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

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Abstract

Drug-producing, or source countries, routinely destroy illegal crops under international pressure to curb drug supply, despite the high costs imposed on crop-growing communities. I study the domestic political consequences of crop eradication in Mexico. The Mexican army incinerates thousands of fields yearly in a context shaped by US counternarcotics pressure. I argue that, for the marginalized communities that bear its costs, eradication erodes trust in the federal government and depresses turnout. To test, I build a novel precinct-level measure from 50,000 satellite-detected fields and NASA fire data. I compare precincts where detected fields were eradicated before elections to similar precincts in the same military zone with comparable pre-election detected cultivation. I show that eradication depresses turnout and, exploiting the timing of a national survey, erodes trust in federal security institutions. International pressure that keeps coercive policy off the electoral menu may weaken accountability by pushing affected citizens toward political withdrawal.

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For over six decades, the United States has funded and promoted the destruction and interdiction of illegal substances abroad as part of its drug abuse prevention strategy (Bagley, 2015; Cedillo, 2021). Such "source country" counternarcotics tactics are motivated by the idea that reducing the supply of imported drugs should, in turn, mechanically reduce consumption (Tokatlian, 2015; Isacson, 2015). To incentivize foreign governments to carry out these costly operations, the United States conditions millions of dollars in aid to major drug-producing or drug transit countries on an annual certification extended only to those that "cooperated fully with the United States in drug control efforts" (Storrs, 2002). While source-country operations are ineffective at reducing supply in the long run (Moreno Sanchez, Kraybill and Thompson, 2003; Mejía, Restrepo and Rozo, 2015; Prem, Vargas and Mejía, 2023), drug-producing countries gain favor with the US by consistently carrying them out, thus facilitating their yearly certification.

In this paper, I examine the domestic political consequences of a paradigmatic example of US-sponsored source country operations: illegal crop eradication in Mexico. Extant research has conceptualized law enforcement policy as endogenous to domestic politics, responding to electoral incentives and preexisting social inequalities (Magaloni, Franco-Vivanco and Melo, 2020; Kronick, 2014; González and Mayka, 2022; Visconti, 2020; Holland, 2013). In contrast, conditional US aid creates incentives for recipient governments to prioritize foreign security preferences over domestic constituencies. This dynamic is particularly salient in Latin America, where drug production and trafficking are closely monitored by the US (DEA, 2021). Indeed, of the 22 countries the US government identified as major drug transit or illegal drug-producing nations in 2022, 17 are in Latin America or the Caribbean.[1]

Drawing from ethnographic and journalistic sources, I argue that crop-growing communities experience eradication—carried out by the Mexican army—as the federal government behaving punitively towards them by interfering with their already precarious well-being (Le Cour Grandmaison, 2021; Álvarez Rodriguez, 2021*b*; Alvarez, Gaussens and Frissard, 2022). As one poppy

[1] Presidential Determination on Major Drug Transit or Major Illicit Drug Producing Countries for Fiscal Year 2021. Available at `federalregister.gov`.

farmer in Guerrero's Sierra put it: "We are not the bad guys, we are the poor ones."[2] I conceptualize institutional trust as the belief that a representative government agent will tend to act in one's interest (Bhattacharya, Devinney and Pillutla, 1998; Hardin, 2003) and follow work positing that the quality of interactions with state agents informs this belief (Blair et al., 2021; Abril et al., 2024; Slough and Torreblanca, 2024). The coercive nature of eradication-spurred interactions should therefore lead people to update their beliefs about the trustworthiness of the federal government and the bureaucracies responsible for eradicating illegal crops downwards.

I further expect eradication to depress turnout in federal elections rather than engendering electoral backlash. While aggrieved eradicated-crop growers may be motivated to incentivize change in federal counternarcotics policy through electoral means, they lack the opportunity to do so. The importance of US security aid, coupled with the political weakness of crop-growing communities, effectively excludes eradication from the menu of reformable policy—a dynamic confirmed by elite interviews across three administrations. For the federal government, the financial benefits of receiving lucrative foreign aid outweigh the costs of angering and alienating small, poor, crop-growing communities. Elite interviews confirm this political calculus; an ex-presidential Chief of Staff explained, "In high-level decision-making, the bilateral relationship and systemic benefits were prioritized over micro-level impact. These impacts were recognized but managed as collateral damage."[3] Thus, no political force has an incentive to propose reforming counternarcotics policy, as doing so risks losing the foreign country's favor and financial backing. Unable to channel their discontent through vote choice, I expect residents of eradicated communities to be more likely to abstain from voting.

To test these hypotheses, I develop a novel precinct-level measure of eradication. The army most commonly destroys illegal fields through incineration. Accordingly, my measure integrates satellite imagery of over 50,000 illegal crop fields detected by the Mexican army with NASA fire records that indicate the timing and location of field incinerations. Each season, the army detects more fields than it can eradicate. I therefore compare electoral precincts in which detected fields

[2] Jan Martínez Ahrens, "La ley del opio en las montañas de Guerrero," *El País*, June 19, 2016.
[3] Interview with the author, May 2025.

were eradicated to those in which fields were detected but not eradicated, conditional on the detected fields being of comparable size. This comparison is made within the same military zone, during the same election year, and only among sections with similar prior eradication-to-cultivation rates. To reduce model dependence and ensure covariate overlap, I pre-process the sample using entropy balancing (Hainmueller, 2012) on pre-election satellite-detected fields, schooling, and employment share. I supplement the precinct-level analysis with a municipal-level analysis that draws on official army eradication records and leverages within-municipality variation across a four-election panel (2009 to 2018).

First, the results show that eradication reduces trust in the federal security apparatus. Leveraging the timing of survey collection across seven waves of an annual nationally representative survey and official eradication data published by the army at the municipality level, I show that rural dwellers in municipalities eradicated before survey collection report significantly lower trust in the army, the navy, and the federal police than individuals in comparable areas eradicated after survey collection. However, eradication does not affect trust in non-federal or non-policing institutions, nor does it affect urban inhabitants who do not personally observe eradication operations.

I further show that eradication before a federal election reduces turnout in affected precincts relative to similar, detected-field precincts with comparable cultivation intensity in the same military zone and election year. At the extensive margin, any eradication reduces turnout by 2.4 percentage points; at the intensive margin, the median eradicated precinct loses approximately 1.9 percentage points, or 12% of a standard deviation. A within-municipality panel using official army eradication data across four elections confirms the direction and magnitude, reducing concern that the result is an artifact of any single identification strategy or imperfect measure.

A natural concern when interpreting the empirical analysis causally is that the army strategically chooses where to eradicate in ways that covary with turnout. That the army is strategic in general is almost surely true: its eradication protocol explicitly lists geography, weather, and logistical capacity among the factors determining which detected fields are scheduled for destruction. But broad strategic targeting is not the relevant design concern. The relevant theoretical counterfactual is not random assignment of eradication across space, but whether, among precincts with detected illegal cultivation subject to similar enforcement conditions, those eradicated before the election would otherwise have exhibited turnout similar to comparable precincts not eradicated before the election. The identifying assumption is therefore conditional: within military zone, election year, and eradication-history cells, and among precincts with comparable detected cultivation and baseline characteristics, pre-election eradication is unrelated to potential turnout.

Consequently, the threat to identification is the narrower claim that within these cells, the army systematically selects the most politically disengaged precincts. Several pieces of evidence counter the argument that pre-existing disengagement drives the results. First, the army eventually eradicates almost every precinct where illegal crops were detected (Table A7), so the variation exploited by the design is not between communities the army targets and those it spares, but in the timing and intensity of eradication within zone-year-prior-eradication-rate-bin cells, among precincts with comparable cultivation intensity and eradication histories. If the army nevertheless selected within these cells on the basis of political disengagement, precincts that were later eradicated should have exhibited lower turnout before being eradicated. However, differences in pre-treatment turnout between treated and control precincts are small and not statistically significant[4]. Additionally, within zone-year-prior-eradication-rate-bin cells, distance to military headquarters does not predict which detected fields are eradicated, ruling out the concern that the army selects on operational convenience and that convenient targets happen to be the most politically disengaged. Finally, if the army targeted persistently disengaged communities, eradication in any cycle should generally predict lower turnout, not just eradication in the cycle immediately preceding the election. Instead, only contemporaneous pre-election eradication does.

Beyond selection, I assess the plausibility of three alternative mechanisms: migration, income loss, and violence, and find no support for any. Using official address-change data, I show that eradication does not trigger differential out-migration between eradicated and control precincts. I also show that the effect of eradication on turnout holds when controlling for homicide rates.

[4]Appendix Table A8

This finding is inconsistent with a violence-mediated mechanism where illegal crop destruction produces violent retaliation. Last, I leverage the 2018 poppy price collapse and examine changes in precinct-level nighttime luminosity. I show that the evidence is inconsistent with mechanically reducing turnout by making people resource-constrained. Instead, eradication produces similar short-run effects on turnout in both high- and low-price years. This pattern is consistent with eradication informing residents about federal policing priorities through an income shock on an asset that is fungible only in the mid- and long-run.

This paper makes three contributions. First, I show that externally conditioned coercive policy can undermine electoral accountability not by weakening *de jure* democratic institutions but by rendering elections futile for affected citizens. Existing research on foreign aid and accountability finds that aid is largely benign for state legitimacy when experienced as beneficial (Blair and Roessler, 2021), and that when external flows do erode accountability, they target oversight institutions rather than elections themselves (Ping, Wang and Chang, 2022; Lührmann, Marquardt and Mechkova, 2020). These findings leave open the question of what happens when foreign conditionality drives coercive domestic policies. I show that in this setting, elections lose part of their disciplining power, as citizens withdraw from an electoral channel they perceive as incapable of altering the policy that harms them.

Second, I bring the study of eradication's political consequences to Mexico and, in doing so, develop a framework for understanding how the domestic response to counternarcotics enforcement varies across settings. Recent work on Colombia has documented that eradication generates electoral backlash, increased violence, and targeted killings of community leaders (Gélvez, 2024; Abadie et al., 2014; Marín Llanes, 2022). Those findings emerge from a context where eradication was a partisan policy and where armed organizations had a direct financial stake in crop production. Mexico presents contrasting conditions on both dimensions: eradication has persisted throughout consecutive governments of three different parties, and illegal crops are marginal to trafficking organizations, so the costs fall directly on smallholder growers. I argue that these differences predict not backlash but electoral withdrawal, as the affected communities lack both a partisan vehicle and the organizational infrastructure to channel their grievances through formal political institutions.

Third, I show that law enforcement operations in Latin America can reshape political behavior in the short run and through a channel not mediated by criminal or drug-trafficking organization violence. Existing work on the consequences of law enforcement in the region has focused on effects that operate through criminal violence (Dell, 2015; Trejo and Ley, 2020) or that accumulate over years of sustained militarization (Flores-Macías, 2018; Osorio, Schubiger and Weintraub, 2021). Conversely, the direct, contemporaneous political consequences of counternarcotics operations have received little attention, in part because the data are difficult to obtain. I overcome this constraint by constructing a precinct-level treatment measure from satellite imagery and NASA fire records, independent of government self-reports, and by building the first georeferenced map of Mexico's military zones and regions from freedom-of-information requests.

Conditional Security Aid as a Multiple Principals Problem

In traditional accounts of the dynamic production of policy, governments are electorally incentivized to be responsive to their domestic constituencies. For instance, observing crime and its punishment is posited to inform citizens about security policy and implementation, shaping their law enforcement preferences and participation decisions (Ley, 2018; Kronick, 2014; Visconti, 2020; Bateson, 2012). Citizens' preferences, in turn, influence which security policies politicians propose and enact; politicians cater to relevant constituencies by tailoring security policy to their tastes (Holland, 2013; Magaloni, Franco-Vivanco and Melo, 2020; González, 2020).

Conditional US aid, however, creates a channel parallel to electoral accountability through which foreign policy preferences influence domestic policy. Since 1986, the US has explicitly conditioned security aid on drug-producing countries' supply-reduction efforts, requiring presidential certification of their cooperation and potentially withholding 50% of security assistance from noncompliant countries (Storrs, 2003). This certification creates a powerful incentive for drug-producing and drug-transit countries to invest resources into drug interdiction and crop eradication, regardless of domestic preferences.[5]

Governments in source countries must therefore tailor security policy to the preferences of two principals: their domestic voters and the US. When principals have divergent interests and differ in bargaining power, the agent crafts policy closer to the preferences of the stronger principal (Voorn, van Genugten and van Thiel, 2019; Dixit, Grossman and Helpman, 1997). When the stronger principal prevails, domestic input into the conditioned policy domain is effectively foreclosed. IMF conditionality offers a precedent: loan conditions insulated fiscal policy from electoral pressure even in functioning democracies, decoupling public spending decisions from voter preferences (Vreeland, 2003; Nooruddin and Simmons, 2006).

The Political Consequences of Crop Eradication

US-sponsored counternarcotics aid to source countries conditions assistance on the application of coercive force against the recipient governments' own citizens, regardless of domestic preferences. Yet the main finding in the literature is that eradication is largely ineffective at reducing cultivation in a sustained fashion (Farrell, 1998; Moreno Sanchez, Kraybill and Thompson, 2003; Mejía, Restrepo and Rozo, 2015). Given the contested nature of the policy and the aggressiveness of its implementation, a growing body of work has asked whether eradication generates unintended behavioral consequences among exposed populations. Gélvez (2024) shows that exposure to aerial coca eradication in Colombia reduced vote shares for pro-eradication presidential candidates, while Abadie et al. (2014) find that eradication increased guerrilla violence as armed groups fought to retain control of coca fields, and Campos-Contreras, Nieto-Matiz and Schenoni (2025) find that aerial spraying increased non-state armed violence more broadly.

However, the nature of the political response to eradication has varied across settings. Where eradication has been associated with a specific political actor, voters have directed their grievances

[5] Although the certification process became somewhat laxer in 2002, it remains a yearly hurdle for drug-producing countries, with drug interdiction activities serving as critical evidence of compliance. Presidential Determinations continue to highlight this pressure, citing Colombia's "extraordinary growth of coca cultivation" in 2018 or expressing concern that "illegal drug crops have expanded over successive years in Colombia, Mexico, and Afghanistan" in 2019. Presidential Determination on Major Drug Transit or Major Illicit Drug Producing Countries for Fiscal Year 2019 and 2020. Available at `federalregister.gov`.

at the ballot box. In Colombia, President Uribe championed aerial spraying and the left explicitly opposed it, giving voters a partisan alternative that channeled grievances into electoral backlash (Gélvez, 2024). In Bolivia, coca growers lacked a partisan champion but had decades of union organizing, coca's quasi-legal status tied to indigenous identity, and a political opening that allowed them to build the Movimiento al Socialismo into a national electoral vehicle (Anria, 2013).

Crop Eradication in Mexico: Theoretical Expectations

I explore the political consequences of eradication for the communities that grow illegal crops in Mexico. To derive theoretical expectations, I draw on ethnographic research, journalistic accounts, and elite interviews, and situate the Mexican case within the framework developed above. Mexico has been eradicating illegal crops since 1969, when Operation Condor inaugurated large-scale counternarcotics cooperation with the United States (Cedillo, 2021).[6] Counternarcotics operations quickly became central to the US-Mexico bilateral relationship, and have continued uninterrupted through the governments of three different parties since then.

Eradication policy is governed at the federal level (Delgado, 2021). Specifically, since 2007, all eradication duties have been the responsibility of the Mexican army, with the navy playing a marginal role.[7] Unlike the aerial spraying campaigns that have characterized eradication in the Andean region (Dion and Russler, 2008; Campos-Contreras, Nieto-Matiz and Schenoni, 2025), the Mexican army overwhelmingly eradicates manually: soldiers locate the field, secure the area, cut the plants, and incinerate them. In 2015 and 2018, the army destroyed 95% and 98% of all fields this way.[8]

Ethnographic research reveals how, for the remote and marginalized communities where poppy and marijuana grow, the army is the most visible face of the federal government. Le Cour Grandmaison, Morris and Smith (2019*b*) narrate how illegal crop fields were very visible and even

[6]Two illegal crops grow in Mexico: poppy and marijuana. Unlike the Andean region, coca plants do not grow in Mexico. Coca requires acidic soil, high humidity, and particular mountain elevations found in the Andean region. Mexico's predominantly alkaline soils and different topography make large-scale cultivation difficult, though isolated experimental plantings have been occasionally reported.

[7]Between 2013 and 2020, the navy was responsible for 2% of all destroyed poppy or marijuana hectares.

[8]Source: Freedom of information request folio 0000700198921.

close to the main street in town. In her ethnography of a crop-growing community in Guerrero, Álvarez Rodriguez (2021*b*) remarks:

> "The most evident expression of the presence of the state is the eradication of poppy fields carried out by the Mexican army. Indeed, people often refer to the armed forces as 'government.'"

Growers interpret the destruction of their fields as the government behaving punitively towards them, the weakest link in the drug-trafficking chain, rather than pursuing the criminals who generate violence (Le Cour Grandmaison, 2021; Álvarez Rodriguez, 2021*b*). As Álvarez Rodriguez (2021*b*) puts it:

> "What local people find unjust is that the force of the law is applied, always, on the growers, never on those who make their living by extorting them."

Eradication persists despite the fact that it does not significantly affect drug-trafficking organizations' (DTOs) profits. Growers, by and large, own their crops and bear the economic risk of cultivation. They sell the raw material to local intermediaries known as *coyotes*, who purchase the opium gum or marijuana in situ and transport it outside the growing region (Alvarez, Gaussens and Frissard, 2022). Production is artisanal, irregular, and largely limited to small plots. While DTOs may exercise monopolistic control over certain growing regions, determining which *coyotes* can operate and extracting rents from the trade, they do not own the crops themselves and do not suffer direct economic losses from eradication, the step with the least value added in the drug-trafficking chain. Further, the economic independence of DTOs from illegal crop cultivation has intensified as synthetic drugs like fentanyl have gained popularity (DEA, 2021).

If institutional trust is the belief that a representative government agent will tend to act in one's interest (Bhattacharya, Devinney and Pillutla, 1998; Hardin, 2003), eradication teaches residents of crop-growing communities the opposite. The state's only sustained presence in their communities is coercive. As one farmer summarized: "They come, they destroy, and they leave."[9] A poppy

[9] "Zozobra en la sierra de Guerrero por quema de plantíos de amapola," *La Jornada*, April 14, 2018.

farmer in Guerrero's Sierra captures the structural trap that keeps growers tied to the illegal economy despite the government's persecution: "If you plant, they [the government] persecute you; if you plant, they [the buyers] rip you off. That is always how it goes." Asked why he keeps planting: "Because if you do not plant, you starve to death. That is the law."[10]

H1: Eradication erodes trust in the federal security institutions responsible for counternarcotics enforcement.

Aggrieved crop-growing communities have no formal recourse to channel their discontent into electoral support for an opposition candidate. Eradication policy is inelastic and never on the electoral menu. Regardless of what party heads it, the federal government profits from sustained eradication that facilitates US bilateral security assistance, and the electoral losses of small, poor, remote communities are marginal in comparison.

Top-level Mexican politicians understand this multiple-agent political calculus. I conducted four elite interviews with high-ranking Mexican politicians in May 2025 to ascertain the political logic of eradication from the source country's perspective.[11] A former federal deputy explained: "They [the federal government] go after the poorest people. Neither heroin nor marijuana is a [financial] priority for organized crime anymore. It [eradication] is done to maintain good relations [with the US]." A former federal Senator described the logic as one of political inertia: "It's part of the script: we have to pay tribute to the flag, we have to play the bugle, we have to follow the regulations, well, we have to go burn crop fields." An ex-presidential Chief of Staff phrased it bluntly: "The US prevailed [over crop-growing communities]. Not meeting their [the US's] expectations could lead to 'decertification,' which would be a major geopolitical blow. The local community hardly had a voice in these foreign policy decisions."

I expect eroded trust, in a context where viable electoral channels are lacking, to depress turnout. Political participation requires both motivation and viable channels for influence (Verba, Nie and Kim, 1978; Franklin, 2004). Crop-growing communities have the motivation: they are aggrieved and distrust the federal government. But they lack the channel. No political force pro-

[10]Jan Martínez Ahrens, "La ley del opio en las montañas de Guerrero," *El País*, June 19, 2016.
[11]See Section Appendix C in the Appendix for details.

poses reforming eradication policy. An inhabitant of a crop-growing community illustrates the resulting skepticism: "Who is going to help? The government? The government never helps" (Giménez Delgado, 2022, p. 225).

H2: Eradication depresses turnout in federal deputy elections.

Federal deputy elections offer a particularly clean test of this expectation, as they are held every three years and candidates are largely unknown to voters. Turnout in these elections reflects engagement with the federal government rather than the personalistic dynamics that shape presidential races.

Data

I draw on official data from the National Electoral Authority (INE) and the National Institute of Geography and Statistics (INEGI), as well as data I obtained from the Mexican army. Municipal-level data on illegal crop eradication operations come from official statistics published by the Mexican army. Additionally, using a freedom of information request, I obtained a list of all satellite-detected illegal crop fields identified by the army between February 2013 and June 2021, which I use to measure eradication at the electoral precinct level. Last, I use several waves of a national survey to measure changes in institutional trust. I explain each data source in more detail in the following subsections.

Outcomes of Interest. To measure trust in government institutions, I use institutional trust responses from seven waves (2013–2019) of the yearly National Survey of Crime Victimization and Public Safety (ENVIPE) (INEGI, 2019), which asks respondents to rate their trust in several law enforcement institutions.

For turnout, I collected data on the four federal deputy elections held after the military took over eradication duties. I focus on federal deputy elections for both substantive and practical reasons. Substantively, unlike presidential elections, which are often shaped by personalistic dynamics, deputy elections offer repeated, comparable observations of political engagement with the federal government. This is especially so given that many voters do not know who the candidates are, making these elections more abstract reflections of broader political attitudes. On the practical side, deputy elections are held every three years, unlike other federal elections, which run on a six-year cycle, allowing me to include more electoral events in the analysis.

I rely on turnout data at the municipality and the electoral precinct levels. Electoral precincts are the basic geographic unit of Mexican elections. Each precinct has at least 100 voters, at most 3,000, and an average of about 1,200 registered voters (Challú, Seira and Simpser, 2020). In rural communities, a single precinct can often straddle the entire settlement, making that level of analysis well-suited to analyze the electoral consequences of highly localized eradication operations. Last, I exclude municipalities that select their authorities via indigenous self-governance (usos y costumbres) because their electoral processes and political structures differ substantially from the standard party-based system (Magaloni, Gosztonyi and Thompson, 2022). Given the location of illegal fields, this culling excludes only 24 precincts from the sample.

Crop eradication. For information on the share and location of destroyed crops, I rely on two data sources based on official Mexican army information. The first consists of the type, number, and size of all illegal fields manually destroyed by the army, broken down by year, month, and municipality. These data were collected, cleaned, and published by the Mexican NGO MUCD (2021) (México Unido Contra la Delincuencia). The smallest geographic unit for which the Mexican army reports crop eradication operations is the municipality, but municipalities can be large, whereas illegal crop-growing communities are often small and rural. Using a freedom of information request to overcome data limitations, I obtained a novel dataset containing the latitude and longitude of all poppy and marijuana fields detected by the army using satellite imagery between 2013 and June 2021, the two illegal crops grown in Mexico. Besides the coordinates and the date of detection, these data report the number of harvested hectares per field and whether the army validated the detected field as a true positive or a false positive.

Figure 1 shows the date and number of illegal crop fields detected via satellite per crop type. The Mexican army detected and destroyed poppy and marijuana fields during the entire period, but, as the figure shows, starting in 2015, poppy fields made up the majority of detected and

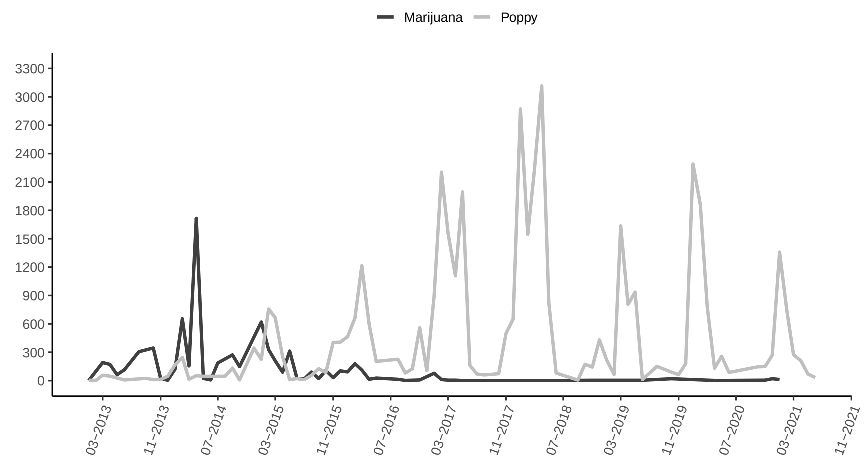

Figure 1: The figure shows the monthly count of illegal fields detected via satellite, as reported by the Mexican army, according to crop type. Data spans from February 2013 until June 2021. The count excludes fields that the army determined to be false-positives.

destroyed fields. Since the data includes latitude and longitude, I can match each satellite image to an electoral precinct and count the number of satellite-detected fields in each precinct each month.

The army does not report whether or not it later eradicated the fields it detected. Therefore, to identify eradicated fields and date their destruction, I make use of the fact that the army incinerates most of the fields it destroys. I compare the geographic data on field detection with historical satellite data on fires[12], provided by the Fire Information for Resource Management System (FIRMS) (Giglio et al., 2018). I classify fields as "eradicated" or "not eradicated" with the following algorithm:

1. Construct a 2km buffer around the coordinates of the illegal field. The buffer size accounts for measurement error in the satellite fire data, which reports the center of a 1km pixel, and for potential differences between field coordinates and the actual eradication location (for instance, soldiers may gather plants to the side rather than the center of a field).

2. Keep all high-quality fires recorded within the 2km buffer for the three months after the illegal field was detected. Three months is the most stringent specification since fields can

[12]Following Hassan and O'Mealia (2018) I use the Moderate Resolution Imaging Spectroradiometer (MODIS) data.

be harvested at most three times per year (Le Cour Grandmaison, Morris and Smith, 2019*b*), or every four months.

3. If any fires were recorded within the 2km buffer in the specified time window, mark that field as eradicated.

4. If only one fire occurred inside the 2km buffer within three months, assign the eradication date as the fire date. If multiple fires meet the criteria, assign eradication to the fire geographically closest to the original field coordinates.

Out of the 53,509 illegal fields, 17,701 were detected in 2015 or 2018, the two federal election years with overlapping satellite imagery collection. The algorithm predicts that the army destroyed 16.6% of those 17,701 fields within three months of detection, 2,757 fields within three months of the election, and 187 more within 6 to 4 months before. Further, it predicts eradication to have taken place in seven different states, 58 municipalities, and 286 unique electoral precincts; These 58 municipalities are concentrated in the two areas of most intensive illegal crop harvesting: the state of Guerrero in southwest Mexico and the so-called "Golden Triangle," formed by the states of Sinaloa, Durango, and Chihuahua in the northwest. In the next section, I discuss this measure's validity in depth and present several placebo tests to corroborate that it indeed captures eradication by incineration.

Constructing a measure of illegal field eradication using automated satellite imagery rather than ground patrols ensures that the treatment is orthogonal to bureaucratic capacity and criminal activity. However, it is important to note that the army uses additional data sources to detect illegal crop fields, such as ground patrols and intelligence from other institutions. Consequently, the universe of municipalities where the army reports eradicating illegal crop fields is larger than the universe of municipalities where it detected illegal fields via satellite. Figure 2 shows the municipalities where the army reports having eradicated illegal crops manually, as well as municipalities where it detected illegal fields via satellite during 2015 or 2018. Table A2 reports basic summary statistics. While the army reports at least one eradication operation in 433 municipalities, 91.6% of all

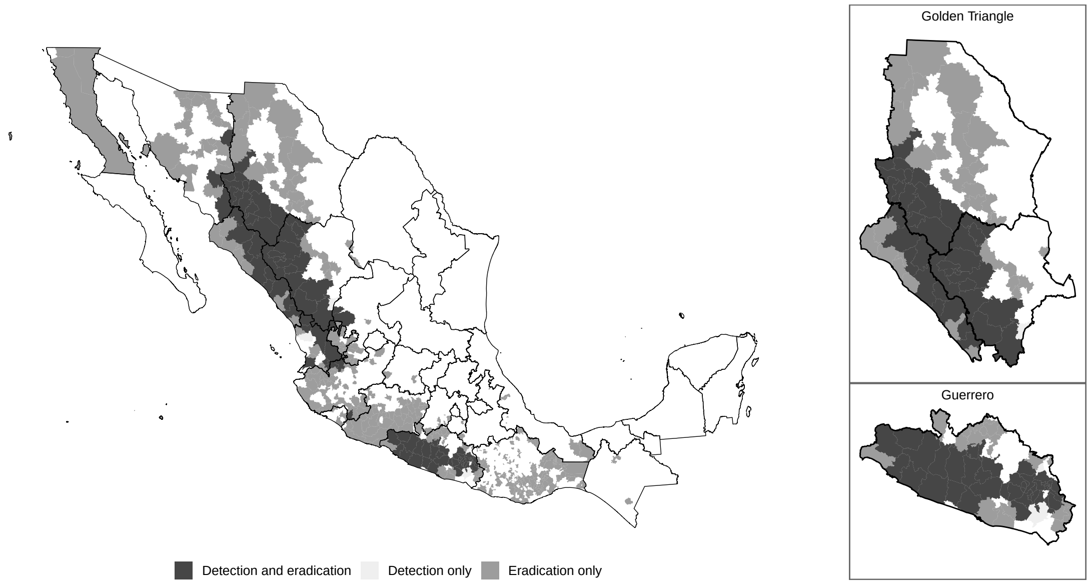

Figure 2: Map marks municipalities where the Mexican army reports having conducted eradication operations in either 2015 or 2018, as well as municipalities that had at least one positive satellite-detection of illegal crop fields. Insets zoom into the two principal crop-producing regions: the Golden Triangle (Sinaloa, Durango, Chihuahua) and Guerrero.

hectares eradicated in 2015 or 2018 were destroyed in municipalities with satellite-detected illegal fields, suggesting a large overlap between the two measures of eradication.

Empirical Strategy

The following section presents the empirical strategy for measuring the effect of eradication on institutional trust. Additionally, it explains two empirical strategies for measuring the effect of eradication on turnout.

Trust. I test the effects of illegal crop eradication on institutional trust using responses from seven waves (2013–2019) of the ENVIPE, a yearly nationally representative survey. The analytical leverage in this design stems from the temporal variation in survey collection timing relative to eradication operations within the same calendar year for a given municipality. I leverage the timing of the survey to compare self-reported attitudes toward institutions in municipalities where the government eradicated illegal fields before survey collection began with those of respondents living in municipalities where the government eradicated fields *after* the survey was collected. All seven waves of the ENVIPE were collected by the National Institute of Statistics (INEGI) between

March and April, with some municipalities surveyed in the first month and others in the second, depending on INEGI logistics.

I pooled the seven survey waves and included respondents living in a municipality where the army reported having eradicated at least one illegal field during the year the survey was collected. Since illegal crops are grown in rural and remote areas, I focus on respondents living in rural communities inside these eradicated municipalities for the main analysis, later using urban respondents (unlikely to have witnessed eradication) as a placebo. Critically, because ENVIPE fieldwork spans March and April, eradication operations occurring during these months create timing ambiguity: a respondent surveyed in March could live in a municipality where eradication occurs in April. To ensure clean temporal ordering, I exclude municipalities where eradication occurs *only* during the survey collection months, retaining only municipalities where eradication occurred clearly before the survey (January–February) or clearly after (May onward). After pooling, I have data from 20,382 respondents in rural communities across 283 municipalities observed over seven survey waves, 51 of which switch treatment status during the analysis period. I fit a series of models with the following specification:

$$Y_{it[m]} = \gamma EradicationBefore_{tm} + \beta \boldsymbol{X_{it}} + \mu_t + \theta_m + \varepsilon_{it[m]} \quad (1)$$

Where $Y_{it[m]}$ is respondent i's self-reported attitude towards a state institution in year t for a respondent living in municipality m. $EradicationBefore_{tm}$ is a dummy that takes the value of 1 if municipality m was eradicated in January or February of year t, before ENVIPE fieldwork begins, and zero if eradication occurred only after the survey was collected. μ_t are year fixed-effects, θ_m are municipality fixed-effects, and $\boldsymbol{X_{it}}$ are respondent-level sociodemographic characteristics. Robust errors are clustered at the municipality level.

The estimand is the average treatment effect on the treated (ATT): the change in institutional trust among rural respondents in municipalities eradicated before the survey, relative to what their trust would have been absent eradication exposure. Formally, $\hat{\gamma}$ in equation (1) estimates

$E[Y_i(1) - Y_i(0) \mid D_m = 1, \theta_m, \mu_t]$, where the treatment $D_m = 1$ indicates that municipality m was eradicated before survey collection. The identifying assumption is that, absent treatment, municipalities eradicated before survey collection would exhibit similar institutional trust as municipalities eradicated after, conditional on municipality and year fixed effects. This assumption is plausible because eradication decisions (made by the army) are independent of survey timing (determined by the statistics institute).

A secondary assumption necessary for identification is that there are no heterogeneous changes in sample composition between the treatment and control groups. In this design, individuals (surveyed once) are nested in municipalities. If eradication alters which types of individuals respond to the survey, estimates could be biased. Appendix Section A7.2 shows that eradication timing does not predict differences in respondent demographics, familiarity with institutions, or non-response patterns.

Lastly, while ENVIPE is nationally representative at the urban-rural level, it is not designed to be representative at the municipal level. This limits the generalizability of effect magnitudes to the national population but does not threaten identification, which requires only comparability between early- and late-eradicated municipalities within the sample. Appendix Section A7.2 assuages such concerns, showing no difference in sample composition or rates of response for treatment relative to control respondents.

Crop eradication: Electoral precinct. Two features of Mexico's eradication program structure the identification strategy: the army's geographic organization and its finite operational capacity. Together, they generate variation in which satellite-detected precincts are eradicated before a given election that is plausibly exogenous conditional on zone, year, and cumulative targeting history.

The army organizes operations around military zones, each commanded by an officer responsible for all operations within the zone, including eradication (SEDENA, 2012). Using freedom of information requests, I assigned each municipality—and by extension each electoral precinct—to its military zone.[13] Zone fixed effects, therefore, absorb the zone-level factors that govern how

[13]Figure A1 in the Appendix shows the result. Military regions encompass several zones; a region can span two

eradication is allocated across detected precincts: common resource constraints, targeting priorities, and operational infrastructure.

The second design pillar is finite capacity. The army detects far more illegal crop fields than it can destroy in any campaign period: the eradication protocol (SDN-MD-11) lists geography, weather, and logistical capacity as the factors governing which detected fields are scheduled for eradication (SEDENA, 2012).[14] Importantly, this capacity constraint does not mean the army permanently spares any community: Appendix Table A7 shows that eradication is nearly inevitable for persistently detected precincts over time. The identifying variation is therefore in the timing of eradication relative to elections, not in selection into the program.

Beyond zone and year, I constrain the comparison along an additional dimensions: each precinct's history as an eradication target. Two precincts in the same zone with detected crops in the same year can still be poor counterfactuals if their cultivation trajectories or eradication histories, conditional on cultivation, differ sharply. Repeated prior eradication reflects both persistent local cultivation and accumulated army operational experience in that precinct, both of which may predict current-period selection within a given zone-year and outcomes of interest. The prior eradication rate—the share of prior election cycles in which the precinct was both detected and eradicated—captures this history; I block on it by splitting precincts into a high-rate stratum (above 50%) and a low-rate stratum (at or below 50%).[15] The fixed effects are military zone $\times$ election year $\times$ prior-eradication-rate bin.

The sample consists of 1,037 precinct-year observations from the 2015 and 2018 federal deputy elections. All precincts in the sample had at least one satellite-detected illegal crop field during the relevant election period. Treatment is the eradication of at least one detected field before the election, measured either as a binary indicator or as the log-transformed count of destroyed fields

to five states, while a single zone can straddle municipalities in different states. A single state can contain as many as five military zones, as in Chiapas.

[14]This account was corroborated in an interview with a high-ranking armed forces officer responsible for overseeing eradication operations, conducted by the author.

[15]The 50% cutoff is arbitrary. Appendix Figure A5 shows that point estimates are negative across all cutoffs from 10% to 90%, and statistically distinguishable from zero from 30% onward, where the two bins are more evenly split, and the stratification is more informative.

or hectares.

The three balancing covariates follow from the research design. Pre-election satellite-detected fields controls for cultivation intensity: precincts with more detected crops face a higher baseline probability of eradication, and denser cultivation may independently shape community mobilization. Schooling achievement and employment share capture the socioeconomic profile of the precinct, which may correlate both with the army's targeting priorities within a zone and with baseline electoral participation. Within a given zone–year–rate-bin cell, the comparison is therefore between precincts with similar cultivation environments and similar socioeconomic characteristics.

To reduce model dependence and target the average treatment effect on the treated, I preprocess the sample using entropy balancing (Hainmueller, 2012). Entropy balancing reweights control precincts to exactly match the first moments of treated precincts on these three characteristics; because control units are reweighted to mirror the covariate distribution of treated units, the estimator recovers the ATT rather than a weighted average that depends on the covariate distribution of controls. Appendix Table A8 reports standardized mean differences before and after balancing. I fit the following weighted fixed-effects regression:

$$Y_{pt} = \gamma D_{pt} + \beta \boldsymbol{X}_p + \mu_{z(p)\times t\times h(p,t)} + \varepsilon_{pt} \tag{2}$$

where Y_{pt} is the turnout rate of electoral precinct p during election year t. D_{pt} is the treatment: either a dummy for any pre-election predicted eradication or the $(log + 1)$ count of fields or hectares predicted eradicated. $\boldsymbol{X}_p$ are controls (pre-election satellite-detected fields, schooling, and employment share), included as linear covariates in one specification and replaced by entropy balancing weights on the same covariates in the preferred specification. $\mu_{z(p)\times t\times h(p,t)}$ are zone $\times$ year $\times$ prior-eradication-rate-bin fixed effects, where $h(p, t)$ indicates whether precinct p's prior eradication rate is above or below 50%. Robust standard errors are clustered at the electoral precinct level.

The estimand is the average treatment effect on the treated (ATT) among precincts with satellite-detected illegal crop fields:

$$\tau_{ATT} = E\big[Y_p(1) - Y_p(0) \;\big|\; D_p = 1,\; \boldsymbol{X}_p,\; \mu_{z(p)\times t\times h(p,t)}\big]\,, \tag{3}$$

the effect of election-year eradication on turnout for precincts where eradication occurred, conditional on being in the same zone-year, having similar prior targeting histories, and conditional on the observed covariates. The population of inference is precincts already participating in the illegal crop economy. The identifying assumption is conditional ignorability within zone-year-prior-eradication-rate cells:

$$\{Y_p(1), Y_p(0)\} \perp\!\!\!\perp D_p \;\mid\; \boldsymbol{X}_p,\; \mu_{z(p)\times t\times h(p,t)}. \tag{4}$$

Two pieces of evidence support the plausibility of conditional ignorability within cells. The geographic selection test in Table A11 shows that distance to army headquarters only weakly predicts eradication within zone-year cells and is indistinguishable from zero within the full zone-year-prior-eradication-rate-bin cells. More broadly, the full set of geographic covariates—altitude, road access, and land cover—explains very little of the within-cell variation in which fields are eradicated ($R^2 = 0.075$), indicating that the three-way fixed-effects structure absorbs the geographic component of targeting. Table A8 in the Appendix confirms that entropy balancing achieves demographic comparability between treated and control precincts within strata. I assess additional threats to identification in the Alternative Explanations section.

Crop eradication: Municipality. My second empirical strategy uses official municipal-level data on eradication, published monthly by the Mexican army. This design relies on different assumptions than the precinct-level analysis: whereas the precinct-level design relies on conditional exogeneity within zone-year-prior-eradication-rate cells, the municipal design exploits within-municipality variation over time in a four-election panel (2009, 2012, 2015, and 2018). My sample includes all municipalities where the army eradicated fields in a given year. For comparability with the precinct-level results, I exclude municipalities that select their authorities through Indigenous self-governance from the sample. The final sample includes 1,664 municipality-year observations from 597 municipalities, covering 96% of the hectares destroyed by the army. Of these, 1,253 municipality-year observations had eradication either before or after the election in a given year; this is the estimation sample for the TWFE specification.[16]

The variation I leverage for identification comes from the timing of field eradication. I compare municipalities with fields manually eradicated *before* the election to turnout in municipalities with fields manually eradicated *after* the election, within the same year. The central identifying assumption is that, absent treatment, municipalities eradicated before the election would have experienced the same trends in electoral turnout as those eradicated after the election. I estimate the following two-way fixed-effects model:

$$Y_{mt} = \gamma EradicationBefore_{mt} + \mu_t + \theta_m + \varepsilon_{mt} \tag{5}$$

where Y_{mt} is the turnout rate in municipality m during election year t and $EradicationBefore_{mt}$ is a dummy variable that takes the value of 1 if the army eradicated illegal fields manually in year t and municipality m during the months before the federal election and 0 if it eradicated fields only after the elections, or the (log+1) count of fields or hectares eradicated manually in the same period. μ_t are year fixed-effects, and θ_m are municipality fixed-effects. Robust standard errors are clustered at the municipality.

The estimand is the average treatment effect on the treated (ATT) among municipalities where the army eradicated in a given year: $\hat{\gamma}$ in equation (5) estimates $E[Y_m(1) - Y_m(0) \mid D_{mt} = 1, \theta_m, \mu_t]$, the effect of pre-election eradication on turnout for municipalities that experienced it, relative to what their turnout would have been had eradication occurred after the election.

The identifying assumption is parallel trends: absent pre-election eradication, treated municipalities would have followed the same turnout trajectory as municipalities eradicated after the

[16] The municipal sample is larger than the precinct-level sample (1,037 observations) for two reasons. First, the municipal analysis uses official army eradication reports covering all eradication operations across all four election cycles (2009–2018), while the precinct analysis is restricted to precincts with satellite-detected fields in the two election years (2015, 2018) with overlapping satellite imagery. Second, municipalities appear in multiple election cycles in the panel, while each precinct-year is a single observation.

election. Because eradication is a recurring treatment that varies within municipalities across election cycles rather than a one-time shock, standard pre-trends tests are inapplicable. Instead, I test the timing structure directly by estimating variants of equation (5) that replace current eradication with lagged eradication from the prior election cycle or with post-election eradication. If confounding rather than eradication drives the result, these placebo treatments should also predict turnout. I report the results in columns 2–4 of Table 2.

Results

Trust. First, I examine how eradication affects people's self-reported levels of trust in law enforcement and justice institutions, using data from the ENVIPE survey. The results for rural respondents, shown in dark gray in Figure 3, indicate that eradication in rural areas reduced people's trust in federal security institutions. Reported trust in the army is 0.08 standard deviations lower when respondents lived in a municipality eradicated before survey collection. Trust in the navy (-0.12 SD) and federal police (-0.13 SD) are also significantly lower. These effects are consistent with the theory, which predicts that eradication informs communities about the priorities of the federal government. All three institutions are perceived as expressions of the federal state apparatus; the absence of effects on non-federal and non-policing institutions, where trust is unchanged, is consistent with communities updating specifically about institutions they identify with federal authority. For non-federal or non-policing agencies, there is no difference in trust.

Next, I use respondents from *urban* localities as a placebo. Since illegal crops are grown in remote areas, urban respondents are less likely to witness eradication. I test for differences in trust with this different sample and report the results in light gray in Figure 3. Reassuringly, the estimates for trust in the army and other federal security agencies are precisely zero or very close to zero among urban respondents, despite the sample being more than twice as large. The contrast between rural and urban patterns supports the interpretation that eradication, rather than a broader municipal trend, drives the observed decline in trust.

While survey evidence indicates that people exposed to eradication then trust the armed forces

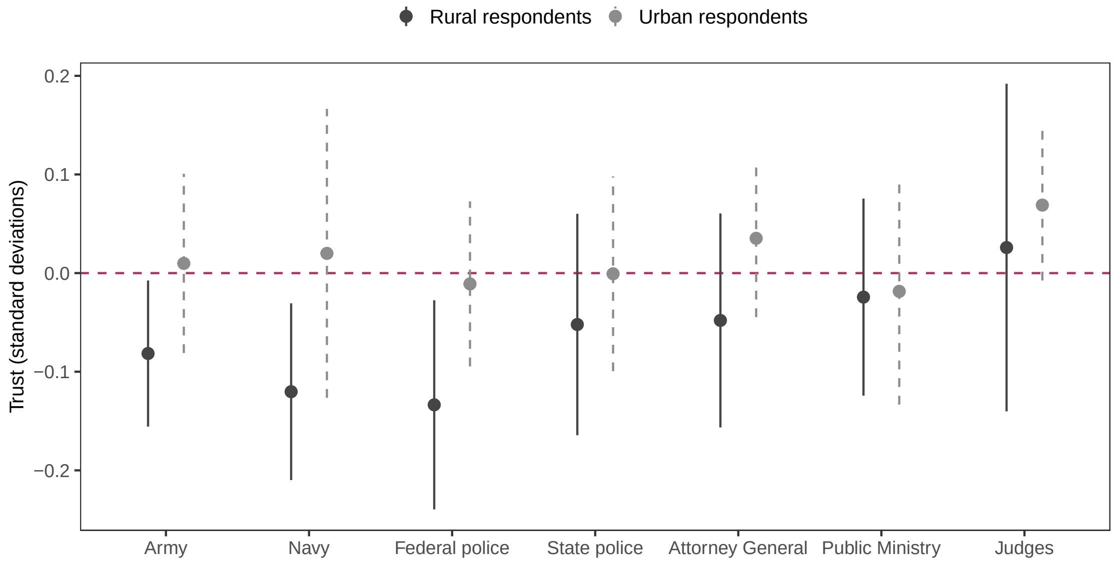

Figure 3: 95% confidence intervals of the difference in self-reported trust ($EradicationBefore_{t,m}$ in specification 3) for respondents in municipalities eradicated before vs. after survey collection. The sample excludes municipalities where eradication occurs only during survey months to ensure clean temporal ordering. All specifications include year- and municipality-fixed effects and controls for respondent-level characteristics (age, sex, and educational attainment). Robust errors are clustered at the municipality level.

and the federal police less, one possible threat to identification is that people in communities eradicated earlier in the year are generally less trusting and thus participate less in elections. I test this possibility by comparing their responses to self-reported trust in family and neighbors, and report the results in Figure A9. I find no significant difference in these measures, and the point estimates are very close to zero. Additionally, I consider whether changes in the sample composition or differential missingness due to eradication, rather than changes in respondents' trust, could explain the decrease in trust, and report the findings in Section A7.2 of the Appendix. Eradication before survey collection does not predict either increased missingness or changes in the demographic characteristics of rural survey respondents. Results suggest that the observed reduction in trust after eradication is not an artifact of differences in sample composition or selective non-response.

Crop eradication: Electoral precinct. Table 1 presents the precinct-level results from specification (2), contrasting precincts where the army detected but did not eradicate illegal fields against precincts where it both detected and eradicated, within the same zone-year-prior-eradication-rate cell. The table reports three specifications in order of increasing covariate adjustment: no controls

(Row 1), three linear controls for pre-election satellite-detected fields, schooling, and employment share (Row 2), and entropy balancing on the same three covariates (Row 3, preferred). All three rows include zone $\times$ year $\times$ prior-eradication-rate-bin fixed effects.

Without covariate adjustment, all three treatment measures are negative and significant. Any eradication reduces turnout by 2.1 percentage points; log eradicated hectares and log fields reduce turnout by 1.06 and 1.32 percentage points, respectively. Adding three linear controls (Row 2) modestly attenuates the estimates, with all three measures remaining negative and marginally significant. This pattern is expected: covariate adjustment removes variation in turnout attributable to observable differences rather than eradication, so some attenuation and loss of precision are the anticipated directions of change.

In the entropy-balanced specification (preferred), all three treatment measures are negative and statistically significant at the 5% level. Any eradication reduces turnout by 2.4 percentage points; a one-unit increase in log eradicated hectares reduces turnout by 1.13 percentage points; and a one-unit increase in log destroyed fields reduces turnout by 1.37 percentage points. To contextualize the magnitude: the median eradicated area among treated precincts is approximately 6 hectares; at this value, the estimated effect is $\ln(6+1) \times -1.13 \approx -2.2$ percentage points, approximately 13% of a standard deviation. At the median number of destroyed fields among treated precincts (3), the field-based estimate implies $\ln(3+1) \times -1.37 \approx -1.9$ percentage points, approximately 12% of a standard deviation.[17]

A complementary check asks whether eradication redirects rather than suppresses participation. If communities blame a specific party and vote strategically against it, PRI's vote shares would shift without a decline in turnout. Appendix Table A4 reports the effect of eradication on PRI vote share. The EB-weighted estimates are small, statistically imprecise, and inconsistent in sign across the three treatment measures. There is no evidence that eradication redirects votes toward or away from any party, supporting the interpretation that communities withdraw from electoral participation rather than channeling grievances through vote choice.

[17] Table A5 in the Appendix presents results using untransformed counts. The findings remain consistent.

	Turnout		
	Dummy (1)	Eradicated ha (log) (2)	Fields (log) (3)
Row 1: No controls			
	-2.102*	-1.056**	-1.318**
	(0.964)	(0.404)	(0.510)
	$[p = 0.030]$	$[p = 0.009]$	$[p = 0.010]$
Row 2: + Controls			
	-1.676+	-0.922+	-1.102+
	(0.991)	(0.472)	(0.599)
	$[p = 0.091]$	$[p = 0.051]$	$[p = 0.066]$
Row 3: Entropy balanced			
	-2.425*	-1.129*	-1.368*
	(1.214)	(0.476)	(0.601)
	$[p = 0.046]$	$[p = 0.018]$	$[p = 0.023]$
FE: Zone × Year × Prior-erad-rate bin	Yes	Yes	Yes
Num.Obs.	1037	1037	1037

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

Table 1: 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 electoral precinct. Row 1 includes only the fixed effects. Row 2 adds three linear controls: pre-election satellite-detected fields, schooling, and employment share. Row 3 uses entropy balancing (Hainmueller, 2012) on the same three covariates to reweight control units (preferred). Robust standard errors clustered at the electoral precinct level.

Measurement concerns

One concern when interpreting the results in Table 1 is that the effects capture something unrelated to eradication, since treatment is predicted rather than observed. I conduct several ancillary analyses to validate the quality of the measure.

First, I use the 8.1% of the satellite detection data observations that the army labeled as false positives. I classify these "false" fields as eradicated using the same algorithm described in the previous section and estimate the effect of their "destruction" on turnout. One way to conceptualize the effect of "false field eradication" on turnout is as the effect of wildfires or controlled burns in places *without* illegal fields. Table A6 reports the results of this placebo test. Suppose predicted eradication in true illegal fields systematically captures false positives. In that case, the effects reported in Table 1 should be similar in direction and magnitude to what we observe in columns 1-2 of Table A6. However, in the entropy-balanced specification, the estimated effect of eradicating any "false" field on turnout is -2.14 (SE $= 2.19$), but the estimated effect of destroying an additional log field is *positive* 0.54 (SE $= 1.78$). Neither coefficient is distinguishable from zero ($N = 284$). Results lend credibility to the fire-based algorithm. Importantly, however, both false positives and false negatives in the treatment measure would attenuate the estimated coefficients. False positives dilute the treatment group with non-eradicated units, and false negatives contaminate the control group with eradicated ones. The reported estimates are therefore likely conservative.

Second, I contrast the algorithm's results, aggregated at the municipality level, with the official municipal-level data published by the army. I then check municipalities where the algorithm predicts eradication during months when the army reports no cases. Predicted destroyed fields in municipalities during months when no official eradication occurred accounted for only 12.2% and 11.8% of all predicted eradicated fields in 2015 and 2018. This low percentage of false positives does not account for incorrectly dated true positives: fields destroyed in these municipalities, but in the prior or subsequent month from the one predicted by the algorithm.

Last, I benchmark the satellite measure against official geolocated army eradication data for 2019 and 2020. Comparing the fire-based predictions to army-reported destruction, 9.45% of control units were possibly misclassified as treated, and 22.8% of treated units were possibly misclassified as control. The latter figure is almost certainly an overestimate: the army eradicates fields detected by satellite, ground, and aerial surveillance, so fields that the algorithm does not match to a fire may have been found through other detection methods rather than missed. In either case, both types of misclassification bias the estimated effect toward zero, so the reported estimates remain conservative. Appendix Figure A6 shows how the distribution of the unadjusted coefficient shifts as misclassification rates vary; more than 40% misclassification in either direction is required before the estimated effect approaches zero.

Crop eradication: Municipality. Next, I present the results of specification (5). All municipal-level analyses use official monthly data on crop eradication reported by the army, along with a four-election panel (2009–2018). This design relies on within-municipality variation over time rather than within-zone-year cross-sectional variation, and it uses administrative eradication records rather than the satellite-based measure. If both designs converge, the result is unlikely to be an artifact of either identification strategy alone.

Table 2 reports the effects of current pre-election eradication on turnout in column 1, with three treatment measures as rows: log hectares, log fields, and any eradication. A one-unit increase in log hectares decreases turnout by 0.98 percentage points; the log fields estimate implies a 0.49 percentage-point reduction. The continuous measures are estimated more precisely than the binary indicator, as expected from the added variation. To contextualize, going from no eradication to the median among eradicated municipality-years (2.5 hectares, or 18 fields) reduces turnout by approximately $[ln(2.5+1) \times -0.98 \approx]$ 1.2 percentage points using the hectares measure, or $[ln(18+1) \times -0.49 \approx]$ 1.4 percentage points using the fields measure.

Year- and municipal-fixed effects control for time-invariant unit-specific confounders and year-specific shocks common to all municipalities. The timing tests in columns 2–4 provide additional evidence against confounding: if an unobserved time-varying confounder drove the results, we would expect lagged eradication to also predict turnout, which it does not.[18] The sharp contemporaneity of the effect is difficult to reconcile with the systematic selection of eradicated places that obeys baseline levels or differential trends in marginalization. Instead, this pattern is consistent with a causal effect that operates through the experience of eradication in the run-up to an election.

For the continuous treatment specifications to recover an average causal effect, treatment effects must be reasonably constant across dosages (Callaway, Goodman-Bacon and Sant'Anna, 2024). I fit the same models with a flexible ten-knot cubic regression spline and plot the results in Figure A3. The effects are plausibly constant across dosages for log hectares but heterogeneous

[18] The sample size drops from 1,253 to 657 in columns 2 and 4 because constructing lagged eradication requires a prior election cycle. Municipality-years from 2009, the first period in the panel, have no prior cycle to lag from and are therefore excluded from these specifications.

	Current (1)	Lagged (2)	Post-election (3)	Current + Lagged (4)
Hectares (log)				
Eradication	−0.981** (0.344) [$p = 0.004$]	0.179 (0.389) [$p = 0.646$]	−0.056 (0.348) [$p = 0.873$]	−0.815+ (0.458) [$p = 0.076$]
Lagged				−0.024 (0.403) [$p = 0.952$]
Fields (log)				
Eradication	−0.494* (0.233) [$p = 0.034$]	0.158 (0.305) [$p = 0.605$]	0.072 (0.231) [$p = 0.754$]	−0.564+ (0.318) [$p = 0.077$]
Lagged				−0.005 (0.307) [$p = 0.988$]
Any eradication (dummy)				
Eradication	−1.763+ (0.927) [$p = 0.058$]			
FE: Municipality, Year	Yes	Yes	Yes	Yes
N	1,253	657	1,253	657

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. Cluster-robust SEs at municipality level.

Table 2: Illegal crop eradication and turnout: municipal-level TWFE. The dependent variable is turnout as a share of registered voters. "Current" is pre-election eradication; "Lagged" is eradication from the prior election cycle; "Post-election" is eradication after the election; column 4 includes current and lagged jointly. N drops from 1,253 to 657 in columns 2 and 4 because the first panel year (2009) has no prior cycle.

for log fields, consistent with the precinct-level findings where the fields measure is more precisely estimated.

Alternative Explanations

Selection. The precinct- and municipal-level results both show that eradication reduces turnout. A central concern is the possibility that the army systematically targets the most marginalized communities within zone-year-prior-eradication-rate-bin cells, and that these communities are also the places with the lowest political participation. If this were the case, the negative turnout results would speak to who gets targeted rather than how targeting changes electoral behavior. I assess this possibility in detail and summarize four pieces of evidence that are inconsistent with it.

First, if the army targeted already-disengaged communities, electoral participation in eradicated precincts should have been lower than in neighboring crop-growing precincts even before eradication began. To test this, I compare treated and control precincts on political characteristics from elections held before the satellite detection program started and not included in the entropy balancing. Prior turnout in 2012 and 2009 shows small, statistically insignificant differences between groups, indicating that eradicated precincts were not more politically disengaged before the program (Appendix Table A8). Prior PRI vote share in 2012 shows no meaningful difference, indicating no distinct partisan profile either.

Second, the municipal-level timing tests reported in Table 2 provide a different cut at the same concern. If the selection story is correct, the communities the army targets should be persistently disadvantaged, not temporarily so. Persistent disadvantage would show up as a correlation between eradication in any period and lower turnout. But lagged eradication from the prior election cycle does not predict current turnout: the lagged coefficient is small and statistically indistinguishable from zero for both hectares and fields. Only contemporaneous, pre-election eradication predicts turnout. The effect is sharply tied to the timing of destruction, not to the characteristics of the places where destruction occurs.

Third, I assess whether soldiers systematically choose to eradicate more accessible fields. The army's own *Protocolo Nacional de Actuación para la Destrucción de Plantíos Ilícitos* states that operational teams create action plans "taking into account the geography of the location, meteorological and demographic conditions" when planning eradication operations. Table A11 in the Appendix reports field-level linear probability models regressing an indicator for eradication on distance to military zone headquarters, with progressively more demanding fixed effects. Distance to headquarters predicts eradication unconditionally, as expected, and within zone $\times$ year cells. Within the full zone $\times$ year $\times$ prior-eradication-rate-bin cells used for identification, however, the coefficient attenuates by 19% to -0.026 and is no longer statistically distinguishable from zero. Geographic proximity does not predict which detected fields are eradicated within the cells that define the causal comparison in the design.

Fourth, the treatment coefficients are stable across the three specifications in Table 1: no controls, linear census controls, and entropy balancing. This pattern suggests that the main result is not highly sensitive to adjustment for the observable precinct characteristics addressed by these specifications, and is therefore unlikely to be driven primarily by those imbalances alone.

Income. Thus far, results show that eradication decreases turnout and trust in the army. While the hypothesized mechanism hinges on changes in citizens' beliefs and electoral incentives, a reasonable concern is that income losses could mechanically depress participation, as resource-constrained individuals might be forced to abstain from voting.

Research on income and voting in Latin America generally finds a null or weak association (Carreras and Castañeda-Angarita, 2014). Eradication is nonetheless a negative resource shock for crop-growing communities, where the illegal economy is central to local livelihoods.[19] The income effect of eradication is unlikely to operate in the short run, however, because raw opium gum and marijuana are not immediately fungible: growers must wait for intermediaries to purchase the harvest, and the income loss materializes over subsequent months rather than at the moment of destruction. Still, the channel is important to assess.

To test, I first leverage the 2018 collapse of the price of poppy due to the increased demand

[19] For instance, Le Cour Grandmaison, Morris and Smith (2019*b*) estimate that 75% of individuals in a crop-growing town in Nayarit and 95% in a crop-growing town in Guerrero profited directly from illegal harvesting.

for fentanyl. While poppy was selling for record prices between 2014 and 2017, its price fell by around 50% in 2018 (Le Cour Grandmaison, Morris and Smith, 2019*a*; Sáez, 2024). Thus, the lost income from an eradicated field in 2015 was significantly higher than that from one eradicated just three years later. Section A6.1 in the Appendix provides a detailed description of the analysis. The estimated effects of eradication on turnout are comparable in magnitude in both 2015 and 2018. Further, contrary to what we would expect if the loss of income drove the effects, the point estimates for 2015 are less negative than those for 2018 in both cases, suggesting that the negative economic shock of eradication cannot explain the results, at least in isolation.

To probe the income channel further, I leverage precinct-level luminosity data (Magar, 2021), a measure often used as a proxy for economic activity (Henderson, Storeygard and Weil, 2012). I report the results in Section A6.1 in the Appendix. Results show no significant differences in luminosity between eradicated and non-eradicated precincts two years before or after treatment. While a coarse measure, this null effect is consistent with the ethnographic understanding of crop-growing economies as processes undertaken in remote and marginalized locations that require time and intermediaries to convert into tangible wealth. These findings further suggest income constraints are not the main channel through which eradication affects turnout in the short run.

Compositional changes. I consider the possibility that population changes could explain the observed effects. I first consider whether arrests for eradication could mechanically decrease turnout by reducing the population. Using a freedom of information request, I obtained data on the number of people arrested for illegal crop farming each year. Although growing illegal crops is a felony, the data show that the penalty is not routinely enforced. Specifically, while the mean yearly number of sentences for the crime of illegal crop harvesting between 2007 and 2020 was only 59, according to the Attorney General's office, the mean number of yearly eradicated fields in the period was 188,691.

Second, one could worry that eradication operations force people to leave their communities and find work elsewhere. Ethnographic work has documented migration away from crop-growing communities. However, the phenomenon is linked to changes in crop-harvesting profitability, not government activity. In fact, historians have pointed to the profitability of crop harvesting as a tool that has allowed communities to *resist* pressures to emigrate to cities (Le Cour Grandmaison, Morris and Smith, 2019*a*). Additionally, the control group should account for any changes in migratory pressures common to crop-growing communities.

However, given extant work on displacement and coca fumigation in Colombia (Dion and Russler, 2008), this channel is essential to examine. To do so, I use data on voter address changes between electoral precincts. While this measure will fail to pick up individuals who do not keep their address up to date with the electoral authority, in Mexico, more than 97% of those eligible have a valid voting ID card (Finan, Seira and Simpser, 2021). I find that 3.2 and 5.8 people per precinct moved from eradicated areas to non-eradicated ones in 2015 and 2018, respectively. Conversely, during those same periods, 3.3 and 5.9 people moved in the opposite direction, from non-eradicated to eradicated precincts. The resulting net difference is not only small but slightly positive, offering no evidence of population displacement as a mechanism.

Violence. Lastly, I consider whether eradication operations might affect participation by altering criminal violence, possibly disrupting DTOs' revenue streams, fostering competition between criminal organizations, or incentivizing them to resort to extortion to compensate for financial losses. This mechanism is improbable for several reasons: growers rather than cartels typically own the crops and absorb the economic costs (Álvarez Rodriguez, 2021*a*; Farfán-Mendez, 2021); cartels have diversified toward synthetic drugs like fentanyl (DEA, 2021), which remain unaffected by eradication; cultivation represents the lowest value-added segment in the trafficking chain, making it unlikely to trigger violent reorganization; and insights from my interviews with high-level Mexican politicians (Appendix C) confirm that DTOs have largely moved away from crop-based revenues.

However, I assess further empirical evidence to determine whether cartel violence could plausibly mediate the relationship between eradication and turnout. Ideally, I would test this mechanism by directly controlling for cartel violence. While measures of municipality-level cartel presence exist based on news reporting (Esberg, 2025), no such measures for cartel violence exist, nor could they likely be constructed without non-random measurement error due to heterogeneous reporting incentives and capacity across geographies and time periods. Instead, I focus on official homicide data, as homicides are the best-measured crime and least susceptible to underreporting in low-trust contexts (World Health Organization, 2014). I conduct the analysis at the municipal level, the smallest geographic unit for which both lethal violence and turnout are consistently reported. I use both overall homicides and firearm homicides, which are commonly more sensitive to DTO violence (Vela Barba and Atuesta, 2022).

I replicate the specification from equation 5, adding lagged municipal homicide rates as controls. 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 to depress turnout, we would expect violence itself to have a negative association with turnout.

Table A12 in the Appendix shows the results. It demonstrates that violence is unlikely to mediate this relationship. Across all specifications, eradication effects remain substantively unchanged when controlling for lagged municipal homicides, with coefficients nearly identical to those in Table 2. Both firearm homicides and total homicides show small but statistically significant *positive* associations with turnout, the opposite of what we would expect to see if violence, not eradication, depressed turnout. The empirical evidence provides little support for this alternative explanation.

Overall, any remaining confounding explanation would need to be consistent with the full pattern of evidence: null differences in pre-treatment turnout and PRI vote share, no lagged effect of eradication on turnout, comparable effects across income-varying contexts, symmetric voter address flows between eradicated and non-eradicated precincts, and eradication coefficients that are stable when controlling for municipal homicide rates. It would also need to vary across precincts within zone-year-prior-eradication-rate-bin cells and not be well captured by pre-election-detected cultivation, schooling, or employment shares. The conjunction of those requirements substantially narrows the set of plausible alternatives.

Discussion

Illegal crop eradication in Mexico erodes trust in the federal security apparatus and depresses turnout in federal elections instead of engendering electoral backlash. The reason is straightforward: no political force in Mexico has an incentive to reform a policy that facilitates the disbursment of profitable US aid to protect remote, politically marginal crop-growing communities. Elite interviews confirm that politicians understand this calculus. Ethnographic work, too, reveals that the communities subjected to eradication understand it. In the words of one elderly grower, "the government treats us like third-class citizens" (Álvarez Rodriguez, 2021*b*).

This configuration of policy inelasticity and unintended political consequences is not unique to Mexico. Eradication operations across the region generate domestic political friction wherever aid requires governments to coerce their own citizens. Existing work on Colombia has documented electoral backlash and increased violence in a context where eradication was partisan and armed groups profited directly from coca (Gélvez, 2024; Abadie et al., 2014). In Bolivia, organizational infrastructure enabled growers to channel grievances into a national political movement (Anria, 2013).

The theoretical scaffolding developed in this paper helps organize these divergent outcomes. By studying Mexico, the second-largest recipient of US counternarcotics assistance after Colombia, I identify two scope conditions that the existing literature has left implicit that shape reactions to illegal crop eradication: who absorbs the economic cost of eradication, and whether the political landscape offers affected populations a channel to contest the policy. Where partisan alternatives to the policy exist, the response is electoral backlash, like in Colombia. Where organizational infrastructure exists, like the indigenous cocaleros in Bolivia, the response is mobilization. Conversely, when smallholders bear the costs, and no formal political alternative exists, the response is withdrawal from electoral politics, as in Mexico.

Crop-growing communities in Mexico are among the most marginalized populations in the country. My results show that eradication pushes these already peripheral communities living on the margins of legality further from the state. However, the evidence in this paper does not suggest that affected citizens become apathetic. They withdraw from elections as if they have concluded that voting cannot change the policy that harms them. If electoral participation loses its instrumental purpose, non-electoral political action becomes a natural alternative. Whether sustained policy inelasticity pushes affected communities from electoral disengagement toward contentious collective action or para-state governance is a first-order question for future research.

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