DON'T READ ALL ABOUT IT: DRUG TRAFFICKING ORGANIZATIONS AND MEDIA REPORTING OF VIOLENCE IN MEXICO *

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An accountable political system rests on citizens, challengers, and civil society organizations being informed about public affairs. However, media outlets—the primary suppliers of politically-relevant information—may be pressured to report news selectively. In contrast with prior studies emphasizing government efforts to influence reporting, we highlight the importance of non-state actors by estimating the prevalence and impact of drug trafficking organizations (DTOs) on news coverage. We conduct a nationwide survey of 1,153 local media outlets in Mexico and show that a quarter of newspapers are pressured to alter their reporting by DTOs. This level of pressure is comparable to that applied by local governments. We further leverage a difference-in-differences design to show that DTO presence in a municipality reduces reporting on incidents of violent crime by approximately 40%. These findings show that non-state actors can shape the information environment, which may subsequently limit accountability and facilitate state capture.

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1 Introduction

An independent media is central to political accountability. The threat of media exposure can motivate politicians and bureaucrats to address citizen demands (**??**), while news coverage can help voters select competent or ideologically-congruent politicians (**??**). Both functions rely on media outlets accurately reporting information on public affairs.

However, broadcast and print media outlets—the primary originators of politically-relevant information in most countries—may withhold or slant their reporting of salient news. Prior research has shown that outlet owners and benefactors impose their ideological preferences on the content their outlets produce (e.g. **??**). Governments can also exert influence less directly via legal restrictions (**?**), bribery (**?**), and the threat of withholding licenses or advertising revenues (**?**). In contrast, far less is known about how non-state actors affect public information flows (**?**).

In this research note, we illuminate the role of organized crime in influencing local media reporting in Mexico by establishing both the prevalence of pressure exerted by drug trafficking organizations (DTOs) and its effects on whether violent crime is reported. While Mexico—where DTOs are believed to have assassinated over 100 journalists and over 100 elected politicians in the last decade—may initially appear to be a somewhat unique case, non-state actors' influence on media exists more broadly across weakly institutionalized states. Groups ranging from businesses in Kenya and Zimbabwe (**?**) to separatist and religious extremist organizations in Colombia, Iraq, Ukraine, and Yemen have regularly sought to affect information flows in other weak states—often invoking similar economic or physical threats.

We find that DTO pressure substantially influences local news reporting. First, we leverage a nationwide survey of local newspapers and radio stations in 2018 to estimate the prevalence of DTO pressure on journalists. Estimates from direct questioning and a list experiment reveal that around a quarter of newspapers had been pressured by DTOs within the last year. Although pressure by local governments is more common, news editors report feeling greater pressure to address requests from organized criminals. Second, we estimate the effect of DTO pressure on media reporting of violent crime using a generalized difference-in-differences design that leverages the geographic expansion of DTOs across Mexican municipalities between 2000 and 2010. Our results indicate that DTO presence reduced reporting on homicides in a given municipality by around 40%.

These findings illuminate when politically-salient information becomes public in several ways. First, and most generally, we provide systematic evidence that news coverage is endogenous to the presence of non-state actors in a weakly institutionalized context. Even where citizens are aware of media manipulation, such manipulation can still alter policy and accountability dynamics (**?**). Second, we extend the literature focusing on government influence over media outlets (see **?**) by showing that non-state actors can also exert substantial influence, and thereby highlight the importance of integrating non-state actors into models of media bias—as well as the electoral arena (**?**). Third, our analyses advance the literature examining the political role of DTOs in Mexico. Extant studies establish the existence of DTO pressure (**???**) and highlight when DTOs resort to violence against journalists (**??**) and politicians (**?**). We advance this literature by documenting the prevalence and geographic distribution of direct pressure across Mexico and—most distinctively—by showing that DTO presence ultimately influences news reporting.

2 Incentives and capacity to alter news production in Mexico

Following decades of hegemonic control of public media outlets by the Institutional Revolutionary Party (PRI), Mexican media outlets became increasingly independent and competitive in the late 1990s (**??**). Nevertheless, there remained both opportunities and strong incentives for state and non-state actors to control information flows.

Increasingly competitive elections since the 2000s magnified electoral incentives to influence media content, especially given the substantial effects of political ads and news reports on voting behavior (**????**). This was reflected in federal and state governments using their vast advertising budgets to condition the financial survival of local broadcasters on supporting preferred candidates (**?**). Moreover, Mexican journalists' low salaries—estimated to be only 60% of the national average wage in 2018[1]—made them particularly susceptible to influence (**?**). Reporters Without Borders ranked media freedom in Mexico 147th of 179 countries in 2018.

Non-state actors also possess incentives to control news content, especially locally, in order to project power. Drug trafficking from and through Mexico to the United States expanded rapidly in the 1990s, while conflict between Mexican DTOs and with the federal government exploded after President Calderón initiated his "War on Drugs" in December 2006. These trends engendered increasingly wealthy and violent DTOs with economic and security incentives to use local politicians and media outlets to protect and expand their operations. By pressuring local media, DTOs can avoid publicity for their activities and strategic liabilities, but also convey messages to the public and rival organizations (**?**). Combined with limited sales revenues, low wages, and local impunity for DTOs in many municipalities, these incentives suggest that DTOs may seek to substantially influence news coverage.

In line with this expectation, a growing body of research suggests that DTOs have bribed and threatened media outlets, induced self-censorship, and restricted reporter access to crime scenes. Recent studies based on ethnography, interviews, or content analyses of several media outlets suggest that DTO members can silence reporters or shape their reporting on criminal activity by offering bribes or threatening force (**????**). DTOs further demand positive coverage of their *narcomensajes* (narco-messages), *narcocorridos* (narco-ballads), and other spectacles (**?**) or negative coverage of rivals (**??**). Some journalists also report preemptively self-censoring their content (**??**). As **?**'s (**?**) case study of the newspaper *El Bravo* illustrates, "structural violence" against the media has created a gap between the incidence and reporting of homicides in Tamaulipas and also reduced the factual content of reporting by *El Bravo* relative to reports on the same events by *The Brownsville Herald* across the Texan border.

While the existence of DTO intervention is widely acknowledged, the *extent* and especially the *effect* of criminal pressure on local media outlets remains uncertain.[2] Our contribution is to

[1] See mom-rsf.org/en/countries/mexico.

[2] **?** and **?** also analyze a survey of more than 100 media outlets. This smaller-scale survey does not use list experiments to mitigate potential social desirability biases or estimate the consequences of DTO pressure.

systematically investigate these pressing challenges to the informational foundations of Mexican democracy.

3 Prevalence of DTO pressure on Mexican local media outlets

We first establish the prevalence of pressure on local media outlets from DTOs across Mexico. Between September and November 2018, our enumerators sought to survey the head of news at 456 newspapers with regional or city-specific circulation and 1,148 AM and FM radio stations across Mexico.[3] Our 15-minute telephone survey was completed by 311 newspapers and 855 radio stations that ever report news, yielding a 73% response rate. The summary statistics in Appendix Table **??** show that 94% reported news at least once a day, while 97% and 96% reported on politics and security, respectively, at the municipal or state level. Most outlets have operated for at least a decade. While many of these local outlets use international, national, and local news sources to aid their news production, 95% reported sourcing stories internally too.

We measure DTO pressure using direct and indirect survey items, benchmarking each against pressure from local (municipal or state) government. First, we directly asked respondents on a five-point scale "How frequently do people related to [local organized crime/municipal or state government] request that someone in your organization alter the way that they report a news story or not report that story at all?" Second, to address the possibility that respondents may be unwilling to truthfully report pressure out of fear or embarrassment, we later used a list experiment to indirectly estimate the share of journalists for whom "a person connected to [local organized crime/municipal or state government] requested that someone in your organization change how you would report a news story or not report that story at all" within the last year. To avoid the ceiling and floor effects that can confound list experiments, each version of this sensitive item was randomly included alongside two relatively common experiences and one less common experience that was likely to correlate negatively with one common experience.[4]

[3] Appendix section **??** describes how this list was generated.

[4] Appendix section **??** reports the exact wording, while section **??** shows that experimental conditions are balanced across predetermined covariates and that we find no evidence of design effects.

../Data/Output/Figures/criminalinterference

Figure 1: Self-reported frequency of criminal interference

../Data/Output/Figures/stateinterference.jp

Figure 2: Self-reported frequency of state interference

Our descriptive results indicate that pressure from DTOs afflicts around a quarter of local newspapers and radio stations. Figure **??** reports evidence from the direct questions, indicating that 34% of newspapers and 23% of radio stations were contacted by DTOs to alter or withhold content at least once a year. Although contact is mostly infrequent, a single visit could convey a blanket ban or induce self-censorship. The list experiment paints a similar picture: our regression estimates in Table **??** indicate that 27% of newspapers had experienced pressure to alter their reporting within the last year. In contrast, our estimate for radio stations is only 4%, and is statistically indistinguishable from zero. The consistently lower rates of pressure experienced by radio stations likely reflect their more limited news coverage and their newsrooms borrowing heavily from better-resourced news sources.

Pressure exerted by local state actors is more prevalent than pressure exerted by DTOs. Figure **??** shows that 36% of newspapers and 30% of radio stations report having been pressured by local governments to alter their reporting at least once within the last year, while the list experiment results in Table **??** indicate that 46% of newspapers and 17% of radio stations were pressured by a local government. To the extent that local governments pressure journalists on behalf of DTOs, the statistically significant difference between government and DTO pressure should be considered an upper bound. Furthermore, Figures **??** and **??** indicate that journalists report being more careful when reporting on DTOs: whereas 60% of outlets indicated that they reported very carefully to avoid antagonizing the government, 81% of surveyed outlets acknowledged reporting very care-

../Data/Output/Figures/crimecare.jpg ../Data/Output/Figures/stat

(a) Local organized crime

(b) State and municipal government officials

Figure 3: Care taken when reporting on organized crime and local government

Table 1: List experiment estimates of organized crime and local government pressure on newspapers and radio stations

	Outcome: Number of experiences		
	All outlets	Newspapers	Radios
	(1)	(2)	(3)
Local organized crime treatment	0.103**	0.268**	0.044
	(0.050)	(0.107)	(0.058)
Local government treatment	0.226***	0.462***	0.166***
	(0.055)	(0.115)	(0.063)
Difference: Local government treatment	0.123**	0.194*	0.122*
- Local organized crime treatment	(0.055)	(0.105)	(0.066)
Observations	1,153	308	845
Outcome mean	2.22	2.25	2.21
Outcome standard deviation	0.77	0.78	0.77

Notes: All specifications include state fixed effects to increase precision and are estimated using OLS. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

fully to avoid antagonizing organized criminals. These findings suggest that DTO pressure may be more effective than the average local government's pressure, which aligns with **?**'s (**?**) finding that journalists surveyed between 2013 and 2015 perceived that they possessed less autonomy when reporting on criminal organizations relative to political actors.

Pressure on the media is concentrated along Mexico's drug trafficking routes. Although the list experiment estimates by geographic clusters of states are noisy, Figure **??** shows that almost half of the newspapers that we surveyed reported recently being pressured by DTOs in northwestern states, where conflict between DTOs over valuable entry points to the US has been particularly great.[5] In the rest of the country, around a quarter of newspapers were pressured. Interestingly, Figure **??** indicates that local government pressure is also greatest in the parts of the country where DTO presence is greatest. Near the US border, around two thirds of newspapers have been pressured by local governments. While our aim is not to establish how these forms of pressure relate to or affect electoral politics, our findings suggest that DTO presence may hinder accountability by limiting news reporting.

[5] Appendix Table **??** reports similar results by electoral region.

(a) Organized crime (b) Local government

Figure 4: List experimental estimates of interference by geographic region

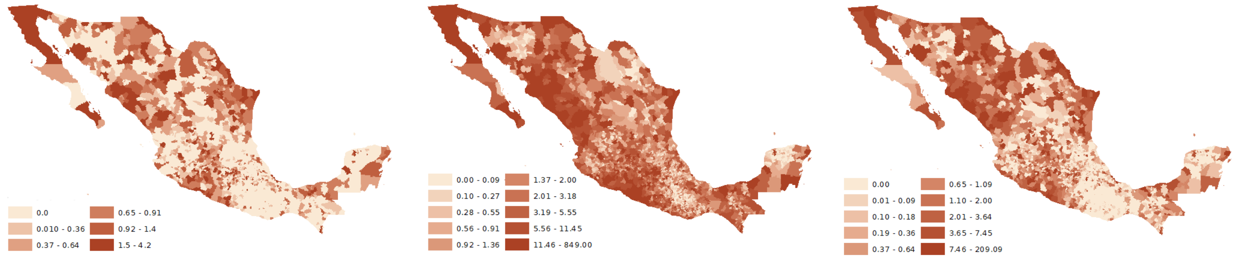
Notes: All specifications include state fixed effects and are estimated using OLS by regional subsample. Regions represent common geographical groupings of states, as there are no administrative regions above states. States borders are in gray. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4 Effects of DTO presence on media reporting on violence

We next examine whether DTOs affect media outlets' reporting of violent crime. Such crime ranks among the most sensitive activities that DTOs frequently engage in, is locally newsworthy (**?**), and can significantly affect electoral behavior (**?**). While the manner of reporting may also change, we establish whether DTOs limit overall reporting on violent crime. Accordingly, we leverage a generalized difference-in-differences design to estimate the effect of DTO presence in a municipality—usually a precondition for exerting pressure, whether directly or indirectly—on the degree to which violent crime in that municipality is reported between 2000 and 2010.[6]

To first approximate the extent to which violent crime is reported, we examine the correlation between the *incidence* and *reporting* of crime. We measure incidence by aggregating homicides—as defined by coroner reports made public by Mexico's independent National Institute of Statistics and Geography—by the date and municipality in which they occurred. To approximate news reporting of such incidents, we use data collected by **?**. He drew on articles from 105 news sources, mostly comprising government websites and the larger local newspapers that are well-represented in our survey sample (and likely aggregate information from smaller newspapers). **?** used man-

[6]We only study up to 2010 due to data availability constraints, although this was the period of greatest growth in DTO presence—our source of causal identification.



(a) Average number of DTOs present

(b) Average number of homicides

(c) Average number of incidents of inter-DTO violence reported

Figure 5: Average DTO presence, homicides, and between-DTO violence reported per year by municipality, 2000-2010

Note: The aggregated data in Figures **??**, **??**, and **??** are, respectively, derived from **?**, INEGI, and **?**.

ual and automated methods to identify reporting of violent events that occurred between DTOs and the location and date of each incident; see Appendix section **??** for additional details. Although the resulting count of incidents does not exclusively include homicides, they are among the most prevalent and newsworthy crimes that occurred in Mexico over the 2000-2010 period which **?**'s (**?**) data cover. To match our indicator of DTO presence, we aggregate each measure to the municipality-year level.

The incidence and reporting of violent crime are, unsurprisingly, highly correlated. We estimate this correlation using the following linear regression:

$$Reports_{mt} = \alpha + \beta Homicides_{mt} + \varepsilon_{mt}, \tag{1}$$

where $Reports_{mt}$ and $Homicides_{mt}$ respectively capture the count of reports relating to inter-DTO violence and the count of homicides in municipality m in year t. Column (1) of Table **??** shows that, on average, 0.25 more reports of between-DTO violence are registered for every homicide that occurs. Given the large number of homicides, the common occurrence of multiple-homicide events that are likely to be reported together, and the fact that **?** does not include content from all newspapers, we caution readers against reading too much into the coefficient magnitude itself. Rather, our goal is to explain variation in the strength of this correlation.

Table 2: Relationship between incidence of homicides and reporting of inter-DTO violent crime

	Outcome: Reported inter-DTO violence	
	(1)	(2)
Homicides	0.254***	
	(0.028)	
DTO		2.230***
		(0.352)
Homicides × DTO		-0.095**
		(0.042)
Observations	27,015	27,015
Outcome mean	1.71	1.71
Outcome standard deviation	13.85	13.85
Homicides mean	5.05	5.05
Homicides standard deviation	35.30	35.30
DTO mean		0.14
Municipality fixed effects		✓
Year fixed effects		✓
Municipality-specific linear homicide terms		✓
Year-specific linear homicide terms		✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. The lower-order homicides term in column (2) is subsumed by the linear homicide terms. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We now turn to our primary quantity of interest: the effect of DTO presence on media reporting of incidents of violent crime. Measuring DTO presence in a municipality is challenging because DTOs may seek to remain hidden from authorities and governments lack incentives to make their information public. We mitigate these concerns by using **?**'s (**?**) DTO presence variables, which are based on Google News hits that mention Mexico's main DTOs in specific municipalities; Appendix section **??** describes this measure in detail. We code DTO presence as an indicator for the years after any DTO first enters a municipality.[7] We then estimate the effect of DTO presence on the correlation between the incidence of homicides and reporting on between-DTO violence by cross-multiplying equation (**??**) with a generalized difference-in-differences design exploiting variation

[7]While DTO presence increased between 2000 and 2010, we exclude treatment reversals to avoid equating DTO entry and exit. Appendix Table **??** reports similar results when we account for DTO presence reversion.

in DTO entry into municipalities across time.[8] This entails estimating the following regression:

$$Reports_{mt} = (\beta_m + \beta_t)Homicides_{mt} + \gamma DTO_{mt} + \delta\big(Homicides_{mt} \times DTO_{mt}\big) + \mu_m + \eta_t + \varepsilon_{mt}, \quad (2)$$

where DTO_{mt} is an indicator for at least one DTO having entered municipality m by year t. In addition to the municipal fixed effects (μ_m) and year fixed effects (η_t) that absorb time-variant differences across municipalities and common period effects, the correlation between homicides and reporting is also allowed to vary by both municipality and year. The inclusion of these flexible terms adjusts for municipality-specific propensities to report on inter-DTO violence and general trends in such reporting, thereby ensuring that δ—our quantity of interest—captures the effect of DTO presence on reporting for a given homicide count. Our primary identifying assumption is that trends in the correlation between the incidence of homicides and reporting of inter-DTO violence in municipalities that a DTO enters would have been similar to the analogous trends in municipalities that a DTO did not enter.[9]

We find that DTO presence causes media outlets to almost halve their baseline propensity to report on violent crime. Specifically, the interaction coefficient in column (2) of Table **??** shows that DTO presence significantly reduces the relationship between the incidence and reporting of crime by almost 0.10 points. This represents a 40% reduction in reporting for a given number of homicides, relative to the sample average of 0.25 in column (1). The observed effect is also similar in magnitude to estimated effects of government pressure; **?** finds that incidents of government pressure are associated with a reduction in headlines that are critical of the government by comparable percentages in the state of Veracruz.

Appendix section **??** demonstrates the robustness of this finding in several important ways. First, and consistent with the parallel trends assumption, Table **??** includes leads of DTO presence to show that reporting on violent crime was not already declining in municipalities that DTOs sub-

[8]The difference-in-differences equation is: $Reports_{mt} = \gamma DTO_{mt} + \mu_m + \eta_t + \varepsilon_{mt}$.

[9]Because DTOs first enter municipalities at different points in time, our design also requires time-invariant treatment effects. Appendix Table **??** shows that treatment effect heterogeneity across cohorts does not drive our estimates.

sequently entered. Second, Table **??** similarly reports a strong negative relationship when using the natural logarithm of homicides and reports of inter-DTO violence. Third, Table **??** reports similar results using alternative automated measures of DTO presence computed by **?** and **?**; Appendix section **??** describes these news-based measures in detail.

Furthermore, the results are not driven by DTO presence increasing the number of homicides to levels that media outlets cannot or will not cover. Although Table **??** shows that DTO presence coincides with 1.4 more homicides a year, our estimates are no greater in municipalities with above-median homicide counts in 1999. Moreover, Table **??** reports similar results if the number of homicides is held constant at its 1995-1999 annual average, and thus cannot be affected by DTO entry.

5 Conclusion

We show that non-state actors can play an important role in controlling the information citizens receive via local media. First, we demonstrate that DTO efforts to pressure radio stations and especially newspapers are substantial throughout Mexico. More than a quarter of newspapers were approached by DTOs about altering their content in 2018. Second, we further demonstrated that a DTO's presence reduces coverage of local violent events. Together, these findings emphasize the importance of considering non-state actors in understanding what politically-relevant information becomes public.

This research note raises various questions for future research. Descriptively, it is important to explore how other dimensions of coverage—including tone and the types of facts that are reported—changed amid DTO presence. Research is also required to establish if and how reporting on topics less related to criminal activities is affected, and how this compares with pressures exerted by political actors, e.g. through outlet ownership ties. Furthermore, our findings challenge future studies to explore the mechanisms by which pressure on the media influences reporting. To what degree does this reflect explicit instructions from DTOs, self-censorship, or structural responses within news organizations? And under what conditions are different types of pressure more impactful?

Our findings also raise broader theoretical questions. How does the market for editorial control operate when state and non-state actors compete or collude? What are the implications for policy choices, political accountability, the selection of politicians, and organized crime's role in state activities? What policies could be used to foster journalistic independence? These questions demand attention in weakly institutionalized nations across Africa and Latin America, where criminal, military, and separatist actors often vie for power and control in electoral and non-electoral arenas.

A Online Appendix

Contents

A.1 Details about the design of the media outlet survey

The 2018 survey of local media outlets is part of a three-year experiment that studied the distinct question of how information and media market incentives drive news reporting on incumbent politician malfeasance and performance. A survey of media outlets was conducted in August-October of 2016, 2017, 2018. Only the 2018 survey, which asked in detail about pressures to report exerted by organized crime groups and local governments, is used in this article.

Our sampling frame is the set of all local newspapers and radio stations across Mexico. Local outlets were defined at the beginning of the project in 2016 by their non-national circulation, which entailed including regional versions of newspapers such as *Milenio* that produce local editions in addition to their nationwide editions. All national circulation newspapers, like *El Universal*, were excluded. Similarly, many radio stations are part of networks owned by the same group and carry the same station branding. We included the vast majority of radio stations that did not entirely retransmit content. By virtue of including all local media outlets that we could find in 2016, our sample is unusually nationally representative, and includes a variety of local public radio stations serving indigenous communities.

Each year from 2016 to 2018, enumerators sought to interview all 1,604 local newspapers and radio stations that were identified as regularly reporting news; telephone numbers and email addresses were initially collected from publicly-available government catalogs, outlet websites, and hard copies of newspapers, and were continually updated throughout the project. The head of news, or an analogous figure in the organization, in each eligible outlet was approached—multiple times if necessary—via telephone to complete the survey each year. All enumerators were trained on the survey protocols by our local team coordinator, and the quality of the surveys was monitored at least weekly by the research team.

Before starting the survey, each respondent was informed of the purpose of the survey (to study reporting on issues relating to accountability by the media in Mexico), that their responses would remain anonymous, and the survey's expected duration. Verbal consent was then obtained from the respondent before the enumerator proceeded to administer the survey. The identity of individual respondents was not recorded to protect the identities, while respondents were also informed that the identity of their media outlet would remain anonymous. Indeed, we decided to de-identify our final survey data to ensure that no state or non-state actor could target newspapers on the basis of their answers to our survey. Given that reporting on issues of corruption, violence, and political accountability can be sensitive issues in Mexico, this represents an appropriate precaution against the possible risks that respondents and media outlets could incur by revealing their vulnerability to pressure or previous reporting on sensitive issues. Otherwise, the short telephone survey posed little risk of harm for respondents. Neither survey respondents nor their organizations were remunerated for completing the survey.

The survey was completed by 73% of eligible outlets in 2018. Very few outlets refused to complete a survey after being reached, although our target contacts and their contact details changed over time and some outlets went out of business.

A.2 Survey question wording

Our two main survey items are part of a 31-question survey of media outlets. Each question appears toward the end, with the organized crime and local government direct questions being the 23rd and 25th questions of the survey respectively, and the indirect list experiment question being the 29th question.

The full wording of the direct questions is given below in Spanish and then English:

> ¿Qué tan frecuente personas relacionadas con el [crimen organizado local/gobierno municipal o estatal] le exigen a alguien de su organización, que cambien la forma en que van a informar acerca de una noticia o que no publiquen la noticia en absoluto? Más de una vez a la semana, Una vez a la semana, Una vez al mes, Una vez por año, Nunca, o No sabe?
>
> How frequently do people related to [local organized crime/municipal or state government] request that someone in your organization alter the way that they report a news

story or not report that story at all? More than once a week, Once a week, Once a month, One a year, Never, or Do not know?

Respondents were also allowed not to respond.

The full wording of the list experiment question is given below in Spanish and then English:

Ahora le voy a leer una lista de eventos que muchas [estaciones de radio/periódicos] experimentan. Una vez que lea los eventos, le voy a preguntar cuántos eventos su organización experimentó este último año. No estamos interesados en qué eventos su organización experimentó, sino el número de eventos que experimentó.

1. Informó activamente sobre la actuación del equipo mexicano de fútbol en la Copa del Mundo.
2. Informó sobre un homicidio o evento violento que ocurrió en su municipio.
3. Recibió un premio por su labor periodística.
4. **Randomized item**: [.../Una persona relacionada con el crimen organizado local le pidió a alguien de su organización que cambie la forma en que iba a informar acerca de una noticia o que no publicara la noticia en absoluto/Una persona relacionada con el gobierno municipal o estatal le pidió a alguien de su organización que cambie la forma en que iba a informar acerca de una noticia o que no publicara la noticia en absoluto].

¿Cuántos eventos su organización experimentó este último año?

I am going to read some things that many media outlets do. After I read these items out, please tell me how many your radio station/newspaper has experienced in the last year. We are not interested in which things, only how many your radio station/newspaper experienced.

1. Actively reported on the Mexican football team's performance at the World Cup.
2. Reported on a homicide or violent event that occurred in your municipality.

3. Received an award for outstanding journalism.

4. **Randomized item**: [.../A person connected to local organized crime requested that someone in your organization change how you would report a news story or to not report that story at all/A person connected to municipal or state government requested that someone in your organization change how you reported a news story or not report that story all].

How many events did your organization experience in the last year?

Respondents were also allowed not to respond.

A.3 Additional tests for estimating the prevalence of pressure on local media outlets

Several tests support the key assumptions required for our list experiment to identify the proportion of outlets that engaged in the sensitive activity. Consistent with our randomization, Tables **??** and **??** confirm that the inclusion of one of the two sensitive activities—i.e. pressure by a DTO member or local government official—in the list that a media outlet receives is generally uncorrelated with predetermined characteristics of the media outlet and its media market. We define media markets based on the primary municipalities in which local outlets circulate or cover; this largely aligns with metropolitan areas. Table **??** further supports the assumption of no design effects: in no case do we find any evidence to suggest that π_{y1}—the difference between the share of control and treated respondents that claim to have experienced at most y items—or π_{y0}—the difference between the share of treated respondents that claim to have engaged in at most y activities and the share of control respondents that claim to have engaged in at most $y-1$ activities—is negative for any value of y. Indeed, we reject the null hypothesis of no design effects using the test proposed by **?** for both variants of the list experimental treatment.

We further validate the list experiment estimates by demonstrating a correlation between the direct and indirect survey approaches. To do so, we interact the list experiment treatment conditions

Table A1: Effect of local organized crime and local government treatments on pre-treatment survey variables

Variable	Control (1)		Local organized crime treatment (2)		Local government treatment (3)		Difference		
	N	Mean	*N*	Mean	*N*	Mean	(1)-(2)	(1)-(3)	(2)-(3)
Years in operation	395	29.089 (0.943)	387	31.912 (1.005)	398	30.975 (0.931)	-2.824**	-1.886	0.937
Audience cares about corruption	400	4.650 (0.033)	380	4.611 (0.035)	383	4.601 (0.035)	0.039	0.049	0.010
Report on corruption	400	0.912 (0.014)	383	0.880 (0.017)	383	0.896 (0.016)	0.033	0.017	-0.016
Report on national/state corruption	400	0.703 (0.023)	383	0.710 (0.023)	383	0.689 (0.024)	-0.008	0.013	0.021
Report on municipal corruption	400	0.515 (0.025)	383	0.504 (0.026)	383	0.501 (0.026)	0.011	0.014	0.003
Audience cares about legislator performance	398	4.417 (0.044)	380	4.316 (0.048)	382	4.264 (0.049)	0.101	0.153**	0.051
Report on legislative performance	400	0.935 (0.012)	383	0.924 (0.014)	383	0.924 (0.014)	0.011	0.011	0.000
Report on national/state legislator performance	400	0.708 (0.023)	383	0.606 (0.025)	383	0.661 (0.024)	0.102***	0.047	-0.055
Frequency of news reporting	329	3.544 (0.053)	283	3.527 (0.059)	295	3.634 (0.052)	0.018	-0.090	-0.107
Cover national/state politics	400	0.825 (0.019)	383	0.867 (0.017)	383	0.883 (0.016)	-0.042	-0.058**	-0.016
Cover municipal/local politics	400	0.980 (0.007)	383	0.984 (0.006)	383	0.956 (0.011)	-0.004	0.024*	0.029**
Cover national/state economy	400	0.812 (0.020)	383	0.802 (0.020)	383	0.833 (0.019)	0.011	-0.020	-0.031
Cover municipal/local economy	400	0.965 (0.009)	383	0.963 (0.010)	383	0.930 (0.013)	0.002	0.035**	0.034**
Cover national/state security	400	0.830 (0.019)	383	0.838 (0.019)	383	0.820 (0.020)	-0.008	0.010	0.018
Cover municipal/local security	400	0.955 (0.010)	383	0.958 (0.010)	383	0.956 (0.011)	-0.003	-0.001	0.003
Cover international affairs	400	0.815 (0.019)	382	0.796 (0.021)	383	0.809 (0.020)	0.019	0.006	-0.014
Cover celebrity affairs	400	0.657 (0.024)	380	0.687 (0.024)	383	0.687 (0.024)	-0.029	-0.029	0.000
Cover sports	400	0.877 (0.016)	382	0.895 (0.016)	383	0.883 (0.016)	-0.018	-0.005	0.013
Wages as a major cost	391	0.494 (0.025)	373	0.491 (0.026)	376	0.489 (0.026)	0.003	0.004	0.001
Transportation as a major cost	390	0.382 (0.025)	373	0.445 (0.026)	374	0.398 (0.025)	-0.063*	-0.016	0.047
Access as major cost	389	0.252 (0.022)	371	0.315 (0.024)	374	0.278 (0.023)	-0.063**	-0.026	0.037
Threats from politicians as major cost	388	0.111 (0.016)	363	0.116 (0.017)	372	0.129 (0.017)	-0.005	-0.018	-0.013
Threats from organized crime as a major cost	388	0.085 (0.014)	362	0.086 (0.015)	372	0.094 (0.015)	-0.001	-0.009	-0.008
Costs prohibit reporting	394	1.860 (0.017)	363	1.840 (0.019)	376	1.894 (0.016)	0.020	-0.033	-0.053**
Use own sources	399	0.955 (0.010)	382	0.950 (0.011)	382	0.953 (0.011)	0.005	0.002	-0.003
Use national media as news source	400	0.647 (0.024)	383	0.634 (0.025)	381	0.643 (0.025)	0.013	0.004	-0.009
Use local media as news source	400	0.573 (0.025)	383	0.577 (0.025)	381	0.622 (0.025)	-0.005	-0.050	-0.045
Use national agencies as news source	399	0.692 (0.023)	383	0.721 (0.023)	381	0.656 (0.024)	-0.029	0.036	0.064*
Use international agencies as news source	398	0.548 (0.025)	382	0.539 (0.026)	381	0.501 (0.026)	0.008	0.046	0.038

Notes: The value displayed for t-tests are the differences in the means across the groups. Regressions are estimated using state fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Effect of local organized crime and local government treatments on pre-treatment media market characteristics

Variable	Control (1)		Local organized crime treatment (2)		Local government treatment (3)		Difference		
	N	Mean	N	Mean	N	Mean	(1)-(2)	(1)-(3)	(2)-(3)
Population (in 1000s)	523	877.326	515	990.259	523	1094.592	-112.933	-217.266*	-104.333
		(67.577)		(94.266)		(107.088)			
Share of homes with internet	523	0.223	515	0.223	523	0.218	0.000	0.005	0.005
		(0.004)		(0.004)		(0.004)			
Share of homes with a television	523	0.934	515	0.936	523	0.934	-0.002	0.000	0.002
		(0.004)		(0.003)		(0.003)			
Share of homes with car	523	0.486	515	0.474	523	0.478	0.012	0.008	-0.004
		(0.007)		(0.007)		(0.007)			
Share of homes with a cell phone	523	0.704	515	0.700	523	0.697	0.004	0.007	0.003
		(0.007)		(0.006)		(0.006)			
Share of homes with radio	523	0.792	515	0.789	523	0.788	0.003	0.004	0.001
		(0.004)		(0.004)		(0.004)			
Share of homes with computer	523	0.307	515	0.307	523	0.302	-0.000	0.005	0.005
		(0.004)		(0.004)		(0.004)			
Share of homes with landline telephone	523	0.419	515	0.419	523	0.410	-0.001	0.009	0.009
		(0.006)		(0.006)		(0.006)			
Share of homes with a washing machine	523	0.700	515	0.696	523	0.683	0.004	0.018*	0.013
		(0.007)		(0.007)		(0.007)			
Share of homes with a fridge	523	0.858	515	0.858	523	0.848	-0.000	0.010	0.011
		(0.006)		(0.005)		(0.005)			
Share of homes with water, drainage, electricity	523	0.839	515	0.837	523	0.836	0.002	0.003	0.001
		(0.007)		(0.006)		(0.006)			
Share of homes with electricity	523	0.978	515	0.979	523	0.978	-0.000	0.000	0.001
		(0.002)		(0.001)		(0.001)			
Share of homes with drainage	523	0.913	515	0.918	523	0.918	-0.004	-0.005	-0.000
		(0.005)		(0.005)		(0.004)			
Share of homes with water	523	0.888	515	0.886	523	0.883	0.002	0.005	0.003
		(0.005)		(0.005)		(0.005)			
Share of homes with a toilet	523	0.963	515	0.963	523	0.962	-0.000	0.001	0.001
		(0.002)		(0.002)		(0.002)			
Share of homes without a dirt floor	523	0.934	515	0.934	523	0.932	0.000	0.002	0.001
		(0.003)		(0.003)		(0.003)			
Average occupants per room	523	1.094	515	1.095	523	1.107	-0.001	-0.013	-0.012
		(0.008)		(0.008)		(0.008)			
Share working age	523	0.640	515	0.642	523	0.641	-0.002	-0.000	0.001
		(0.001)		(0.001)		(0.001)			
Share economically active	523	0.405	515	0.407	523	0.408	-0.002	-0.002	-0.001
		(0.002)		(0.001)		(0.001)			
Share married	523	0.554	515	0.552	523	0.552	0.002	0.001	-0.000
		(0.001)		(0.001)		(0.001)			
Average children per woman	523	2.291	515	2.295	523	2.290	-0.004	0.001	0.006
		(0.012)		(0.012)		(0.011)			
Share married	523	0.554	515	0.552	523	0.552	0.002	0.001	-0.000
		(0.001)		(0.001)		(0.001)			
Average years of schooling	523	8.862	515	8.879	523	8.802	-0.018	0.059	0.077
		(0.049)		(0.047)		(0.048)			
Share without healthcare	523	0.294	515	0.303	523	0.310	-0.009	-0.016***	-0.007
		(0.004)		(0.005)		(0.005)			
Share of state workers in health care	523	0.063	515	0.066	523	0.065	-0.003	-0.002	0.001
		(0.002)		(0.002)		(0.002)			
Average occupants per dwelling	523	3.871	515	3.862	523	3.885	0.009	-0.014	-0.023
		(0.013)		(0.012)		(0.012)			
PRI federal vote share	523	0.319	515	0.302	523	0.308	0.016***	0.011*	-0.006
		(0.004)		(0.004)		(0.004)			
PAN federal vote share	523	0.231	515	0.225	523	0.224	0.006	0.007	0.001
		(0.005)		(0.006)		(0.005)			
PRD federal vote share	523	0.081	515	0.089	523	0.086	-0.008	-0.005	0.003
		(0.004)		(0.004)		(0.004)			
Morena federal vote share	523	0.072	515	0.081	523	0.077	-0.008**	-0.004	0.004
		(0.002)		(0.003)		(0.002)			

Notes: The value displayed for t-tests are the differences in the means across the groups. Regressions are estimated using state fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Estimated respondent types for list experiment

	Local organized crime treatment				Local government treatment			
y value	π_{y0}	SE	π_{y1}	SE	π_{y0}	SE	π_{y1}	SE
0	−0.0003	0.005	0.005	0.004	0.002	0.004	0.003	0.003
1	0.040	0.027	0.146	0.019	0.038	0.027	0.148	0.019
2	0.035	0.034	0.458	0.032	0.089	0.034	0.405	0.032
3	0.024	0.009	0.288	0.025	0.079	0.014	0.233	0.027

Notes: The table shows the estimated proportion of respondent types, π_{yz}, where $y_{i(0)}$ is the (latent) count of 'yes' responses to the control items and z_i is the (latent) binary response to the sensitive item. The Bonferroni-corrected p-values of 0.959 and 1 for the local organized crime and local government treatments, respectively, indicate failure to reject the null hypothesis of no design effects for each treatment arm. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

with the response to the direct questions; if they are capturing the same latent factor, we should expect to observe positive interaction coefficients. The results in Tables **??**, **??**, and **??** show that this is generally the case in the pooled, newspaper-only, and radio-only samples.

Finally, panels A-C of Table **??** report the list experiment estimates by region in our pooled, newspaper-only, and radio-only samples. These estimates are shown graphically in Figures **??** and **??**. Table **??** reports similar results by the five electoral regions that each contribute 40 representatives to Mexico's Chamber of Deputies via the proportional representation component of the legislature's mixed electoral system.

A.4 Measurement of reporting on inter-DTO violence

We measure media reporting on violent crime using data collected by **?**. He constructed the Organized Criminal Violence Event Database (OCVED), which contains events of drug-related violence in Mexico at the municipal level from 2000 to 2010. The data includes information from over 41,000 news reports based on 105 Mexican sources: federal government agencies (4), state government agencies (32), national-level newspapers and magazines (11), and local-level newspapers (58). Because the information is principally obtained from media outlets and state agencies do not report on all incidents of violent crime, we regard this dataset as reflecting events *that were reported*, rather than a complete list of events that occurred.

Table A4: List experiment validation using all outlets

	Outcome: Number of experiences			
	(1)	(2)	(3)	(4)
Local organized crime treatment	0.017	-0.011	-0.005	-0.049
	(0.062)	(0.061)	(0.063)	(0.068)
Criminal interference (self-reported)	-0.065	-0.211**		-0.235***
	(0.077)	(0.087)		(0.090)
Local organized crime treatment × Criminal interference (self-reported)	0.359***	0.518***		0.492***
	(0.126)	(0.125)		(0.141)
Local government treatment	0.148**	0.122*	0.044	0.069
	(0.070)	(0.068)	(0.069)	(0.075)
Local government interference (self-reported)	0.122*		-0.009	0.103
	(0.066)		(0.082)	(0.080)
Local government treatment × Local government interference (self-reported)	0.317**		0.425***	0.256*
	(0.127)		(0.130)	(0.144)
Local government treatment × Criminal interference (self-reported)		0.491***		0.381**
		(0.131)		(0.150)
Local organized crime treatment × Local government interference (self-reported)			0.254**	0.099
			(0.117)	(0.128)
Observations	896	958	1,001	896
Outcome mean	2.23	2.23	2.24	2.23
Outcome standard deviation	0.77	0.76	0.78	0.77

Notes: All specifications include state fixed effects and are estimated using OLS. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: List experiment validation using newspapers

	Outcome: Number of experiences			
	(1)	(2)	(3)	(4)
Local organized crime treatment	-0.022	-0.091	0.046	-0.084
	(0.150)	(0.134)	(0.145)	(0.155)
Criminal interference (self-reported)	-0.433***	-0.456**		-0.551***
	(0.158)	(0.196)		(0.211)
Local organized crime treatment × Criminal interference (self-reported)	0.527**	0.737***		0.610**
	(0.236)	(0.229)		(0.288)
Local government treatment	0.240*	0.211	0.178	0.163
	(0.143)	(0.135)	(0.150)	(0.153)
Local government interference (self-reported)	0.320**		0.082	0.316*
	(0.144)		(0.183)	(0.185)
Local government treatment × Local government interference (self-reported)	0.306		0.492*	0.261
	(0.251)		(0.269)	(0.298)
Local government treatment × Criminal interference (self-reported)		0.393		0.284
		(0.256)		(0.296)
Local organized crime treatment × Local government interference (self-reported)			0.361	0.083
			(0.240)	(0.285)
Observations	223	252	245	223
Outcome mean	2.22	2.23	2.24	2.22
Outcome standard deviation	0.79	0.77	0.79	0.79

Notes: All specifications include state fixed effects and are estimated using OLS. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: List experiment validation using radios

	Outcome: Number of experiences			
	(1)	(2)	(3)	(4)
Local organized crime treatment	0.023	-0.002	-0.015	-0.028
	(0.071)	(0.071)	(0.074)	(0.081)
Criminal interference (self-reported)	0.046	-0.106		-0.145
	(0.086)	(0.089)		(0.089)
Local organized crime treatment × Criminal interference (self-reported)	0.275*	0.389***		0.444***
	(0.156)	(0.145)		(0.165)
Local government treatment	0.143*	0.106	0.047	0.071
	(0.082)	(0.079)	(0.080)	(0.088)
Local government interference (self-reported)	0.123		0.043	0.126
	(0.077)		(0.089)	(0.088)
Local government treatment × Local government interference (self-reported)	0.282*		0.322**	0.189
	(0.144)		(0.144)	(0.159)
Local government treatment × Criminal interference (self-reported)		0.491***		0.415**
		(0.150)		(0.164)
Local organized crime treatment × Local government interference (self-reported)			0.150	0.039
			(0.134)	(0.146)
Observations	672	706	756	672
Outcome mean	2.24	2.23	2.24	2.24
Outcome standard deviation	0.77	0.76	0.78	0.77

Notes: All specifications include state fixed effects and are estimated using OLS. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The database generation process consisted of trained coders first running a systematic query in *Infolatina*, a large repository of newspapers. Using the output, coders next manually identified news reports of violence committed by either criminal groups or government authorities conducting law enforcement activities.

The main inclusion criteria that coders followed were including reports of events associated with violent actions such as armed clashes, murders, killings, shootings, ambushes, attacks, assassination attempts, wounding, kidnapping, torture or mutilation that involve the participation of presumed members of criminal organizations as perpetrators or victims. Reports without these explicit mentions were also to be included if their *modus operandi* involved one of more of the following: use of assault weapons, two or more victims, execution style killings, participation of at least one group of armed men, participants traveling in convoys of vehicles, signs or messages associated with organized crime, or bodies found in various containers. Reports that were associated with kidnappings, extortion or money laundering were to be included even if they did not explicitly mention criminal or drug trafficking organizations. These criteria are consistent with those used

Table A7: Average treatment effects by geographic region

	Outcome: Number of experiences					
	Region 1	Region 2	Region 3	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All outlets						
Local organized crime treatment	0.083	0.140	0.156	0.029	0.040	0.207
	(0.105)	(0.142)	(0.152)	(0.124)	(0.093)	(0.136)
Local government treatment	0.095	0.530***	0.128	0.428***	0.058	0.261*
	(0.109)	(0.148)	(0.152)	(0.135)	(0.112)	(0.156)
Observations	261	187	145	149	298	113
Outcome mean	2.45	2.40	1.83	2.05	2.19	2.20
Outcome standard deviation	0.74	0.86	0.78	0.70	0.71	0.67
Panel B: Newspapers						
Local organized crime treatment	0.505	0.241	0.226	0.117	0.231	0.245
	(0.327)	(0.386)	(0.283)	(0.220)	(0.191)	(0.232)
Local government treatment	0.635**	0.683**	0.475	0.352	0.340	0.450
	(0.275)	(0.334)	(0.324)	(0.249)	(0.229)	(0.293)
Observations	51	41	32	58	94	32
Outcome mean	2.45	2.51	2.03	2.05	2.15	2.47
Outcome standard deviation	0.78	0.93	0.74	0.74	0.73	0.67
Panel C: Radios						
Local organized crime treatment	-0.036	0.087	0.165	0.068	-0.036	0.219
	(0.113)	(0.157)	(0.187)	(0.158)	(0.105)	(0.161)
Local government treatment	0.036	0.478***	0.083	0.559***	-0.084	0.196
	(0.118)	(0.165)	(0.174)	(0.164)	(0.126)	(0.187)
Observations	210	146	113	91	204	81
Outcome mean	2.45	2.37	1.78	2.04	2.21	2.10
Outcome standard deviation	0.73	0.85	0.79	0.68	0.69	0.64

Notes: **Region 1** includes the following states: Baja California, Baja California Sur, Chihuahua, Durango, Sinaloa, and Sonora. **Region 2** includes: Coahuila, Nuevo León, and Tamaulipas. **Region 3** includes: Colima, Jalisco, Michoacán, and Nayarit. **Region 4** includes: Hidalgo, Estado de México, Morelos, Puebla, Querétaro, and Tlaxcala. **Region 5** includes: Campeche, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatán. **Region 6** includes: Aguascalientes, Guanajuato, San Luis Potosí, and Zacatecas. All specifications include state fixed effects and are estimated using OLS. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Average treatment effects by electoral region

	Outcome: Number of experiences				
	Region 1	Region 2	Region 3	Region 4	Region 5
	(1)	(2)	(3)	(4)	(5)
Panel A: All outlets					
Local organized crime treatment	0.110	0.146	0.061	-0.048	0.196
	(0.092)	(0.100)	(0.098)	(0.127)	(0.180)
Local government treatment	0.159*	0.397***	0.037	0.349***	0.271
	(0.095)	(0.106)	(0.119)	(0.128)	(0.194)
Observations	349	319	276	100	109
Outcome mean	2.27	2.30	2.22	2.04	2.02
Outcome standard deviation	0.80	0.79	0.72	0.57	0.84
Panel B: Newspapers					
Local organized crime treatment	0.432	0.247	0.252	-0.224	0.384
	(0.261)	(0.211)	(0.204)	(0.215)	(0.274)
Local government treatment	0.633**	0.559**	0.333	-0.093	0.622*
	(0.239)	(0.225)	(0.244)	(0.227)	(0.311)
Observations	71	78	86	33	40
Outcome mean	2.31	2.46	2.15	2.06	2.10
Outcome standard deviation	0.82	0.80	0.76	0.56	0.81
Panel C: Radios					
Local organized crime treatment	0.020	0.116	-0.005	-0.006	0.110
	(0.101)	(0.114)	(0.110)	(0.161)	(0.244)
Local government treatment	0.110	0.356***	-0.106	0.558***	0.075
	(0.104)	(0.123)	(0.134)	(0.133)	(0.259)
Observations	278	241	190	67	69
Outcome mean	2.26	2.24	2.25	2.03	1.97
Outcome standard deviation	0.80	0.79	0.70	0.58	0.86

Notes: **Region 1** includes the following states: Baja California, Baja California Sur, Chihuahua, Durango, Jalisco, Nayarit, Sinaloa, and Sonora. **Region 2** includes: Aguascalientes, Coahuila, Guanajuato, Nuevo León, Querétaro, San Luis Potosí, Tamaulipas, and Zacatecas. **Region 3** includes: Campeche, Chiapas, Oaxaca, Quintana Roo, Tabasco, Veracruz, and Yucatán. **Region 4** includes: Guerrero, Morelos, Puebla, and Tlaxcala. **Region 5** includes: Colima, Estado de México, Hidalgo, and Michoacán. All specifications include state fixed effects and are estimated using OLS. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

by the Mexican government for classifying a homicide as being presumed to be associated with organized crime.

Selected reports then served as input text for automated event coding using Eventus ID, a software for automated event coding from Spanish text. The software uses a set of actors and verb dictionaries to recognize specific words in news reports that identify the key components of the event data. An event is defined by three elements: a source (actor or perpetrator of the action), action, and target. Using these elements, data were aggregated to conform to the following categories: inter-DTO violence, violent law enforcement, arrests, seizure of arrests, seizures of drugs, seizures of weapons, and violent retaliation. Our variable of interest, inter-DTO violence, measures the number of violent events between criminal groups that occurred in a municipality-week. The measure considers episodes where both the source and target are members of criminal organizations, and the event is associated with the following actions: "attack," "shoot," "clash," "kidnap," "burn," "wound," "torture," "mutilate," and "kill."

A.5 Definition of DTO presence

As noted in the main text, we use **?**'s measure of yearly municipal DTO presence constructed using a search algorithm that queries archived publications from Google News. The algorithm codes a DTO as present in a municipality if the frequency of hits for a given municipality-organization pair exceeds the threshold determined by the searchable material for a given municipality-year pair. The results of the search indicate the presence of 13 criminal organizations across 713 of Mexico's 2,441 municipalities from 1991-2010 (**?**).

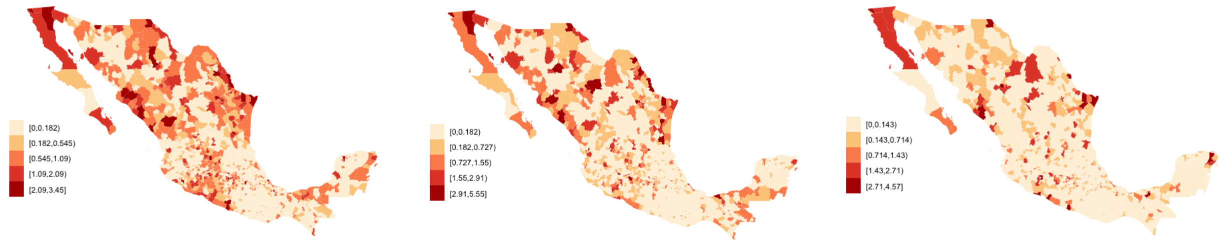
We are aware of two other measures of municipal DTO presence that are available nationwide for comparable periods. First, **?**'s (**?**) measure, which is available from 2000 to 2010, is constructed using the same automated method used to identify the location and timing of violent events that occurred between DTOs. This method involved four stages: first, trained coders ran a systematic query in Infolatina, a large repository of newspapers which yielded relevant reports. Second, they followed certain rules to identify news reports or violence committed by either DTOs or government authorities engaged in law enforcement. Third, selected reports served as input text for automated coding using Eventus ID, a software for coding event data from news reports written in Spanish. Finally, the data were validated, aggregated, and recoded, resulting in the final database consisting of 251,167 geo-referenced events. From these event data, **?** creates a disaggregated measure of DTO presence that identifies "main DTOs" (presence of one of the size most prominent DTOs in Mexico) and "secondary DTO" (presence of smaller DTOs that may have emerged as spin-offs of larger groups or developed independently). The activity of a criminal organization is imputed for the entire year in a location once it is mentioned in a report to reduce the risk of false negatives.

Second, **?** constructs a measure of DTO presence for every Mexican municipality in each year from 1990-2016. She uses a web crawler to extract articles related to a municipality-DTO pair from Google News Mexico, which identified 770 local, 33 national, and 83 international media outlets reporting in Spanish (**?**). After collecting articles whose main body mentions a Mexican municipality and the name of one of nine major DTOs, she uses a sentence extractor to filter the sentences from these articles that include a municipality-DTO pair. The sentences are analyzed using a semi-supervised CNN, a deep learning set of algorithms, to validate whether an article is actually discussing a DTO being active in that municipality. The algorithm was trained by manually classifying 5,000 sentences as indicating DTO presence or not.

We favor the **?** measure over the **?** because of the more extensive DTO identification rules applied by **?**; the **?** measure is constructed similarly, but is less reliable before 2004. However, all measures are positively correlated. Figure **??** shows the yearly average numbers of DTOs present in each municipality for each of the three measures of presence, and we show below in Table **??** that we obtain similar results using each of these measures.

A.6 Robustness tests for estimating the effect of DTO presence

This section details the results of several robustness tests. First, we use leads of DTO presence to provide support for the identification assumptions. Second, we document robustness to using alternative definitions of DTO presence. Third, we show that the results are not driven by DTO presence



(a) **?** (b) **?** (c) **?**

Figure A1: Average number of DTOs present

Note: The aggregated data in Figures **??**, **??**, and **??** depict average yearly DTO presence and are derived, respectivley, from **?**, **?**, and **?**.

increasing the number of homicides. Fourth, we report similar results when further exploiting DTO exit to estimate the effect of DTO exits from municipalities.

A.6.1 Effect of DTO presence by treatment year cohort

Generalized difference-in-differences designs leverage both units that are never treated *and* units that were treated earlier (or later) to compute counterfactual trends. The latter group may be problematic if treatment effects vary over time, and thus counterfactual trends partly reflect treatment effect heterogeneity. Since our main specification pools both types of counterfactuals, we next demonstrate that the results are not driven by the municipalities that a DTO had already earlier. To do so, we estimate standard difference-in-differences specifications separately for each cohort of municipalities that DTOs first entered by restricting our sample to municipalities that are never treated and municipalities that first became treated in a given year. Within these subsets of our dataset, all treated municipalities became treated in the same year. The results in Table **??** show remarkably stable negative and statistically significant estimates for the interaction between homicides and DTO presence across the municipalities first treated in each cohort between 2001 and 2008. Only in the municipalities that DTOs first entered in 2009 and 2010 do we fail to observe this negative effect, which could simply reflect it taking time for the effect of DTO presence to materialize. In sum, these results indicate that our estimates are not driven by treatment effect heterogeneity over time.

A.6.2 Including leads of DTO presence

Generalized difference-in-differences designs rely on the parallel trends assumption. We provide evidence consistent with this assumption by examining whether municipalities that became treated were already exhibiting lower rates of reporting on inter-DTO violence for a given level of homicides *before* DTOs entered the municipality. Specifically, we add one or two leads of our treatment variable to equation (**??**), at the cost of dropping the last year or two years of data from the sample (because the lead of DTO presence is missing in 2010 and the second lead of DTO presence is missing in 2009, since our dataset does not extend beyond 2010). Specifically, we estimate equations with τ leads as follows:

$$\begin{aligned} Reports_{mt} &= (\beta_m + \beta_t) Homicides_{mt} + \gamma DTO_{mt} + \delta\big(Homicides_{mt} \times DTO_{mt}\big) \\ &+ \sum_{\tau=1}^{T} \kappa\big(Homicides_{mt} \times \mathbb{1}[Years\ until\ DTO\ entry_{mt} = \tau]\big) + \mu_m + \eta_t + \varepsilon_{mt}, \end{aligned} \tag{A1}$$

where $\mathbb{1}[Years\ until\ DTO\ entry_{mt} = \tau]$ is an indicator for the year τ periods before a DTO first enters municipality m.

The results reported in Table **??** provide support for the principal identifying assumption. Because the sample is truncated by the inclusion of leads, we first show in columns (1) and (3) that our main estimates remain relatively similar in the subsamples used to estimate the effects of leads. Turning to our tests of the parallel trends assumption, column (2) shows that inclusion of one lead barely alters the point estimate for the quantity of interest. Furthermore, the statistically insignificant coefficient on the lead provides no evidence to suggest that violence is reported significantly differently in municipalities that DTO are about to enter. Column (4) includes a second lead. In this specification, we observe some evidence of a pre-trend, although this likely reflects the baseline category against which estimates are benchmarked—the set of observations for which periods at least 3 years before a DTO enters a municipality—becoming smaller and more idiosyncratic.

Table A9: Effect on DTO presence on reports of inter-DTO violent crime by year

	Outcome: Reported inter-DTO violence									
	Treated municipalities restricted to those first treated in year:									
	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DTO	4.325***	3.975*	9.556**	8.212***	2.513***	2.515***	1.525***	0.813*	-0.130	-0.609
	(1.441)	(2.316)	(4.860)	(2.108)	(0.905)	(0.750)	(0.557)	(0.459)	(0.662)	(0.396)
Homicides × DTO	-0.151***	-0.263**	-0.229	-0.251***	-0.139*	-0.203***	-0.202**	-0.170**	-0.019	0.089
	(0.057)	(0.104)	(0.173)	(0.071)	(0.082)	(0.071)	(0.095)	(0.068)	(0.128)	(0.100)
Observations	19,502	19,315	19,557	19,667	20,338	20,371	20,316	20,976	19,942	19,678
Outcome mean	1.26	1.00	1.17	1.14	1.11	1.05	1.02	0.99	0.95	0.94
Outcome standard deviation	15.12	12.11	12.67	12.31	11.94	11.85	11.81	11.61	11.87	11.94
Homicides mean	3.84	2.73	2.91	2.94	3.04	2.97	2.75	2.99	2.64	2.64
Homicides standard deviation	40.27	21.45	21.63	21.60	21.13	21.03	20.84	20.72	20.92	21.06
DTO mean	0.04	0.04	0.04	0.05	0.06	0.06	0.05	0.05	0.04	0.03
Municipality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality-specific linear homicide terms	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-specific linear homicide terms	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effect of DTO presence on reports of inter-DTO violent crime, including leads of DTO presence

	Outcome: Reported inter-DTO violence			
	(1)	(2)	(3)	(4)
DTO	2.324***	2.288***	2.317***	2.393***
	(0.353)	(0.358)	(0.379)	(0.359)
Homicides × DTO	-0.097**	-0.115**	-0.115***	-0.177***
	(0.040)	(0.055)	(0.042)	(0.058)
1st Lead DTO		-0.410		-0.230
		(0.250)		(0.255)
Homicides × 1st Lead DTO		-0.028		-0.079*
		(0.041)		(0.044)
2nd Lead DTO				-0.159
				(0.163)
Homicides × 2nd Lead DTO				-0.053**
				(0.022)
Observations	24,559	24,559	22,103	22,103
Outcome mean	1.38	1.38	1.11	1.11
Outcome standard deviation	12.02	12.02	10.33	10.33
Homicides mean	4.59	4.59	4.20	4.20
Homicides standard deviation	26.45	26.45	19.67	19.67
DTO mean	0.12	0.12	0.11	0.11
Homicides × DTO = Homicides × 1st Lead DTO (p value)		0.01		0.01
Homicides × DTO = Homicides × 2nd Lead DTO (p value)				0.01
Homicides × 1st Lead DTO = Homicides × 2nd Lead DTO (p value)				0.45
Municipality fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality-specific linear homicide terms	✓	✓	✓	✓
Year-specific linear homicide terms	✓	✓	✓	✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Importantly, the t-tests at the foot of the table indicate that we find no evidence of a pre-trend in the two years before a DTO first enters the municipality. Furthermore, the tests at the foot of the table demonstrate that the effect is significantly larger than the effect in the two years preceding treatment, indicating that DTO entry indeed caused a substantial change in reporting.

A.6.3 Logarithmic transformation of homicides and reporting on violent crime

To ensure that the results are not driven by the positively skewed distribution of homicides and reporting on inter-DTO violence, we show that the results are robust to a logarithmic transformation of these variables. Specifically, we take the natural logarithm of each raw variable (plus one), allowing us to interpret the correlation between the two variables as an elasticity. The estimate in column (1) of Table **??** thus implies that a one percent increase in homicides results in a 0.3 percent increase in reporting on inter-DTO violence. Column (2) shows that the presence of a DTO reduces this elasticity by around 20%.

A.6.4 Alternative definitions of DTO presence

We obtain similar results when using the alternative definitions of DTO presence described above in section **??**. For the **?** and **?** definitions of DTO presence, Table **??** reports similarly negative and statistically significant effects of DTO presence on the association between homicides and reporting on violent crime between DTOs. Although they are notably noisier (because we drop the year 2000-2003, due to the limited number of Google News hits that were captured), the estimates using the **?** measure suggest that there is almost no correlation between the incidents of homicides and reporting on homicides in the presence of a DTO.

A.6.5 Effects of DTO presence on the incidence of homicides

While we argue that the DTOs reduce incentives for local media outlets to report on violent crime that occurs locally, DTO presence is also likely to increase the homicide rate too. Indeed, **??** estimates a simple difference-in-differences specification to show that a DTO indeed increases the

Table A11: Relationship between incidence of homicides and reporting of inter-DTO violent crime using log-transformations

	Outcome: Log reported inter-DTO violence	
	(1)	(2)
Log homicides	0.303***	
	(0.016)	
DTO		0.493***
		(0.046)
Log homicides × DTO		-0.047**
		(0.024)
Observations	27,015	27,015
Outcome mean	0.28	0.28
Outcome standard deviation	0.73	0.73
Homicides mean	0.84	0.84
Homicides standard deviation	1.04	1.04
DTO mean	0.14	0.14
Municipality fixed effects		✓
Year fixed effects		✓
Municipality-specific linear homicide terms		✓
Year-specific linear homicide terms		✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. The lower-order homicides term in column (2) is subsumed by the linear homicide terms. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

number of homicides that occur in a treated municipality by 2.6 on average.

This is hardly surprising, but could affect the identification or interpretation of our estimates. In terms of identification, the increase in the homicide count raises the possibility of post-treatment bias arising from the homicides variable is itself a function of DTO presence. To ensure that post-treatment bias is not driving our estimates, we replace the time-varying homicides variable by a measure of the average number of homicides registered between 1995 and 1999—a measure of homicides that could not be affected by the changes in DTO presence that occur subsequently. We then estimate the following regression:

$$Reports_{mt} = \beta_t Homicides_m + \gamma DTO_{mt} + \delta\left(Homicides_m \times DTO_{mt}\right) + \mu_m + \eta_t + \varepsilon_{mt}, \quad \text{(A2)}$$

Table A12: Effect of DTO presence on reports of inter-DTO violent crime using alternative cartel indicators

	Outcome:			
	Reported inter-DTO violence	Actual homicides	Reported inter-DTO violence	Reported inter-DTO violence
	(1)	(2)	(3)	(4)
Panel A: ? cartel measure				
Homicides	0.254***			
	(0.028)			
DTO		0.756	3.126***	3.314***
		(0.962)	(0.527)	(0.506)
Homicides × DTO				-0.164***
				(0.052)
Observations	27,015	27,015	27,015	27,015
Outcome mean	1.71	5.05	1.71	1.71
Outcome standard deviation	13.85	35.30	13.85	13.85
Homicides mean	5.05	5.05	5.05	5.05
Homicides standard deviation	35.30	35.30	35.30	35.30
DTO mean		0.12	0.12	0.12
Panel B: ? cartel measure				
Homicides	0.270***			
	(0.035)			
DTO		6.811***	5.523***	6.089***
		(2.524)	(1.077)	(2.297)
Homicides × DTO				-0.321
				(0.197)
Observations	17,191	17,191	17,191	17,191
Outcome mean	2.56	5.61	2.56	2.56
Outcome standard deviation	17.27	42.69	17.27	17.27
Homicides mean	5.61	5.61	5.61	5.61
Homicides standard deviation	42.69	42.69	42.69	42.69
DTO mean		0.07	0.07	0.07
Municipality fixed effects		✓	✓	✓
Year fixed effects		✓	✓	✓
Municipality-specific linear homicide terms				✓
Year-specific linear homicide terms				✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Effect of DTO presence on homicides and reports of inter-DTO violent crime

	Outcome:	
	Actual homicides	Reported inter-DTO violence
	(1)	(2)
DTO	1.400**	2.594***
	(0.590)	(0.418)
Observations	27,015	27,015
Outcome mean	5.05	1.71
Outcome standard deviation	35.30	13.85
DTO mean	0.14	0.14
Municipality fixed effects	✓	✓
Year fixed effects	✓	✓
Municipality-specific linear homicide terms		✓
Year-specific linear homicide terms		✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

where $Homicides_m$ is now a time-invariant pre-sample measure of homicides, normalized by the growth rate in homicides between 1995 and 1999—computed either for all municipalities or all municipalities but municipality m. Note that we exclude the interaction between municipality fixed effects and the average pre-sample homicide count because this is subsumed by the municipality fixed effects. The results in Table **??** show that we obtain similar estimate for δ as we do in the main specification. This suggests that the increase in homicides that results from DTO presence is not driving the decline in reporting on inter-DTO violence for a given homicide count.

Turning to alternative interpretations, the decline in reporting per homicide could reflect media fatigue with reporting on homicides, rather than DTO pressure. We assess this interpretation by splitting the sample between municipalities with above and below median homicide counts between 1995 and 1999. If the decline in reporting reflects the media reaching saturation point in its reporting on violent crime, we should expect DTO presence—and the additional homicides—that it brings to reduce reporting more in above-median municipalities where homicides were already common. However, contrary to this alternative interpretation, a comparison across panels A and B of Table **??** indicates that the decline in reporting was larger in magnitude, albeit noisier, in

Table A14: Effect of DTO presence on reports of inter-DTO violent crime using predetermined homicide rates

	Outcome:			
	Reported inter-DTO violence	Actual homicides	Reported inter-DTO violence	Reported inter-DTO violence
	(1)	(2)	(3)	(4)
Panel A: Growth rate based on all municipalities				
1995-1999 homicides (adj.)	0.252***			
	(0.081)			
DTO		2.122***	2.594***	2.479***
		(0.291)	(0.418)	(0.392)
1995-1999 homicides (adj.) $\times$ DTO				-0.146**
				(0.074)
Observations	27,015	27,015	27,015	27,015
Outcome mean	1.71	5.05	1.71	1.71
Outcome standard deviation	13.85	18.80	13.85	13.85
Homicides mean	5.05	5.05	5.05	5.05
Homicides standard deviation	18.80	18.80	18.80	18.80
DTO mean		0.14	0.14	0.14
Panel B: Growth rate based on all but municipality *m*				
1995-1999 homicides (adj.)	0.244***			
	(0.078)			
DTO		2.135***	2.594***	2.243***
		(0.291)	(0.418)	(0.370)
1995-1999 homicides (adj.) $\times$ DTO				-0.099*
				(0.052)
Observations	27,015	27,015	27,015	27,015
Outcome mean	1.71	5.04	1.71	1.71
Outcome standard deviation	13.85	18.73	13.85	13.85
Homicides mean	5.04	5.04	5.04	5.04
Homicides standard deviation	18.73	18.73	18.73	18.73
DTO mean		0.14	0.14	0.14
Municipality fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality-specific linear homicide terms				✓
Year-specific linear homicide terms				✓

Notes: Panel A uses the 1999 homicide count in a municipality adjusted for the growth rate in homicides over the sample period. Panel B uses the same measure but excludes the municipality's homicide count when computing the growth rate. All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

municipalities with below-median homicide counts.

A.6.6 Allowing for reversals in DTO presence

In the main analysis, we coded a municipality as treated in all periods after a DTO first enters the municipality. We did so because to avoid combining potentially heterogeneous effects of DTO presence depending on whether a DTO arrives or leaves and to reduce the risk of measurement error. However, this also limits the variation that we can exploit. Table **??** reports the results when we allow DTO presence to reverse over time, indicating that our estimates are relatively unaffected.

A.6.7 Heterogeneity by number of DTOs present

Table **??** compares the effect of DTO presence between municipalities that only ever had at most one DTO versus municipalities that experienced at least two DTOs being present. The results suggest that there is no effect of additional DTOs on reporting on inter-DTO violence.

Table A15: Heterogeneity in reporting by by 1995-1999 municipal homicide counts

	Outcome:			
	Reported inter-DTO violence	Actual homicides	Reported inter-DTO violence	Reported inter-DTO violence
	(1)	(2)	(3)	(4)
Panel A: Municipalities below 1995-1999 median number of homicides				
Homicides	0.771***			
	(0.219)			
DTO		1.019***	4.160***	3.805***
		(0.211)	(0.733)	(0.775)
Homicides × DTO				-0.359
				(0.351)
Observations	14,607	14,607	14,607	14,607
Outcome mean	0.78	0.54	0.78	0.78
Outcome standard deviation	6.72	1.74	6.72	6.72
Homicides mean	0.54	0.54	0.54	0.54
Homicides standard deviation	1.74	1.74	1.74	1.74
DTO mean		0.07	0.07	0.07
Panel B: Municipalities above 1995-1999 median number of homicides				
Homicides	0.254***			
	(0.028)			
DTO		-1.028	0.201	1.337***
		(1.657)	(0.842)	(0.405)
Homicides × DTO				-0.076*
				(0.043)
Observations	12,408	12,408	12,408	12,408
Outcome mean	2.81	10.36	2.81	2.81
Outcome standard deviation	19.03	51.56	19.03	19.03
Homicides mean	10.36	10.36	10.36	10.36
Homicides standard deviation	51.56	51.56	51.56	51.56
DTO mean		0.23	0.23	0.23
Municipality fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality-specific linear homicide terms				✓
Year-specific linear homicide terms				✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Effect of DTO presence on reports of inter-DTO violent crime with reversing DTO indicators

	Outcome:			
	Reported inter-DTO violence	Actual homicides	Reported inter-DTO violence	Reported inter-DTO violence
	(1)	(2)	(3)	(4)
Homicides	0.254***			
	(0.028)			
DTO		2.001***	3.799***	3.139***
		(0.561)	(0.419)	(0.381)
Homicides × DTO				-0.115***
				(0.025)
Observations	27,015	27,015	27,015	27,015
Outcome mean	1.71	5.05	1.71	1.71
Outcome standard deviation	13.85	35.30	13.85	13.85
Homicides mean	5.05	5.05	5.05	5.05
Homicides standard deviation	35.30	35.30	35.30	35.30
DTO mean		0.14	0.14	0.14
Municipality fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality-specific linear homicide terms				✓
Year-specific linear homicide terms				✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Heterogeneity in reporting effects by number of DTOs

	Outcome: Reported inter-DTO violence (1)
DTO	2.341***
	(0.369)
Homicides × DTO	-0.097**
	(0.042)
DTO × Ever 2 (or more) DTOs	-1.844***
	(0.595)
Homicides × DTO × Ever 2 (or more) DTOs	0.025
	(0.075)
Observations	27,015
Outcome mean	1.71
Outcome standard deviation	13.85
Homicides mean	5.05
Homicides standard deviation	35.30
DTO mean	0.14
Ever 2 (or more) DTOs mean	0.02
Municipality fixed effects	✓
Ever 2 (or more) DTOs × Year fixed effects	✓
Municipality-specific linear homicide terms	✓
Ever 2 (or more) DTOs × Year-specific linear homicide terms	✓

Notes: All specifications are estimated using OLS, and fixed effect coefficients are omitted. Standard errors are clustered by municipality. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.