The Political Consequences of "Source Country" Operations: Evidence from Crop Eradication in Mexico
Supplemental Information

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Contents

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Appendix A Military Geography

While Mexico is politically divided into 32 states, the Mexican Army is territorially organized around 13 military regions and 46 military zones. Each region is headed by a Division General and encompasses whole states, while military zones are headed by lower-ranking Brigade Generals and can incorporate municipalities from one, two, or three different states. Zone commanders have operational autonomy in the territory they head and can appoint the commanders of sectors and subsectors within their territory. In addition, the president has the prerogative to appoint both zone and region commanders directly for an indeterminate length of time. Figure A1 shows the overlap between military zones and regions.

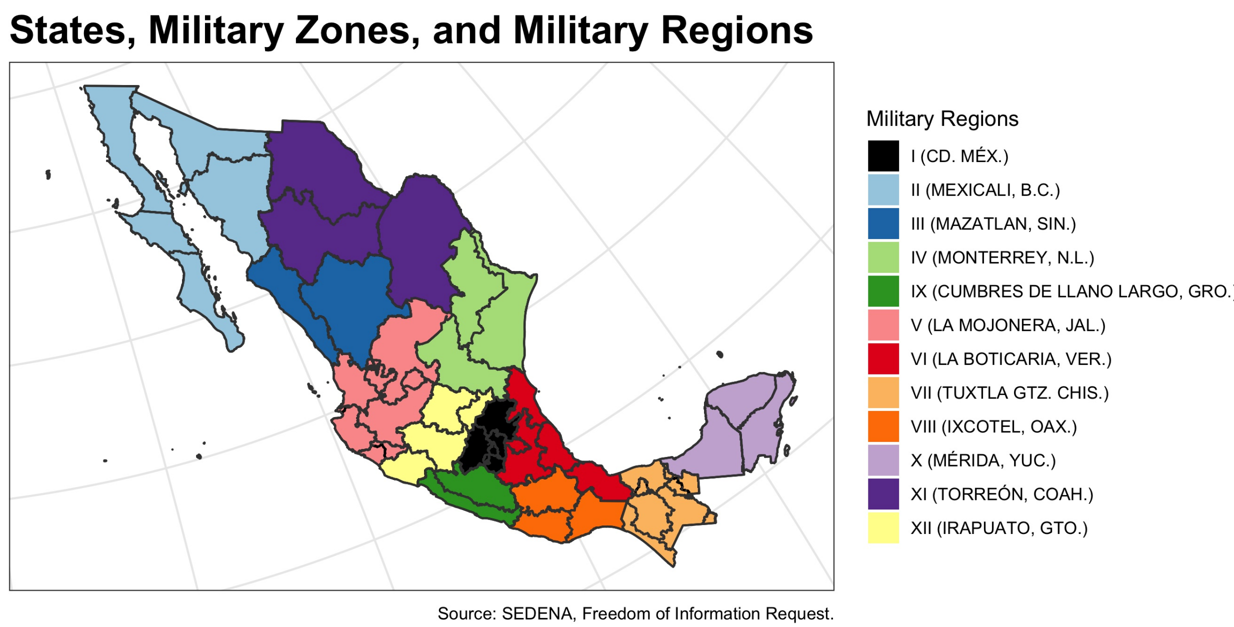

Figure A1: The map colors municipalities by the military region to which they belong and shows the borders of military zones in black.

A1.1 Military Organization and Eradication Operations

Mexico's crop eradication program is executed by the Secretaría de la Defensa Nacional (SEDENA) through the army's territorial command structure. Two primary documents describe this organizational architecture: SEDENA's accountability report for the 2006–2012 period, *Memoria Documental: Operaciones Contra el Narcotráfico* (SDN-MD-11), and the interagency *Protocolo Nacional de Actuación para la Destrucción de Plantíos Ilícitos*. Together, these sources reveal a hierarchical system in which broad campaign planning is centralized at the Military Region level, resource allocation and performance monitoring occur at the Military Zone level, and troop deployment takes place at the subsector level.

Command hierarchy and decentralized execution. The 2007–2012 "Directiva para el Combate Integral al Narcotráfico" established the strategic framework under which eradication operations are planned and conducted. As SDN-MD-11 explains, SEDENA implemented new procedures aimed at granting "*mayor libertad a los Comandantes para realizar sus operaciones en forma descentralizada*" (greater freedom for Commanders to carry out their operations in a decentralized manner), while privileging aerial resources, technological tools, and statistical methods to estimate cultivated area and production capacity (SDN-MD-11, p. 1). The eradication section of the report specifies the responsibility structure: "*los Comandantes de Región Militar son los responsables de concebir, preparar y conducir las operaciones previstas*" (the Military Region Commanders are responsible for conceiving, preparing, and conducting the planned operations; SDN-MD-11, p. 52). The document further describes a system of "*planeamiento centralizado y ejecución descentralizada*" (centralized planning and decentralized execution; SDN-MD-11, p. 58) where Zone commanders are central pieces.

Eradication as a systematic, state-based campaign. Eradication campaigns are organized as sequential, state-level sweeps. SDN-MD-11 lists all "Operaciones de Alto Impacto de Erradicación" and "Operaciones Regionales de erradicación" by year, and these are uniformly labeled by state and sequential number: Michoacán I, II, III; Guerrero I, II, III; Sinaloa I, II, III; Durango I, II, III; and so forth (SDN-MD-11, pp. 53–56). Each operation covers a defined time period within the year, and the directive's stated objective was to "*disminuir gradualmente las áreas de cultivo de plantíos ilícitos*" (gradually reduce the areas of illicit crop cultivation; SDN-MD-11, p. 52).
The *Operaciones de Alto Impacto* are large, time-bounded seasonal sweeps concentrated in a single state; the *Operaciones Regionales* are the ongoing, lower-intensity eradication activity that zone commanders conduct continuously within their jurisdictions between major campaigns. In both modalities, the program's objective is to reduce cultivated area, not to weaken or target particular criminal networks.

Field detection via satellite imagery. The army's Proyecto de Fotointerpretación de Imágenes Satelitales (Satellite Image Photointerpretation Project) further supports the technology-driven nature of field detection. SDN-MD-11 explains that the project was developed "*para minimizar el tiempo de localización de plantíos ilícitos en áreas de incidencia en el territorio Nacional*" (to minimize the time required to locate illicit plantings in areas of incidence throughout the national territory; SDN-MD-11, p. 50). The program relies on imagery from the SPOT satellite constellation, received through the Estación de Recepción México de la Constelación Spot (ERMEXS), which began operating in 2003. In November 2008, an interinstitutional Grupo de Análisis de Imágenes Satelitales (Satellite Image Analysis Group) was established between SEDENA and the Secretaría de Marina to conduct illicit crop detection activities using satellite imagery (SDN-MD-11, p. 59).

Appendix B The Drug-Trafficking Chain

Growers, not drug-trading organizations (DTOs), often own the illegal crops the army eradicates in Mexico. Thus, eradication operations do not affect DTOs economically, and the negative economic shock is absorbed by growers that, as Figure A2 shows, sell their crops to intermediaries.

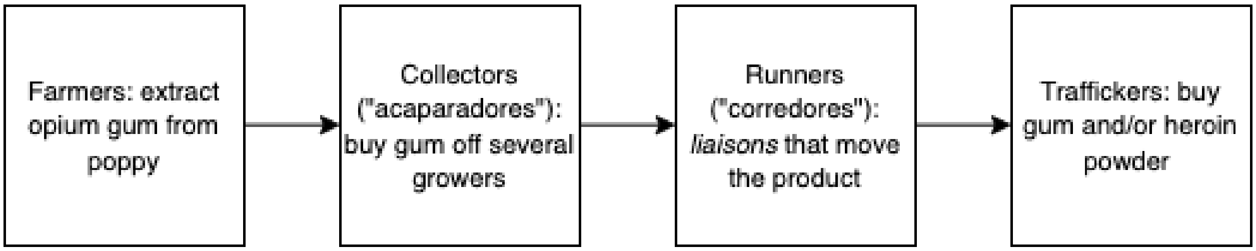

Figure A2: The figure describes the drug-trafficking process, starting from the selling of poppy farming in a town of the Sierra de Guerrero, as described in Álvarez Rodriguez (2021a).

Appendix C Semi-Structured Interviews with Mexican Officials

To better understand the domestic political dynamics driving eradication policy in Mexico, I conducted four semi-structured interviews with former high-level Mexican politicians in May 2025.

A3.1 Interview Subjects

- Former federal senator
- Former federal deputy
- Former federal deputy and President of Congress
- Former Presidential Chief of Staff

All interviews lasted 40-60 minutes and followed a standardized protocol covering drug policy, US-Mexico relations, and eradication considerations.

A3.2 Key Findings

The interviews reveal three consistent themes:[1]

US Influence in Drug Policy: All interviewees emphasized how US preferences fundamentally constrain Mexican counternarcotics policy choices, limiting domestic political debate.

> *Former Senator:* "Counternarcotics policy is a bargaining chip. It serves the purpose of facilitating political negotiation where Latin American governments historically accept US government demands because it was politically convenient for them."

> *Former Deputy:* "They're being monitored by the *gringos* (US authorities), and if they didn't burn any plantations, they'd get into trouble."

> *Former Deputy:* "You know this whole certification thing [...] but it's clear that they're told 'act here' and they act here, and then they get certified. If they did nothing, they'd be at fault with the *gringo* (US) government."

[1] All quotes are the author's translation from Spanish.

> *Former CoS:* "During the nineties, eradication was practically a tacit requirement to avoid losing certification. In practice, it became an indispensable diplomatic gesture in the relationship with Washington."

> *Former CoS:* "Certification wasn't just a formality: it was a thermometer of presidential credibility [...] It became a kind of 'annual exam of responsible sovereignty."

Eradication as Politically Expedient: Officials described crop eradication as useful for political purposes *not* curbing drug production.

> *Former Deputy:* "It's propaganda. For Trump, the easiest place to show trophies is Mexico, in the fight against cartels... In the US they are very concerned about narco-politics, eradication also serves the purpose of emphasizing that Mexico's government are not narcos."

> *Former Deputy:* "They do it for propaganda; they already know that little is exported; there's much less in exports. The issue is fentanyl."

> *Former Deputy:* "They're just covering the male's eye (i.e., putting up a façade or making a superficial gesture); they need to justify themselves."

> *Former CoS:* "It [eradication] responded primarily to the US annual certification process, which evaluated anti-drug cooperation as a condition for maintaining assistance and bilateral legitimacy. This pressure shaped the national agenda, although efforts were made to align it with domestic narratives of sovereignty and crime fighting."

Policy Inelasticity: Despite recognition that DTOs no longer mainly profit from crops like marijuana and poppy, and despite strides to legalize the recreational use of marijuana in the US, drug policy reform remains off the policy reform menu.

> *Former Senator:* "The problem is that we can't even discuss the issue. Prohibition doesn't allow us to discuss why."

> *Former Deputy:* "It's not a priority for the federal government. Nothing is more important than propaganda."

> *Former CoS:* "During the certification era, the US prevailed [over crop-growing communities]. Not meeting their [the US] expectations could lead to ''decertification,' which was a major geopolitical blow. The local community hardly had a voice in these foreign policy decisions."

A3.3 Compliance with APSA's Principles and Guidance for Human Subjects Research

This subsection discusses the elite interview study's compliance with APSA's principles. Principles are reproduced in bold.

- **Power: When designing and conducting research, political scientists should be aware of power differentials between researcher and researched and the ways in which such power differentials can affect the voluntariness of consent and the evaluation of risk and benefit.** The interviews were conducted with former high-ranking Mexican politicians, including federal senators, deputies, and the presidential chief of staff. These participants represent powerful political actors rather than vulnerable populations, minimizing concerns about power imbalances that could compromise voluntary participation.

- **Consent: Political science researchers should generally seek informed consent from individuals who are directly engaged by the research process [...]** All respondents explicitly consented to participate in the interview after being informed about the research project and were informed about their right to stop the interview or refuse to answer any question. No coercion or influence was used to encourage participation. The study involved minimal risk of harm.

- **Deception: Political science researchers should carefully consider any use of deception and the ways in which deception can conflict with participant autonomy.** No deception was used at any point during the interviews. Participants were informed about the research goal and the topics to be discussed prior to the session.

- **Harm and trauma: Political science researchers should consider the harms associated with their research. The study involved minimal risk of harm.** Interview questions focused on policy perspectives and institutional dynamics rather than personal or traumatic experiences. Participants retained full control over their level of engagement and could decline to answer any questions.

- **Confidentiality: Political science researchers should generally keep the identities of research participants confidential; when circumstances require, researchers should adopt the higher standard of ensuring anonymity.** While participants' general roles (e.g., "former federal senator") are disclosed to provide necessary context for their perspectives, specific names and identifying details are kept confidential to protect participants' privacy.

- **Impact: Political science researchers conducting studies on political processes should consider the broader social impacts of the research process as well as the impact on the experience of individuals directly engaged by the research.** The interviews did not intervene in any ongoing political processes and focused on participants' retrospective assessments of completed policy periods. The research contributes to academic understanding of policy dynamics without compromising political processes.

- **Laws, Regulations, and Prospective Review: Political science researchers should be aware of relevant laws and regulations governing their research-related activities.** This study received IRB approval from the University of Pennsylvania Institutional Review Board (858571).

Appendix D Municipal level results

The municipal-level results presented in the main paper rely on municipal fixed effects for identification. Besides the absence of time-variant confounders, the effects must be constant across groups, periods, and dosages for the effect estimated from the continuous measure of eradication to recover the desired causal contrast. In figure A3, I plot the result of estimating a flexible ten-knot cubic regression spline with the same specification. The effects of eradication are plausibly constant across different dosages for the log number of eradicated hectares; however, the effects across dosages are heterogeneous for the log number of eradicated fields.

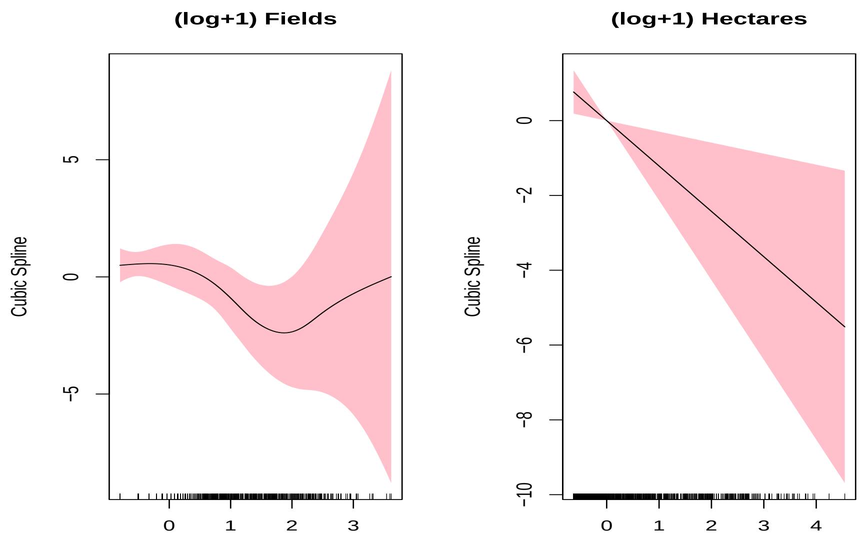

Figure A3: Figure shows the result of fitting smoothing cubic splines with 10 knots on a model with hectares or fields as the independent variable and turnout as the dependent variable.

Appendix E   Eradication: Precinct level results

A5.1   Sample

The sample for the precinct-level analysis consists of all electoral precincts where the Mexican army detected an illegal field during a federal election year. To measure eradication, I combine data on fires detected by NASA satellites with geolocated information on detected fields. Using these data, I construct a measure of precinct-level eradication as described in the Crop Eradication subsection of the main paper. Table A2 provides basic summary statistics for the resulting sample. Along with the treatment variables, I include summary statistics for the response variables: turnout rates (as a share of registered voters) and vote shares for the two main parties—the incumbent in both elections (PRI) and the opposition party (PAN).

Table A1: Summary Statistics

Predicted eradication?			No					Yes			
	N	Mean	Min	Max	Sd	N	Mean	Min	Max	Sd	Diff
Detected fields (count)	702	8.46	1	206	17.14	337	29.20	1	417	46.2	20.74
Destroyed fields (count)	702	–	–	–	–	337	7.72	1	97	12.99	–
Turnout (%)	702	52.36	5.49	100	16.6	337	56.86	5.9	100	15.41	4.50*
PAN vote share (%)	702	7.22	0	55.56	8.3	337	6.39	0	63.74	8.94	-0.83
PRI vote share (%)	702	22.66	0	94.36	13.49	337	20.71	0	89.43	15.1	-1.95

$^{*}p < 0.05$

Table A2: Table shows basic summary statistics of treatment and response variables, according to the presence or absence of predicted eradication at the precinct level.

While the Mexican army does not provide technical details on the minimum detectable size of an illegal crop field, Table A3 reports the observed size in hectares for each type of field in the data.

Variable	**Min.**	**1st Qu.**	**Median**	**Mean**	**3rd Qu.**	**Max.**
Marijuana Field Surface	0.00	1.50	4.80	22.34	12.60	753.30
Poppy Field Surface	0.00	2.70	8.90	33.00	30.32	656.51

Table A3: Summary statistics of field surface area (in hectares) by crop type. Values represent hectares per field. Statistics are calculated only for positive detections.

Table A2 shows that turnout was 4.5 percentage points higher in precincts with predicted eradication compared to those without, on average. Such a difference in turnout might raise concerns about a potential reversal-to-the-mean dynamic driving the results. Specifically, if high-turnout precincts were systematically targeted for eradication or systematically different precincts across characteristics correlated with high turnout, a regression toward the mean in turnout levels could be mistaken for a negative treatment effect of eradication. However, note that this difference is in raw turnout rates. Conversely, in my main specification, I use zone $\times$ year $\times$ prior-eradication-rate-bin fixed effects, as well as entropy balancing on census covariates. So, to consider whether we might be concerned about a reversal to the mean dynamic, first, I residualized turnout with respect to the cell fixed effects and all covariates included in the main specification, then tested for differences in these residuals across treatment groups. The t-test results show that the difference is substantially reduced from 4.5 percentage points to approximately 1.2 percentage points and is no longer statistically significant (t = -1.42, p = 0.155, 95% CI: [-2.82, 0.45]). Notably, the sign reverses: after conditioning, treated precincts show slightly lower residual turnout than controls, consistent with the direction of the main results. This suggests that the fixed effects and controls in my empirical strategy successfully account for baseline turnout differences between groups.

As an additional examination, in Figure A4, I plot the relationship between turnout rate in 2015 for precincts and the log count of detected fields in the same precinct in 2018. The figure distinguishes between precincts that were eradicated in 2018 (red) and those that were not eradicated that year (blue). Since turnout in 2015 predates detection in 2018, this figure helps us examine baseline electoral participation patterns. We observe higher turnout rates in places with fewer detected fields, suggesting that certain social and demographic characteristics might influence both electoral participation and illegal crop cultivation. However, there is no significant difference in 2015 turnout between precincts that would later be eradicated in 2018 versus those that would not, suggesting that while there are underlying electoral dynamics, these are likely orthogonal to the army's eradication decisions. Additionally, these results help assuage mean-reversal concerns: if eradication were targeted at precincts with already declining turnout, we'd expect the red dots to consistently show lower 2015 turnout than blue dots with similar detection levels - but they don't. The nearly parallel regression lines with overlapping confidence intervals indicate that future eradication decisions are not systematically related to prior turnout levels, limiting the possibility that the results simply capture pre-existing differences between treated and control precincts.

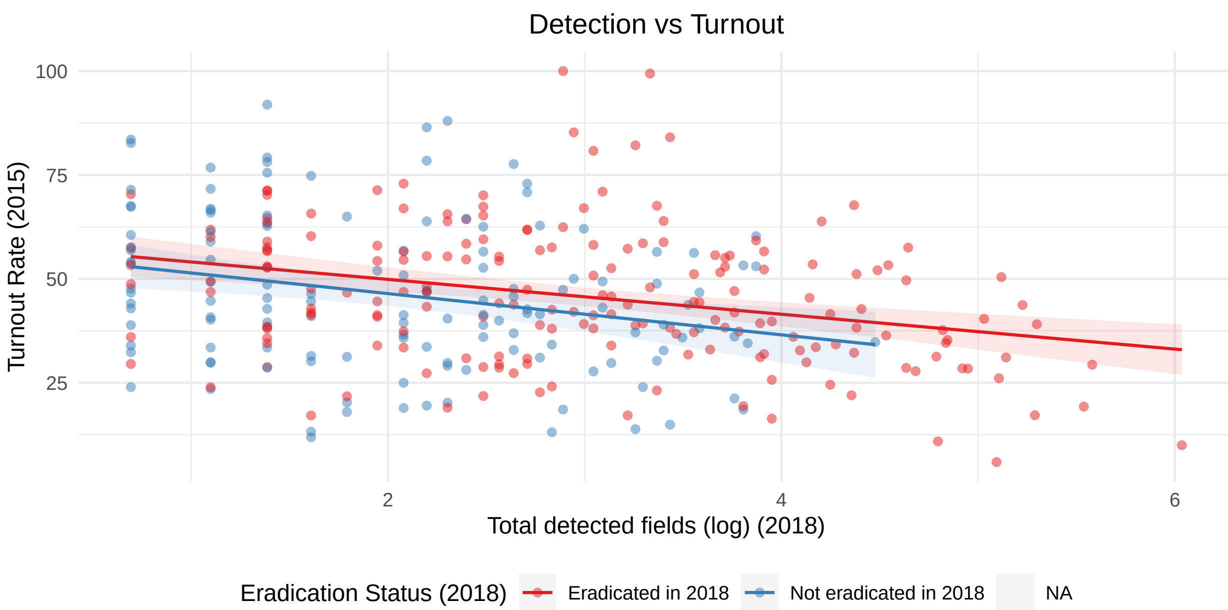

Figure A4: Figure shows the relationship between turnout rate in 2015 and the log count of detected fields in the same precinct in 2018, with colors indicating whether the precinct was eradicated (red) or not eradicated (blue) in 2018.

A5.2 Vote for PRI

In the main paper, I show that eradication at the precinct level reduced turnout. Table A4 demonstrates that this decrease in turnout did not result in systematic changes in the vote share received by the incumbent party, the PRI. I focus on the PRI vote share for two reasons. First, as the summary statistics show, many of these electoral precincts have vote shares for PAN in the single digits, which could lead to noisy measurements. Second, the PRI was the incumbent party during both election years, making it the most relevant party to examine for potential electoral consequences of eradication.

Since both turnout and PRI vote share are measured as shares of registered voters, comparing the two coefficients directly tests whether the turnout decline reflects anti-incumbent backlash or broad demobilization. If voters punished the PRI for eradication, PRI vote share should decline alongside turnout. As shown in Table A4, all PRI coefficients are statistically insignificant at conventional levels. All coefficients for the hectares and fields measures are statistically insignificant at conventional levels across all three specifications. Further coefficients have heterogeneous directions, some suggesting the diffrence is positive, others negative. The evidence is inconsistent with targeted anti-incumbent backlash.

	(1) Any eradication	(2) Eradicated ha (log)	(3) Fields (log)
No controls			
Eradication	0.420	-0.074	-0.030
	(0.941)	(0.359)	(0.445)
	$[p = 0.656]$	$[p = 0.836]$	$[p = 0.946]$
+ 3 controls			
Eradication	0.754	0.116	0.301
	(1.012)	(0.417)	(0.512)
	$[p = 0.456]$	$[p = 0.782]$	$[p = 0.556]$
Entropy balanced			
Eradication	0.352	-0.159	-0.098
	(1.042)	(0.388)	(0.471)
	$[p = 0.736]$	$[p = 0.683]$	$[p = 0.836]$
Num.Obs.	1,037	1,037	1,037
FE: Zone × Year × Erad-rate bin	Yes	Yes	Yes
Concurrent election control	Yes	Yes	Yes

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Exact p-values in brackets.
Robust SEs clustered at the electoral precinct level.

Table A4: Effect of eradication on PRI vote share: precinct-level results. Dependent variable is PRI votes as a share of all registered voters. All specifications include zone × year × prior-eradication-rate-bin fixed effects. The first row includes no individual controls; the second adds three linear controls: pre-election satellite-detected fields, schooling, and employment share; the third uses entropy balancing on the same three covariates.

A5.3 Scale Sensitivity

While the main results in Table 1 include a binary measure of any/no eradication that is not subject to concerns about the scale of the constant, one might worry that the results linking the number of fields and the number of eradicated hectares to lower turnout are artifacts of the logarithmic transformation used in coding these variables. To address this potential concern, Table A5 demonstrates consistency in the sign and direction of the effect of eradication (in hectares and count of fields) when using untransformed variables. Both the count of eradicated fields ($p = 0.020$) and the count of eradicated hectares ($p = 0.021$) are statistically significant at the 5% level.

	(1)	(2)
Destroyed fields (count)	-0.1610*	
	(0.0682)	
	$[p = 0.019]$	
Destroyed hectares (count)		-0.0788*
		(0.0335)
		$[p = 0.019]$
Num.Obs.	1037	1037
FE: Zone × Year × Erad-rate bin	Yes	Yes

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Robust SEs clustered at the electoral precinct level.

Table A5: Illicit-crop eradication and turnout in federal elections: precinct-level results with untransformed variables on the entropy-balanced sample. All specifications include zone × year × prior-eradication-rate-bin fixed effects and three controls: pre-election satellite-detected fields, schooling, and employment share.

To contextualize the size of the fields effect, when the eradicated field is used as a count, one additional eradicated field leads to a 0.15 percentage point decrease in turnout. Given that the mean eradication is 7.7 fields (median of 3), an increase to the mean level reduces turnout by approximately one percentage point.

A5.4 False-Positive Placebo Test

Table A6 reports the estimated effect of eradicating fields that the army's satellite program labeled as false positives (negative detections) on electoral turnout. If the fire-based algorithm were capturing general disturbance rather than eradication of genuine illicit fields, false-positive eradication should also predict turnout changes. Neither the binary indicator nor the log count of destroyed fields approaches conventional levels of statistical significance ($N = 284$). The two point estimates go in opposite directions—the binary measure is negative while the field count is positive—which is inconsistent with any systematic effect of false-positive fires on turnout and confirms that the algorithm captures real eradication rather than general disturbance.

	(1) Any eradication	(2) Log fields
No controls		
Eradication (false positive)	-0.977	0.783
	(1.996)	(1.738)
	$[p = 0.625]$	$[p = 0.653]$
+ 3 controls		
Eradication (false positive)	-1.115	0.789
	(2.006)	(1.860)
	$[p = 0.579]$	$[p = 0.672]$
Entropy balanced		
Eradication (false positive)	-2.137	0.544
	(2.192)	(1.779)
	$[p = 0.331]$	$[p = 0.760]$
Num. Obs.	284	284
FE: Zone × Year × Erad-rate bin	Yes	Yes

Robust SEs clustered at the electoral precinct level.

Table A6: Placebo test: effect of eradicating satellite-detected false-positive fields on turnout. Fields in this sample were investigated by the army and determined not to contain illicit crops. Specifications mirror Table 1 of the main paper.

A5.5 Detection Persistence and Eradication Frequency

Table A7 reports the cross-tabulation of detection persistence (years in which a precinct had at least one satellite-detected illegal crop field) and eradication frequency (years with predicted eradication) over the 2013–2020 period. The table shows that eradication is nearly inevitable for persistently detected precincts: only 1 of 73 precincts detected in all eight years was never eradicated. The variation that identifies the main results is therefore in the timing of eradication relative to federal elections, not in whether eradication occurs at all.

Table A7: Detection persistence and eradication frequency (2013–2020). Cell entries are precinct counts ($N = 712$ unique precincts).

	Years eradicated						
Years detected	0	1	2	3	4	5	6
1	83	13	0	0	0	0	0
2	65	30	12	0	0	0	0
3	37	27	18	2	0	0	0
4	30	45	21	9	1	0	0
5	21	32	21	13	7	0	0
6	10	19	22	15	9	0	0
7	4	17	18	14	12	10	2
8	1	8	7	21	15	13	8

A5.6 Rate-Bin Cutoff Robustness

The main specification splits precincts at a 50% prior eradication rate. Figure A5 shows the entropy-balanced coefficient on log destroyed fields as the cutoff varies from 10% to 90%. At the lowest cutoffs (10%–20%), nearly all precincts are classified as high-rate, the binary split is uninformative, and confidence intervals are wide. From 30% onward, the negative effect on turnout becomes statistically distinguishable from zero and remains stable across all higher cutoffs, suggesting the result is not an artifact of the chosen 50% threshold.

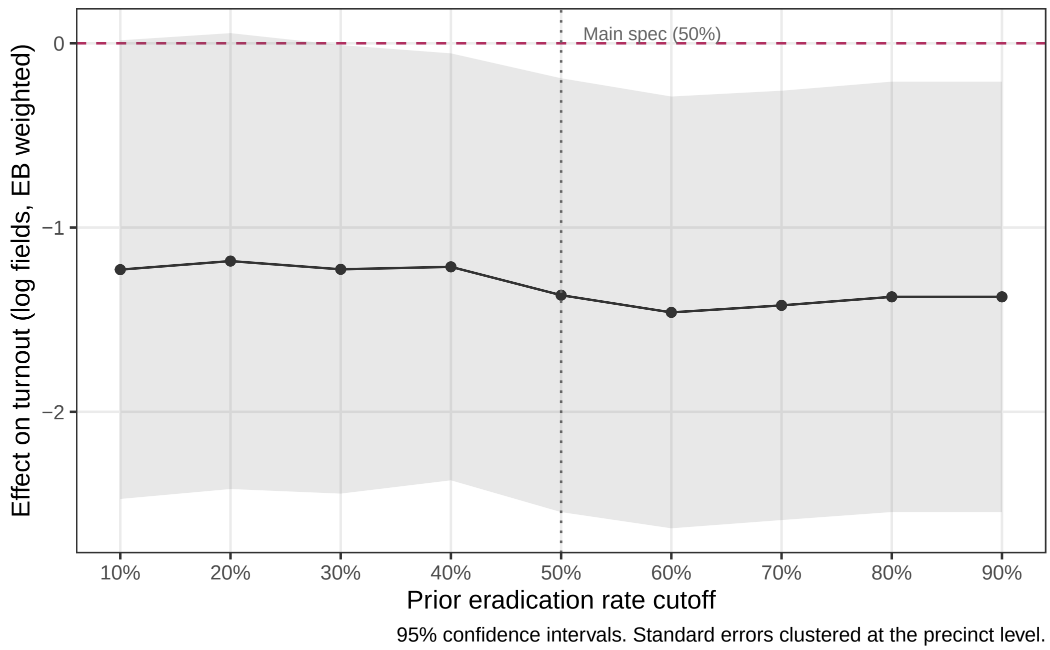

Figure A5: Entropy-balanced coefficient on any eradication (log fields) as the prior-eradication-rate cutoff varies from 10% to 90%. The dotted vertical line marks the main specification cutoff (50%). Shaded band shows 95% confidence intervals; standard errors clustered at the electoral precinct level.

A5.7 Balance Before and After Entropy Balancing

Table A8 reports covariate balance between treated and control precincts before and after entropy balancing on 3 covariates (Hainmueller, 2012). The top panel shows covariates included in the entropy balancing; by construction, their weighted means are equalized across treatment groups (SMD ≈ 0). The bottom panel shows political covariates not included in the balancing, which serve as a diagnostic for whether eradication is selective on politically relevant characteristics.

The preferred specification balances on log pre-election satellite-detected fields, schooling, and employment share. Treated precincts have substantially more detected fields (raw SMD $= +0.85$) and somewhat lower schooling and employment; entropy balancing eliminates all three imbalances exactly. Among the out-of-sample diagnostic covariates, prior turnout from 2012 and 2009 is more balanced after EB than before (raw SMD -0.11 and -0.09; EB SMD -0.05 and $+0.02$). Prior PRI vote share (2012) shows a modest EB SMD of -0.12, up from $+0.05$ raw.

A5.8 Within-Cell Distribution of Treated and Control Precincts

The identification relies on within-cell variation. Table A9 reports the number of treated and control precincts in each of the zone $\times$ prior-eradication-rate-bin $\times$ year cells that define the fixed effects structure. Cells with fewer than five treated or five control observations contribute little identifying variation and are flagged implicitly by the small cell sizes shown below. Most identifying variation comes from Durango, Guerrero, Sinaloa, and Chihuahua: the four military zones headquartered in those states account for 94.3% of all satellite-detected fields in the sample, reflecting that illicit crop cultivation in Mexico is heavily concentrated in the Golden Triangle and Guerrero.

Variable	Raw SMD	EB-weighted SMD
Covariates in entropy balancing		
Log detected fields	+0.85	+0.00
Schooling (sd)	−0.14	−0.00
Employment (sd)	−0.15	−0.00
Diagnostic covariates (not in EB)		
Prior turnout (2012)	−0.11	−0.05
Prior turnout (2009)	−0.09	+0.02
Prior PRI share (2012)	+0.05	−0.12

Table A8: Balance between treated (any predicted eradication) and control precincts, before and after entropy balancing on log detected fields, schooling, and employment share. SMD denotes the standardized mean difference (treated minus control, divided by pooled SD). Covariates in the top panel are balanced exactly by construction. Diagnostic covariates in the bottom panel are not included in the balancing.

	2015		2018	
Military zone	Treated	Control	Treated	Control
DURANGO, DGO.	18	124	76	88
GUADALUPE, ZAC.	–	–	2	2
TEPIC, NAY.	3	19	19	11
LA MOJONERA, JAL.	1	7	1	1
COLIMA, COL.	–	–	1	0
PIE DE LA CUESTA, GRO.	3	10	21	13
CHILPANCINGO, GRO.	38	49	71	125
HERMOSILLO, SON.	0	6	0	1
HIDALGO DEL PARRAL, CHIH.	6	81	46	42
MIAHUATLAN, OAX.	0	1	–	–
Culiacán, Sin.	14	66	36	35
Total	83	363	273	318

Treated = predicted eradication in the election year. Control = detected fields, no predicted eradication.

Table A9: Within-cell distribution of treated and control precincts. Each row is one military zone; cells with zero observations in both years are omitted. The main specification interacts zones with a binary prior-eradication-rate-bin split and election year. Most identifying variation comes from Durango, Guerrero, Sinaloa, and Chihuahua.

A5.9 Sector × Year × Eradication-Rate-Bin Fixed Effects

The main specification uses zone × year × eradication-rate-bin fixed effects, where zones are the primary command unit documented in SDN-MD-11. Below zones, the army organizes operations into sectors and subsectors, which are the finer units that actually execute field operations. Replacing zone fixed effects with sector fixed effects is therefore an interesting robustness check: it asks whether the result holds within the finest organizational unit directly responsible for eradication, absorbing any sector-level differences in operational capacity or targeting priorities that zone FE leaves unaddressed.

This specification has two limitations. First, sectors are smaller and more numerous than zones, so each sector-year-rate bin contains fewer observations; this mechanical loss of within-cell variation tends to attenuate estimates and inflate standard errors, particularly for the binary measure. Second, because sector commanders operate under the direction of zone commanders, sector-level operational decisions may partly reflect the same zone-level priorities absorbed by the main FE, meaning the two specifications are not fully independent. For these reasons, the zone-level specification is preferred.

Notwithstanding these limitations, Table A10 shows that coefficients remain consistently negative across all specifications. The hectares measure is marginally significant without controls ($p = 0.064$); once controls are added the estimates attenuate and lose significance, consistent with the reduced within-cell variation noted above. The direction of the results is stable across all columns, providing additional evidence that the main findings do not depend on the particular level of the army hierarchy used to define the comparison group.

	(1) Any eradication	(2) Eradicated ha (log)	(3) Fields (log)
No controls			
Eradication	-1.153	-0.794^{+}	-0.876
	(1.026)	(0.428)	(0.539)
	$[p = 0.261]$	$[p = 0.064]$	$[p = 0.104]$
+ 3 controls			
Eradication	-0.882	-0.676	-0.660
	(1.045)	(0.490)	(0.626)
	$[p = 0.399]$	$[p = 0.168]$	$[p = 0.292]$
Entropy balanced			
Eradication	-1.253	-0.799	-0.839
	(1.215)	(0.496)	(0.645)
	$[p = 0.303]$	$[p = 0.108]$	$[p = 0.194]$
Num.Obs.	1,037	1,037	1,037
FE: Sector × Year × Erad-rate bin	Yes	Yes	Yes
Concurrent election control	Yes	Yes	Yes

+ p < 0.1, * p < 0.05, ** p < 0.01. Exact p-values in brackets.

Unweighted. Robust SEs clustered at the electoral precinct level.

Table A10: Sector × year × prior-eradication-rate-bin fixed effects. “No controls” includes only the treatment variable and cell fixed effects. “+ 3 controls” adds pre-election satellite-detected fields, schooling, and employment share. Unweighted sample; robust standard errors clustered at the electoral precinct level.

A5.10 Misclassification

To benchmark a plausible proportion of misclassified units in the precinct-level analysis, I compare the estimated eradication measure with official geolocated data from the army on all eradication operations for 2019 and 2020. For each field detected between 2019 and 2020, I replicate the algorithm described in Section 4.3 but use reported eradication instead of NASA fire data to measure eradication. I compare the classification of all fields when eradication is predicted with NASA fire data to the classification when it is predicted with official army data. Importantly, the army eradicates fields that are detected using all techniques, not only via satellite. Thus, the estimated proportion of false positives is likely overstated. Benchmarking the fire-based measure of predicted eradication to reported eradication, I estimate the former measure to be 61% accurate. When aggregated into electoral precincts, I estimate a conservative proportion of 9.45% of false negative and 22.8% of false positive units.

Figure A6 shows how the distribution of the unadjusted binary dummy coefficient shifts as misclassification rates vary, using the zone × year × prior-eradication-rate-bin specification without entropy balancing. For each hypothesized misclassification rate, I draw 200 probabilistic reassignments of treatment status and re-estimate the model. The left panel varies the proportion of treated units misclassified as control (false negatives), holding false positives fixed; the right panel varies the proportion of control units misclassified as treated (false positives), holding false negatives fixed. The estimated effect remains negative and the distribution stays largely below zero up to approximately 40% misclassification in either direction.

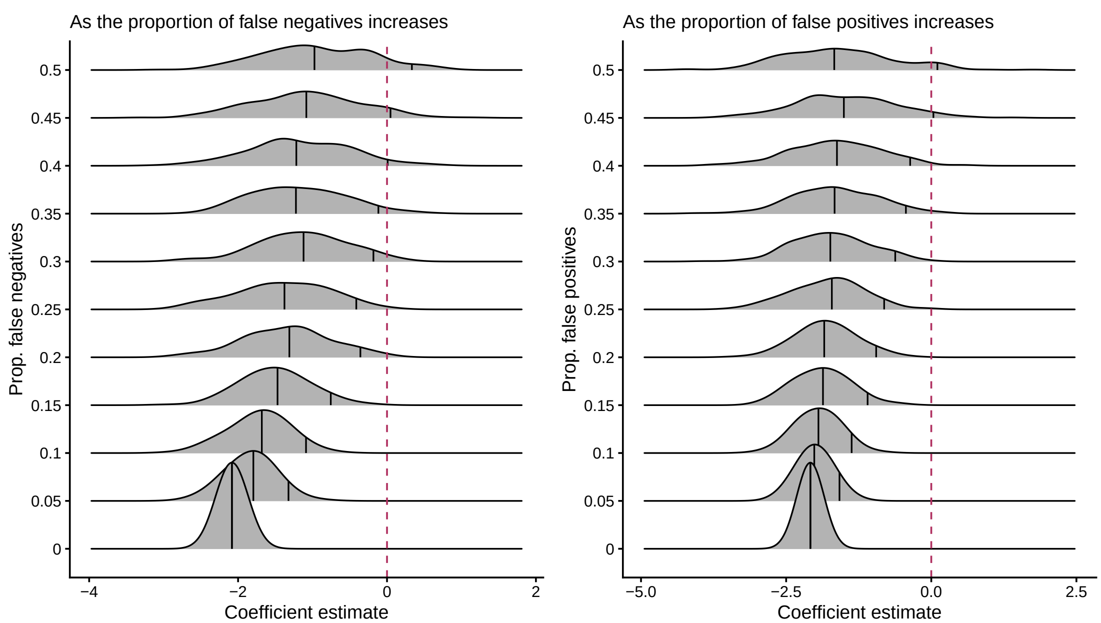

Figure A6: Sampling distribution of the unadjusted binary eradication coefficient under varying misclassification rates. Left panel: proportion of treated units misclassified as control (false negatives), with false positives held fixed. Right panel: proportion of control units misclassified as treated (false positives), with false negatives held fixed. Specification: zone × year × prior-eradication-rate-bin fixed effects, no entropy balancing. Each ridge shows 200 bootstrap draws.

A5.11 Geographic determinants of eradication

To assess whether geographic characteristics predict selection into eradication, I regress a binary indicator for eradication on a full set of geographic covariates—distance to military zone headquarters, altitude, paved road access, and land cover proportions (grasslands, agriculture, forest)—across three specifications: unconditional, within zone $\times$ year, and within zone $\times$ year $\times$ prior-eradication-rate-bin cells. The unit of observation is a detected field in the analysis sample (2015 and 2018 election years, $N = 15{,}939$). The distance coefficient is negative throughout, indicating that fields closer to headquarters are more likely to be eradicated, consistent with the logistical constraints of deploying troops to remote terrain. Table A11 shows that distance reaches conventional significance within zone-year cells ($p = 0.044$) but loses it once the prior-eradication-rate bin is added ($p = 0.110$). Most other geographic covariates that are significant unconditionally also lose significance within the triple-cell structure; the main exception is forest cover, which is marginally negative within cells ($p = 0.042$), suggesting the army slightly favors accessible, non-forested terrain even within otherwise comparable cells. The R^2 across all specifications remains very low (0.010 to 0.075), indicating that the full set of geographic characteristics explains little of the overall variation in which fields are eradicated. This confirms that the triple fixed-effects structure absorbs the geographic component of targeting and that the remaining within-cell variation is not systematically driven by terrain accessibility.

	Unconditional	Zone $\times$ Year FE	Zone $\times$ Year $\times$ Rate-bin FE
Distance to HQ	-0.005^{+}	-0.032^{*}	-0.026
	(0.003)	(0.016)	(0.016)
	$[p = 0.076]$	$[p = 0.044]$	$[p = 0.110]$
Altitude	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)
	$[p = 0.542]$	$[p = 0.562]$	$[p = 0.622]$
Paved road (dummy)	-0.010	-0.030	-0.030
	(0.021)	(0.020)	(0.019)
	$[p = 0.646]$	$[p = 0.129]$	$[p = 0.124]$
Grasslands/shrublands	0.544^{**}	0.302^{+}	0.273^{+}
	(0.161)	(0.164)	(0.163)
	$[p = 0.001]$	$[p = 0.065]$	$[p = 0.095]$
Agriculture	0.187	0.135	0.131
	(0.156)	(0.163)	(0.162)
	$[p = 0.230]$	$[p = 0.408]$	$[p = 0.419]$
Forest	-0.046^{+}	-0.048^{+}	-0.054^{*}
	(0.027)	(0.026)	(0.027)
	$[p = 0.085]$	$[p = 0.067]$	$[p = 0.042]$
N	15,939	15,939	15,939
R^2	0.010	0.071	0.075

$^{+}$ p < 0.1, * p < 0.05, ** p < 0.01. Exact p-values in brackets.
Unit of observation is a detected field in the 2015 or 2018 election year in the analysis sample.
Outcome is an indicator for eradication. Standard errors clustered at the electoral precinct level.
Distance in decimal degrees; altitude in meters.

Table A11: Geographic determinants of eradication: linear probability models with full set of geographic covariates.

Appendix F Alternative Explanations

A6.1 Income

In this subsection I consider the possibility that eradication operates on participation mechanically through changes in people's income.

To test, I first use the 2017 collapse of poppy prices. While poppy was selling for record prices between 2014 and 2017, its price fell by around 50% in 2018. I subset the precinct-level data on eradication and keep only electoral precincts detected illegal poppy fields. I define the treatment as the $(log + 1)$ number of poppy fields the algorithm predicts were eradicated before the elections or the $(log + 1)$ number of destroyed hectares. I plot the marginal effect of eradication on turnout for each of the two years in Figure A7. The effect is more precisely estimated for 2018 than 2015 because the army detected many more poppy fields in the former year than in the latter. The estimated effects are of comparable magnitude across years, and we cannot reject the null that the coefficients are the same. If the income channel drove the result, effects should be larger in 2015, when poppy prices were high and a destroyed field represented a greater economic loss. Instead, point estimates are slightly larger in 2018, the low-price year, which is the opposite of what an income-mediated mechanism predicts.

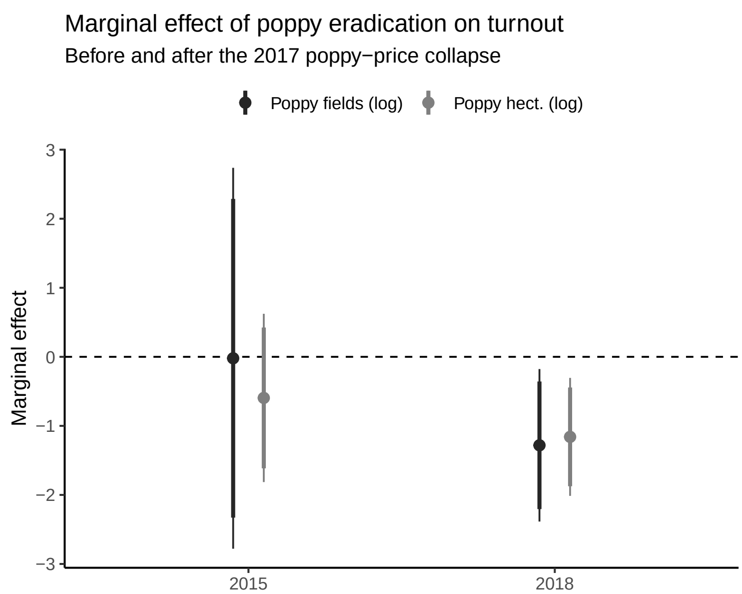

Figure A7: Figure plots the marginal effect per year of a model with turnout as a dependent variable and the log+1 number of eradicated poppy fields/hectares as the main independent variable. All controls and fixed effects are included. Only electoral precincts with detected poppy fields are included in the control group.

Additionally, I examine whether eradication affected economic activity at the precinct level by analyzing nighttime light data. Using luminosity as a proxy for economic activity, I compare precincts eradicated in 2015 to control precincts in the same military zones. I focus on 2015 since luminosity data at the precinct level ends in 2018 (Magar, 2021). I maintain the same covariate structure as in the main analysis, ensuring that the estimated treatment effects are conditional on the same set of demographic, geographic, and political controls. Figure A8 shows the results: there are no significant differences in luminosity between eradicated and non-eradicated precincts two years before or after treatment. Together with the price comparison analysis, these findings strongly suggest that while eradication certainly has economic consequences for growers, changes in income are not the primary channel through which eradication affects electoral turnout.

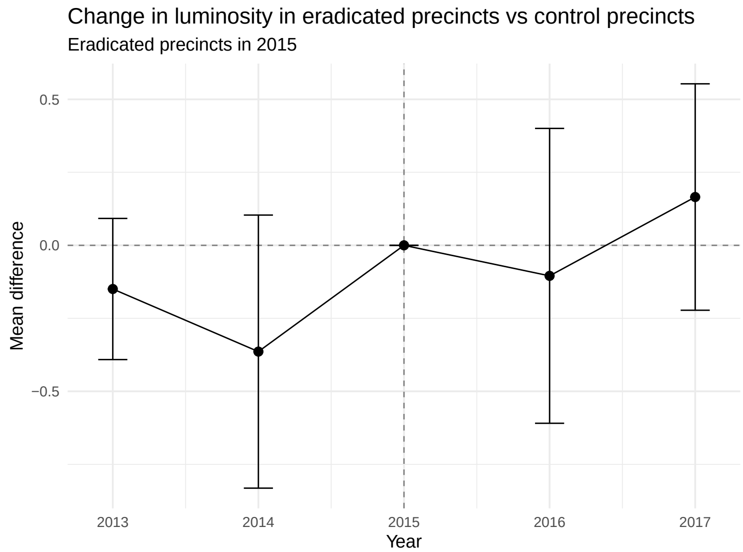

Figure A8: Figure plots the mean yearly difference in luminosity for precincts eradicated in 2015, relative to control precincts in the same military zone. Luminosity data at the precinct level comes from Magar (2021).

A6.2 Violence

A potential concern is that eradication affects turnout indirectly through changes in local violence rather than through the direct political mechanisms I propose. If drug trading organizations (DTOs) respond to eradication by attempting to capture new territory, or if weakened DTOs invite attacks from competitors, eradication might increase violence that subsequently depresses electoral participation. While DTOs may also respond with non-lethal violence like extortion, such phenomena are difficult to measure reliably in low-trust environments due to reporting issues. I therefore focus on homicide data, the best-measured crime, as well as firearm homicides, which is often considered a more precise indicator of DTO-related violence.

To test whether violence mediates the eradication-turnout relationship, I add lagged municipal homicide rates to my main municipal-level turnout specifications. For the coefficient to identify the mediated effect of violence on turnout, the sequential ignorability assumption must hold (Imai, Keele and Tingley, 2010): conditional on observed pretreatment covariates, eradication must be independent of all potential values of the outcome and mediating variables, and the observed mediator (violence) must be independent of all potential outcomes given the observed treatment and pretreatment covariates. While this assumption is strong, the exercise remains informative. If violence were the primary channel through which eradication affects participation, controlling for violence should substantially attenuate the eradication coefficients. Moreover, if eradication operates through violence, we would expect violence itself to depress turnout.

	Turnout (1)	Turnout (2)	Turnout (3)	Turnout (4)	Turnout (5)	Turnout (6)
Any eradication (dummy)	-1.620^{+}			-1.590^{+}		
	(0.937)			(0.937)		
Manually er. fields (log)		-0.475^{*}			-0.481^{*}	
		(0.233)			(0.233)	
Manually er. hects. (log)			-0.956^{**}			-0.972^{**}
			(0.342)			(0.342)
Firearm homicides (t-1)	0.021^{*}	0.021^{*}	0.020^{*}			
	(0.009)	(0.009)	(0.009)			
Firearm homicides (t-2)	0.000	0.000	0.000			
	(0.003)	(0.003)	(0.003)			
Total homicides (t-1)				0.015^{**}	0.015^{*}	0.015^{*}
				(0.006)	(0.006)	(0.006)
Total homicides (t-2)				0.001	0.001	0.001
				(0.003)	(0.003)	(0.003)
Observations	1,248	1,248	1,248	1,248	1,248	1,248
Adjusted R^2	0.659	0.660	0.662	0.659	0.660	0.662
Municipality FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A12: Illicit-crop eradication and turnout in federal elections for deputies: municipal-level results controlling for violence. Dependent variable measures turnout as the share of all registered voters in the municipality. Robust standard errors clustered at the municipality level are shown in parentheses. All models include municipality and year fixed effects.

Table A12 shows that lethal violence is unlikely to mediate the relationship between eradication and turnout. Across all specifications, eradication effects remain substantively unchanged when controlling for lagged municipal homicides and firearm homicides. The coefficients for eradication are nearly identical to those in Table 3, consistent with eradication affecting turnout through channels independent of municipal violence.

Muñiz-Sánchez, Fuerte-Celis and Méndez-Ramírez (2022) document a positive correlation between officially reported eradicated hectares and army-classified cartel homicides across municipalities in eight northern states selected precisely because they had the highest execution counts during the Calderón administration (2006–2011). The analysis pools all between-municipality variation in a region where eradication intensity and cartel presence are jointly determined by the same underlying drug-trafficking economy, with no controls for trafficking activity, cartel territorial control, or prior violence, and in a period and geography where cartel conflict was at its most intense. A positive coefficient on eradicated hectares in this setting is consistent with the army eradicating more where there are more drugs, and more drugs generating more cartel violence, with no causal role for eradication. At the municipal level, isolating pre-election from post-election eradication and controlling directly for homicide rates, I find no evidence that the turnout effect operates through this channel (Table A12).

This null result is consistent with Campos-Contreras, Nieto-Matiz and Schenoni (2025), who find that while aerial spraying of coca crops increases armed violence in Colombia, manual eradication does not — suggesting the violence mechanism is specific to the aerial modality that Mexico abandoned well before the period under study.

Moreover, both firearm homicides and total homicides show small but statistically significant *positive* associations with turnout, contradicting the hypothesis that violence depresses participation in these contexts. This finding aligns with research suggesting that security concerns can mobilize rather than demobilize voters (Ley, 2022), and with insights from my interviews with high-level Mexican politicians who emphasized that DTOs have shifted toward synthetic drugs, making crop eradication largely irrelevant to their operations.

Appendix G Trust

A7.1 Trust in family and neighbors

In this paper, trust in law enforcement agencies is characterized as a belief that is updated when people acquire new information about the authorities through eradication operations. The results would be biased if trust operated not as a belief but as a personal proclivity, whose distribution in the population covaries with the timing of crop eradication operations. If individuals living in municipalities eradicated before survey collection were more trusting, generally, than people living in municipalities eradicated after, then the results would be biased. Figure A9 shows that trust in family or neighbors is not the case. The timing of eradication is not correlated with differences in trust in either of these groups.

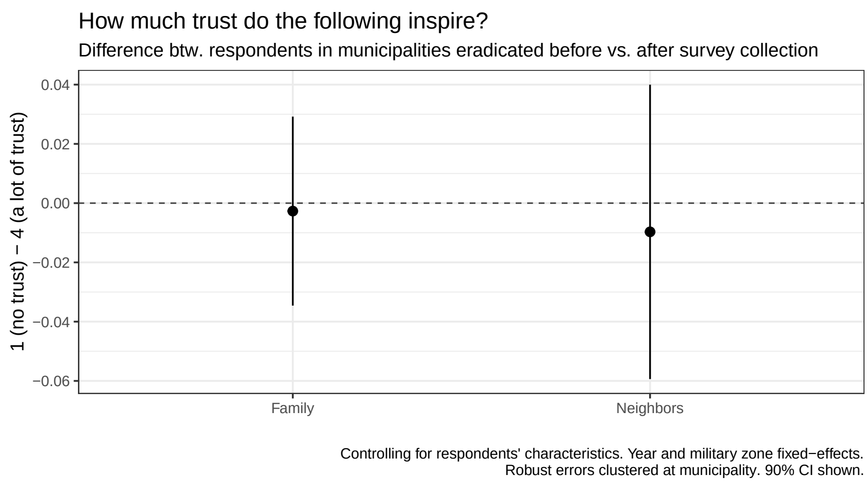

Figure A9: Difference-in-means in two measures of trust reported by ENVIPE respondents living in municipalities eradicated before vs after survey was collected.

A7.2 Measurement Concerns

In the paper, I show that rural respondents living in municipalities eradicated before survey collection report lower trust in federal authorities than those in municipalities eradicated after. I also demonstrate that urban dwellers in these same municipalities exhibit no such difference. However, a potential concern is that non-random missingness could bias the results. For instance, individuals affected by eradication might be less likely to answer trust questions for reasons related to eradication. Although this type of bias would likely attenuate the findings, pushing the results toward zero, I test for systematic non-reporting and present the results in Figure A10, finding that such non-reporting is unlikely to drive the results or meaningfully bias the findings.

Non-responses can arise in two different ways. First, respondents may not know the authority in question. The survey uses conditional questioning: only individuals who affirmatively report knowing each institution are subsequently asked about their trust in that institution. Second, even those who report knowing an institution may decline to rate it on trust. The proportion of respondents who know each institution by year is reported in the top left panel of Figure A10. The army is the best-known institution, with almost all respondents after 2013 reporting they can identify it. The Navy follows, then the state police and the federal police. The least known institutions are those related to criminal investigation and prosecution: the public ministry, the attorney general, and the judges. Importantly, there are no differences in identification trends across years between rural and urban respondents. The proportion of respondents who did not answer the trust question, despite knowing the institution, is reported in the top right panel. Generally, this type of missingness remains between 1% and 4% across all years and institutions.

Since non-random missingness can occur either through differential knowledge or differential willingness to report trust correlated with treatment, I test whether eradication at the municipal level correlates with either type of missingness in the bottom left panel of Figure A10. Eradication does not predict missingness for any authority among rural respondents, except the attorney general, which alleviates concerns about eradication changing reporting willingness. It does, however, predict responses for the army and Navy in the placebo group of urban respondents. For the army specifically, eradication makes it less likely that urban respondents failed to answer the trust question for any reason (lack of knowledge or unwillingness to disclose). Since the main results show eradication doesn't shift trust among urban respondents but does reduce missingness in Army trust questions, this suggests that "newly responding" urban residents have similar trust levels. Overall, results show low missingness levels that are uncorrelated with the treatment.

A related concern is that while eradication might not change missingness patterns, it could still alter the types of respondents who answer the survey, particularly across sociodemographic characteristics that correlate with trust. To assess such potential differences in sample composition, I test whether eradication is associated with meaningful differences in the age, education, and gender of respondents in the bottom right panel of Figure A10. I find that these differences are close to zero and not statistically significant.

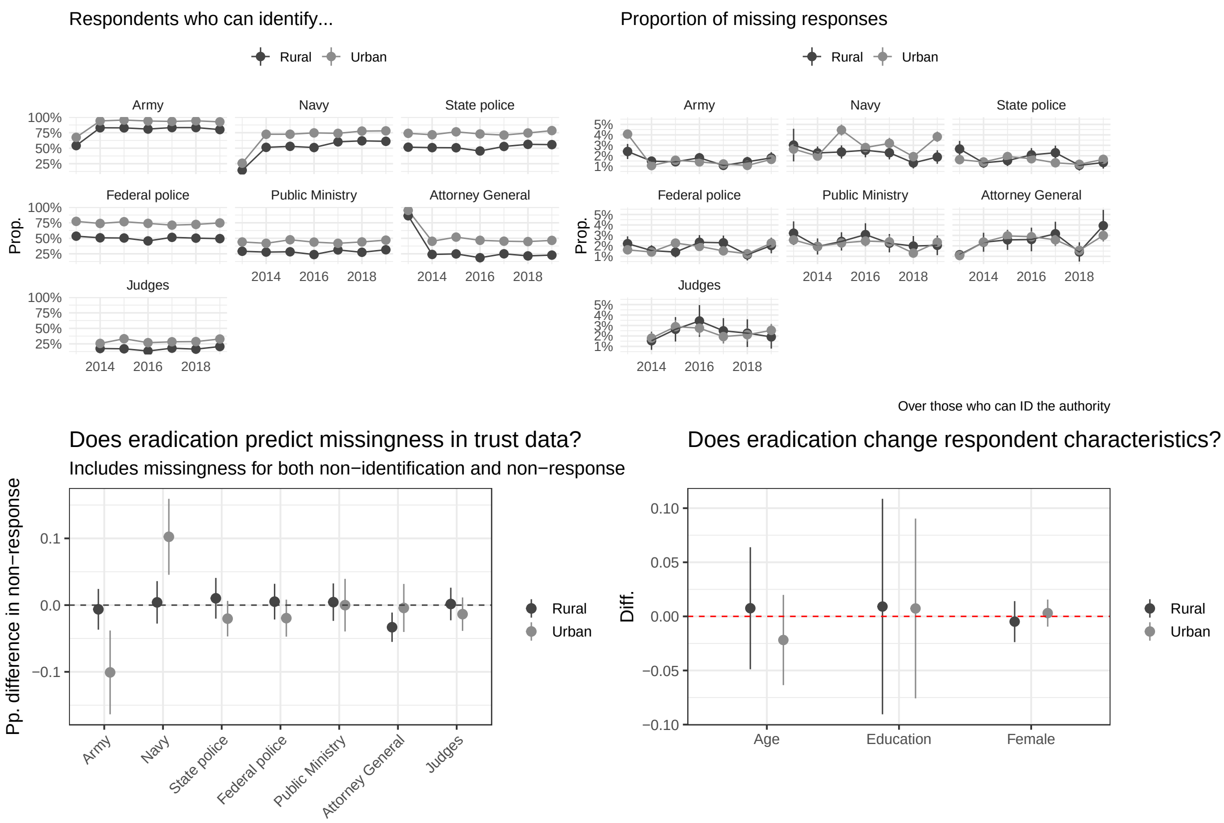

Figure A10: The top left panel shows the proportion of respondents (urban placebo in light gray and rural in dark gray) who reported being familiar with each institution, by year. The top right panel shows the proportion of those familiar with each institution who did not provide a valid response to institutional trust questions by authority and year. The bottom left panel shows the percentage point difference in the proportion of non-responses, either from lack of knowledge or unwillingness to disclose, for individuals in municipalities eradicated before versus after being surveyed. The bottom right panel examines whether eradication is associated with differences in respondent characteristics (age, education, and gender). All models for the bottom panels include municipality and year-fixed effects, with robust standard errors clustered at the municipality level.

Supplementary Appendix: References

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