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

August 25, 2025

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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.

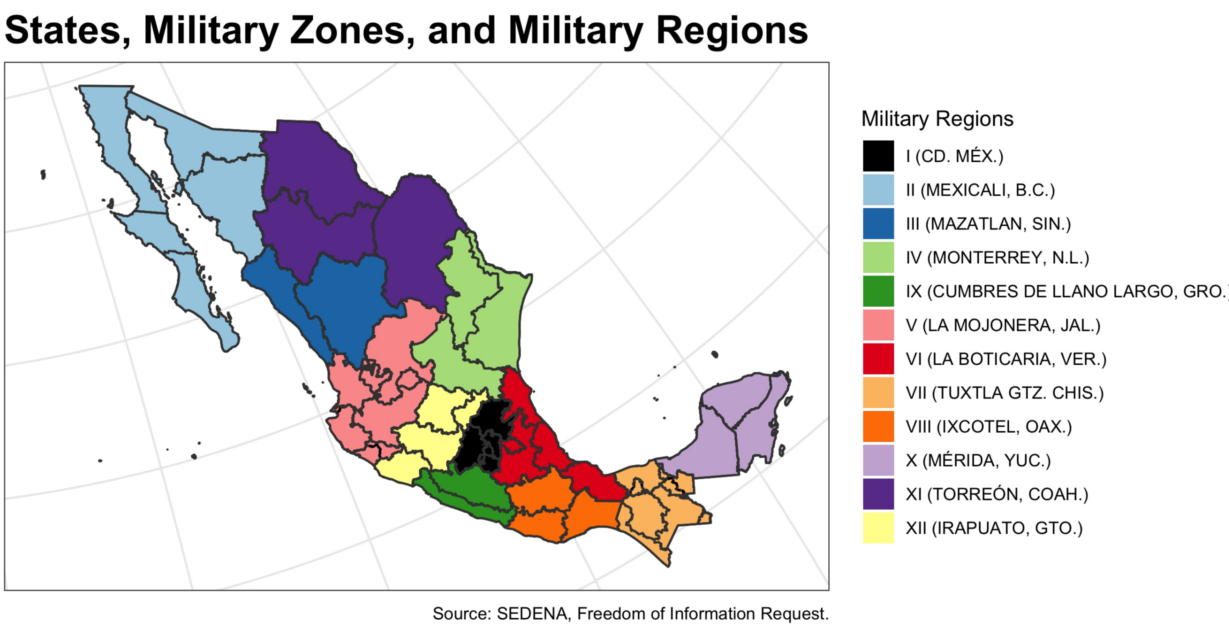

Figure A1: The map colors municipalities by the military region to which they belong and shows the borders of military zones in black.

Appendix B The Drug-Trafficking Chain

Growers, not drug-trading organizations (DTOs), often own the illicit 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.

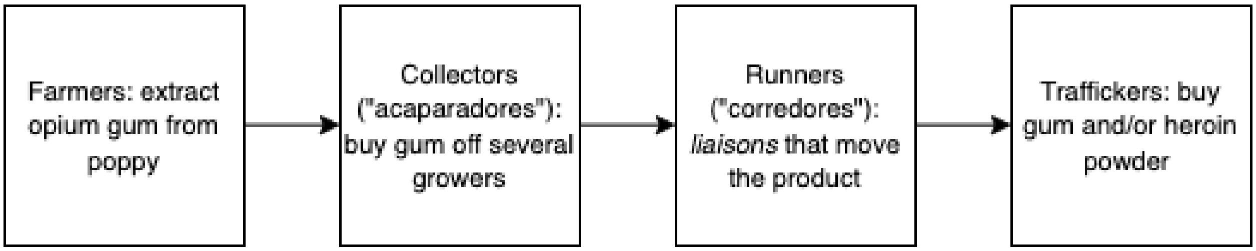

Figure A2: The figure describes the drug-trafficking process, starting from the selling of poppy farming in a town of the Sierra de Guerrero, as described in Álvarez Rodriguez (2021a).

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 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.

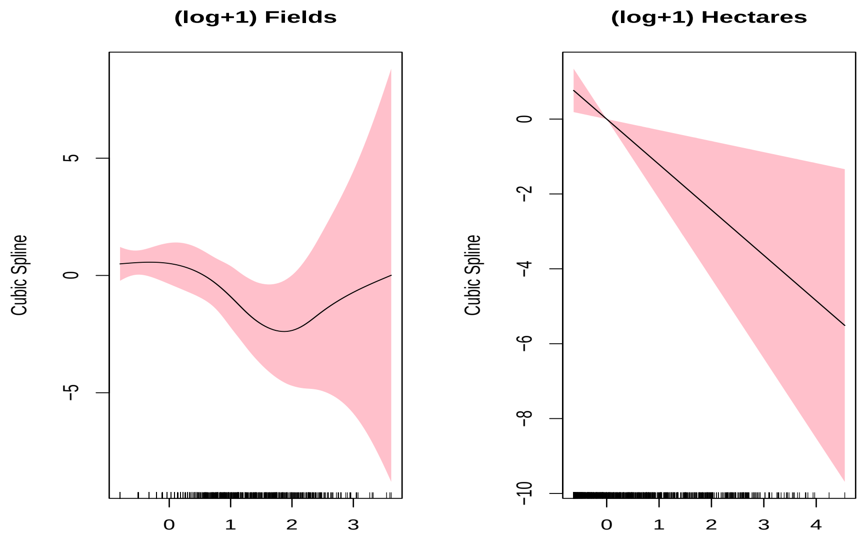

Figure A3: Figure shows the result of fitting smoothing cubic splines with 10 knots on a model with hectares or fields as the independent variable and turnout as the dependent variable.

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 illicit 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 section 4.3 of the main paper. Table A5.1 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 illicit crop field, Table A5.1 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 A5.1 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 fixed effects, as well as a battery of controls. So, to consider whether we might be concerned about a reversal to the mean dynamic, first, I residualized turnout with respect to the zone $\times$ year 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 =

1.46, p = 0.15, 95% CI: [-0.43, 2.93]). 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 illicit 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.

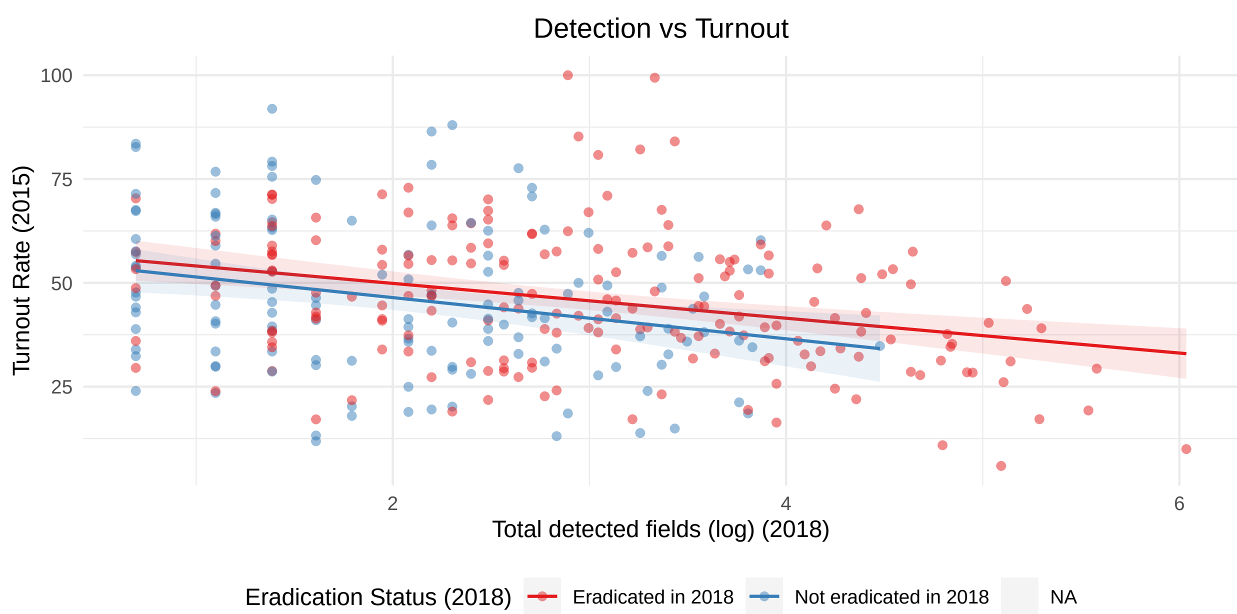

Figure A4: Figure shows the relationship between turnout rate in 2015 and the log count of detected fields in the same precinct in 2018, with colors indicating whether the precinct was eradicated (red) or not eradicated (blue) in 2018.

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

	(1)	(2)	(3)
Any eradication (dummy)	1.786+		
	(0.978)		
Destroyed fields (log)		0.273	
		(0.477)	
Destroyed hectares (log)			−0.587+
			(0.351)
Num.Obs.	1039	1039	1039
R2 Adj.	0.198	0.195	0.197

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Effect of Eradication on PRI Vote Share: precinct-level results. Dependent variable measures vote share as the share of votes for PRI of all registered voters in the electoral precinct. Robust standard errors clustered at the electoral precinct

examine for potential electoral consequences of eradication.

As shown in Table A4, the coefficients vary across specifications and, contrary to the results for turnout, do not follow a systematic pattern: positive for the eradication dummy and log fields models but negative for the log hectares model. Importantly, none of these coefficients are statistically significant at conventional 95% confidence levels. The evidence suggests that the vote share for the PRI did not significantly change due to eradication. Instead, fewer voters participated overall, regardless of their party preferences, which is consistent with the demobilization hypothesis.

A5.3 Scale Sensitivity

While the main results in Table 2 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. While the effects of the count of hectares (the most error-prone of the satellite measures) are smaller in magnitude and not statistically significant, the effects of the count of eradicated fields remain robust and significant.

To contextualize the size of the latter effect, when the eradicated field is used as a count, one additional eradicated field leads to a 0.2 percentage point decrease in turnout. Given that the mean eradication is 7.7 fields (median of 3), an increase to the median level reduces turnout by almost one percentage point, which represents a meaningful effect on electoral participation.

	(1)	(2)
Destroyed fields (count)	-0.193^{**}	
	(0.060)	
Destroyed hectares (count)		-0.010
		(0.006)
Num.Obs.	1039	1039
R2 Adj.	0.405	0.399

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

Table A5: Illicit-crop eradication and turnout in federal elections: precinct-level results with untransformed variables. Robust standard errors clustered at the electoral precinct level.

A5.4 Results with municipal fixed effects

Table A6 replicates the precinct-level analysis from section 6.1 in the main paper but substitutes the military-zone fixed effects with municipal fixed effects. While this specification is very stringent, as most of the variation comes from within military zones, the magnitude and direction of the results corroborate that eradication depresses turnout, albeit estimated more imprecisely.

	Turnout (1)	Turnout (2)	Turnout (3)
Any eradication (dummy)	-0.503		
	(1.065)		
Destroyed hectares (log)		-0.496	
		(0.349)	
Destroyed fields (log)			-0.782
			(0.530)
Num.Obs.	1039	1039	1039
R2	0.596	0.597	0.597
R2 Adj.	0.530	0.531	0.531
Fixed effects: Year x Municipality	Yes	Yes	Yes

Cluster-robust standard errors shown in parentheses.
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table A6: Illicit-crop eradication and turnout in federal elections for deputies: precinct-level results. The dependent variable measures turnout as the share of all registered voters in the precinct. Robust standard errors clustered at the precinct level.

A5.5 ICWa construction

I construct the following inverse covariance weighted average that synthesizes a battery of sociodemographic characteristics. For each electoral precinct i, the ICW will equal

$$ICW_i = (\mathbf{1'\hat{\Sigma}^{-1}1})^{-1}(\mathbf{1'\hat{\Sigma}^{-1}}\boldsymbol{X_i})^{-1}$$

Where $\boldsymbol{X_i}$ is a vector of the following standardized precinct-level covariates: mean school achievement, the share of employed residents, the share of residents who can read and write, the share of dwellings that have T.V., the share of dwellings that have internet, the share of dwellings without a dirt floor, share of homes headed by a man, share of residents with healthcare. $\mathbf{\hat{\Sigma}^{-1}}$ is the inverted covariance matrix, and $\mathbf{1}$ is a column vector of 1's (Anderson, 2008).

A5.6 Results with Additive Controls

In the main specification, I use an ICW index to summarize variation in a battery of sociodemographic controls meant to refine the causal contrasts: only similar precincts in the same year and military zone are compared to each other. While using an index is an efficient solution to control for these characteristics while maintaining statistical power, one might worry that the results do not hold if covariates are included additively instead of through the index. To address this concern, I rerun the precinct-level analysis with linear inclusion of all covariates and present the results in Table A5.6. As expected from the loss of statistical power, the results are more imprecisely estimated across the board and somewhat attenuated in magnitude. However, the direction remains the same for all three measures of eradication, and the continuous measure of eradicated fields remains significant and negative, buttressing the validity of the main findings. The consistency of these results provides additional confidence that the demobilizing effects of eradication are not an artifact of how the control variables are specified in the empirical model.

	(1)	(2)	(3)
Any eradication (dummy)	−0.960		
	(0.957)		
Destroyed hectares (log)		−0.566	
		(0.378)	
Destroyed fields (log)			−1.194*
			(0.522)
Num.Obs.	1039	1039	1039
R2 Adj.	0.427	0.428	0.430

Cluster-robust standard errors shown in parentheses.
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A7: Illicit-crop eradication and turnout in federal elections for deputies: precinct-level results. Robust standard errors clustered at the precinct level. Results from models where all pre-treatment sociodemographic precinct-level covariates are introduced additively instead of summarized with an ICW index

A5.7 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.

To test the robustness of the results to the inclusion of false negatives and false positives, I first assess the sensitivity of the results to each, independently and then simultaneously. To start, I assign control/treatment units to treatment/control probabilistically by sampling 500 new outcomes from Bernoulli processes with a probability of success equal to the hypothesized shares of each type of misclassified unit. Next, I re-estimate the model 500 times, each using one of the 500 new probabilistically-drawn outcomes. Figure A7 shows the results of the simulation. The top panel shows the sampling distribution of the difference-in-means estimator, assuming the benchmarked proportions of false positives and negatives: 9.45% and 22.8%, respectively. Results show that the difference in means, although overstated, is still statistically significant under this assumed misclassification proportion. The left panel shows the sampling distributions of the difference-in-means estimator under different assumptions of the “true” proportion of false negatives, holding false positives fixed. The right panel shows the corresponding distributions as the “true” proportion of false positives changes, holding the proportion of false negatives fixed. Results show that the “true” estimated effect is statistically different from zero with up to 40% of misclassified control units or 45% of misclassified treatment units.

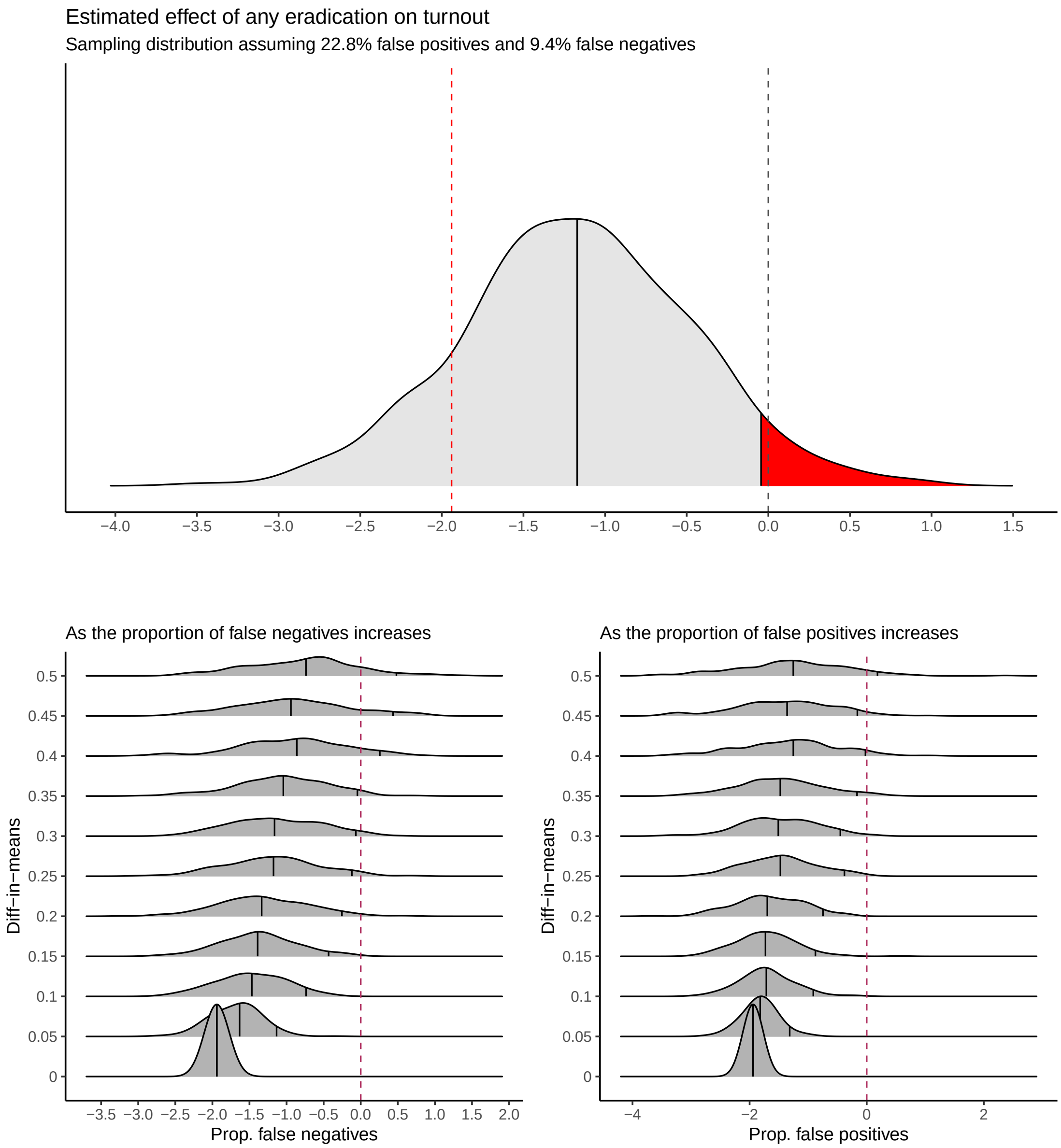

Figure A5: The top panel plots the estimated sampling distribution of the effect of eradication on turnout, assuming 22.8% of the observations classified as 'eradicated' are false positives and 9.4% of the observations classified as 'not eradicated' are false negatives. The bottom left panel shows the sampling distribution of the difference-in-means estimator as the proportion of false negatives changes, holding false positives fixed. The bottom right panel show the sampling distribution of the difference-in-means estimator as the proportion of false positives changes, holding false negatives fixed.

A5.8 Geographic determinants of eradication

In this subsection, I present the results of the predictive exercise detailed in section 6.1.2 of the main paper. To test for the possibility of strategic eradication, motivated by geographic characteristics, I model the probability θ that illicit field i in electoral precinct p was counted as eradicated as follows:

$$\theta_{i[p]} = g^{-1}(\gamma DistanceToArmy_i + \beta \boldsymbol{X}_p + \mu_t + \theta_z)$$

Where $DistanceToArmy_i$ is the distance from illicit field i to the corresponding military zone's headquarters in decimal degrees, X_p is a vector of precinct-level covariates, including the proportion of precinct p's surface area that is occupied by grassland, agriculture, forest, and human settlements, and a dummy variable that takes the value of one if any paved roads pass through the electoral precinct and zero otherwise, μ_t are year fixed-effects, θ_z are military zone fixed-effects, and $g(.)$ is the logistic link function.
Table A8 shows this model's confusion matrix. Geographic characteristics do a very poor of predicting eradication: only 0.13% of all eradicated fields are correctly predicted to be eradicated, lending credence to the identifying assumption.

	Destroyed (DV)	Not destroyed (DV)
Destroyed (Fitted)	9	13
Not destroyed (Fitted)	6507	27338

Table A8: Do geographic characteristics predict field eradication? Confusion table from predicting eradication using the geographic characteristics of detected fields.

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 illicit 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 A6. 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. However, the estimated effects are of comparable magnitude, and we cannot reject the null that the coefficients are the same with 95% confidence. Further, contrary to what we would expect if the loss of income drove the effects, the point estimates for 2015 are less negative than in 2018 for both cases, suggesting that the negative economic shock of eradication cannot explain the results, at least in isolation.

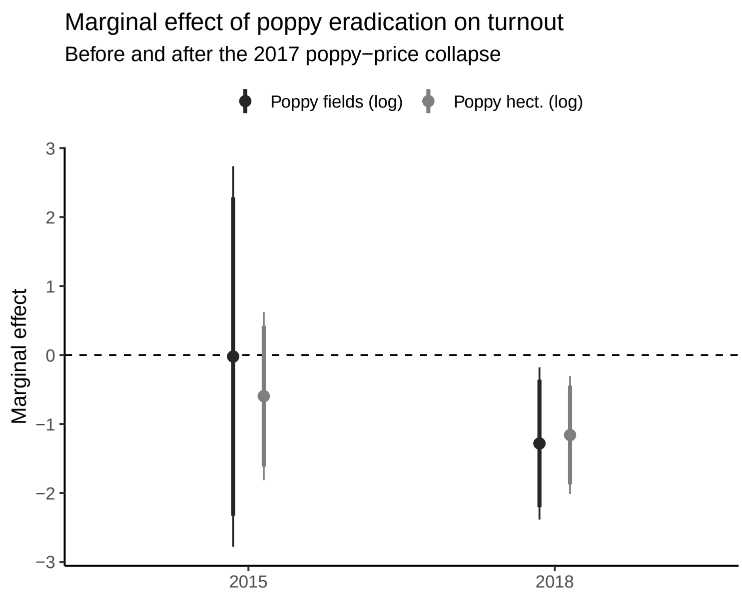

Figure A6: Figure plots the marginal effect per year of a model with turnout as a dependent variable and the log+1 number of eradicated poppy fields/hectares as the main independent variable. All controls and fixed effects are included. Only electoral precincts with detected poppy fields are included 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. Put simply, make sure to keep the causal contrasts the same. Figure A6.1 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.

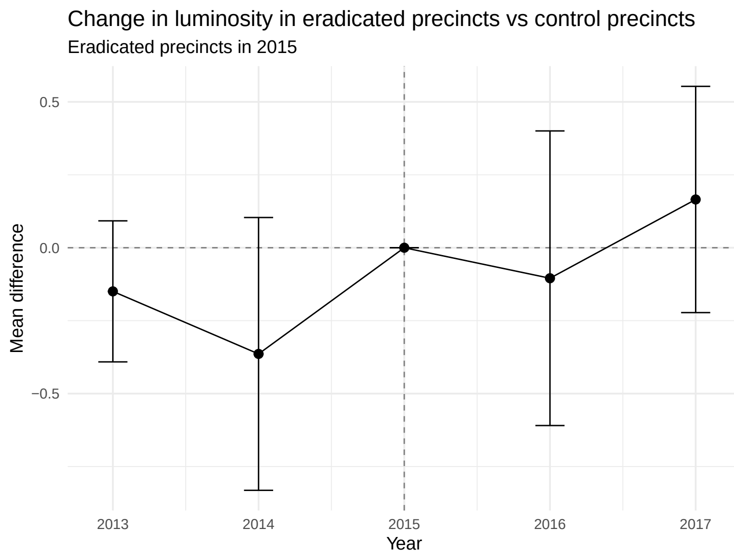

Figure A7: Figure plots the mean yearly difference in luminosity for precincts eradicated in 2015, relative to control precincts in the same military zone. Luminosity data at the precinct level comes from Magar (2021).

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 A9: 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 A6.2 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. 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 A8 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.

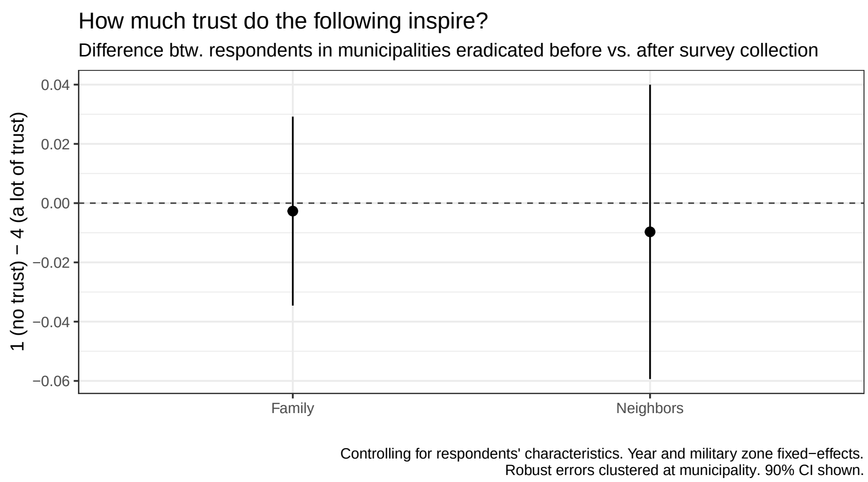

Figure A8: Difference-in-means in two measures of trust reported by ENVIPE respondents living in municipalities eradicated before vs after survey was collected.

A7.2 Figure 3: Full Results

Table A10 shows the full results of the difference-in-means in self-reported trust in law enforcement institutions, plotted in Figure 3.

A7.3 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 A9, 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

DV:Trust in...	Diff-in-means rural	Female rural	Education rural	Age quintile rural	Diff-in-means urban	Female urban	Education urban	Age quintile urban
Army	-0.052 [0.028] n = 19520	-0.241 [0.016] n = 19520	0.005 [0.005] n = 19520	-0.014 [0.006] n = 19520	0.000 [0.041] n = 49970	-0.220 [0.017] n = 49970	-0.029 [0.003] n = 49970	-0.010 [0.005] n = 49970
Navy	-0.067 [0.041] n = 12443	-0.238 [0.023] n = 12443	0.012 [0.005] n = 12443	-0.020 [0.008] n = 12443	0.021 [0.068] n = 35792	-0.192 [0.0156] n = 35792	-0.015 [0.002] n = 35792	-0.018 [0.005] n = 35792
Federal police	-0.057 [0.039] n = 12418	-0.111 [0.022] n = 12418	-0.010 [0.005] n = 12418	-0.0351 [0.008] n = 12418	-0.001 [0.035] n = 42468	-0.108 [0.0132] n = 42468	-0.030 [0.002] n = 42468	-0.0419 [0.004] n = 42468
State police	-0.004 [0.038] n = 12774	0.040 [0.018] n = 12774	-0.022 [0.005] n = 12774	-0.038 [0.007] n = 12774	-0.003 [0.039] n = 42215	0.041 [0.015] n = 42215	-0.035 [0.003] n = 42215	-0.0189 [0.007] n = 42215
Attorney General	-0.000 [0.041] n = 7632	-0.058 [0.025] n = 7632	-0.010 [0.007] n = 7632	-0.040 [0.010] n = 7632	0.050 [0.058] n = 32612	-0.100 [0.020] n = 32612	-0.034 [0.003] n = 32612	-0.040 [0.008] n = 32612
Public Ministry	-0.047 [0.036] n = 6943	0.000 [0.021] n = 6943	-0.030 [0.006] n = 6943	-0.050 [0.009] n = 6943	-0.020 [0.044] n = 2507	0.052 [0.015] n = 2507	-0.036 [0.003] n = 2507	-0.057 [0.005] n = 2507
Judges	-0.045 [0.057] n =3658	-0.017 [0.029] n =3658	-0.025 [0.009] n =3658	-0.040 [0.014] n =3658	0.069 [0.039] n = 12846	0.027 [0.015] n = 12846	-0.023 [0.004] n = 12846	-0.035 [0.009] n = 12846

Table A10: Table corresponds to Figure 3 in the main paper. Robust standard errors clustered at the municipality are shown in brackets. The dependent variable is the standardized response to the question "How much trust do the following authorities inspire?" measured on a 1-4 scale. Columns show the estimated coefficients for each of the individual-level covariates used in the adjustment, along with the difference-in-means in self-reported trust in each authority for people living in municipalities eradicated before vs. after the survey was collected. Columns labeled "rural" show the results of models fitted only with rural respondents, while columns labeled "urban" show the results of models fitted exclusively with urban respondents. All models include year and municipality-fixed effects.

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 A9. 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 A9. 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 A9. I find that these differences are close to zero and not statistically significant.

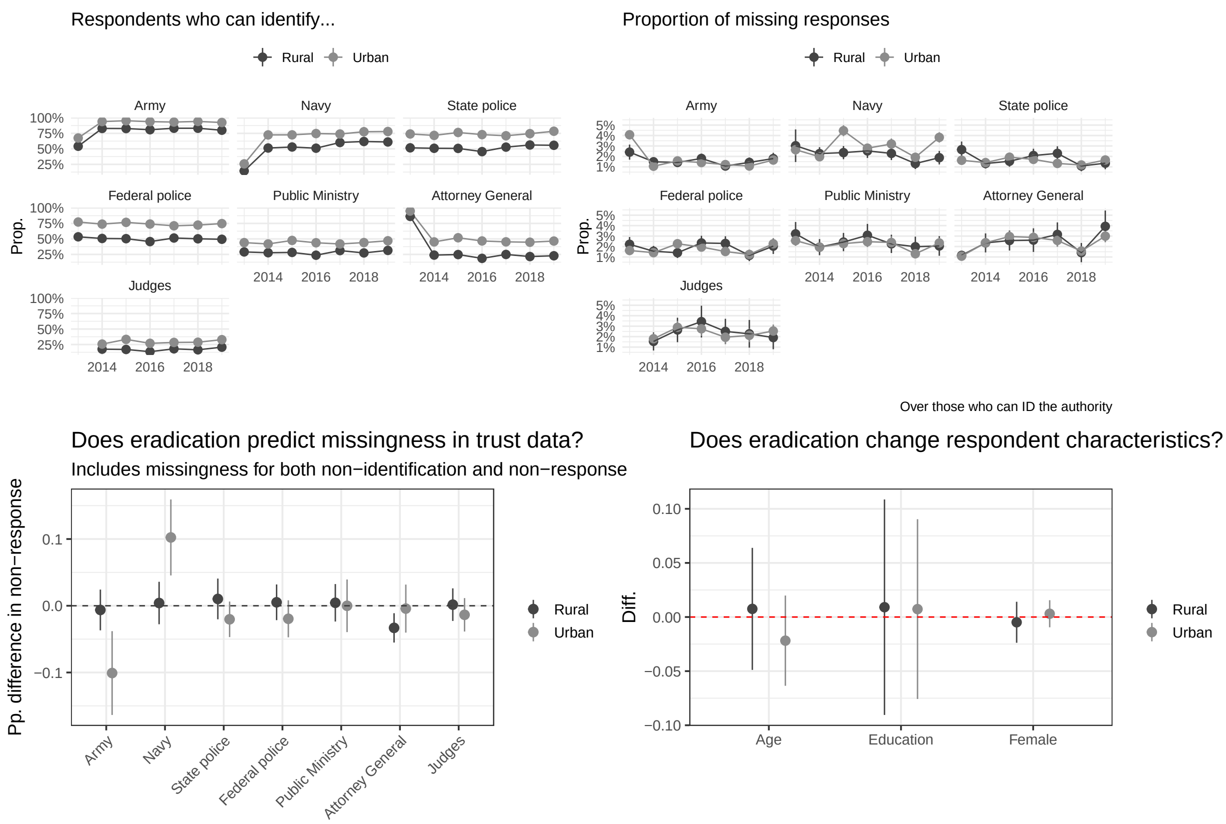

Figure A9: The top left panel shows the proportion of respondents (urban placebo in light gray and rural in dark gray) who reported being familiar with each institution, by year. The top right panel shows the proportion of those familiar with each institution who did not provide a valid response to institutional 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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