evidence that voters hold politicians to account for their performance on the issues most salient to voters, and have clear implications for the design of NGO information campaigns. However, the welfare implications of many voters' reliance on pre-election signals are mixed. While I find that that poorly-informed voters process performance indicators like homicides in relatively sophisticated ways, electoral outcomes are often driven by the idiosyncratic component of noisy signals because voters cannot differentiate this idiosyncratic component from the incumbent quality component of homicide counts that they seek to draw inferences about.

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A.3.8 Differential effects of homicide shocks across parties A27
A.4 Differential effects of homicide shocks by incumbent ad shares A30

A.1 Data description

A.1.1 Months and years of municipal elections

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

A.1.2 ENCUP survey data

The ENCUP surveys 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.[47] Each round draws stratified random samples of around 4,500 Mexican voters for face-to-face interviews from pre-selected electoral precincts within urban and rural strata defined by the electoral register.[48]

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

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

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

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

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

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

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

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

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

Notes: Emboldened elections are counted as upcoming local elections, i.e. surveys conducted within 4 months of local election, in the survey analysis. State-level elections were held in Hidalgo in February 2002 without concurrent municipal elections, and are therefore 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 and Oaxaca 2013, missingness reflects the fact that data from the preceding election required to define the change in vote share was not available. For Baja California 2001 and 2004, the precinct numbering changed across elections and thus cannot be matched. For Oaxaca 2013, precinct level data was unavailable.

tuition? (2001) Which party holds your state governorship? (2001, 2003, 2005, 2012) What is the name of your state Governor? (2001)

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

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

- *Average monthly homicide rate (last 12 months)*. Average monthly homicide rate per 100,000 people within the municipality over the 12 months preceding the survey (excluding the current month).

- *Average monthly homicide rate (last 3 years)*. Average monthly homicide rate per 100,000 people within the municipality over the 36 months preceding the survey (excluding the current month).

- *Public insecurity the major problem in the community*. Indicator coded 1 if, in an open response, a respondent lists violence, crime or public security as the main problem facing their community (including as 0s respondents that listed no problem). *Number of organizations*. The number of organizations that a respondent reports being a member of, or previously being a member of. The number of possible organizations slightly varies across survey.

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

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

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

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

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

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

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

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

A.1.3 Municipal- and precinct-level homicide and electoral data

The main analyses use the following variables:

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

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

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

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

- *Homicide shock*. Defined in equation (8) 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.

- *Placebo homicide shock (6 months earlier)*. Defined as in equation (8) of the main paper, with the exception that all months are shifted 6 months forward in time.

- *Pre-election homicide rate*. The average monthly homicides rate per 100,000 people, as registered as occurring in the given municipality by INEGI, in the two months preceding a municipal election.

- *Average monthly homicide rate (12 months/3 years before election)*. Average number of residents suffering a homicide per month within the municipality in the 12 months/3 years prior to the municipal election, again based on INEGI homicide data.

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

- *Lower expectations*. Indicator coded 1 for municipalities where (i) the current incumbent party was in power before the previous election and oversaw a homicide shock; (ii) the incumbent party in power before the previous election was from a different party and did not oversee a homicide shock; or (iii) there were no homicides around the previous election.

- *Drug-related homicide shock*. Defined in equation (8) of the main paper, using the Calderón government's monthly statistics classifying the occurrence of drug-related homicides between 2007 and 2011 in a given municipality.

- *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.

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

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

A.1.4 Precinct local media coverage data

The main analyses use the following additional variables:

- *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).

- *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.1.5 Media demand and supply mechanisms data

The following variables from additional datasets are used in the mechanisms analysis:

- *Number of inter-DTO violence reports per month-year*. The number of reports in collected newspapers at the municipal-month level. The data are from Osorio's (2015) online replication archive.

- *Homicides per month*. The number of homicides that occur in a given municipality-month, using the INEGI data described above.

- **Google search topics**. A measure of search frequency for a variety of terms. Within each states, scores across the term categories range from 0 to 100, based on relative search propensity within the state. This data was downloaded directly from Google Trends.

A.1.6 Map of municipalities included in different samples

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

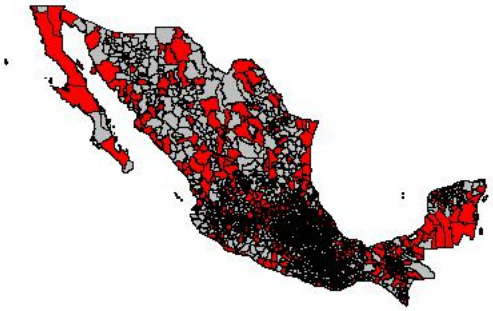
A.2 Evidence supporting the identification assumptions

Tables A2-A7 show the results of balance tests for the three main sets of empirical findings in the paper: Table A2 shows that upcoming local elections are well balanced across individual and municipal characteristics in the ENCUP survey data; Tables A4 and A5 shows that homicide shocks are well balanced across a wide variety of covariates, where municipality fixed effects are excluded for time-invariant Census and geographic variables; and Table A7 shows that the number of local media stations is well-balanced across these same covariates. In addition, Table A3 shows that homicides are well-balanced in the ENCUP surveys.

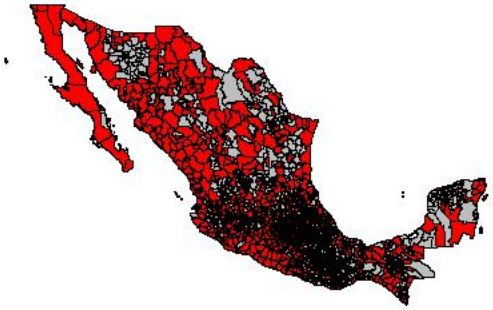
I also provide several additional tests to support the exogeneity of pre-election homicide shocks. First, Figure A2 shows that the distribution of homicide rates in the two-month windows prior to the window used to define homicide shocks does not vary by whether a municipality ultimately experienced a homicide shock. For each set of months, Kolmogorov-Smirnov tests fail to reject the null of equality of distribution: respectively, the p-values associated with the combined test for 9 and 10 months before the election, 7 and 8 months before the election, 5 and 6 months before the election, and 3 and 4 months before the election are 0.377, 0.988, 0.684, and 0.997. Second, Table A6 shows that election outcomes are uncorrelated with homicide rates in the two months after elections. In particular, columns (2) and (3) find no correlation between the identity of the winning party and post-election homicide rate either throughout the sample or during the Calderón administration. This does not conflict with Dell (2015), who focuses on drug-related homicides over the *subsequent* mayor's term, or as many months as possible of that mayor's term.

A.3 Additional results

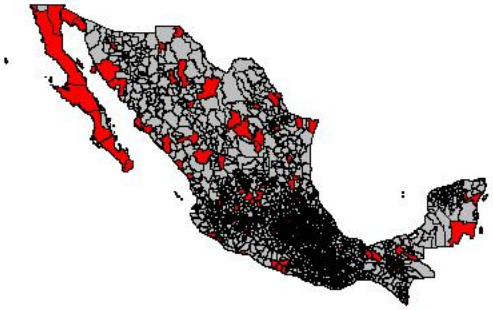
The following subsections present the results of additional analyses cited in the main paper.



(a) Municipalities in the ENCUP survey samples



(b) Municipalities in the homicide shock sample



(c) Municipalities in the local media neighbor sample

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

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

	Female	Age	Education	Employed last year	Own econ. in month	Number of org.s	Org.s talk about politics	Number of group meetings	Discuss community problems
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Upcoming local election	-0.013	-1.449	0.252**	-0.066	-0.072	-0.176	-0.023	0.066	-0.025
	(0.065)	(1.242)	(0.102)	(0.054)	(0.145)	(0.203)	(0.085)	(0.288)	(0.102)
Unique states	31	31	31	31	31	31	31	31	31
Observations	15,976	15,976	11,756	11,756	12,322	15,976	12,576	15,976	12,576
Outcome mean	0.55	40.76	1.70	0.47	1.57	1.06	0.26	1.52	0.70
Local election mean	0.06	0.06	0.03	0.03	0.07	0.06	0.07	0.06	0.07
	Homicide shock	Homicide last year	Homicide last 3 years	Incumbent win margin (lag)	Incumbent won (lag)	Police per voter (last year)	PAN governor	PRI governor	PRD governor
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Upcoming local election	-0.072	4.516	4.398	-0.016	0.111***	-0.063	-0.336***	-0.056	-0.056
	(0.203)	(3.080)	(3.147)	(0.024)	(0.041)	(1.293)	(0.111)	(0.320)	(0.315)
Unique states	31	31	31	31	31	31	31	31	31
Observations	12,664	15,941	15,941	15,976	15,833	13,666	15,896	15,896	15,896
Outcome mean	0.44	4.50	4.82	0.15	0.54	2.30	0.29	0.08	0.08
Local election mean	0.06	0.06	0.06	0.06	0.06	0.05	0.06	0.06	0.06

Notes: All specifications include survey year fixed effects, and are estimated using OLS. Bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

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

	Female	Age	Education	Employed last year	Own econ. in month	Number of org.s	Org.s talk about politics	Number of group meetings	Discuss community problems
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Homicide shock	-0.002	-0.267	-0.010	-0.006	0.024	0.026	-0.010	0.029	-0.015
	(0.007)	(0.273)	(0.053)	(0.012)	(0.016)	(0.064)	(0.014)	(0.062)	(0.020)
Observations	12,664	12,664	9,464	9,464	9,591	12,664	9,764	12,664	9,764
Outcome mean	0.55	40.58	1.79	0.47	1.57	1.05	0.26	1.45	0.69
Homicide shock mean	0.44	0.44	0.45	0.45	0.46	0.44	0.46	0.44	0.46
	Upcoming local election	Homicide last year	Homicide last 3 years	Incumbent win margin (lag)	Incumbent won (lag)	Police per voter (last year)	PAN governor	PRI governor	PRD governor
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Homicide shock	-0.017	-2.357*	-3.143*	0.000	-0.000	-0.139	-0.018	0.013	0.013
	(0.019)	(1.267)	(1.860)	(0.010)	(0.041)	(0.106)	(0.040)	(0.020)	(0.020)
Observations	12,664	12,664	12,664	12,664	12,521	10,584	12,584	12,584	12,584
Outcome mean	0.06	5.62	6.03	0.15	0.54	2.17	0.28	0.08	0.08
Homicide shock mean	0.44	0.44	0.44	0.44	0.44	0.45	0.44	0.44	0.44

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

Table A4: Municipal-level balance on 99 variables over pre-election homicide shocks

Outcome	Effect of homicide shock Coef.	SE	Obs.	Outcome	Effect of homicide shock Coef.	SE	Obs.	Outcome	Effect of homicide shock Coef.	SE	Obs.
Electorate	8050.4324**	(3,594.6290)	2,838	Reports of violent enforcement before election	0.0467	(0.0995)	1,906	Non-local media	0.3888	(0.6710)	2,835
Electorate density (log)	-0.0079	(0.0076)	2,823	Reports of arrests before election	0.8854	(0.5708)	1,906	Occupants per room	0.0044	(0.0097)	2,810
Incumbent vote share (lag)	0.0015	(0.0061)	2,801	Reports of drug seizures before election	1.3563	(0.9128)	1,906	Share with 2 bedrooms	-0.0054	(0.0051)	2,810
Incumbent win (lag)	0.0475	(0.0328)	2,317	Reports of asset seizures before election	-0.0867	(0.2142)	1,906	Share 3+ bedrooms	-0.0055	(0.0054)	2,810
Win margin (lag)	0.0016	(0.0087)	2,801	Reports of gun seizures before election	0.6084*	(0.3263)	1,906	Share female	-0.0007	(0.0005)	2,810
ENPV (lag)	-0.0140	(0.0287)	2,838	Gun-related share of homicides before election	-0.0067	(0.0240)	1,810	Share working age	0.0005	(0.0017)	2,810
Turnout (lag)	-0.0094***	(0.0028)	2,631	Chemical substance-related share of homicides before election	0.0020	(0.0028)	1,810	Children per woman	0.0056	(0.0138)	2,810
PRI incumbent	-0.0251	(0.0311)	2,838	Hanging-related share of homicides before election	-0.0043	(0.0115)	1,810	Female years of schooling	-0.0298	(0.0681)	2,810
PAN incumbent	0.0263	(0.0290)	2,838	Drowning-related share of homicides before election	0.0035	(0.0035)	1,810	Male years of schooling	-0.0449	(0.0753)	2,810
PRD incumbent	-0.0080	(0.0247)	2,838	Explosives-related share of homicides before election	0.0019	(0.0016)	1,810	Share illiterate	0.0011	(0.0017)	2,810
Mayor's party aligned with President	0.0545**	(0.0276)	2,838	Smoke/fire-related share of homicides before election	-0.0006	(0.0046)	1,810	Share economically active	-0.0022	(0.0020)	2,810
Mayor's party aligned with Governor	-0.0274	(0.0317)	2,838	Cutting object-related share of homicides before election	-0.0200	(0.0148)	1,810	Share born out of state	0.0002	(0.0075)	2,810
Mayor's party aligned with President and Governor	0.0103	(0.0139)	2,838	Blunt object-related share of homicides before election	0.0026	(0.0054)	1,810	Share Catholic	0.0021	(0.0026)	2,810
Total municipal spending	13.6004	(61.9228)	1,751	Share of homicide registrations delayed before election	0.0022	(0.0070)	1,810	Share indigenous speakers	0.0001	(0.0030)	2,810
Police per voter	0.0381	(0.0630)	2,838	Male share of homicide victims before election	0.0230	(0.0149)	1,810	Share without health care	-0.0025	(0.0038)	2,810
Homicide per 100,000 12 months before election	-0.2886*	(0.1555)	2,838	Average age of homicide victims before election	1.7469***	(0.5848)	1,763	Share with electricity, running water, and drainage	-0.0077	(0.0060)	2,810
Homicide per 100,000 11 months before election	-0.1454	(0.2052)	2,838	Share of victims with no schooling before election	0.0065	(0.0143)	1,666	Share washing machine	0.0003	(0.0066)	2,810
Homicide per 100,000 10 months before election	-0.2957	(0.1959)	2,838	Share of victims with incomplete primary schooling before election	-0.0042	(0.0153)	1,666	Share landline telephone	-0.0091	(0.0100)	2,810
Homicide per 100,000 9 months before election	-0.2208	(0.1635)	2,838	Share of victims with complete primary schooling before election	-0.0295	(0.0247)	1,666	Share radio	-0.0017	(0.0038)	2,810
Homicide per 100,000 8 months before election	-0.2496**	(0.1238)	2,838	Share of victims with incomplete secondary schooling before election	-0.0140	(0.0225)	1,666	Share fridge	0.0006	(0.0051)	2,810
Homicide per 100,000 7 months before election	-0.1203	(0.1512)	2,838	Share of single victims before election	-0.0219	(0.0213)	1,707	Share cell phone	-0.0012	(0.0061)	2,810
Homicide per 100,000 6 months before election	-0.1091	(0.1443)	2,838	Job-related share of homicides before election	0.0034	(0.0188)	1,212	Share television	-0.0007	(0.0023)	2,810
Homicide per 100,000 5 months before election	-0.0374	(0.1013)	2,838	Share of homicides where organs were examined before election	0.0039	(0.0078)	1,570	Share car or truck	0.0026	(0.0055)	2,810
Homicide per 100,000 4 months before election	0.0177	(0.1182)	2,838	Family-incident share of homicides before election	-0.0050	(0.0068)	1,754	Share computer	-0.0046	(0.0074)	2,810
Homicide per 100,000 3 months before election	-0.1598	(0.1328)	2,838	Urban share of homicide victims before election	0.0134	(0.0183)	1,395	Share internet	-0.0037	(0.0066)	2,810
Average monthly homicide rate (prior 3 years)	-0.0212	(0.0690)	2,838	Non-Mexican share of homicide victims before election	0.0032	(0.0032)	1,810	Non-homicide crimes shock	0.0676	(0.0674)	505
Average monthly homicide rate (prior year)	-0.1609	(0.1254)	2,838	Neighbor average homicide shock	-0.0238	(0.0190)	2,721	Robbery shock	0.0928	(0.0672)	483
Top homicide quartile (prior year)	-0.0112	(0.0254)	2,838	Average non-homicide deaths (prior 3 years)	-0.1205	(0.0992)	2,838	Property theft shock	-0.0468	(0.0615)	461
Top homicide decile (prior year)	0.0099	(0.0213)	2,838	Average non-homicide deaths (prior year)	-0.0847	(0.1240)	2,838	Carjacking shock	0.0509	(0.1300)	121
Presence of DTO in municipality	-0.0490	(0.0317)	2,000	Average child mortalities (prior 3 years)	-0.0014	(0.0284)	2,107	Criminal injury shock	0.0704	(0.0695)	454
Criminal-electoral violence events	-0.0209	(0.0213)	2,499	Average child mortalities (prior year)	-0.0038	(0.0330)	2,107	Sexual assault shock	0.1276	(0.0944)	307
Average drug-related monthly homicide rate (prior year)	0.0000	(0.0267)	2,838	Area (km^2)	430.6155	(274.9787)	2,838	Kidnapping shock	-0.3520**	(0.1428)	93
Reports of inter-DTO violence before election	-0.8574	(0.5871)	1,906	Local media	-1.2808	(0.8195)	2,835	Other crime shock	0.1266*	(0.0697)	503

Notes: Specifications include municipality and state × year fixed effects, and are estimated using OLS. 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 (occupants per dwelling-share internet)—and pre-election shocks regarding other types of crime (data are only available from 2011 onwards) exclude municipality fixed effects. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A5: Precinct-level balance over 105 variables by pre-election homicide shocks

Outcome	Effect of homicide shock			Outcome	Effect of homicide shock			Outcome	Effect of homicide shock		
	Coef.	SE	Obs.		Coef.	SE		Obs.	Coef.	SE	Obs.
Electorate	68.5188*	(39.1734)	166,355	Criminal-electoral violence events	-0.0208	(0.0208)	151,442	Area (km^2)	2.0194	(1.7517)	163,968
Municipal electorate	8087.6270**	(3,576.3795)	166,355	High-risk electoral precinct	0.0014	(0.0037)	50,279	Local media	-1.3315*	(0.7987)	164,327
Electorate density (log)	-0.0001	(0.0059)	163,968	Average drug-related monthly homicide rate (prior year)	-0.0005	(0.0263)	166,355	Non-local media	0.4810	(0.6737)	164,327
Incumbent vote share (lag)	0.0008	(0.0059)	166,355	Reports of inter-DTO violence before election	-0.8600	(0.5751)	126,679	Occupants per room	0.0064	(0.0092)	166,023
Municipal-level incumbent vote share (lag)	0.0015	(0.0059)	165,477	Reports of violent enforcement before election	0.0484	(0.0975)	126,679	Share with 2 bedrooms	-0.0052	(0.0050)	165,895
Incumbent win (lag)	0.0479	(0.0320)	138,678	Reports of arrests before election	0.9011	(0.5614)	126,679	Share 3+ bedrooms	-0.0055	(0.0052)	165,895
Win margin (lag)	-0.0011	(0.0069)	166,355	Reports of drug seizures before election	1.3818	(0.8982)	126,679	Share female	-0.0009*	(0.0005)	165,906
Municipal-level win margin (lag)	0.0013	(0.0085)	165,477	Reports of asset seizures before election	-0.0784	(0.2107)	126,679	Share working age	0.0008	(0.0016)	165,906
Municipal-level ENPV (lag)	-0.0119	(0.0234)	166,352	Reports of gun seizures before election	0.6200*	(0.3223)	126,679	Children per woman	0.0068	(0.0134)	166,023
ENPV (lag)	-0.0150	(0.0281)	166,355	Gun-related share of homicides before election	-0.0070	(0.0231)	145,302	Female years of schooling	-0.0469	(0.0686)	166,023
Turnout (lag)	-0.0098***	(0.0027)	157,175	Chemical substance-related share of homicides before election	0.0020	(0.0027)	145,302	Male years of schooling	-0.0622	(0.0764)	166,023
Municipal-level turnout (lag)	-0.0092***	(0.0027)	156,573	Hanging-related share of homicides before election	-0.0042	(0.0111)	145,302	Share illiterate	0.0011	(0.0017)	165,895
PRI incumbent	-0.0240	(0.0305)	166,355	Drowning-related share of homicides before election	0.0032	(0.0033)	145,302	Share economically active	-0.0022	(0.0020)	165,906
PAN incumbent	0.0268	(0.0285)	166,355	Explosives-related share of homicides before election	0.0019	(0.0016)	145,302	Share born out of state	0.0002	(0.0071)	165,906
PRD incumbent	-0.0090	(0.0241)	166,355	Smoke/fire-related share of homicides before election	-0.0007	(0.0045)	145,302	Share Catholic	0.0019	(0.0024)	165,906
Mayor's party aligned with President	0.0548**	(0.0271)	166,355	Cutting object-related share of homicides before election	-0.0194	(0.0143)	145,302	Share indigenous speakers	-0.0002	(0.0029)	165,895
Mayor's party aligned with Governor	-0.0278	(0.0309)	166,355	Blunt object-related share of homicides before election	0.0025	(0.0052)	145,302	Share without health care	-0.0018	(0.0038)	165,906
Mayor's party aligned with President and Governor	0.0106	(0.0135)	166,355	Share of homicide registrations delayed before election	0.0031	(0.0067)	145,302	Share with electricity, running water, and drainage	-0.0079	(0.0058)	165,895
Total municipal spending	14.0449	(60.7808)	110,803	Male share of homicide victims before election	0.0226	(0.0143)	145,302	Share washing machine	-0.0005	(0.0064)	165,895
Police per voter	0.0419	(0.0605)	166,355	Average age of homicide victims before election	1.6920***	(0.5624)	144,250	Share landline telephone	-0.0111	(0.0094)	165,895
Homicide per 100,000 12 months before election	-0.2908*	(0.1518)	166,355	Share of victims with no schooling before election	0.0059	(0.0138)	141,709	Share radio	-0.0025	(0.0037)	165,895
Homicide per 100,000 11 months before election	-0.1506	(0.2011)	166,355	Share of victims with incomplete primary schooling before election	-0.0035	(0.0147)	141,709	Share fridge	0.0003	(0.0049)	165,895
Homicide per 100,000 10 months before election	-0.2965	(0.1899)	166,355	Share of victims with complete primary schooling before election	-0.0295	(0.0237)	141,709	Share cell phone	-0.0013	(0.0059)	165,895
Homicide per 100,000 9 months before election	-0.2226	(0.1599)	166,355	Share of victims with incomplete secondary schooling before election	-0.0144	(0.0217)	141,709	Share television	-0.0006	(0.0023)	165,895
Homicide per 100,000 8 months before election	-0.2436**	(0.1206)	166,355	Share of single victims before election	-0.0226	(0.0205)	142,985	Share car or truck	0.0012	(0.0053)	165,895
Homicide per 100,000 7 months before election	-0.1214	(0.1460)	166,355	Job-related share of homicides before election	0.0038	(0.0178)	126,596	Share computer	-0.0061	(0.0072)	165,895
Homicide per 100,000 6 months before election	-0.1112	(0.1397)	166,355	Share of homicides where organs were examined before election	0.0043	(0.0075)	138,415	Share internet	-0.0052	(0.0065)	165,895
Homicide per 100,000 5 months before election	-0.0418	(0.0988)	166,355	Family-incident share of homicides before election	-0.0045	(0.0065)	142,513	Non-homicide crimes shock	0.0670	(0.0656)	32,370
Homicide per 100,000 4 months before election	0.0172	(0.1159)	166,355	Urban share of homicide victims before election	0.0140	(0.0177)	123,074	Robbery shock	0.0869	(0.0651)	32,155
Homicide per 100,000 3 months before election	-0.1591	(0.1304)	166,355	Non-Mexican share of homicide victims before election	0.0031	(0.0031)	145,302	Property theft shock	-0.0394	(0.0588)	31,207
Average monthly homicide rate (prior 3 years)	-0.0215	(0.0673)	166,355	Neighbor average homicide shock	-0.0238	(0.0186)	163,149	Carjacking shock	0.0566	(0.1219)	7,450
Average monthly homicide rate (prior year)	-0.1621	(0.1223)	166,355	Average non-homicide deaths (prior 3 years)	-0.1105	(0.0970)	166,355	Criminal injury shock	0.0665	(0.0663)	31,421
Top homicide quartile (prior year)	-0.0105	(0.0249)	166,355	Average non-homicide deaths (prior year)	-0.0723	(0.1214)	166,355	Sexual assault shock	0.1221	(0.0898)	25,109
Top homicide decile (prior year)	0.0100	(0.0208)	166,355	Average child mortalities (prior 3 years)	-0.0009	(0.0277)	133,829	Kidnapping shock	-0.3479***	(0.1251)	12,566
Presence of DTO	-0.0497	(0.0308)	130,678	Average child mortalities (prior year)	-0.0034	(0.0322)	133,829	Other crime shock	0.1300*	(0.0664)	32,335

Notes: Specifications include municipality and state $\times$ year fixed effects, and are estimated using OLS. 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 (occupants per dwelling-share internet)—and pre-election shocks regarding other types of crime (data are only available from 2011 onwards) exclude municipality fixed effects. Standard errors clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

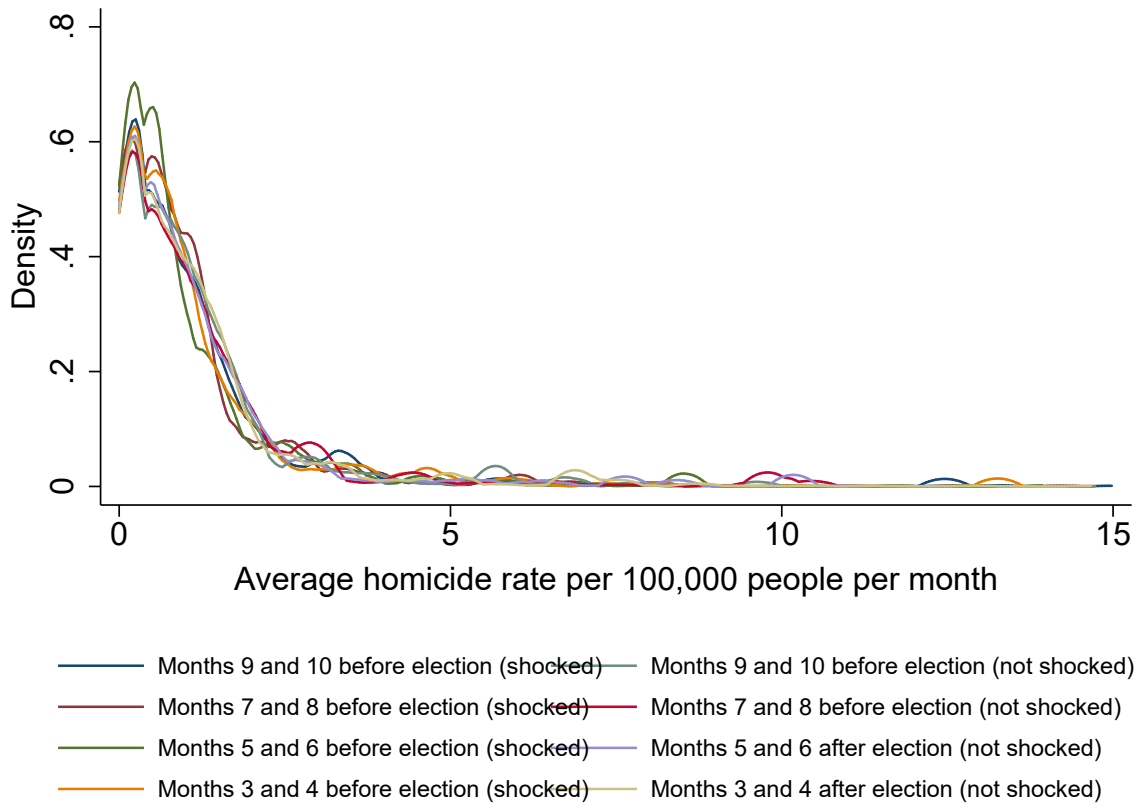


Figure A2: Distribution of pre-election homicide rates, by homicide shock status

A.3.1 Effects on turnout

Table A9 examines how homicide shocks affect turnout and incumbent vote share (as a proportion of registered voters). The results show that a homicide shock significantly increases turnout,[49] but barely affects incumbent vote shares as a proportion of registered voters. Bearing in mind the limits of ecological inference, these results suggest that the decline in incumbent vote share (as a proportion of those that turned out) principally reflects a rise in turnout for non-incumbent parties. The sample sizes are slightly smaller than reported in the main text because turnout at the previous election is not always available, and thus the outcome cannot be constructed for some cases. Column (1) demonstrates that the overall effect on vote share continues to hold in this subsample.

The results in Table A14 report the effect of drug-related homicide shocks just before an election, defined according to equation (8) but instead using drug-related homicides. Given that only

[49] The corresponding municipal level estimate is identical to 3 decimal places.

Table A6: Correlation between election outcomes and post-election homicides

	Average homicide rate per 100,000 people in the 2 months after an election		
	(1)	(2)	(3)
Incumbent party win	0.210		
	(0.159)		
PAN win		0.241	0.420
		(0.296)	(0.582)
PRI win		0.210	-0.207
		(0.219)	(0.501)
PRD win		0.443	0.890
		(0.276)	(0.543)
Observations	2,838	2,838	965
Unique municipalities	905	905	479
Outcome mean	1.66	1.66	2.34
Election outcome mean	0.55		

Notes: All specifications include municipality and state $\times$ year fixed effects, and are estimated using OLS. Column (3) restricts the sample to elections during the Calderón administration. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

five years of data exist, and thus many municipalities only appear once, I use state fixed effects instead of municipality fixed effects. Although the large drop in sample size unsurprisingly reduces precision substantially, the point estimates are similar to those reported in Table 2. These findings reinforce the claim in the main text that voters respond similarly to different types of homicide.

A.3.2 Additional robustness checks for the individual-level survey results

Table A10 shows that the information consumption results are robust to defining upcoming local elections by any number of months between 1 and 12 before the election.

Table A11 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.

A.3.3 Additional robustness checks for the aggregate electoral results

Table A12 reports the results from Table 2 using incumbent vote share instead of the difference in incumbent vote share with respect to the previous election. The results do not meaningfully differ, but are—as expected—somewhat less precisely estimated than for the difference in the incumbent

Table A7: Precinct-level balance over 93 variables by the number of local media stations

Outcome	Effect of homicide shock			Outcome	Effect of homicide shock			Outcome	Effect of homicide shock		
	Coef.	SE	Obs.		Coef.	SE		Obs.	Coef.	SE	Obs.
Homicide shock	0.0001	(0.0001)	29,704	Homicide per 100,000 4 months before election	0.0002	(0.0003)	29,704	Share of single victims before election	0.0001	(0.0001)	27,996
Area (km^2)	-0.0027	(0.0060)	29,704	Homicide per 100,000 3 months before election	0.0001	(0.0005)	29,704	Job-related share of homicides before election	0.0000	(0.0000)	26,234
Electorate	-28.5186*	(14.6964)	29,704	Average monthly homicide rate (prior 3 years)	0.0002	(0.0002)	29,704	Share of homicides where organs were examined before election	0.0000	(0.0000)	27,610
Municipal electorate	17.1619	(16.338)	29,704	Average monthly homicide rate (prior year)	0.0001	(0.0003)	29,704	Family-incident share of homicides before election	0.0000	(0.0000)	27,472
Electorate density (log)	-0.0123	(0.0087)	29,669	Top homicide quartile (prior year)	0.0001*	(0.0000)	29,704	Urban share of homicide victims before election	0.0000	(0.0000)	24,701
Distance from centroid to municipal head (log)	-0.0051	(0.0035)	29,704	Top homicide decile (prior year)	0.0000	(0.0001)	29,704	Non-Mexican share of homicide victims before election	0.0000*	(0.0000)	28,167
Non-local media	-0.0299	(0.2371)	29,704	Average drug-related monthly homicide rate (prior year)	0.0000	(0.0000)	29,704	Neighbor average homicide shock	0.0000	(0.0001)	28,576
Incumbent vote share (lag)	-0.0001	(0.0004)	29,704	Presence of DTO	0.0000	(0.0001)	22,779	Average non-homicide deaths (prior 3 years)	0.0004	(0.0004)	29,704
Incumbent win (lag)	-0.0001	(0.0002)	24,161	Criminal electoral violence events	0.0000	(0.0001)	25,735	Average non-homicide deaths (prior year)	0.0002	(0.0004)	29,704
Win margin (lag)	-0.0001	(0.0006)	29,704	High-risk electoral precinct	0.0016	(0.0017)	7,377	Average child mortalities (prior 3 years)	0.0000	(0.0001)	23,095
Municipality win margin (lag)	0.0000	(0.0000)	29,633	Reports of inter-DTO violence before election	-0.0010	(0.0011)	22,070	Average child mortalities (prior year)	0.0000	(0.0001)	23,095
ENPV (lag)	0.0009	(0.0016)	29,704	Reports of violent enforcement before election	0.0001	(0.0002)	22,070	Occupants per room	-0.0044**	(0.0017)	29,704
Municipality ENPV (lag)	-0.0001	(0.0001)	29,704	Reports of arrests before election	0.0019	(0.0012)	22,070	Share with 2 bedrooms	0.0001	(0.0006)	29,704
Turnout (lag)	0.0006	(0.0004)	28,754	Reports of drug seizures before election	0.0022	(0.0021)	22,070	Share 3+ bedrooms	0.0011	(0.0007)	29,704
Municipality turnout (lag)	0.0000	(0.0000)	28,723	Reports of asset seizures before election	-0.0010*	(0.0005)	22,070	Share female	0.0001	(0.0002)	29,704
PRI incumbent	0.0000	(0.0001)	29,704	Reports of gun seizures before election	0.0005	(0.0006)	22,070	Share working age	-0.0003	(0.0002)	29,704
PAN incumbent	0.0000	(0.0001)	29,704	Gun-related share of homicides before election	0.0001	(0.0001)	28,167	Children per woman	-0.0026	(0.0017)	29,704
PRD incumbent	0.0000	(0.0000)	29,704	Chemical substance-related share of homicides before election	0.0000	(0.0000)	28,167	Female years of schooling	0.0137	(0.0109)	29,704
Mayor's party aligned with President	0.0000	(0.0001)	29,704	Hanging-related share of homicides before election	0.0000	(0.0001)	28,167	Male years of schooling	0.0233**	(0.0114)	29,704
Mayor's party aligned with Governor	-0.0001	(0.0001)	29,704	Drowning-related share of homicides before election	0.0000	(0.0000)	28,167	Share illiterate	-0.0003**	(0.0001)	29,704
Mayor's party aligned with President and Governor	0.0000	(0.0000)	29,704	Explosives-related share of homicides before election	0.0000	NA	28,167	Share economically active	0.0002	(0.0003)	29,704
Total municipal spending	0.0794	(0.1524)	19,788	Smoke/fire-related share of homicides before election	0.0000	(0.0000)	28,167	Share born out of state	0.0007	(0.0006)	29,704
Police per voter	0.0004*	(0.0002)	29,704	Cutting object-related share of homicides before election	0.0000	(0.0001)	28,167	Share Catholic	0.0001	(0.0005)	29,704
Homicide per 100,000 12 months before election	0.0004	(0.0004)	29,704	Blunt object-related share of homicides before election	0.0000	(0.0000)	28,167	Share indigenous speakers	0.0000	(0.0002)	29,704
Homicide per 100,000 11 months before election	0.0003	(0.0005)	29,704	Share of homicide registrations delayed before election	0.0000	(0.0000)	28,167	Share without health care	-0.0003	(0.0004)	29,704
Homicide per 100,000 10 months before election	-0.0007	(0.0006)	29,704	Male share of homicide victims before election	0.0000	(0.0001)	28,167	Share with electricity, running water, and drainage	0.0016*	(0.0008)	29,704
Homicide per 100,000 9 months before election	0.0004	(0.0006)	29,704	Average age of homicide victims before election	-0.0030	(0.0018)	27,916	Share washing machine	0.0008	(0.0009)	29,704
Homicide per 100,000 8 months before election	0.0003	(0.0003)	29,704	Share of victims with no schooling before election	0.0000	(0.0000)	27,917	Share fridge	0.0006	(0.0005)	29,704
Homicide per 100,000 7 months before election	0.0000	(0.0004)	29,704	Share of victims with incomplete primary schooling before election	0.0000	(0.0000)	27,917	Share cell phone	0.0004	(0.0005)	29,704
Homicide per 100,000 6 months before election	0.0004	(0.0005)	29,704	Share of victims with complete primary schooling before election	0.0000	(0.0001)	27,917	Share car or truck	0.0015	(0.0013)	29,704
Homicide per 100,000 5 months before election	0.0001	(0.0005)	29,704	Share of victims with incomplete secondary schooling before election	-0.0001	(0.0001)	27,917	Share computer	0.0023*	(0.0013)	29,704

Notes: All specifications include neighbor group and state $\times$ year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters divided by he 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 clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A8: Precinct-level balance over 93 variables by pre-election homicide shocks (in the local media sample)

Outcome	Effect of homicide shock			Outcome	Effect of homicide shock			Outcome	Effect of homicide shock		
	Coef.	SE	Obs.		Coef.	SE	Obs.		Coef.	SE	Obs.
Homicide shock	0.0059	(0.0045)	29,704	Homicide per 100,000 4 months before election	-0.2421	(0.4769)	29,704	Share of single victims before election	0.0424	(0.0497)	27,996
Area (km^2)	0.0006	(0.0015)	29,704	Homicide per 100,000 3 months before election	0.1094	(0.5633)	29,704	Job-related share of homicides before election	0.0307	(0.0554)	26,234
Electorate	71.6305**	(28.0041)	29,704	Average monthly homicide rate (prior 3 years)	-0.1141	(0.2931)	29,704	Share of homicides where organs were examined before election	0.0636***	(0.0211)	27,610
Municipal electorate	14,250.6458***	(4,552.9630)	29,704	Average monthly homicide rate (prior year)	-0.3879	(0.5552)	29,704	Family-incident share of homicides before election	0.0050	(0.0119)	27,472
Electorate density (log)	0.0130	(0.0090)	29,669	Top homicide quartile (prior year)	-0.0575	(0.0560)	29,704	Urban share of homicide victims before election	-0.0029	(0.0314)	24,701
Distance from centroid to municipal head (log)	-0.0007	(0.0014)	29,704	Top homicide decile (prior year)	0.0734	(0.0596)	29,704	Non-Mexican share of homicide victims before election	-0.0047	(0.0117)	28,167
Non-local media	0.0027	(0.0022)	29,704	Average drug-related monthly homicide rate (prior year)	-0.0081	(0.0326)	29,704	Neighbor average homicide shock	-0.0847**	(0.0371)	28,576
Incumbent vote share (lag)	-0.0086	(0.0159)	29,704	Presence of DTO	-0.1134*	(0.0655)	22,779	Average non-homicide deaths (prior 3 years)	-0.4249*	(0.2174)	29,704
Incumbent win (lag)	-0.0059	(0.0777)	24,161	Criminal electoral violence events	-0.0033	(0.0669)	25,735	Average non-homicide deaths (prior year)	-0.5299*	(0.2827)	29,704
Win margin (lag)	0.0072	(0.0219)	29,704	High-risk electoral precinct	0.0139	(0.0086)	7,377	Average child mortalities (prior 3 years)	-0.0096	(0.0561)	23,095
Municipality win margin (lag)	0.0297	(0.0233)	29,633	Reports of inter-DTO violence before election	-2.4641	(1.4865)	22,070	Average child mortalities (prior year)	0.0452	(0.0592)	23,095
ENPV (lag)	0.0348	(0.0476)	29,704	Reports of violent enforcement before election	-0.0034	(0.2650)	22,070	Occupants per room	0.0007	(0.0005)	29,704
Municipality ENPV (lag)	0.0628	(0.0421)	29,704	Reports of arrests before election	2.0682	(1.4837)	22,070	Share with 2 bedrooms	-0.0002	(0.0003)	29,704
Turnout (lag)	-0.0075	(0.0060)	28,754	Reports of drug seizures before election	3.9320*	(2.2720)	22,070	Share 3+ bedrooms	-0.0004	(0.0003)	29,704
Municipality turnout (lag)	-0.0055	(0.0054)	28,723	Reports of asset seizures before election	0.2741	(0.4902)	22,070	Share female	0.0000	(0.0000)	29,704
PRI incumbent	0.0975*	(0.0565)	29,704	Reports of gun seizures before election	0.1980	(0.5206)	22,070	Share working age	-0.0002**	(0.0001)	29,704
PAN incumbent	-0.0988*	(0.0550)	29,704	Gun-related share of homicides before election	0.0197	(0.0626)	28,167	Children per woman	-0.0004	(0.0009)	29,704
PRD incumbent	-0.0605	(0.0478)	29,704	Chemical substance-related share of homicides before election	-0.0017	(0.0017)	28,167	Female years of schooling	-0.0026	(0.0036)	29,704
Mayor's party aligned with President	-0.0102	(0.0503)	29,704	Hanging-related share of homicides before election	-0.0184	(0.0215)	28,167	Male years of schooling	-0.0018	(0.0043)	29,704
Mayor's party aligned with Governor	0.0099	(0.0494)	29,704	Drowning-related share of homicides before election	-0.0005	(0.0051)	28,167	Share illiterate	0.0000	(0.0001)	29,704
Mayor's party aligned with President and Governor	-0.0082	(0.0130)	29,704	Explosives-related share of homicides before election	0.0000	NA	28,167	Share economically active	-0.0001	(0.0001)	29,704
Total municipal spending	70.0739	(121.1612)	19,788	Smoke/fire-related share of homicides before election	-0.0023	(0.0029)	28,167	Share born out of state	-0.0003	(0.0003)	29,704
Police per voter	0.3195	(0.1964)	29,704	Cutting object-related share of homicides before election	-0.0232	(0.0281)	28,167	Share Catholic	-0.0003**	(0.0001)	29,704
Homicide per 100,000 12 months before election	-0.4055	(0.6128)	29,704	Blunt object-related share of homicides before election	-0.0002	(0.0112)	28,167	Share indigenous speakers	0.0000	(0.0000)	29,704
Homicide per 100,000 11 months before election	0.0755	(0.8496)	29,704	Share of homicide registrations delayed before election	0.0211	(0.0187)	28,167	Share without health care	0.0002	(0.0003)	29,704
Homicide per 100,000 10 months before election	-1.3220	(1.1828)	29,704	Male share of homicide victims before election	0.0319**	(0.0153)	28,167	Share with electricity, running water, and drainage	0.0001	(0.0003)	29,704
Homicide per 100,000 9 months before election	-0.6641	(0.7138)	29,704	Average age of homicide victims before election	-0.4640	(1.2085)	27,916	Share washing machine	0.0000	(0.0002)	29,704
Homicide per 100,000 8 months before election	-0.2090	(0.4180)	29,704	Share of victims with no schooling before election	0.0266	(0.0204)	27,917	Share fridge	0.0001	(0.0002)	29,704
Homicide per 100,000 7 months before election	-0.5886	(0.4431)	29,704	Share of victims with incomplete primary schooling before election	-0.0013	(0.0252)	27,917	Share cell phone	0.0000	(0.0003)	29,704
Homicide per 100,000 6 months before election	-0.3768	(0.5126)	29,704	Share of victims with complete primary schooling before election	-0.0515	(0.0406)	27,917	Share car or truck	-0.0004	(0.0003)	29,704
Homicide per 100,000 5 months before election	-0.2559	(0.3142)	29,704	Share of victims with incomplete secondary schooling before election	-0.0377	(0.0364)	27,917	Share computer	-0.0004	(0.0004)	29,704

Notes: All specifications include neighbor group and state ×year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters divided by he 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 clustered by municipality in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A9: Effect of pre-election homicide shocks on precinct-level turnout

	Change in incumbent party vote share (share of turnout)		Change in turnout		Change in incumbent vote share party (share of registered voters)	
	(1)	(2)	(3)	(4)	(5)	(6)
Homicide shock (one month)	-0.017**		0.009**		-0.003	
	(0.007)		(0.004)		(0.004)	
Pre-election homicide rate		-0.015**		0.008**		-0.003
		(0.007)		(0.003)		(0.003)
Observations	157,175	157,175	157,175	157,175	157,175	157,175
Unique municipalities	904	904	904	904	904	904
Outcome range	[-0.96,0.89]	[-0.96,0.89]	[-0.76,0.67]	[-0.76,0.67]	[-0.93,0.81]	[-0.93,0.81]
Outcome mean	-0.06	-0.06	0.00	0.00	-0.03	-0.03
Outcome std. dev.	0.14	0.14	0.11	0.11	0.09	0.09
Homicide shock mean	0.41	0.41	0.41	0.41	0.41	0.41
First stage F statistic		134.0		134.0		134.0

Notes: Odd-numbered specifications are estimate equation (9) using OLS and even-numbered specifications estimate equation (10) using 2SLS, and all observations are weighted by the number of registered voters in the electoral precinct. All observations are weighted by the number of registered voters. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A10: The effect 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***	0.051*	0.051*	0.048*	0.051**	0.051**	0.052**	0.037**	0.037**
	(0.017)	(0.017)	(0.016)	(0.016)	(0.030)	(0.029)	(0.025)	(0.025)	(0.025)	(0.026)	(0.015)	(0.015)
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 range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
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	0.19	0.19	0.28	0.30	0.30	0.30	0.42	0.42
Panel B: Outcome: Watch and listen to news and political programs weekly												
Upcoming local election	0.124***	0.124***	0.125***	0.125***	0.081*	0.081*	0.067*	0.064*	0.064*	0.069*	0.075***	0.075***
	(0.026)	(0.027)	(0.023)	(0.022)	(0.045)	(0.045)	(0.038)	(0.035)	(0.035)	(0.036)	(0.025)	(0.025)
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 range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
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	0.19	0.19	0.28	0.30	0.30	0.30	0.42	0.42
Panel C: Outcome: Watch and listen to news and political programs scale												
Upcoming local election	0.438***	0.438***	0.433***	0.433***	0.280*	0.280*	0.256**	0.250**	0.250**	0.261**	0.261***	0.261***
	(0.088)	(0.088)	(0.077)	(0.079)	(0.155)	(0.153)	(0.127)	(0.121)	(0.123)	(0.125)	(0.079)	(0.080)
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 range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	2.55	2.55	2.55	2.55	2.55	2.55	2.55	2.55	2.55	2.55	2.55	2.55
Outcome standard deviation	1.48	1.48	1.48	1.48	1.48	1.48	1.48	1.48	1.48	1.48	1.48	1.48
Upcoming local election mean	0.02	0.02	0.07	0.07	0.19	0.19	0.28	0.30	0.30	0.30	0.42	0.42
Panel D: Outcome: Topical political knowledge												
Upcoming local election	0.482***	0.329*	0.510**	0.510**	0.330***	0.330***	0.212*	0.192*	0.172	0.153	0.118*	0.118*
	(0.039)	(0.195)	(0.254)	(0.255)	(0.125)	(0.126)	(0.113)	(0.109)	(0.106)	(0.104)	(0.070)	(0.071)
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 range	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]	[-2.11,1.56]
Outcome mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	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	0.24	0.25	0.27	0.36	0.36
Unique states	31	31	31	31	31	31	31	31	31	31	31	31

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

Table A11: The effect of upcoming local elections on political news consumption and topical political knowledge, excluding each ENCUP survey wave

	Watch and listen to news and political programs ever			Watch and listen to news and political programs weekly			Watch and listen to news and political programs scale			Topical political knowledge		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Excluding the 2001 ENCUP survey												
Upcoming local election	0.077***			0.125***			0.433***			0.596***		
	(0.016)			(0.022)			(0.079)			(0.067)		
Upcoming local election (two-month)		0.096***			0.124***			0.438***			0.482***	
		(0.017)			(0.026)			(0.087)			(0.040)	
Months until next election			-0.002**			-0.004***			-0.015***			-0.006*
			(0.001)			(0.001)			(0.004)			(0.004)
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	0.62	0.62	0.62	2.55	2.55	2.55	-0.01	-0.01	-0.01
Outcome standard deviation	0.34	0.34	0.34	0.48	0.48	0.48	1.48	1.48	1.48	1.08	1.08	1.08
Independent variable mean	0.07	0.02	17.66	0.07	0.02	17.66	0.07	0.02	17.66	0.07	0.02	17.66
Panel B: Excluding the 2003 ENCUP survey												
Upcoming local election	0.068***			0.125***			0.430***			0.521*		
	(0.019)			(0.027)			(0.100)			(0.271)		
Months until next election			-0.001			-0.004**			-0.012**			-0.003
			(0.001)			(0.002)			(0.005)			(0.004)
Observations	7,620		7,620	7,620		7,620	7,620		7,620	11,613	11,613	11,613
Outcome mean	0.88		0.88	0.63		0.63	2.57		2.57	-0.00	-0.00	-0.00
Outcome standard deviation	0.33		0.33	0.48		0.48	1.42		1.42	1.01	1.01	1.01
Independent variable mean	0.07		20.06	0.07		20.06	0.07		20.06	0.06	0.01	20.78
Panel C: Excluding the 2005 ENCUP survey												
Upcoming local election	0.096***			0.124***			0.438***			0.329*		
	(0.017)			(0.026)			(0.087)			(0.190)		
Upcoming local election (two-month)		0.096***			0.124***			0.438***			0.329*	
		(0.018)			(0.026)			(0.086)			(0.193)	
Months until next election			-0.001			-0.005***			-0.015***			-0.004**
			(0.001)			(0.002)			(0.005)			(0.002)
Observations	7,763	7,763	7,763	7,763	7,763	7,763	7,763	7,763	7,763	11,756	11,756	11,756
Outcome mean	0.86	0.86	0.86	0.62	0.62	0.62	2.56	2.56	2.56	0.01	0.01	0.01
Outcome standard deviation	0.35	0.35	0.35	0.49	0.49	0.49	1.50	1.50	1.50	0.92	0.92	0.92
Independent variable mean	0.03	0.03	18.54	0.03	0.03	18.54	0.03	0.03	18.54	0.03	0.03	19.77
Panel D: Excluding the 2012 ENCUP survey												
Upcoming local election	0.077***			0.125***			0.433***			0.510**		
	(0.016)			(0.022)			(0.077)			(0.254)		
Upcoming local election (two-month)		0.096***			0.124***			0.438***			0.329*	
		(0.017)			(0.027)			(0.088)			(0.190)	
Months until next election			-0.003**			-0.005***			-0.017***			-0.007
			(0.001)			(0.002)			(0.005)			(0.006)
Observations	8,583	8,583	8,583	8,583	8,583	8,583	8,583	8,583	8,583	12,576	12,576	12,576
Outcome mean	0.85	0.85	0.85	0.62	0.62	0.62	2.53	2.53	2.53	0.00	0.00	0.00
Outcome standard deviation	0.35	0.35	0.35	0.49	0.49	0.49	1.52	1.52	1.52	0.98	0.98	0.98
Local election mean	0.09	0.03	14.75	0.09	0.03	14.75	0.09	0.03	14.75	0.07	0.03	17.10
Unique states	31	31	31	31	31	31	31	31	31	31	31	31

Notes: All specifications include survey-year fixed effects, and are estimated using OLS. The outcomes in columns (1)-(9) were not collected in the 2001 survey. Some coefficients in panel B could not be estimated because no election occurred within 2 months of a local election in the 2005 and 2008 surveys. Bootstrapped standard errors clustered by state (10,000 replications) are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

party's vote share.

In addition to the robustness checks reported in the main paper, Table A13 demonstrates that the results are also not driven by the operationalization of the electoral outcome. First, columns (1) and (2) shows that the reduced form and IV results are robust to removing elections that occurred in the second half of the month. As noted in the main text, these elections are more challenging to classify with respect to homicides occurring before the election because they are more scattered within the month. Second, columns (3) and (4) restrict attention to PAN, PRD, and PRI incumbents, demonstrating that the results are not driven by the few mayors from smaller parties. Third, and consistent with a clearer assignment of responsibility, columns (5) and (6) report slightly larger effects, especially on vote share, when restricting attention to single-party incumbents.

A.3.4 Electoral effects of drug-related homicides

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

The results in Table A14 report the effect of drug-related homicide shocks just before an election, defined according to equation (8) but instead using drug-related homicides. Given that only five years of data exist, and thus many municipalities only appear once, I use state fixed effects instead of municipality fixed effects. Although the large drop in sample size unsurprisingly reduces precision substantially, the point estimates are similar to those reported in Table 2. These findings reinforce the claim in the main text that voters respond similarly to different types of homicide.

A.3.5 Panel fixed effects approach to identifying the effects of pre-election homicides

The results in the main text focus on pre-election homicide shocks coded as a binary variable. This approach is theoretically appealing because its short-term nature both captures difference relative to the municipality's general baseline as well as closely maps to cycles in political information

Table A12: Effects of pre-election homicide shocks on municipal incumbent vote share

	Baseline spec.	No fixed effects	Additional control variables	Incumbent party trends	Municipality -specific trends	Election month effects	Placebo 6 months earlier	Violence bin effects	Fewer than 25 homicides per month	Few drug homicides	Non-DTO states	One-month shock	Three-month shock	Four-month shock
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Reduced form estimates														
Homicide shock	-0.015**	-0.013*	-0.011*	-0.012*	-0.013	-0.015**		-0.015**	-0.014**	-0.015**	-0.012			
	(0.006)	(0.008)	(0.006)	(0.006)	(0.008)	(0.006)		(0.006)	(0.007)	(0.006)	(0.008)			
Placebo homicide shock							0.002							
							(0.008)							
Homicide shock (one month)												-0.016**		
												(0.007)		
Homicide shock (three month)													-0.018***	
													(0.005)	
Homicide shock (four month)														-0.020***
														(0.005)
Panel B: Instrumental variables estimates														
Pre-election homicide rate	-0.014**	-0.012	-0.011*	-0.012*	-0.014	-0.014**		-0.015**	-0.014**	-0.014**	-0.014	-0.022**	-0.032***	-0.041***
	(0.006)	(0.007)	(0.006)	(0.006)	(0.009)	(0.006)		(0.006)	(0.007)	(0.006)	(0.010)	(0.010)	(0.010)	(0.013)
Pre-election homicide rate mean	1.6	1.6	1.6	1.6	1.6	1.6		1.6	1.3	1.6	1.0	1.7	1.5	1.4
Pre-election homicide rate std. dev.	3.1	3.1	3.1	3.2	3.1	3.1		3.1	2.5	3.1	1.3	3.2	3.0	3.0
First stage coefficient	1.033***	1.073***	0.989***	1.029***	0.812***	1.033***		0.965***	1.046***	1.033***	0.751***	0.731***	0.609***	0.502***
	(0.085)	(0.111)	(0.076)	(0.090)	(0.088)	(0.085)		(0.064)	(0.086)	(0.085)	(0.061)	(0.081)	(0.075)	(0.077)
First stage F statistic	144.4	92.5	163.2	128.6	84.8	144.4		230.8	186.1	144.4	160.3	81.3	58.4	43.6
Observations	166,355	166,355	162,061	156,907	166,355	166,355	144,993	166,355	156,794	166,355	101,365	141,453	180,031	188,049
Unique municipalities	905	905	895	822	905	905	819	905	904	905	615	619	1,062	1,170
Outcome range	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]	[0,1]
Outcome mean	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.40	0.43	0.42	0.41
Outcome std. dev.	0.15	0.15	0.14	0.14	0.15	0.15	0.14	0.15	0.15	0.15	0.14	0.14	0.15	0.15
Homicide shock mean	0.41	0.41	0.40	0.41	0.43	0.41	0.40	0.41	0.41	0.41	0.40	0.44	0.43	0.42

Notes: All specifications in panel A are estimated using OLS and all specifications in panel B are estimated using 2SLS, and all observations are weighted by the number of registered voters in the electoral precinct. Standard errors clustered by municipality are in parentheses. Column (1) is the baseline specifications in equation (9) and (10), which include municipality and state × year fixed effects and (in the case of panel B) the prior monthly average homicide rate per 100,000 people. Column (2) excludes municipality and state × year fixed effects. Columns (3) includes the controls in 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 incumbent party-specific incumbent year trends. Column (5) includes municipality-specific year trends. Column (6) includes election month fixed effects. Column (7) uses a placebo homicide shock defined six months before the election. Column (8) includes fixed effects for the total number of homicides over the four-month window in bins of size ten. Column (9) excludes municipalities averaging more than 25 homicides per month over the two months either before or after the election. Column (10) excludes municipalities that average more than one drug-related homicide a month over the 12 months before an election between 2006 and 2011. Column (11) excludes states with high-level of DTO activity (see footnote 34). Columns (12)-(14) respectively define homicide shocks over one-, three-, and four-month windows. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A13: Robustness of definition of electoral outcomes

	Elections from first half of month		PAN, PRD, or PRI incumbent		Single-party incumbent	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome: Incumbent party win						
Homicide shock	-0.097***		-0.095***		-0.088**	
	(0.034)		(0.033)		(0.041)	
Pre-election homicide rate		-0.087***		-0.091***		-0.097**
		(0.031)		(0.032)		(0.047)
Observations	2,620	2,620	2,533	2,533	1,745	1,745
Unique municipalities	854	854	822	822	638	638
Outcome range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}	{0,1}
Outcome mean	0.55	0.55	0.55	0.55	0.55	0.55
Outcome std. dev.	0.50	0.50	0.50	0.50	0.50	0.50
Homicide shock mean	0.41	0.41	0.41	0.41	0.38	0.38
First stage F statistic		134.4		122.0		103.0
Panel B: Outcome: Change in incumbent party vote share						
Homicide shock	-0.017**		-0.015**		-0.023**	
	(0.007)		(0.007)		(0.009)	
Pre-election homicide rate		-0.015**		-0.014**		-0.026**
		(0.006)		(0.007)		(0.011)
Observations	152,047	152,047	156,907	156,907	103,745	103,745
Unique municipalities	854	854	822	822	638	638
Outcome range	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]	[-0.96,0.89]
Outcome mean	-0.05	-0.05	-0.05	-0.05	-0.04	-0.04
Outcome std. dev.	0.14	0.14	0.14	0.14	0.15	0.15
Homicide shock mean	0.41	0.41	0.41	0.41	0.38	0.38
First stage F statistic		139.4		127.5		110.1

Notes: Odd-numbered specifications estimate equation (9) using OLS and even-numbered specifications estimate equation (10) using 2SLS, and all observations are weighted by the number of registered voters in the electoral precinct. Columns (1) and (2) include only elections that take place on or before the 15th of the month. Columns (3) and (4) include only elections where the PAN, PRD, or PRI are an incumbent. Columns (5) and (6) include only observations where the incumbent is not a coalition. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A14: Effect of pre-election drug-related homicide shocks on municipal incumbent electoral outcomes

	Incumbent party win	Change in incumbent party vote share
	(1)	(2)
Drug-related homicide shock	-0.065	-0.019
	(0.080)	(0.018)
Observations	380	36,149
Unique municipalities	330	334
Outcome range	{0,1}	[-0.84,0.87]
Outcome mean	0.51	-0.05
Homicide shock mean	0.46	0.44

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

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. However, I also now consider an alternative approach to capturing the effects of short-run shocks around local elections that does not rely on homicide data after the election.

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.[50] I thus use a panel fixed effects design to identify how *deviations* in the homicide rate just before elections, relative 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-

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

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. The identifying assumption is that municipalities experiencing different homicide rate deviations from election cycle trends before elections otherwise follow parallel trends in incumbent support.

Table A15 reports estimates from the analog of equation (11), and provides evidence suggesting that this alternative approach to capturing pre-election homicide rate deviations also decreases the incumbent party's vote share. Although the estimates are often borderline statistically significant, columns (1) and (2) first show that an increase in the raw pre-election deviation decreases the probability of the incumbent winning and the incumbent's vote share. This estimates are more precisely estimated when separating above and below median deviations in columns (3) and (4). Columns (5)-(8) report similar results when considering the proportional deviation instead.

A.3.6 Differential effects of homicide shocks by presence of a municipal police force

Table A16 compares the effects of pre-election homicide shocks across municipalities with and without their own police. The latter group, which includes delegations within Mexico City, were excluded from the main analysis. Although the analysis is not sufficiently powered to detect a differential effect, the difference in the effect is large in magnitude and the tests at the foot of the table indicate that pre-election homicide shocks do 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.3.7 Effects of pre-election homicides on higher levels of government

Table A17 examines the extent to which pre-election homicides influence the selection of higher-level politicians. As noted in the main text, columns (1)-(3) show that the parties of higher-level incumbents are not punished at the municipal level, while columns (4) and (5) further demonstrate that pre-election homicide rates also do not affect the performance of higher-level incumbent parties in their own elections. These findings suggest that voters primarily hold municipal incumbents to account for municipal homicide rates.

Although there is no evidence to suggest that state and federal incumbent parties are held responsible for homicide shocks by voters, the punishment of the municipal incumbent party could also spill up to state or national level elections. However, when municipal and higher-level elections are held concurrently, columns (6) and (7) of Table A17 show that the state and federal deputy vote shares of the municipal incumbent's party are not significantly affected by a homicide shock.

Table A15: Panel fixed effects approach to estimating the effect of pre-election deviations in homicide rates on municipal incumbent electoral outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Outcome: Incumbent party win								
Deviation from average monthly homicide rate	-0.0069	-0.0087						
	(0.0067)	(0.0164)						
Above median deviation from average monthly homicide rate			-0.0683*	-0.0601				
			(0.0385)	(0.0566)				
Proportional deviation from average monthly homicide rate					-0.0069	-0.0093		
					(0.0056)	(0.0122)		
Above median proportional deviation from average monthly homicide rate							-0.0675*	-0.0534
							(0.0392)	(0.0554)
Observations	2,838	2,838	2,838	2,838	2,748	2,748	2,748	2,748
Unique municipalities	905	905	905	905	866	866	866	866
Outcome mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Homicide rates measure mean	0.35	0.35	0.49	0.49	0.42	0.42	0.50	0.50
Homicide rate measure std. dev.	2.23	2.23	0.50	0.50	1.94	1.94	0.50	0.50
Panel B: Outcome: Change in incumbent party vote share								
Deviation from average monthly homicide rate	0.0015	0.0032						
	(0.0010)	(0.0025)						
Above median deviation from average monthly homicide rate			-0.0280***	-0.0325***				
			(0.0078)	(0.0122)				
Proportional deviation from average monthly homicide rate					0.0006	0.0014		
					(0.0013)	(0.0024)		
Above median proportional deviation from average monthly homicide rate							-0.0270***	-0.0314***
							(0.0077)	(0.0119)
Observations	166,355	166,355	166,355	166,355	165,863	165,863	165,863	165,863
Unique municipalities	905	905	905	905	901	901	901	901
Outcome mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Homicide rates measure mean	0.36	0.36	0.49	0.49	0.43	0.43	0.50	0.50
Homicide rate measure std. dev.	2.24	2.24	0.50	0.50	1.98	1.98	0.50	0.50
Municipality-specific time trends		✓		✓		✓		✓

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

Table A16: Electoral effects of homicide shocks, by presence of municipal police force

	Incumbent party win (1)	Change in incumbent party vote share (2)
Homicide shock	-0.103***	-0.017***
	(0.031)	(0.006)
No municipal police force	0.038	0.007
	(0.080)	(0.014)
Homicide shock	0.075	0.027
× No municipal police force	(0.083)	(0.021)
Observations	3,334	184,488
Unique municipalities	1,007	1,007
Outcome mean	0.56	-0.05
Homicide shock mean	0.40	0.40
No municipal police force mean	0.09	0.09
× No municipal police force	(0.077)	(0.020)

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

A.3.8 Differential effects of homicide shocks across parties

I also examine differential punishment across parties. Due to the PRI's more extensive clientelistic ties (e.g. Cornelius 1996; Magaloni 2006), PRI voters may be less susceptible to performance information, and thus less inclined to punish PRI incumbents for homicide shocks. Table A18 provides evidence consistent with this expectation. Column (1) shows that the PAN and PRD are punished significantly more than the PRI, for whom no reduction in vote share is registered. Columns (2)-(4) similarly register significant punishment of PAN and PRD mayors. This is potentially because the PAN and PRD control more urban areas where the effects of homicide shocks are larger.

Finally, who do voters turn to in order to reduce local violence? If reducing violence is a major concern, and voters believe that Calderón's tough stance on drug-related crime may help their municipality (see Dell 2015), even PAN mayors may benefit from a homicide shock. However, column (5) finds little evidence for this.

Table A17: Electoral effects of pre-election homicides , by level of government

	Change in municipal vote share of...			Change in state deputy incumbent vote share	Change in federal incumbent vote share	Change in state vote of municipal incumbent	Change in federal vote of municipal incumbent
	...state district party	...state Governor's party	...President's party				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Reduced form estimates							
Homicide shock	-0.002	0.022***	-0.012	-0.002	-0.002	0.002	-0.014
	(0.006)	(0.008)	(0.008)	(0.008)	(0.013)	(0.013)	(0.012)
Panel B: Instrumental variable estimates							
Pre-election homicide rate	-0.002	0.019***	-0.012	-0.002	-0.004	0.004	-0.018
	(0.005)	(0.007)	(0.008)	(0.007)	(0.013)	(0.012)	(0.016)
Pre-election homicide rate mean	1.6	1.7	1.6	1.6	1.3	1.6	1.3
Pre-election homicide rate std. dev.	3.3	3.4	3.1	3.3	2.6	3.3	2.4
First stage F statistic	114.7	114.9	144.4	115.4	20.2	104.7	37.2
Observations	143,049	135,786	166,880	134,899	52,449	127,855	62,200
Unique municipalities	888	845	905	891	390	869	413
Outcome range	[-0.97,0.98]	[-0.94,0.89]	[-0.97,0.98]	[-0.96,0.91]	[-0.75,0.66]	[-0.98,1]	[-0.76,0.63]
Outcome mean	0.00	0.03	0.03	-0.04	-0.05	-0.02	-0.05
Outcome std. dev.	0.13	0.17	0.24	0.14	0.13	0.19	0.12
Homicide shock mean	0.42	0.41	0.41	0.40	0.41	0.41	0.41

Notes: All specifications include municipality and state $\times$ year fixed effects, weight by the number of registered voters, and are estimated using OLS. The IV estimates in panel B additionally control for the average monthly homicide rate over the 10 months preceding the election window. Standard errors clustered by municipality are in parentheses. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table A18: Effects of pre-election homicide shocks on municipal incumbent electoral outcomes, by incumbent party

	Change in incumbent party vote share (1)	Change in PAN incumbent vote share (2)	Change in PRD incumbent vote share (3)	Change in PRI incumbent vote share (4)	Change in PAN incumbent vote share (5)
Homicide shock	0.007	-0.021*	-0.056***	-0.000	0.005
	(0.010)	(0.011)	(0.018)	(0.009)	(0.011)
Homicide shock × PAN	-0.039**				
	(0.016)				
Homicide shock × PRD	-0.056**				
	(0.027)				
Homicide shock × Calderón Presidency					-0.015
					(0.017)
Observations	158,718	56,524	19,770	82,424	166,880
Unique municipalities	895	431	287	765	905
Outcome range	[-0.96,0.89]	[-0.85,0.87]	[-0.96,0.89]	[-0.94,0.88]	[-0.96,0.89]
Outcome mean	-0.05	-0.06	-0.09	-0.04	-0.05
Outcome standard deviation	0.14	0.14	0.17	0.13	0.14
Homicide shock mean	0.41	0.40	0.42	0.41	0.41

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

A.4 Differential effects of homicide shocks by incumbent ad shares

Table A19 reports the interaction between homicide shocks and the (standardized) municipal incumbent party's ad share in the 2009 and 2012 elections. In these elections, ad slots were assigned by a formula that, via cross-state spillovers, generated variation in precinct-level exposure to ads from different parties (Larreguy, Marshall and Snyder 2018*a*). The results suggest that the incumbent party's access to pre-election ads does not the significantly moderate the effects of homicides shocks.

Table A19: Effect of homicide shocks on precinct-level change in incumbent party vote share, moderated by access to local and non-local media

	Change in incumbent party vote share		
	(1)	(2)	(3)
Homicide shock	-0.0217	-0.0207	-0.0206
	(0.0286)	(0.0282)	(0.0283)
AM incumbent party ad share	-0.0055		
	(0.0140)		
Homicide shock × AM incumbent party ad share	-0.0064		
	(0.0183)		
FM incumbent party ad share		-0.0080	
		(0.0103)	
Homicide shock × FM incumbent party ad share		-0.0039	
		(0.0151)	
TV incumbent party ad share			-0.0052
			(0.0094)
Homicide shock × TV incumbent party ad share			-0.0070
			(0.0135)
Observations	33,597	33,597	33,597
Unique municipalities	379	379	379
Outcome range	{-0.77,0.87}	{-0.77,0.87}	{-0.77,0.87}
Outcome mean	-0.07	-0.07	-0.07
Outcome standard deviation	0.13	0.13	0.13
Homicide shock mean	0.42	0.42	0.42
Incumbent party ad share media mean	-0.00	-0.00	0.00
Incumbent party ad share standard deviation	1.00	1.00	1.00

Notes: All specifications include municipality and year fixed effects, and are estimated using OLS. All observations are weighted by the number of registered voters divided by the number of comparison units within each neighbor group. The sample includes only observations from 2009 and 2012, when federal ad shares are available. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.